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UNIVERSITY OF CALIFORNIA, SAN DIEGO

Agency and Structure in the History of DNA Profiling:

The Stabilization and Standardization of a New Technology

A dissertation submitted in partial satisfaction of the

requirements for the degree of Doctor of Philosophy

in

Sociology (Science Studies)

by

Linda Anne Derksen

Committee in charge:

Professor Steven Epstein (Chair)
Professor Martha Lampland
Professor Akos Rona Tas
Professor Lisa Catanzarite
Professor Robert Westman
Professor Gerald Doppelt

2003

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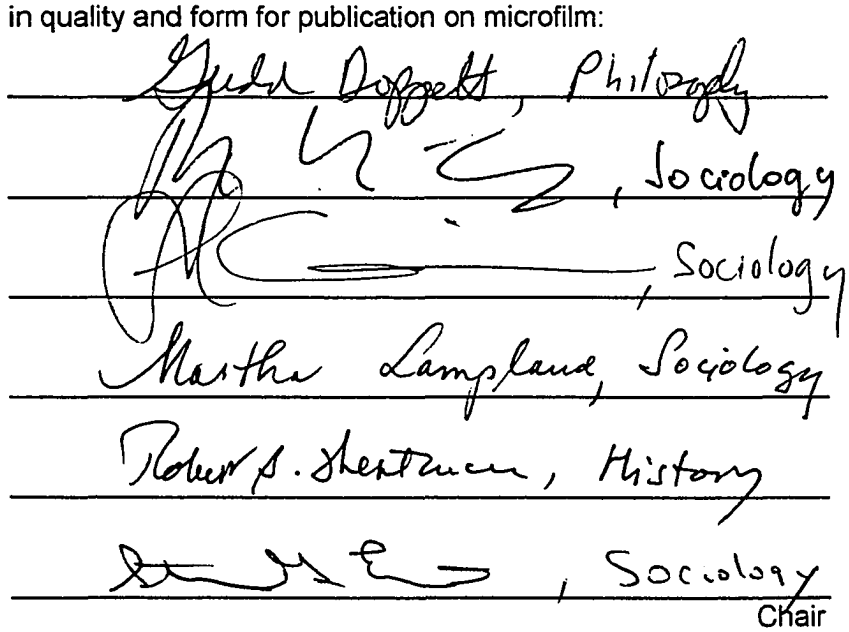

University of California, San Diego

2002

DEDICATION

This dissertation is dedicated to my grandfather, Oliver George Rees (January 9, 1911 – December 14, 1997). Moving to California to complete my doctoral studies cost us both the joy of sharing the last seven years of his life. Although my grandfather did not have the luxury of pursuing formal education, he loved learning and knowledge, and read avidly and educated himself throughout his lifetime. We miss him more than words can say, and continue to feel his presence through his absence.

EPIGRAPH

The Bible tells us that it was the first murder in human history.

Cain was a farmer, Abel a shepherd. Each offered a sacrifice to God. From Cain came produce, from Abel, sheep. The Lord preferred Abel's offering of meat to Cain's offering of produce, and Cain, in a jealous rage, killed his brother.

"Where is your brother Abel?" asked the Lord.

"I do not know," replied Cain. "Am I my brother's keeper?"

"What have you done?" asked the Lord. But then, in an instant, the Lord knew what Cain had done.

"Hark," said the Lord, "your brother's blood cries out to me from the ground!"

That cry, the cry of Abel's blood, told God that Cain had murdered his brother. The Lord immediately sentenced Cain to be a ceaseless wanderer upon the earth, and Cain soon left for the land of Nod, east of Eden.

Thousands of years would go by before blood would cry out again and positively identify a murderer.

From Harlan Levy, *And the Blood Cried Out*, (1996, 20)

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Acknowledgements

If the crop had been good one year a long time ago, my mother, the daughter of a farmer, would have had a different life. She could have pursued her dreams of higher education. Instead, she left school and went to work to support her family, and has given us everything, ever since. Her sacrifices, of all kinds, have enabled me to attain this degree, and it belongs as much to her as it does to me. She has felt deeply every moment of pain and joy. She has believed in me always, especially when I lost the ability to believe in myself. I hope you know Mother, how much I love you and how grateful I am for your support, over a very long haul!

Nobody completes a PhD, or a dissertation, by themselves. I am indebted to more people than I can name in this acknowledgement. While I lived in San Diego, and when I returned home, my brother Bruce selflessly, generously and continuously showed his support in many ways, and in practical ways. My brother gave me more money to pay rent, to move, to keep my car running, to pay for coaches – he was selfless and generous. Without Bruce's assistance, there would be no Ph.D. – and I hope he knows how grateful I am for his help, and how delighted I am with the life he has built for himself. My father, Philip Derksen, has also supported me in many ways throughout the years. He and his wife Margaret surprised me once with a brand new Pentium computer, which *almost* made it to the end of the dissertation, and without which I would have been lost. To this day my father is the only non-academic person who has read *everything* I have written! That is moral support indeed. My grandparents, George and Mary Rees, helped me too, with love and support and money, and always provided a safe, loving haven to which to escape. My grandmother is my "safe person," and now I look forward to spending more time with her.

Intellectually, I am indebted to many people. My professors at the University of Alberta, who still cheer for me and write seemingly endless letters of reference. John Gartrell, Les Hayduk, Mike Gillespie and Detmar Tschofen. Their scholarship was rigorous and sometimes scary, and has stood me in good stead over many years. John Gartrell, my M.A. supervisor, has continued to support me, to give me guidance and feedback and sage advice as I charted the often rocky waters of this degree. He has encouraged me to never give up, to keep moving forward, and assured me I would get there! I studied for a year with Randall Collins at the University of California, Riverside, and he has continued to support my scholarship over the years. I learned a lot in that year, and I am grateful for the superb foundations in social theory he provided.

I am more than grateful to my supervisor, Steven Epstein. Steve took a chance by agreeing to be my supervisor, and he has been very patient waiting for all these bits to become a dissertation. He has encouraged me when I was lost, and throughout our many conversations he has shaped and formed my thinking in ways that I cannot articulate. Steve is a wonderful reader and editor, and he has a talent for finding that one jewel of an insight in a jumble of scattered words. He has made the time in a very busy schedule to accommodate and guide this very long project, and his rigorous standards have made this a much better dissertation.

When Martha Lampland agreed to join my committee, the energy level went up a few hundred watts! Martha has been unflaggingly positive, supportive, encouraging, and has often reminded me that this is my *first* academic endeavour, not my last. She is a wonderful role model, and continues to reconstitute me when I have lost the faith, and become melted Jello on the floor. Martha has great gifts of

perspective and insight, and I am grateful to her for many things. If I end up in the academy, I hope to bring to it the humanity that she does.

We rarely get to know the *huge* effects that small kindnesses or moments can have on other people's lives. Without Bob Westman, there would again be no dissertation. I once had a paper on which I had been working for several years. I was stuck, paralyzed, unable to write, and filled with fear, and I needed to finish it to proceed. One day Bob, whose time is more precious than *real* balsamic vinegar from Modena, made the time to take me to the Faculty Club and sit me on a sofa. He asked me to tell him about the paper, and I must have talked for half an hour, non-stop. He listened, asked questions, and then he smiled at me, and said "Linda, this paper is in your head. You've just 'talked' your paper to me. Now go write it. When you're afraid, write about being afraid. Soon you'll be writing about DNA profiling." And I did just that, writing through the fear, and finished the paper, which eventually resulted in an excellent publication. As an historian, Bob taught me that "God is in the details," which has alternately driven me crazy and improved my scholarship immeasurably. Ted Porter from UCLA also supported me through some difficult academic times, never losing faith in my abilities. I commuted to UCLA for six months to take his history research seminar, and his dry sense of humor always kept me laughing while he solemnly delivered deep insights into my work.

I worked with Lisa Catanzarite for six years, helping with her statistical analyses. What began as a working relationship has blossomed into a deep and cherished friendship. Lisa has dried more tears from a distance than any friend or committee member should have to. Without her absolutely unflagging belief in me and the value of finishing this degree, I could not have kept going. Lisa is one of the very few women that I would like to be like. She and her husband Landon have

always made me feel special, loved, and worthwhile. I cherish them and their family deeply.

I am also grateful to Akos Rona Tas, who was originally kind enough to join my committee so that Lisa could have a honeymoon. At my candidacy exam, Akos asked deep and penetrating questions that have shaped this dissertation, and sometimes changed its focus. At my defense, he pointed out places where my assumptions were implicit. While it was difficult to articulate what I meant, I am so very glad that he saw that need for explicit articulation – this document is far better for his involvement. I am grateful for his continued commitment and participation on my committee, despite an immensely busy schedule.

Jerry Doppelt also supported me very strongly during my years in San Diego, first as a professor in the Science Studies Core Seminar, and later in his position as director of the program. I am grateful that he has maintained this support and goodwill during the years I have been away. I am honored to have him on my committee, and I value his insights. I also owe many thanks to David Blasband, who entered my life at a very crucial, painful moment, and kept me on the road to life, the dissertation and a better self. I cannot repay David, but I will be forever changed because of the efforts he made on my behalf, and the commitment that he made to me and my well-being.

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Friends are the fabric of life for me, people that make me feel better just by being themselves and being with them. Hugh Arbor and John Walk, and their daughters, Rachel and Mary, provided countless dinners, endless laughter, trips to Chuck E. Cheese's -- with the noise made bearable only by beer and the sheer joy of watching the children grow up. We shared many stunning Christmas trees and the joy of excited children at 6:00 am on Christmas morning, a lovable dog named Maxine, hours and hours of coloring and baking cookies and watching movies. My friendship with Hugh and John and their family taught me that I had value beyond my academic talents. Bruce Jones is also a beloved friend, and continues to be so. I miss our lunches, and his gentle hugs that communicate true love, friendship and understanding. I am lucky to still have his calm, loving presence in my life. Bruce has done so many things for me over the years that I've lost count. His generosity of spirit and self is amazing, and he is one of my very favorite people on the planet. Elizabeth Leonard is one of the most loving, wise, giving people I know, and she will always

hold a cherished place in my heart. She is another female role model. I feel great joy at her continued success in all facets of her life. Her deep faith, in God, and in me, has kept me going at times when the end seemed just too far away. Helen Coombs started as a student in a course I was teaching, became an exercise partner, and over the last few years, we have spent endless hours sharing the joy and pain that constitute life.

Mike and Elva Jones are old and cherished friends, whose love is unconditional, and transcends accomplishments. Mike was my high school debating coach. I changed their son's diapers, and a couple of decades later attended his wedding. They have read this dissertation cover to cover, and they got excited even over the dry, technical bits. That's friendship! Elva is absolutely not responsible for any remaining typographical errors or stylistic inconsistencies! I am grateful for their continued faith and support. Janet Dixon is an old and cherished friend – I miss her deeply. When I was doing my masters degree, she decided I needed a computer, and a few days later, a delivery person dropped one off. Janet flew me home when my grandfather died. In her wry, dry way, she said that some day, when she was down and out, I could help her. I hope that I get the chance, one day, and in the meantime I am delighted with the joy she finds in her new job and her life with her son Scott. Jayne and Julian Buchwald provided moral support, care packages upon care packages, gifts, money when (always just when I was wondering "how will I pay for this?"), endless dental care, love – gifts that transcend friendship. Norman Ingram and Stephanie Mah have each continued, in their own very different ways, to provide that love and support, the sense of history, the knowledge of your true self and potential, that only friendships that started in elementary school can provide! They never gave up on me, and helped me not to either.

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I am also grateful for the support, understanding and immense flexibility while I worked in the Department of Human Ecology, by my wonderfully warm, loving and understanding supervisor, Jacquie Eales. Jacquie's faith that I could manage the dual responsibilities of managing a large research project and finishing my dissertation at the same time have helped propel me to the finish line.

My colleagues at UCSD would probably agree that a Science Studies degree is akin to trial by fire – we started when the Science Studies program was a pilot project, and the bar was set very high. I am especially grateful for the friendship of Bart Simon, now with his own new book, a great job and partner, and a new daughter Shayle. I also thank Margaret Garber -- I am delighted at her success, her perseverance, and for her often centering words of wisdom along the very arduous path we had to traverse. Like me, she has refused to give up despite what sometimes seemed like insurmountable odds.

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So you see, this dissertation is a hugely collaborative effort. I had to put the words to paper, but the best of every single one of you is in these pages. To those I have forgotten, please accept my apologies – your contribution is deeply appreciated as well. In closing, I want to thank two people who have changed my life deeply and irrevocably.

Beth Green is incredibly intelligent and has a gift for insight. She is wise, calm, consistent and responsive. She is filled with grace, and overflows with competence, knowledge and plain hard work. She has been there, over and over, and through the way she chooses to live her life, she has helped me to change who I am in the world, for the better. She is the best role model I could wish for, and if I end up even a little like her, I will be extremely pleased and proud. Beth's faith in me and her willingness to hold in *her* heart the vision of me with a doctorate is a gift without price. There are no words for the changes that Beth's hard work has wrought in my life.

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VITA

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Social Theory
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Sociology of Scientific Knowledge
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ABSTRACT OF THE DISSERTATION

Agency and Structure in the History of DNA Profiling:

The Stabilization and Standardization of a New Technology

by

Linda Anne Derksen

Doctor of Philosophy in Sociology (Science Studies)

University of California, San Diego

2003

Professor Steven Epstein, Chair

DNA profiling has been called the most important forensic innovation since the introduction of fingerprints in the early 19th century. Before DNA profiling became a stable form of knowledge, it went through many crises, including "The DNA Wars." Using concepts from the sociology of scientific knowledge, this dissertation contributes to a theory of agency and structure that shows how the dynamics of agency and structure interacted in the case of DNA profiling, to create new knowledge *and* new forms of social structure. Data were drawn from in-depth personal interviews, scientific publications, transcripts of key court cases and Congressional hearings, judges' decisions, FBI documents, two National Research Council (NRC) reports, and technical and lay press coverage of the controversies.

In the DNA Wars the country's most prestigious population geneticists mobilized their personal and institutional credibility to support or derail the new technology. In 1991 the NRC convened a committee of blue-ribbon representatives from the criminal justice system and academia to settle controversies surrounding the technology. Their recommendations met with widespread criticism, forcing the NRC to convene a second committee in 1994. While some groups were fighting bitterly over the procedural and computational aspects of DNA profiling, the FBI was quietly creating a community of practitioners, known as the Technical Working Group in DNA Methods (TWGDAM), who were instrumental in stabilizing, standardizing and disseminating DNA profiling procedures in North America.

This analysis shows that stable knowledge is produced in successful communities of practice – the first NRC committee's proposed solution to the knowledge problem failed, partly because they could not constitute themselves *as* a community. It also reveals that closure to scientific controversies is a complex process that can occur at different times for different groups. In this case "wars" raged in some social worlds (academia, the National Research Council), while order was quietly established in other arenas (Congress, the FBI, TWGDAM). The study also shows that we are misled if we believe that it is only scientists who produce and stabilize knowledge. Sound knowledge about DNA profiling was created from the activities of many individuals pursuing specific goals in disparate institutional contexts.

Chapter One

Agency, Structure and Science Studies -- New Tools for an Old Problem

1) Introduction: DNA – The Power to Establish Unique Identity

Saskatoon, Saskatchewan, Canada is a small prairie city – a university town with prairie farm roots. It is a quiet, pretty city, with many bridges that cross the banks of the beautiful, winding North Saskatchewan River. Canadian prairie winters are often hard and cold, and in Saskatoon on the morning of January 31, 1969, it was 42 degrees below zero. That morning, pediatric nursing assistant Gail Miller set off to catch her bus to her "dream job" in the pediatric ward of City Hospital. Gail Miller never arrived at work that bitterly cold winter morning. Her partially clothed body was discovered in a back lane by children on their way to school (Vancouver Sun 1998, A4). She had been brutally raped, stabbed and slashed more than twenty-two times. A trail of her belongings led from the house of Albert (Shorty) Cadrain – a knife handle ... a boot ... a wallet. The house was the house where 16 year old David Milgaard and his friends had stopped that morning to pick up Shorty for a trip to Alberta. It was also the same house where Larry Fisher lived in the basement with his wife.

The "hippie" teenager David Milgaard was arrested for the rape and murder of Gail Miller. Although Milgaard protested his innocence, on January 31, 1970, he was convicted and sentenced to life in prison with no possibility of parole for 25 years. One year later, the Saskatchewan Court of Appeal rejected an appeal of his case. On November 15, 1971 the Supreme Court of Canada again denied Milgaard's right to appeal. In 1973 Milgaard escaped from the Stony Mountain penitentiary near Winnipeg, but he was caught quickly. In 1980 Milgaard illegally extended a day pass,

and 77 later he was shot in the back, hands held over his head, by a policeman in Toronto, Ontario. Milgaard continued to claim that he was innocent (Tyler 1997, A17).

In 1988 the Milgaard family hired James Ferris, a forensic pathologist from Vancouver, British Columbia. Ferris attempted to use the new technology of DNA testing to prove Milgaard's innocence. The test results were inconclusive. In December of 1988, Milgaard's lawyers went directly to Kim Campbell, then the Federal Justice Minister and asked to have his case reopened, and on February 27, she declined the request. In August of 1991, Milgaard's lawyers again approached the minister, this time with new evidence that implicated Larry Fisher, who had been living in Shorty Cadrain's basement at the time of the rape and murder.

Shortly after Milgaard was first sent to prison In September of 1970, Larry Fisher was apprehended by police while fleeing from an attack on a woman in Fort Garry, a suburb of Winnipeg. Winnipeg is in Manitoba, one province east of Saskatchewan, where the Miller rape had occurred. In Winnipeg, Fisher was arrested on another charge, to which he confessed, and at the same time he also confessed to four brutal sexual assaults in Saskatoon that had occurred in Gail Miller's neighborhood around the same time as her murder (Vancouver Sun 1998, A4).

Incredibly, no one linked Fisher's confession of *four* sexual assaults in Saskatoon to the Milgaard case and to Milgaard's continued protestations of innocence. Fisher's case was tried in Saskatchewan's provincial capital city of Regina, not Saskatoon, where the crimes had been committed. It did not attract much media attention, and even police investigators who were familiar with both cases did not make the link between the Miller rape and murder and the other rapes to which Fisher had confessed.

In 1990 David Milgaard's mother, Joyce Milgaard, made the connection. She

tied Fisher's confessions to the series of sexual assaults around the time of the Miller rape and murder, the brutal death of Gail Miller, and her son's pleas of innocence (Bayin 1999). On September 6, 1991, Milgaard's mother confronted the Canadian Prime Minister, Brian Mulroney, with the facts of her son's case. Mulroney promised he would look into the case, and on November 29, 1991, Justice Minister Kim Campbell referred the case to the Supreme Court for review. On January 21, 1992, before the Supreme Court of Canada, Milgaard testified that he had not killed Gail Miller 23 years before. However, new DNA tests, which had been conducted in the United States, were again inconclusive (Tyler 1997).

On April 14, 1992 the Supreme Court of Canada finally ruled that Milgaard should have a new trial, and David Milgaard was freed after 23 years in prison. Although the Saskatchewan government did not prosecute him again, it also refused to acknowledge his innocence by registering an acquittal, awarding compensation for wrongful imprisonment, or calling for an inquiry into the case (Tyler 1997, A17). The federal government also refused to pardon him. In 1992, a Royal Canadian Mounted Police (RCMP) forensic scientist in Ottawa identified what she believed to be semen stains on Miller's underwear, but no testing was done to confirm this.

Five years later, on May 19, 1997, still seeking official acknowledgement of Milgaard's innocence, David and his mother announced that DNA testing would be tried again. This time the tests were conducted in England, by American, British and Canadian scientists. Although it had been twelve years since the discovery of DNA profiling, the Miller garments were the oldest to ever be subjected to DNA testing for legal purposes (Tyler 1997, A17). By July 18th, 1997, DNA tests were conducted on Miller's underwear, bra, half-slip, clothing and coat. It was determined that the stain on her underwear was not semen, but semen was found on her slip and her coat. On

July 18th, 1997, Milgaard's lawyers announced that the DNA on Miller's garments did not match Milgaard's DNA, proving conclusively that Milgaard had not committed Gail Miller's murder.

When Larry Fisher's DNA was tested, the biologists in the RCMP forensic laboratory stated that there was a 1 in 950,000,000,000,000 chance (this is a *very* small chance) that the semen on Gail Fisher's slip came from someone *other* than Larry Fisher (J.I. 1999)). This was not only strong evidence that Larry Fisher was included in the population of people who could have committed the crime, it proved with virtual certainty that he was responsible for Gail Miller's death.

In 1988 and 1992, DNA tests were unable to show that David Milgaard did not kill Gail Miller. In a total about-face, in 1997, DNA profiling provided conclusive, incontrovertible evidence that Milgaard *could not* be her killer. What changed between 1988 and 1997? The answer which will spring to most readers' minds is that "the science" of DNA testing improved between 1988 and 1997. I would like to immediately derail this train of thought. Our faith in science leads us to believe that it gets better over time, and that an attitude of skepticism on the part of scientists, and the continual testing of theories leads inevitably to the continual improvement of knowledge. This understanding of science is pervasive, but it does not provide a complete understanding of the ways in which new technologies gain the status of "truth." The path to the acceptance of new technologies is not necessarily paved with changes or improvements in the techniques of production. However, the creation of every new technology does change the social, scientific, economic and organizational relations in which the technology is embedded (Bijker and Law 1992).

The reason that I want to derail in the reader's mind the thought that Milgaard's 1997 test proved him innocent because "the science got better over time"

is because the actual procedures involved in conducting DNA profiles using Restriction Fragment Length Polymorphism (RFLP)[1] analysis did not change very much between 1988 and 1997. However, the social, scientific and organizational relations in which the technology of DNA profiling was embedded changed immensely. This dissertation shows how, over time, the actions of many individuals, professional groups, and organizations changed the social, scientific and organizational relations in which DNA profiling was embedded. This finally resulted in the closure of controversies over different aspects of the DNA profiling procedure, sometime between 1996 and 1997.

2) The Story in Brief

The entrance of DNA profiling to the U.S. courts was a little bit like the serene beginning to what turns out to be a frightening movie. In 1987, when DNA profiling was used to establish identity for the first time in a U.S. court, all appeared to be well with the technology. In the beginning DNA profiling was solely the territory of private enterprise. It was conducted by two private companies: Lifecodes Corporation of Valhalla, NY, and Cellmark Diagnostics, of Germantown, Maryland. In the courtroom, high-ranking scientists from those companies spoke to the validity and reliability of DNA profiling, and from the outset it had high credibility in the courts. However, this peaceful period of credibility was short lived.

In 1989 a double murder of a pregnant woman and her two year old child in

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[1] Restriction Fragment Length Polymorphism is a technique for DNA profiling that uses sections along the DNA molecule known as VNTRs (variable number of tandem repeats). These VNTRs vary in length from individual to individual (thus they are "polymorphic"). "Restriction fragment length" refers to the length of the fragment that is "cut" from the DNA molecule with restriction enzymes. These fragment lengths are processed and compared (see Chapter Two for the technical details of DNA profiling). See Appendix A for a Glossary of Terms.

New York City, which became known as the *Castro* case, brought about an unusual convergence of people and interests. The defense lawyers in the *Castro* case were very sharp. Through a series of coincidences, Barry Scheck and Peter Neufeld made the acquaintance of an unusually erudite and accomplished molecular biologist from MIT and Harvard, named Eric Lander. Lander was reluctant to become involved in the case, because he did not want to jeopardize what he saw as a valuable technology, but Scheck and Neufeld pressed him to help them interpret the visual images of the DNA profiles. In the *Castro* case Scheck and Neufeld mounted the first significant challenge to the credibility and trustworthiness of DNA profiling. Their challenges provided the impetus for the scientists who had been called as expert witnesses from the prosecution *and* the defense to get together outside the courtroom and discuss the scientific status of Lifecodes' evidence. In unison, they declared that Lifecodes' evidence did not meet the minimal standards of scientific evidence. Scheck and Neufeld were successful in exposing how at that time, DNA profiling was extremely dependent on local practices and subjective decisions (Jasanoff 1995, 42-68; Derksen 2000). The procedure of DNA profiling lost credibility after the *Castro* case, and it became a problem for academia to address.

After the *Castro* case the FBI was worried about the future of DNA profiling as evidence, and called for the National Academy of Science to convene a committee to investigate and solve the problems associated with the technology. This committee began meeting in 1989, and had its final meeting on December 21, 1991. This is the same day that the journal articles which started the "DNA Wars" were published in the prestigious journal *Science* (Lewontin and Hartl 1991; Chakraborty and Kidd 1991). The DNA Wars were a series of disputes which broke out among scientists once the problems with DNA profiling became more evident to them. They had their genesis in

the courtroom, as the most highly esteemed population geneticists and statisticians were called as expert witnesses in court cases across the country. In the courts, defense and prosecution lawyers were doing their best to present their own expert witnesses as the most credible and trustworthy. The scientists clashed violently with each other in courtrooms, and rushed back to their laboratories to write up their expert witness reports as articles. They then submitted these for peer review and publication in scientific journals.

This all happened within the complex interplay of social worlds: scientists were out of their element and uncomfortable in the agonistic environment of the courtroom. Lawyers spent time in laboratories learning about DNA profiling. A powerful law enforcement institution appealed to another powerful institution – the National Academy of Science, for science to step up to the bat and settle problems with DNA profiling. The National Academy of Science is not like political institutions. It is supposed to act as an independent arbiter of scientific matters for the government. It members, who comprise the nation's most highly respected scientists, have higher than average credibility and trustworthiness in the eyes of the public (Shapin 1996).

The first NRC committee on forensic DNA technology was a mix of individuals from different social worlds. Highly esteemed lawyers, molecular biologists, professors, were all brought together to "solve" the problems with DNA profiling. Here again, individual dynamics in the making of knowledge came into play. This first committee had a weak chairman, and two strong personalities, both with equal scientific credibility. Eric Lander had the largest, roboticized laboratory in the Human Genome Project, and Thomas Caskey had ongoing research grants on DNA profiling from the National Institutes of Justice, and had adopted the FBI protocols in his own laboratory. Over the two year tenure of the committee, these two were constantly at

each other's throats. The committee was characterized by deep cleavages, as members lined up behind Lander or Caskey, or kept quietly to themselves.

This committee represents the gathering together of members of society with unimpeachable scientific credentials under the rubric of the National Research Council, with all the prestige and credibility with which that institution is imbued. They were brought together to exercise their individual agency, to bring their knowledge, logic and opinions to bear on the problem at hand. Here, we see the formation of a new social group in the attempt to solve a problem of knowledge. However, this group was not able to solve problems of order within their own committee, and this affected their final report and the credibility of the "ceiling principle," their proposed solution to the random match probability problem.

The first NRC committee on the forensic uses of DNA profiling produced a large, generally well received report, released in April of 1992. It covered an extremely wide range of issues involved in DNA profiling, from chain of custody, to laboratory proficiency, quality assurance, training of personnel, as well as the proposed solution to the problem of how to correctly calculate the random match probability (NRC 1992). However, the proposed ceiling principle enraged powerful members of the FBI, as well as academic population geneticists, and statisticians – who had not been included on the committee. The hue and cry over the problems of the ceiling principle were so great, that at the behest of the FBI, a second NRC committee was convened in 1994.

In late 1994 just before O. J. Simpson came to trial for the murders of his former wife and her friend, two arch opponents in the DNA Wars – the FBI's Bruce Budowle and Eric Lander and Bruce Budowle, attempted to exercise their individual agency and bring closure to the wars by exercising their personal and scientific

credibility. They published an article in *Nature*, declaring in large headlines that the "DNA Wars Are Over." Lander and Budowle did not want Simpson's defense attorneys to be able to claim that there was no consensus in the scientific community over the status of DNA profiling, thus potentially having it declared inadmissible. It is somewhat paradoxical that these two rivals declared the wars over at virtually the same time as the second NRC committee was beginning to meet.

The second NRC committee (NRC2) on the forensic uses of DNA was another blue-ribbon committee, but this committee included population geneticists and statisticians. In the academic world, these disciplines ostensibly have jurisdiction over the fields of knowledge under which pertain to the knowledge of random match probabilities. The committee's mandate was to solve the "statistical issues" that were felt to be unresolved by the first NRC committee. To this end, NRC2 focused on developing a correction factor to take account of the possibility of population substructure, and laying down, in meticulous detail, mathematical grounding for the calculation of random match probabilities. After much debate, the second committee decided that the FBI and other law enforcement agencies could return to the method of calculating random match probabilities that they had been using in the late 1980s.

The FBI played an enormous role in the stabilization and standardization of DNA profiling in the United States. The FBI is clearly a social institution which pre-dated the controversy. As such, it could use its considerable resources to enable or constrain the agency of many different kinds of actors. From the outset the FBI brought to bear its material and intellectual resources to enable the development, stabilization and standardization of DNA profiling in North America. The FBI facilitated the formation of a group of crime laboratory practitioners who initially knew nothing about DNA. They were named the Technical Working Group on DNA Methods

(TWGDAM). Through individual and group interactions, these people learned about DNA, they learned to produce DNA profiles, and they learned how to interpret them. They helped to hone the protocols for producing DNA profiles to their simplest, most robust form. Together they worked out informal standards for interpretation, and formal standards for quality assurance and proficiency. These standards became part of formal social structure when they were legislated as the standards for forensic DNA laboratories to follow in the 1994 DNA Identification Act. Part of this analysis is to show that a group of individuals, when provided with the necessary material and institutional resources, can bring new forms of knowledge into being.

It is clear that the FBI had an explicit interest in getting every crime laboratory in the United States to perform DNA profiling in essentially the same way. From the outset, they had the vision of a national DNA databank in their minds. In order for this to become a reality, every participating laboratory had to perform its DNA profiling in essentially the same way – using the same protocols, chemical reagents, probes, and analyzing the same sites along the DNA molecule. It took a tremendous amount of work on the part of the FBI and the crime laboratory directors and practitioners in the United States and Canada to standardize and stabilize DNA profiling. This was accomplished to the extent that virtually any lab which wanted to could utilize the FBI's protocol. The big pay-off for all this work was participation in CODIS, the Combined DNA Indexing System, a databank which the FBI hoped would store the DNA profiles of convicted felons, and span the United States and Canada. The story of how an individual's identity, as represented in blood, semen, saliva or hair becomes stored as a number in a databank is intriguing, complex, and involved the work of many people, the power of institutions, and the marshalling of a huge amount of material resources.

The sustained efforts to end the DNA wars, to find a correct way to calculate random match probabilities, and the massive efforts to stabilize and standardize DNA profiling resulted in the creation of many new social structures. These ranged from standards of interpretation of DNA autorads, to legislation, to groups such as the Technical Working Group on DNA Methods (TWGDAM), the DNA Task Force, and the National Commission on the Future of DNA Evidence. Standards of quality assurance and laboratory proficiency were also created through the actions of the FBI and TWGDAM. The Technical Working Group on DNA Methods became such an important part of controlling how DNA profiling was done, that legislation passed granting money to laboratories wanting to conduct DNA profiling tied that money to the laboratory's agreement to use TWGDAM protocols and standards of quality assurance. This took place about six years after TWGDAM's first meeting, when there were only three or four people in the entire group that even knew what the DNA molecule looked like.

One of the conclusions of this study is that it would be a mistake to look at the DNA Wars only from the perspective of "science." If we look only to the scientific domain for the production of knowledge and the solving of controversies, we are left vexingly bereft of an explanation. However, if we follow the knowledge production process across the many social worlds in which "knowledge making" happened, and realize that groups other than scientists make knowledge, then the picture becomes clearer. The two National Research Committees did not bring closure to the DNA Wars. Indeed, even in retrospect it is difficult to say precisely when the Wars were over. The controversy reached closure at different times and in different ways in each of the social worlds. How this occurred will be explored in detail in the following pages.

Returning for a moment to the David Milgaard story, recall that Milgaard's first two DNA tests in 1988 and 1992 did not "work," and the test in 1997 did. It is highly probable that the difference between test results was not due to as much to technical changes in the procedure, as it was to social changes surrounding the technology – that is, the reorganization of the people and organizations that supported and utilized the technology. To understand how these social changes occurred, we need to leave the Milgaard story, the DNA wars, TWGDAM and CODIS, and turn to the arena of social theory. This will help to understand the role of individuals in stabilizing knowledge and technology and in settling scientific controversies.

3) Linking Agency and Structure in the History of DNA Profiling

In this dissertation I undertake two explicit tasks. The main goal of my research is to address a central problem in social theory: to demonstrate empirically how human activity at the micro level of agency results in the creation of macro social structures. To do this, I use the history of DNA profiling as data, to show how the processes involved in creating stable knowledge about DNA fingerprinting resulted in the creation of social structures. In order to use the history of DNA profiling as "data" for my agency/structure argument, I have to provide an historical account of the stabilization of DNA profiling as it unfolded in the United States between 1985 and 1999. This history is deeply shaped by the theories, perspectives and concepts offered by the discipline of science studies, which is itself inter-disciplinary. This is the second major contribution of the dissertation.

The main argument is that in the arena of DNA profiling in the United States, between 1985 and approximately 1999, human beings involved in streams of practical, mundane activities produced stable knowledge about DNA profiling, as well as new social structures that supported that knowledge. The credibility of DNA

profiles as knowledge claims was not secure until many of these social structures were firmly established.

The relationship between the individual actor, their actions, and the social structures that they create is a very old sociological problem, without an agreed upon causal pattern. By approaching this problem historically, I introduce time into my analysis, which will allow the reader to see the genesis and growth of social structures.[2] The methodology involved in historical sociology is particularly suited to parsing out the relationship between human agency and social structure because its primary focus is the interplay between human agency and social structure. Abrams (1982) argues that these problems are best approached empirically, not as abstract theoretical issues.

a) Agency and Structure in Social Theory

Most of the classical social theorists were fundamentally concerned with the emergence of new social structures on the macro level. Marx, Weber and Durkheim each struggled to understand the new forms of social order that with arose with modernity, such as capitalism, bureaucracy and organic solidarity. Most

[2] Much of mainstream quantitative North American sociology uses cross-sectional data (covering one point in time). However, many sociologists use historical methods and perspectives to conduct their analyses. Aside from Marx and Weber, who were deeply informed by their knowledge of history, many of today's prominent sociologists are historical sociologists. Contemporary social theorist Anthony Giddens has suggested that time and space are important theoretical categories (1984), and time sequence is an important aspect of Pierre Bourdieu's analysis of social practice in *Outline of a Theory of Practice* (1977). Other prominent sociological analyses that take history seriously include Charles Tilly's work on the history of revolutions and state control (Tilly 1997; Tilly 1993; Tilly 1990). Philip Abrams is another well known historical sociologist. He wrote a famous treatise on historical sociology (1982); studied the history of towns in society (1978), and wrote about the origins of British sociology (1968). Phillipe Ariès has studied the social history of childhood (1962), and Philip Corrigan and Derek Sayer have argued that the formation of the British state was the outcome of a cultural revolution (1985). Marxist scholar Derek Sayer has also written a history of Czech society (1998), and there are many other examples of sociological analyses which invoke historical methods or perspectives. The discipline of sociology also recognizes the importance of history and historical sociology. The American Sociological Association has a section dedicated to historical sociology, and *The Journal of Historical Sociology* is well respected.

contemporary social theorists have turned away from explaining the emergence of new social structures, and have focused instead on the *reproduction* of existing social structures, mostly the class structure of societies.[3]

The debate as to whether ontological and causal primacy should be assigned to human agency or to social structure dates back to the origins of the discipline (Alexander and Giesen 1987; Coleman 1986, 1320-1327). During the 1980s and 1990s, the micro-macro problem emerged as *the* major theoretical concern for American sociology (Ritzer 1996, 83). In Europe it is referred to as the agency/structure problem, and has "rightly come to be seen as the basic issue in modern social theory" (Archer 1988, ix), and it has arguably garnered even more attention from contemporary European theorists than their North American counterparts. This concern for the link between human agency and social structure is the predominant problematic in the work of contemporary theorists such as Anthony Giddens (structuration theory, 1979, 1982, 1984), Pierre Bourdieu (the concepts of *habitus* and *field*, 1977, 1984) and Margaret Archer (culture, agency, structure, 1988).

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[3] Many contemporary theorists who focus on the reproduction of structure have attempted to augment Marx's theory, or have constructed their theories to circumvent problems in Marx's theoretical schema. Both Giddens and Bourdieu try to advance theoretical programmes which argue that the economy is not the sole means by which inequality is perpetuated. Giddens theorizes that the reproduction of social structure is an unintended consequence of social interaction. He frequently uses the example of speaking grammatical English -- by speaking the language actors reproduce the structure of the language, without explicitly intending to do so (1979, p. 77-8). Bourdieu (1985) argues that inequalities in French society are due to cultural, rather than economic inequality. On the surface the French educational system appears to be a meritocracy which is open to all who are academically qualified, regardless of their economic status. However, Bourdieu argues that success French society requires a set of cultural skills -- a manner of being which is embodied, learned early in life in the family of origin, that the lower classes do not share and cannot attain. This is the genesis of his concept of *habitus*. In the end, his argument is mainly structural: the educational system in France perpetuates class privilege without anyone explicitly working for it to do so by the operation of its own internal logic. Individuals have a particular *habitus* which interacts with the *field* of education in France, and the outcome is the consistent reproduction of the existing class structure (Bourdieu 1985).

In trying to link agency with structure, both Giddens and Bourdieu attempt to transcend the problems of mid-twentieth century social theory where "human agency [became] pale and ghostly in ... functionalism, while structure betook an evanescent fragility in the re-flowering of phenomenology" (Archer 1990, 73). Perhaps Marx summed up the intrinsic tension, duality, complexity -- and the mutually constitutive nature of the relationship between the individual and over-arching social structures in this famous passage from the *Eighteenth Brumaire of Louis Bonaparte*:

> Men make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly found, given and transmitted from the past (Marx [1852] 1978).[4]

We also find in the classical theorists the concepts and terminology that have shaped the contemporary agency/structure discourse. Durkheim had a clear sense that the social structures of societies came from the way they categorized and classified their world. Weber's writings straddled the micro and macro, but many theories of agency are based on his concept of *Verstehen*, or the goal subjective understanding of action. Marx's work is filled with the tension between individual and society, agent and structure. Although he is sometimes interpreted as an economic determinist (taken to extremes in the work of Althusser's structuralist Marxism (Althusser 1966; Craib 1984, 123-146)), throughout his early and late work he critiqued the concepts of "society as subject" and the "abstract-isolated-individual" (Sayer 1990, 235). Marx did not intend for individuals to be opposed to society, either

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[4] Marx wrote this in late 1851 and early 1852. It was originally published in a magazine called *Die Revolution*, which was printed and distributed in the United States, specifically in New York.

analytically or empirically.[5]

Although class struggle is the "motor" of social change in Marx's writings, he is not an economic determinist. The origins of the word "determine" mean the setting of boundaries or setting of limits. This guides the analyst to look at "the predominance of objective conditions at any particular moment in the process" (Williams 1977, 85). Objective conditions are "boundaries" or "limits" to action. In this analysis it will be important to remember that individuals do make history but that capacity to make history is qualified or limited by the "objective conditions" under which individuals act at any given time. As I will argue later, this is the sense of determination as the setting of the parameters or boundaries within which action can vary. I contend that nature itself is one of the boundaries to human action in the knowledge making process. Nature is indifferent to our nominal classifications, but materially it is not infinitely malleable. At some point nature "speaks," or becomes incompatible with a particular interpretation or manipulation.[6]

Graphically, the reciprocal causation involved in the relationship between agency and structure may best be illustrated by a double headed arrow:

agency ←→ structure.

In this dissertation I focus mainly on the agency → structure link, while also keeping in mind the ways that pre-existing structures both enabled and constrained individual

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[5] In his 1844 *Manuscripts* Marx wrote that the individual "*is the social being.*" In the *Theses on Feuerbach* Marx explicitly claims that the only proper subject matter of the human science is "human sensuous activity, practice." In Thesis 3 he rejects any conception of one-way societal determination, and in Thesis 6 he rids us of the idea that the human actor can be isolated from a social context (Marx [1845], in Farganis 2000, 55-56).

[6] Pickering refers to this relationship between the agency of humans and the non-infinite malleability of the natural world as the "mangle of practice" (Pickering 1993). The point at which nature is can no longer be manipulated as "resistance." The literature on the social construction of technology is based on the assumption that producing new technology is the result of conflicts, differences and resistance (Bijker and Law 1992, 8).

agency throughout the history of the stabilization of DNA profiling.[7]

b) The Concepts

Structure: Social structure is a central concept in social theory, rooted in the works of the classical theorists. However, it is often poorly theorized and it is frequently used only as a metaphor (Turner 1998, 470). Theorists who take structure seriously tend to view it as having an ontological status "which privileges it over agency" (Waters 1994, 12). For these theorists, social structure determines the content of conscious experience, and the subject and his or her agency tends to disappear.

In this study I develop and utilize a conception of social structure in which "social structure" takes on a wide range of ontological manifestations. Social structures range from simple to complex, from ephemeral and interpersonal to formal legislation or law. They can be as simple as the rules for interpreting of a DNA profile which come out of group negotiation,[8] the tacit rules for appropriate interaction in a

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[7] One of many empirical studies examining the opposite side of the relationship (structure → agency) is found in Derksen and Gartrell (1993). Here I demonstrated how social structure can increase the probability of a desired behavior (agency). In this study of recycling behavior, I showed that even people who were not concerned about the environment recycled more than people who were very concerned about the environment, *if* the unconcerned people had access to an easy (in house/curbside) recycling program (and obdurate social structure). The social structure of the recycling program increased the probability of observing the desired behavior, independently of people's individual level of environmental concern. While the link between agency and structure is best represented graphically by a double headed arrow (agency ←→ structure), in a story of this magnitude, it is not possible to recount both sides of the story at once. Therefore I have chosen to emphasize the agency → structure dynamic in this dissertation, keeping in mind that the structure → agency dynamic also exists and is very important.

[8] Social structure arising out of interaction has classical roots in the work of Georg Simmel, who conceptualizes structure as the different forms of interaction that underlie and make possible the various activities and relations into which individuals enter. While Simmel wrote on an extremely diverse range of topics, from the city ([1903] 1971), to fashion ([1904] 1971), secrets ([1906] 1950), and the philosophy of money ([1907] 1978), the common theme is the search for underlying patterns – the social structures -- which guided the substantive interaction. In all interactions he sought out patterns of superiority, subordination, competition and division of labor (Simmel 1895). While symbolic interactionism (George Hebert Mead)

culture,[9] or as complex as the tacit and inarticulable stocks of knowledge used to guide action (Schutz 1972). Social structures can be comprised of a combination of obdurate physical structures and human relationships, as in universities, large corporations, or entities like the FBI. Social structures can be complex and difficult to locate in time and space – these are the entities that we usually call institutions: "the economy," "religion," "the family," "the criminal justice system." Legislation and laws are forms of social structure, because they are the outcome of routinized processes of social interaction in institutions and organizations which have the qualities of persistence (in time and space), physical reality, and can be ephemeral (hard to locate or pin down).

Systems of classification can become social structures. Social structure viewed in this way has its roots in Durkheim's early works such as *The Division of Labor in Society* ([1893] 1964) and *The Rules of Sociological Method* ([1895] 1964). Durkheim believed that mental structures reflect the material organization of society, and his work with Marcel Mauss made very clear his belief that human systems of classification come from our social organization:

> The first logical categories were social categories; the first classes of things were classes of men, into which these things were integrated. It was because men were grouped, and thought of themselves in the form of groups, that in their ideas they grouped other things[.]

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also views interaction as primary, it tends to view social structure as being created anew in each interaction (Craib 1984).

[9] Giddens has a fairly loose, agent based conception of social structure. He view it as "rules and resources" which are carried in people's heads, which both constrain and enable individual behavior (1984, 169; 1979, 71, 81). While Giddens recognizes that social structure can be powerfully constraining, it exists only within individual actors, in the form of the rules and resources they use to structure their interaction. Social structure manifest at the level of individual interaction, as agents bring these rules and resources to bear in orienting their

(Durkheim and Mauss [1903] 1963, 82-84).

Durkheim's ideas about the social consequences of classification are echoed in the work of Mary Douglas (1966; 1975) and in contemporary studies of science and technology. Geoff Bowker and Susan Leigh Star's book *Classification and its Consequences* (1999) shows how classifying objects and ideas is a natural human activity. Similarly, I will argue that the classification system used in DNA profiling is the outcome of social action. Bowker and Star show that systems of classification are human products. Classification systems are a complex blend of the ontology of the entities being classified and the complex political and social circumstances which gave rise to the need for classification. Bowker and Star offer as an example the International Classification of Diseases (ICD), which has a long and complex history. They show that classification is not a simple, seemingly objective enterprise like sorting child's blocks by primary color or shape. Their history is filled with contingency, and their ontology mirrors the circumstances of their origins and historical trajectory and the interests of the people and institutions who participated in developing them.

Agency: The sociological concept of agency encompasses behavior, human subjectivity and what happens in individual consciousness during action in the world. It extends to the meanings that individuals apply to their behavior, and the motives they cite for their actions. It also includes how meanings are communicated during interaction, and the ways in which "stable intersubjective social worlds" are established (Waters 1994, 11). By human agency I mean the "process of acting in relation to a set of meanings, reasons or intentions" (Waters 1994, 15).

This perspective has its classical roots in the work of Weber (1978, 4-22), for

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behavior.

whom understanding subjective meaning, or *Verstehen*, was the goal of interpretive sociology. Weber defined sociology as a science concerned with the interpretive understanding of human behavior. The type of human behavior subject to sociological analysis is social action, which occurs when an actor attaches meaning to his or her behavior. Action is social to the extent that it takes into account the behavior of other people, and is thereby directed, or oriented, in its course (Weber 1978, 4). Meaning is attributed by the actor, not the observer, and so solitary action can be social, to the extent that it takes into account, or is shaped by the behavior and expectations of others.[10]

This conceptualization of agency also owes much to the work of George Herbert Mead, who is best known as the founding father of symbolic interactionism (Mead 1934). This theoretical perspective holds that society is constituted by the exchange of symbols and gestures, which signify mental processes. Mead's great insight was that language is the critical element which distinguishes humans and human society, from animals. Communication occurs when two people not only attribute meaning to their own behavior, but also understand the behavior of the other. The key theoretical claims of Mead's theory are that humans become truly social when they are able to take on the role of the other, when they are able to view themselves as objects, and when they achieve the capacity to engage in a sort of imaginative role taking where actors could see themselves acting in a number of different manners, given a situation or different situations (Turner 1998, 474).

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[10] While credited with being the founder of micro theories which privilege agency, Weber actually straddled the fence between agency and structure. One of his major enterprises was a study of the ways in which the form of a society's economy was determined by its religious organization. His very famous work, *The Protestant Ethic and the Spirit of Capitalism* ([1905] 1976) was never meant to stand alone, but was to be the first in a global study of the relationship between economy and religion (Randall Collins, 1991).

c) Agency and Structure in the Work of Anthony Giddens and Pierre Bourdieu

Anthony Giddens and Pierre Bourdieu are two of the most important contemporary social theorists who have attempted to forge a link between individual action and social structure. To support my thesis that new social structures are created through human agency exercised in the production of knowledge, I need to explain how agency and social structure are related, particularly with respect to the origins of new social structure. Giddens' and Bourdieu's theories are both lacking any deep theorizing about the creation of structure, focusing, as noted before, on the reproduction of existing structure. However, both theorists do have concepts which can inform this project. I drawn on Giddens' conceptualization of actors as reflexive beings. For some actions, actors can provide explanations for why they have done what they did. It is no doubt true that one of the ways that social structure exerts its force is through "rules and resources" in peoples' heads which guide their action. However, the reader will recall that while the concept of social structure utilized in this study encompasses internalized rules and resources, social structure also takes on much broader and obdurate forms.

Below I will argue that in the last instance, Bourdieu is a structuralist, and that his concepts of habitus and field do not provide the "missing link" between agency structure. However, his concept of the field, which is populated with groups of actors each intent on serving their interests and reaching their goals is a useful concept. It is most useful when coupled with the understanding of a field as encompassing many social worlds.[11]

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[11] The concept of social worlds is explained later in the chapter, in the literature on Science Studies.

Before exploring the relative merits of Giddens' and Bourdieu's theories, it is important to note that while these two theorists are among the leading theorists attempting to grapple with the link between agency and structure, most of the work which addresses the emergence of new social structure does *not* come from social theory. Indeed, many sociological studies are deeply concerned with the emergence of new structures – often at the grand, macro level of the emergence of new social formations, such as studies of the transitions in post-socialist societies,[12] but more often at a more specific level of studying how new laws, policies, practices, and institutions come into being. However even this literature tends to focus on continuity, rather than innovation. In the study of social movements Doug McAdam argues that restricting the unit of analysis to individual movements, and following the methodological convention of drawing data from case studies has resulted in "a highly static view of collective action that privileges structure over process" (1995, 218).[13]

In this dissertation I will be seeking to explain the emergence of social structure at this level. Although the discipline of science studies is not known for

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[12] Exemplars of work that engage with the emergence of social structures on the grand, macro level such as new democratic constitutions and electoral systems are found in O'Donnell and Schmitter 1986; Linz & Stepan 1996; and Elster, Offe and Preuss 1998. Two examples of empirical studies of the relationship between the micro level of agency and the macro level emergence of new social structures in post-communist societies are Rona Tas (1997) and Lampland (2002). In *The Great Surprise of the Small Transformation* Akos Rona Tas offers a historical analysis of the newly emerging macro-level economic institutions in post-socialist Hungary, which, he argues, are a consequence of micro-level transformations in society. In "The Advantages of Collectivization," Martha Lampland (2002) examines the relationship between the evolution of economics and the emergence of new structures in post-socialist Hungary, as evidenced in the way people talk about contingency and history in the construction of new economic systems.

[13] For studies that address the agency/structure dynamic from a perspective that takes into account the inertia of existing social structure, policy imperatives and expertise in the explanation of the emergence of new social structures see McAdam (1995); Kingdon (1984); DiMaggio and Powell (1983); Weir (1992); Weir, Orloff and Skocpol (1988); and Skocpol (1995).

directly engaging with debates in social theory,[14] I will draw on concepts from science studies which help us to think about one dimension of the emergence of new structures – the emergence of new knowledge, and how knowledge is implicated in the social order.

d) Anthony Giddens: Linking Agency and Structure through Structuration Theory

Anthony Giddens tries to capture the reciprocal nature of the relationship between agency and structure when he argues that "the structural properties of social systems are both the medium and the outcome of the practices that constitute those systems" (Giddens 1979, 69). Marxist scholar Derek Sayer argues that Giddens' conceptions of structure are not terribly new or noteworthy. He points out that within Marx we find the same emphasis on the duality of structure and the emphasis on human agency that Giddens emphasizes. Sayer contends that we find in Marx "intimations of the duality of structure, that is, 'structure as the medium and outcome of the conduct it recursively organizes'" (Sayer 1990, 235). Perhaps Giddens' contribution is more subtle, in that he stresses that structures do not only constrain and coerce human agency, but that they can also enable it (1984, 169; 1979, 71, 81).

Anthony Giddens has a compelling language for talking about the relationship between agency and structure, using phrases such as "mutually constitutive" and the "duality of structure." In the end he grants causal action to human agency, which is not inconsistent with the theory I use in explaining the stabilization of DNA profiling. However, Giddens' conception of structure as "rules and resources" carried in

[14] Exceptions would be Steven Shapin's study of the relationship between the history of "truth" and its relationship to social order, scientific knowledge and credibility (1996), and Pickering's attempt to grapple with the concept of agency in science and the "resistance" of nature (1993).

people's heads, which guide their actions is far too restricted to encompass the range of social structures created by human beings.[15] Institutional structures are created and maintained over time, through the occurrence of routine action (which takes place in time and space, or locales). The intended or unintended effect of interaction is social structure, which are viewed by Giddens as institutionalized patterns of behavior.[16] This is an example of Giddens' duality: institutions are the outcome of agents' practices, while agents' practices are patterned by the routines of institutionalized patterns. Neither institutions nor agents' practices can exist without the other.

Many forms of structural sociology, including Durkheim's, take the position that the structural properties of society act to constrain individual behavior and agency (Giddens 1984, 169). In contrast to this, Giddens has a much looser, agent based conception of social structure, conceptualizing it as "rules and resources" carried inside the heads of actors, which both constrain and enable individual behavior (Giddens 1984, 169; 1979, 71, 81). Social structure exists within and acts through individual actors, in the way the rules and resources structure interaction.

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[15] I do not mean to imply that Giddens does not acknowledge that social structures can be powerfully constraining. However, he does define social structure as rules and resources, not as obdurate entities with an existence all their own. In this sense, his conception of structure is too limited for this analysis.

[16] In discussing the reproduction and building of structure through regionalized (situated?) interaction, Giddens draws on elements of symbolic interactionism, and especially Goffman's work on dramaturgy for his theory of encounters, everyday routines and the routinization of interaction (1984, 68-83). By relying so heavily on symbolic interactionism Giddens is postulating an agent that has a lot of agency -- apart from whatever structure within which his or her actions may be embedded.

Social structure is reproduced, or changed, because these same rules and resources are also reconstituted and changed through the processes of interaction. Giddens defines structure as "the medium and outcome of the conduct it recursively organizes; the structural properties of social systems do not exist outside of action but are chronically implicated in its production and reproduction" (Giddens 1984, 374). What makes Giddens very different from other structural theorists is that he argues that structure is *"not to be conceptualized as a barrier to action, but as essentially involved in its production*[.]" (1979, 70, original emphasis).[17] For Giddens, social structure is integrally involved in the production of action, resulting in the reproduction of existing relations of society. "Structure is thus the mode in which the relation between moment and totality expresses itself in social reproduction" (Giddens 1979, 71).

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[17] It is important to understand that Giddens' concept of structure is vastly different from the structuralism based on the work of Sassure and Claude Levi-Strauss, which is the foundation for contemporary structuralists such as Pierre Bourdieu and Robert Wuthnow. Traditional French structuralism is based on a model of language where reality is seen as a kind of language, and it is the task of the analyst to "decode" its underlying structure. Individual behavior is seen as a surface manifestation of deep, underlying mental structures. Levi-Strauss' structuralism is based on viewing what is empirically observable, i.e., behavior of an individual or group as a system of relationships among elements, where the elements are parts of language, myth systems, kinship systems, etc. The meaning of individual elements comes from their relationship to other parts of the system. The elements and relationships between them are conceptualized in terms of binary opposites, such as good/bad, male/female, sacred/profane. The underlying generative structure is the codes or rules which are used to organize these opposites, and it is the job of the analyst to construct models of the rules which are used to organize the opposition, but first models are constructed of the observable relationships -- for example, in a kinship system. The basic metaphor is that there is an underlying grammar or structure which generates the observable behavior of individuals or groups. Levi-Strauss applied structuralism to the study of myths and kinship systems in tribal cultures, arguing that the exchange of women in kinship systems created the basic structure of tribal societies, within which other kinds of transactions and relationships could then take place. Different cultures had different rules of kinship, akin to rules of grammar in language -- i.e., different underlying structures. Levi-Strauss' work on tribal myths tried to show that myths were coding systems by which the universe and objects in the world were categorized, and his analyses tried to show the underlying structure of the categories. As a method, it has also been applied by Althusser and Poulantzas to interpret Marx, by Lacan to study dreams and the human unconscious, and Barthes in the study of literature, advertisements and other cultural products (Turner, 1991, 492-5; Collins 1988, 302-11).

I agree with Giddens that the construction of new structure, or the reproduction of existing structure, may be an unintended consequence of action (Giddens 1979, 77-78). Giddens is useful for my project because he sees that the potential for the change (creation?) of social structure is always implicit in the reproduction of existing structures:

> It is essential to see that any and every change in a social system logically implicates the totality and thus implies structural modification, however minor or trivial this may be. This is illustrated by linguistic change: modifications in the phonemic, syntactical or semantic character of words in language are effected through and in language use, that is through the reproduction of language; since language only exists in and through its reproduction, such modifications implicate the whole (Giddens 1979, 114).

Giddens' most fundamental idea about individuals and agency is that actors are driven unconsciously and diffusely to seek ontological security to reduce anxiety (Giddens 1979, 128; 1984, 50). This need for security is the motivation for interaction, and over time, these interactions become routinized, predictable and stable, thus reducing anxiety.[18] Giddens actors can give accounts for why they do what they do (Giddens 1979, 57),[19] and they are reflexive -- they monitor their behavior and its effects as they interact with others in different contexts. The contexts in which they act are social systems, which exist in particular times and spaces.[20] He uses the idea

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[18] Giddens draws on Lacan's interpretation of Freud for his theory of the unconscious (Giddens 1979, 120), and Erickson for his ideas about anxiety, trust, and the drive for ontological security (Giddens 1984, 51-60).

[19] Giddens' theory requires a theory of the unconscious, because he says that while agents may be able to give reasons for what they do consciously, they are not always aware of the unconscious motivations for their behavior. Here, in his ideas about discursive and practical consciousness, stocks of knowledge, and reflexivity, Giddens draws heavily on elements of phenomenology and ethnomethodology (Giddens 1984, 7).

[20] One of the very important contributions that Giddens adds to the discourse of social theory is to add the dimensions of time and space to his theory. It may be that many theorists are driven to seek a micro-macro "link" because they focus on only one moment in time, and in that moment, they can see only the effects of agency, or more often, of social structure. For

of "locale" to account for both the physical space in which interaction occurs, and also to account for the knowledge which individuals bring to the interaction about what is appropriate behavior for that context. Actors use their stocks of knowledge to know how to go on within any given context (Giddens 1984, 110-44).

By agency, Giddens means what an actor actually does in a situation that has visible consequences, whether these are intended or unintended (1979). What is important to Giddens is *that* people act, not what their intentions are in acting. Agency implies that the actor could have acted differently – their behavior is not determined by their circumstances (Giddens 1984, 9).

e) Bourdieu: Linking Agency and Structure through Habitus and Field

The concept of *habitus* is Bourdieu's micro-macro link. It is also the cultural mechanism which perpetuates inequality through the tastes people have, and the decisions they make. Through these cultural mechanisms, class structure is reproduced without the presence of an explicit aristocratic ruling class or a direct link to economic structures (Bourdieu 1990, 53).[21] Bourdieu links agency to social structure, by examining the relationship between habitus and field. A "field" is a social

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example, in the case of DNA profiling, by conducting a sociological analysis that includes a length of time (albeit short by most historians' standards), I can show that some new structures are partly the outcome of agency. If we were to "drop in" on the story of DNA profiling at any given moment between 1985 and 2000, we would be unable to see the existing structures as the outcome of situated human labour, because the *process* would not be visible in that moment. While historians may tell their stories without the benefit (or bias) of much theory, many sociologists construct their theories and tell *their* stories as if time (and space) were not important determinants of the outcome.

[21] At first glance it can seem that Bourdieu is offering nothing more than a Weberian explanation of status group – different classes have different tastes in food, music and art. But Bourdieu offers a theory that shows that cultural reproduction matches the reproduction of social classes, which has traditionally been supposed to be "caused" by the economic domain. One of Bourdieu's contributions is to show that members of subordinate classes are doubly indemnified because they are economically constrained and culturally bereft, at least in terms of the "culture" that matters.

space, extending from past practices and the space on which present and future ones are acted out. Fields are objective arenas in which struggles for material resources, cultural capital and academic achievement are played out.

Bourdieu seeks to augment Marx's explanation of class dynamics and social reproduction by explaining the reproduction of class structures without resorting to an economic explanation. Habitus is the relationship between an individual and their social class. Members of a social class share "tastes," which are systems of classification, appreciation, judgments, perceptions and behavior. Taste is a marker of what *appears* to be "natural" superiority, while it is in fact *social* superiority acquired in the family of origin. Bourdieu argues that taste is tied to social class in all aspects of life – humor, art, sports, theatre, clothing, music and food (Bourdieu 1984). People believe that these markers of lifestyle are universal, but Bourdieu argues that they are explicitly related to individual classes.

In France, despite equality of access to the educational system, which is supposed to "level the playing field," class privilege is reproduced because the only place to attain the most powerful tastes, or the most powerful *habitus* is in the family of origin. The class system remains closed because class membership is not determined by academic achievement, but by an individual's *habitus,* which is acquired from a lifetime of socialization to particular tastes. Perception and cognition are rooted in and structured by social class. We perceive the world through socially structured categories, and a person's *habitus* reflects this acquired way of being in the world. Each individual internalizes principles that guide them in all situations, so that they know how to dress and how to make judgments and classifications.

The focus on social reproduction has led Bourdieu and other contemporary

social theorists to under-theorize the production of new behaviors and action. For Bourdieu, new behavior and action is the objective result of the intersection between the actor's position in a *field* and his *habitus* acquired over the course of a lifetime. The production of a cultural product, and its subsequent meaning or value come from the relationship between habitus and field. Bourdieu credits any individual creativity which is not obviously attributable to the relationship between field and habitus to the realm of the unpredictable, to that which is not sociological, and cannot be theorized.

For Bourdieu, sociology *can* offer an account of cultural production by focusing on the production of value (Bourdieu 1993, 139). The substantive products of the producers are not terribly important. Bourdieu focuses on the field of cultural production, and "inseparably from this, the relationship between the field of production and the field of consumers" (1993, 141). Cultural products are created by the field as a whole. The individual artist, writer (or scientist) is only the apparent cause. However, the group is not the "cause" of the production either, "it is the *field of artistic production as a whole"* which creates value (1993, 142).

> What is called 'creation' is the encounter between a socially constituted *habitus* and a particular position that is already instituted or *possible* in the division of the labour of cultural production. The labour through which the artist makes his work and, inseparably from this, makes himself as an artist (and, when it is part of the demands of the field, as an original, individual artist) can be described as the dialectical relationship between his 'post', which often exists prior to him and outlives him (entailing obligations, such as 'the artist's life', attributes, traditions, modes of expression, etc.), and his *habitus*, which more or less totally predisposes him to occupy that post or -- and this may be one of the prerequisites inscribed in the post -- more or less completely to transform it (1993a, 141).

Even when Bourdieu deals with the production of new cultural products, for him the proper object of sociological analysis is the relationship between interrelated spaces, that of the products and the producers (1993a, 145). Individual, autonomous

works do not exist for Bourdieu.[22] For example, in a work of art, the value is not due to the uniqueness of the work, as is commonly believed, but rather due to "the collective belief in the value of the producer and his product" (1993a, 147). What "makes" a reputation and produces value is the field of production. The concept of field is "the system of objective relations between these agents or institutions and ... the site of the struggles for the monopoly of the power to consecrate ... the value of works of art and belief in that value" (1993b, 78). The "field" of the art world is the relationships between the people with the power to consecrate the status of "art" upon an artwork, and the power to determine its value. Bourdieu makes it clear that this power does not reside in any intrinsic or aesthetic properties of the art itself. His is a schema which does not need an agent: the individual does not confer meaning or value on new cultural products.

> The source of 'creative' power, the ineffable *mana* or charisma celebrated by the tradition, need not be sought anywhere other than in the field, i.e., in the system of objective relations which constitute it, in the struggles of which it is the site and in the specific form of energy or capital which is generated there (1993, 81).

Although Bourdieu is critical of structuralism as a method (1990, 9), and he proposes his theory of *habitus* as a concept which combines agency and structure, in the end, what animates his actors is the combination between their habitus and their position

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[22] Of course, I do not want to argue that the value placed on works of art are autonomous, but I do want a space for individual action and agency in the creation of new knowledge, such as cultural products.

in a field. The theory defaults to a structuralist explanation of the relationship between agency and structure.[23]

f) Agency and Structure – What are the Useful Concepts?

By demonstrating that one of the products of human knowledge making practices is the creation of new social structure, I draw on a conception of the actor as having freedom to act, but not in a completely unconstrained manner. The actors' agency is constrained by their environment, their own personal capacities, and by the natural world with which he or she interacts, thus limiting the ways in which he or she could have "acted differently." Agency is also constrained by existing social structures, which the actor may be unintentionally creating, or intentionally trying to change.

Human agency and action are almost elided by Bourdieu because he discounts the complexity of the "practical or conscious evaluation" (1993, 81) which individuals undertake when they act. In Bourdieu's theory movement and change are determined once we know the positioning of an actor in a field, and therefore it is difficult to specify how human agency contributes to change or the genesis of new structure. This was not Bourdieu's intention, as he spent much of his career grappling with the relationship between the freedom of individual action and the constraining power of social structures. On the other hand, the language of Giddens' structuration

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[23] Bourdieu's theory can be used to explain social change when there is a clearly defined field of action, and when the agents in that field are groups that are polarized in very different ways, seeking to meet different goals, and motivated by differing deep seated interests. In his book *Impure Science,* Steven Epstein (1996) adeptly uses Bourdieu to show how AIDS activists were instrumental in democratizing the epistemic practices of doctors and the Food and Drug Administration in conducting clinical trials of medications to treat HIV. What makes Bourdieu's theory more helpful in Epstein's story is not that there were no strong individual actors in his story – there were many. Epstein shows that knowledge development in the AIDS epidemic did not "follow pathways common to science," and that AIDS research can only be understood as taking place on a very broad, public field which is highly contested.

theory is misleadingly seductive for anyone seeking to understand how new social structure is produced. For Giddens social structure is created as an intended or unintended result of human agency. It seems to promise a language for talking about how human agency constructs the social world, and how in turn that social world acts back on human actors by enabling or constraining their agency. In Giddens' view structural sociology's flaw is to assume that human agency can be ignored, as if humans automatically just do and know how to do whatever underlying structures compel them to do. Giddens' actors are intelligent, knowledgeable and capable in their use of rules and resources to bring about their goals. Their action has unintended consequences, and it is through these that structure is built. In the last instance, Giddens defaults to the power of the agent in constructing social structure, but his conceptualization of structure is somewhat vague.

4) A note on Science and Sociology – Is Scientific Knowledge Outside the Realm of Sociological Inquiry?

One of the problems with using sociological theory for the analysis proposed here is that even contemporary social theorists accord the natural sciences a special epistemic status. In doing this, they continue a long tradition of setting the content of scientific knowledge outside the boundaries of what social theory can address appropriately. Contemporary social theorists continue to place a divide between the natural and the social sciences, conferring a different status upon the objects and processes of investigation in the natural sciences than those of the social sciences. Bourdieu calls science an "exceptional social field," and although it is still subject to the general laws which govern all fields, science is "different" and "special." Science is set apart because "those who have a share in it [the scientific field] have an interest in truth, instead of having, as in other games, the truth which suits their interests"

(1975, 31). On Bourdieu's view the natural sciences are special because they seek a non-relativist truth, not a relative truth which can change according to an actor's interests and position in a given academic field.

Giddens also sets science outside the boundaries of sociological inquiry. His concept of the double hermeneutic "refers to the way the structures of the social world were constructed originally by human agents, whereas those of nature were not. In this respect, therefore, a qualitative ontological difference exists between nature and society" (Morrow 1994, 156). Giddens sees a dichotomy where social reality is "constructed," while natural reality is not. I argue that the *materiality* of the natural world is not created in the same way that social structures are -- mountains are given, they are an obdurate physical feature of the natural world. Money has a physical existence, but it is created by humans, and its meaning comes from the web of social relations in which it is embedded. Giddens tries to draw a dichotomy between the objects of the natural world and the "objects" of the social world, claiming that they are ontologically different. Like many sociologists, Giddens uses this difference to set science, particularly the content and form of scientific knowledge, outside the purview of sociological investigation.

Social theorists tend to accept the idea of a universal "scientific method" and many sociologists strive to follow this elusive method, in the hope that it will make the products of their research more scientific (and eligible for the best journals). However, recent research is dismantling this iconic belief in one scientific method. In her book *Epistemic Cultures* (1999) Karin Knorr Cetina shows that different sciences have different cultures of knowing. There is no such thing as one or *the* "scientific method." Through extensive ethnographic research into the social worlds of high energy physics and molecular biology, Knorr Cetina shows that each science has a particular

way of producing knowledge. She contrasts the epistemic cultures of the two sciences by arguing that in molecular biology, the individual scientist *becomes* the measurement instrument: their bodies become highly trained in manipulating instruments and their vision becomes increasingly disciplined over the course of a career. The individual biography of a particular scientist will affect their skill set and their modes of interpretation. In contrast, in high energy physics, the group is the unit that conducts experiments – individual physicists have little to do with a particular experiment, and thus the biographical experience, or the tacit bodily knowledge of any given scientist is irrelevant. Experiments may take years, and involve hundreds of people. And, perhaps most importantly, individual scientists are not able to see or interpret the results of an experiment. All interpretation is done by computer. Knorr Cetina uses the contrasts between these two sciences to argue that there is no one scientific method, no one epistemic culture, but rather that each science has its own epistemological conventions and rules.

I argue that the knowledge attained, the meanings given, to our manipulations of the natural world -- what we "know" or hold to be "true" about nature – is constituted in the same way as the meanings (or knowledge) given to social entitities. The analytic divide that sociology places between the natural and the social sciences is not tenable, as the social and the natural sciences both go through similar processes to create knowledge.

5) New Tools for Sociological Theory: The Sociology of Scientific Knowledge

The agency-structure debates are an old, central problem in social theory and the search continues for the "link" between the two levels of analysis. Here, I utilize concepts from the discipline of Science Studies, which encompasses a variety of approaches to the study of science. All share a commitment to explaining scientific

knowledge as the result of the social activity of human beings. Knowledge claims are the result of practical streams of human activity. Therefore, stable scientific knowledge is the outcome of the interface between human agency and nature. That agency is situated: it is exercised, constrained and enabled within institutional, historical and structural settings. Following are some of the theoretical constructs drawn from science studies which are used to show how the stabilization, standardization and dissemination of credible knowledge claims about DNA profiling occurred through the exercise of human agency and the establishment of social structure.

a) Knowledge, Belief, Trust and Credibility: The Edinburgh School of the Sociology of Scientific Knowledge

The theoretical perspective I take towards science is deeply informed by the Edinburgh School of the Sociology of Scientific Knowledge (SSK). In a powerful series of moves in the 1970s, founding member Barry Barnes shifted or dropped entirely many of the problematics of the sociology of knowledge.[24] Barnes' work has provided the theoretical foundation for the empirical case studies undertaken by the "Edinburgh School" of SSK during its heyday in the 1970s and 1980s.[25]

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[24] Barnes (1977) notes correctly that while Mannheim (1936) attempted to advance a social model of knowledge, ultimately he continued to believe in the contemplative model by granting special status to scientific and mathematical knowledge. The Edinburgh school changed the definition of knowledge to "generally accepted belief," and in so doing set aside Mannheim's problematic, and the linking of social groups or processes with different *forms* of knowledge fell out of Barnes' theoretical model. These moves resulted in a subtle shift of categories, such that while SSK practitioners often use the terms "sociology of knowledge" and "sociology of scientific knowledge" interchangeably -- without the girding provided by Scheler, Mannheim and Marx, sociology of knowledge after Barnes is not the same project as before. Conceptual categories have been shifted, as have the theoretical resources available for sociological explanations of scientific knowledge.

[25] The main "members" of the Edinburgh School include Barry Barnes, David Bloor, Steven Shapin, David Edge and Donald MacKenzie. The work of Andrew Pickering, and the early work of John Law, John Dean, Bill Harvey and Jonathan Harwood are also considered part of the Edinburgh legacy.

The work of Edinburgh SSK is grounded in the following claims or assumptions. First, they define knowledge simply as "accepted belief," and as "publicly available, shared representations" (Barnes 1977, 1). This definition of knowledge challenges the received view of scientific knowledge in which "knowledge" is conflated to "scientific knowledge." The challenge for a group advancing a knowledge claim is to have that claim accepted as widely as possible beyond the group. If it is not accepted, it does not attain the status of knowledge. This problem arose for the first National Research Council's Committee on forensic DNA, as it was unable to have its solution to the problem of random match probabilities attain the status of accepted belief among scientists, and thus it could not attain the status of knowledge.[26]

A second major tenet is that on Edinburgh's view science is only one form of human cultural activity that produces knowledge. Like all cultural activities, the processes by which science produces knowledge are irreducibly social. The Edinburgh School replaces the traditional contemplative model of knowledge, based on *theoria,* where truth is seen as being produced by the isolated individual knower passively and independently apprehending nature through sense experience, with a more active conception of knowledge which is rooted in practical activity. Edinburgh SSK offers a model of science as a collective, situated, practical human activity. Science belongs to the realm of action and praxis, not to the realm of contemplation

[26] Because DNA profiles are not unique to individuals, a probability that the suspect's DNA profile could have come from another person (chosen at random) must be given, so that the jury knows how likely it is the suspect's DNA and that found at the crime scene, or on the victim, truly identify "this" human being.

and *theoria*.[27] All knowledge is the outcome of situated human activity, and all human groups are groups of knowledge makers. During the process of stabilizing knowledge about DNA profiling, several groups tried to produce knowledge with varying degrees of success: the two National Research Council Committees, the FBI, and the Technical Working Group on DNA Methods. Very prominent individual scientists such as Richard Lewontin, Kenneth Kidd and Eric Lander also threw their hats into the fray in attempts to have their beliefs become generally accepted.

The Edinburgh School holds that all knowledge is generated by people who are trying to meet a range of goals or interests. Some of these interests may be deep, and not explicit or even apparent to the individual. Thus they claim that all knowledge is "interested," or the result of interests. It is important to understand that this does not mean that all knowledge is biased, but instead that all knowledge serves an interest, or deep rooted need, of human beings.[28] Knowledge is produced and evaluated "not

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[27] The distinction between *theoria* and *praxis* is an old one, going back to Greek philosophy. *Theoria* holds that the properly philosophical knowledge of things comes from looking, from contemplation. It is a model of science that says that rational and certain knowledge come from contemplation, from an individual knower. It supports the view of scientist as a lone hero, which characterizes much of the history of science of the first part of the twentieth century. Histories of science based on this model are written from the perspective that science changes due to internal, rational causes -- science changes because theories change. That which is social and can affect science is relegated to the external, outside of the knowledge making process. Social factors are seen as producing error and bias, and in this type of account of science, "interests" are usually interpreted as class interests, as an external thing "pushing" science around and biasing it (Shapin 1982).

[28] Barnes used the understanding, that knowledge does not come in different varieties, to set aside the problematics of critical theory as well, specifically Habermas' notion of knowledge interests (Habermas 1968, 1987). Barnes discarded the entire idea of universal knowledge interests, and specifically challenged Habermas' notion that historical/hermeneutic inquiry can provide practical knowledge oriented to "getting along" in society. Barnes argued that all knowledge, including historical, is as instrumental as scientific knowledge (1977, 15). The idea of specific universal knowledge interests disappeared from SSK through Barnes' argument that all knowledge, however it is developed, is oriented to prediction and control (1977, 15-16). After Barnes, different types of rationality, or the construing and linking of particular transcendental knowledge interests to forms of human action were no longer tenable ideas for the sociologists of scientific knowledge. The concept of "interests" was reinterpreted and utilized very differently.

by passively perceiving individuals, but by interacting social groups engaged in particular activities" (Barnes 1977, 2). The claim that all knowledge is "interested" is both one of the most fundamental tenets of SSK, and for many historians and philosophers of science, its most contentious claim.

An example of interest driven knowledge making activity is the FBI's desire, and concomitant efforts, to establish a national databank of DNA profiles. This was an important goal for the FBI, better identified as a deep-seated institutional interest. It arose almost before DNA profiling was discovered, in the early days of molecular biology (Budowle 1997, Personal interview). In the analysis that follows, it will become apparent that all of the FBI's activities aimed towards producing and stabilizing knowledge about DNA were in some way oriented towards the goal of establishing a national DNA databank.

The case studies of the Edinburgh school utilize the four tenets of the "strong programme" in the sociology of knowledge established by David Bloor ([1976] 1991, 7), of which the most enduring is the commitment to symmetry in causal explanations of belief. Bloor's tenets are first, that sociological explanations should be causal, and "concerned with the conditions which bring about belief or states of knowledge." Bloor (1982) argues that there is no contradiction between knowledge being grounded in reality and its being shaped by social conventions and purposes. Drawing on Durkheim and Douglas, he argues that any group's knowledge is ordered in and as its society is ordered. Different purposes and different histories lead to groups making different classifications when apprehending the same natural world. Bloor invokes studies of the history of science to understand why "we" have made the classifications that we have, whereas other groups, confronting the same natural

world, have made different classifications.[29]

The second tenet of the strong programme is that explanations should be non-evaluative or impartial with respect to whether the beliefs are true or false, rational or irrational, successes or failures (Bloor [1976] 1991, 7). The strong programme holds that all beliefs require explanation. The third tenet, which is perhaps the most contentious and widely cited, is that of "symmetry" in explanations of the origins of knowledge. Bloor argues that the same kinds of causes must explain both true and false beliefs.[30] The fourth tenet is that of reflexivity, meaning that the patterns of explanations offered by the strong programme would also apply to its own explanations.[31]

Barnes (1972) borrowed the concept of "normal practice" from Kuhn (1971). Normal scientific practice is the taken for granted way that the scientific community goes on (see also Collins 1985). Dogma, authority and socialization are not sources of error for Kuhn, they are central to the "normal" practice of science. Barnes thus makes the case that normal practice is open to traditional sociological inquiry. One of

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[29] See Dean (1979) as an example of two groups of botanists that classify the same bits of botanical reality differently, given their different technical training, and thus different "interests." Dean argues that "classification is a process of invention rather than discovery, that our classifications of the natural world are 'made' rather than 'found'. If this is the case, then in an important, indeed fundamental sense, classifications of the natural world have the status of *conventions* and are thus sustained and modified in response to changing patterns of social contingencies" (1979, 212; see also Bulmer (1967) as another example of how other groups classify the natural world differently than we do).

[30] The tenet of symmetry runs up against an entrenched asymmetry in the philosophy of science, in traditional sociology of knowledge, and in everyday culture. The taken for granted notion is that what is erroneous needs causal explanation, while the "reality" of the natural world accounts for truth. Traditional sociology of knowledge sets scientific knowledge outside of the scope of sociological inquiry (Mannheim 1936) and invokes the "social," i.e., interests, or bias, only to explain error, ideology, or false beliefs. True beliefs require no "social" explanation because "nature" has spoken.

[31] Malcolm Ashmore (1989) and Steve Woolgar (1988) have written extensively on reflexivity and the sociology of scientific knowledge.

the strongest contributions of early Edinburgh SSK is that scientific knowledge is locally produced through mundane practices (Shapin 1995, 304).[32]

b) Trust and Credibility

In *The Social History of Truth* Steven Shapin argues that far from ridding ourselves of dependence on the credibility and trustworthiness of individuals in our scientific knowledge claims, "no knowledge making practice has accomplished the rejection of testimony and authority, and ... no cultural practice recognizable as such could do so. [He sets himself] the dual task of showing the ineradicable role of what others tell us and of saying how reliance upon testimony achieves invisibility in certain intellectual practices" (1996, xxv). No scientific claim "shines with its own light," or "carries its credibility with it" (1995, 305). Knowledge claims are accepted as true or rejected as false on the basis of "specific processes of argumentation and political action." Trust and credibility are *the* foundations upon which all knowledge claims are built. Science is not *less* trusting or more skeptical than other institutions. Instead, science is *more* trusting (Shapin 1994).

Shapin also argues that what is held to be true about nature bears an enormous moral significance. Arguments about what *is* in the world are very potent resources for establishing what *ought* to be. Mary Douglas contends that the credibility for a knowledge claim to be used in an "ought" debate rests on its status as an "is" ([1978] 1982). "Knowledge is a collective good," meaning that we are dependent on others for the credibility of all knowledge claims. "Trust" is the word that

[32] The Edinburgh School is particularly well known for "controversy studies." Some of the now classic investigations in this area are Steven Shapin's "The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes" (1979); Donald MacKenzie and Barry Barnes, "Scientific Judgment: The Biometry-Mendelism Controversy" (1979); Steven Shapin and Simon Schaffer, *Leviathan and the Air Pump: Hobbes, Boyle and the Experimental Life* (1985).

Shapin uses to indicate the moral character of the relationships which are necessary to hold collective knowledge (1994, xxv).

c) Replication and its Problems

While Durkheim argued that social facts are social categories, the sociology of scientific knowledge takes the position that scientific facts are also social categories (Shapin and Schaffer 1985). As such, scientific facts are items of public, not private knowledge. A fact is made by replicating private sensory experiences in forums in which they are publicly witnessed and become agreed upon facts of nature. This is done by replication, which is "the set of technologies which transforms what counts as belief into what counts as knowledge" (Shapin and Schaffer 1985, 225). A physical replication of the experiment is necessary, but not sufficient for the establishment of a fact. Replications of this type occurred when the FBI and TWGDAM were trying to validate DNA profiling techniques. The validation protocol was a series of experiments to determine the characteristics of DNA in different circumstances. For example – it was not known whether DNA could be extracted from hair. Validation studies established that hair does not contain DNA, but the root of a hair does. Validation studies established that the DNA found in semen is the same as DNA found in blood or saliva. None of these things were known when the FBI and TWGDAM began their investigations. Replication also includes the "virtual witnessing" offered by literary technologies such as publication. Today, in scientific circles, this usually occurs in peer reviewed journals. In a case known as the *Yee* case the FBI lost credibility because they were making knowledge claims in the courtroom but the defense pointed out that they had not published any of their findings in peer-reviewed journals.

In the book *Leviathan and the Air Pump* Steven Shapin and Simon Schaffer (1985) argue that what counts as "replication" and what counts as a "matter of fact" are problematic and inextricably woven together. They draw on Harry Collins' (1984) concept of the "experimenter's regress" and on the negotiations that were supposed to get Shapin and Schaffer's 17th century natural philosophers out of this regress. Conceptually, Shapin and Schaffer begin their explanation of the regress with the observation that Robert Boyle was attempting to make an air pump. His claims about the phenomena associated with the pump could be made into "facts" if others could successfully replicate an air pump. That is, other people had to produce a working pump for the relevant community to agree that what Boyle said was happening was indeed happening. However, to do this, they needed to get a pump that "worked." However, the catch is that the only way to know if their pump "worked" was to get it to do what Boyle's had done. Those wanting to contest, or replicate Boyle's claims had to use his phenomena to "calibrate" their own machines. "To be able to produce such phenomena would mean that a new machine could be counted as a good one" (226). Another catch is that before an experimenter could make the judgment that his machine was working "well," "he would have to accept Boyle's phenomena as matters of fact...and before he could accept those phenomena as matters of fact, he would have to know that his machine would work well" (226).[33] The experimenter's regress is quite a circular phenomenon, and Collins argues that it pervades the practice of science.

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[33] For studies that emphasize the achievement of closure in scientific controversies, see Engelhardt and Caplan (1987).

d) Social Worlds Theory -- Science as Work

Social worlds theory comes out of the work of Anselm Strauss and his students, and from the sociology of work. "Social worlds" are "groups with shared commitments to certain activities, sharing resources of many kinds to achieve their goals, and building shared ideologies about how to go about their business" (Clarke 1991, 131). Their boundaries are not geographical or organizational, but communicational, and are defined by the "limits of effective communication" (Clarke 1991, 131).[34]

As with other science studies perspectives, social worlds theory battled the received view of traditional philosophy of science which elides the situated human labor that goes into making observations, claims and judgments (Star 1991, 267). Work is the link between the material world and the scientific abstractions made to bring order to that world. Susan Leigh Star argues that human labor links the visible and the invisible:

> Visibles are not automatically organized into pre-given abstractions. Someone does the ordering, someone living in a visible world. Every ordering is itself anchored in a series of contingencies, and every anchoring embeds a patterning that can be viewed theoretically (Star, 1991, 265-6).

An example of how social worlds intersect and affect each other is found in the first case in which DNA profiling evidence was declared to be inadmissible. In the 1989 "Castro case" the integrity of DNA profiling as a knowledge claim was challenged.

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[34] Social worlds theory has its foundations in Chicago school interactionism and the work of Anselm Strauss. Its major contributors in the field of science studies are Adele Clarke, Susan Leigh Star, Joan Fujimura, James Griesemer and Eli Gerson.

Sociologist of science Sheila Jasanoff (1995) notes that Eric Lander, one of the expert witnesses for the defense, was successful in showing that DNA profiling was deeply dependent on local practices and local standards – practices and standards that were kept under wraps in the private laboratories of Lifecodes and Cellmark (Lander 1989). Once DNA profiling was destabilized and lost its credibility, it took the labor of many people from disparate social worlds to stabilize it and restore its credibility and truth status.

e) DNA Profiles as Boundary Objects

The concept of "boundary objects" was introduced by Susan Leigh Star and James Griesemer (1989) in a history of Berkeley's Museum of Vertebrate Zoology. Boundary objects are entities that exist in several different social worlds at once, serving different needs and fulfilling different purposes for each group, while retaining enough cohesiveness to maintain an identity across the different worlds. Boundary objects are a means of translating the disparate viewpoints between groups. Each group recognizes enough of the object's identity that the object can act as a translation point for differing interests and goals.[35] Star and Griesemer's argument is based on the premise that scientific work is heterogeneous in that it is done by extremely different groups of actors, and cooperation between these groups is often required. In conducting the work, people from different groups often encounter objects that have a slightly different meaning for each of them. Each social world has a partial

[35] For example, in detailing the history of Berkeley's Museum of Vertebrate Zoology, Star and Griesemer show how the state of California functioned as a boundary object for the different groups, it "live[d] in multiple social worlds and [had] different identities in each" (1989, 409). The different groups involved shared the goal of conserving California and nature, and of classifying variation in the natural world. Their argument is based on the premise that scientific work is heterogeneous, in other words that it is done by extremely different groups of actors, and cooperation between these groups is often required. In conducting the work, people from different groups often encounter objects that have a slightly different meaning for each of them.

claim to the resources of the object, "and mismatches caused by the overlap become problems for negotiation" (Star and Griesemer 1989, 412). It is particularly important that a boundary object is an entity that can cross social worlds, thus linking their disparate interests together and serving as a common point of reference. In their museum case study Star and Griesemer argue that the work involved in creating boundary objects -- which are scientific objects -- is done by a variety of people, including scientists, collectors, and administrators. The objects formed "a common boundary between worlds by inhabiting them both simultaneously" (Star and Griesemer 1989, 412).

The main point to be taken from social worlds theory is that the dichotomies through which we view and experience the world, and the scientific objects which inhabit it, are created through situated human labor done by individuals who often inhabit more than one social world. Each group may have differing goals and interests. One of the tasks of the sociologist of science is to make visible the invisible labor by which these dichotomies and entities are constructed and maintained.

f) Classification and Standards: A Way out of the Laboratory

In their book *Sorting Things Out*, Geoff Bowker and Leigh Star (1999) argue that classification is a basic human activity. In our own personal lives, we all have systems of classification – foods that are appropriate for breakfast but not for dinner, the inbox demanding immediate attention, but never receives it, a list of "things to do" that grows longer, but never shorter. A grocery list is a classification of items needed to keep the bodies in your home fed for another week. There are more formal systems of classification with which we are all familiar. Who does not remember their undergraduate student ID number or their social security number? These are parts of

sometimes huge information infrastructures by which institutions and the government keep track of us, and our behavior. Bowker and Star argue that most of our classification systems are so well known to us that we can no longer identify them as classification systems – they appear to us as natural order: as knowledge. However, classification has a history as long as humanity itself. For example, in 1629, after proposing that using the decimal system would allow all quantities to be numerated to infinity, in a single language (that of numbers), Descartes also proposed that a language that would contain and categorize all human thought was needed. Englishman John Wilkins (1614-1672) took up the task, and began by dividing the world into forty categories (Borges 1964, 102). The "ambiguities, redundancies and deficiencies" of Wilkins' system brought to Borges' mind another system of classification: one attributed by Dr. Franz Kuhn to a Chinese encyclopedia called the "Celestial Emporium of Benevolent Knowledge" (Borges 1964, 102). If you doubt the naturalizing power of our own classifications, consider making your way about in a world which classifies animals in the following way:

> (a) those that belong to the emperor, (b) embalmed ones, (c) those that are trained, (d) suckling pigs, (e) mermaids, (f) fabulous ones, (g) stray dogs, (h) those that are included in this classification, (i) those that tremble as if they were mad, (j) innumerable ones, (k) those drawn with a very fine camel's hair brush, (l) others, (m) those that have just broken a flower vase, (n) those that resemble flies from a distance (Borges 1964, 102).

This delightful, unusual and somewhat maddening system of classification abruptly disrupts our idea that the categories into which we cut the natural world are the only valid or meaningful ways in which it can be divided.

In 1995, Steve Shapin argued that the focus on the local and contingent nature of knowledge production had generated a central problem for future research in SSK: how does knowledge escape the laboratory (Shapin 1995, 307)? Shapin argued that standardization and measurement are the way in which knowledge claims escape the local and mundane methods and locales of their production.[36] However, measurement, and standardization do not come without a cost. "[E]xpensive and labour intensive" metrological practices are required to "move results achieved in the lab out into the world" (O'Connell 1993, 129). Chapter Six details the extensive, expensive and labor intensive practices which were required to stabilize and standardize DNA profiling so that it could disseminate outwards from the FBI's laboratories into crime laboratories across North America.

If classification, standards and measurement are the "way out" of the laboratory, and one way by which scientific knowledge travels and becomes entwined in our lives then the empirical question arises of how, in whose person, and by what forms of labor this occurs. Bowker and Star argue that before aggregated information can be communicated, it has to be classified (Bowker 1998, 259). DNA as classification is a good example of what Bruno Latour calls mutable mobiles

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[36] Shapin has also suggested that another way knowledge gets "out of the laboratory" is by becoming an "obligatory passage point" (Callon and Latour 1981). When others must use, cite or purchase the knowledge claim, then it is an obligatory passage point which others must pass through to attain their own goals (Callon and Latour, 1981).

Bruno Latour's defining aporia is the eradication of dichotomies in all forms. His overarching argument is that we cannot use the "great divides," such as human/non-human, nature/society, rational/primitive, micro/macro as explanations for our knowledge, because the divides themselves are what have to be accounted for (Latour 1993). Having dissolved the divides, in their place Latour puts networks of varying strength. "Truth" exists when a strong, stabilized network which links the interests of human and non-humans together is established. This emphasis on truth by fiat is a perspective which has come to be called "actor-network theory." Truth is the outcome of a controversy, not what settles the controversy. "Nature," "truth," or the ontology of an entity is determined when the controversy is settled. Nature does not close the controversy, it is the outcome of the controversy (Latour 1987, 94-100).

becoming immutable mobiles (Latour 1987).[37] DNA from a crime scene – a blood soaked spot on a concrete sidewalk, the semen left behind on the victim's clothing, blood or saliva collected from the alleged suspect – as each step as these samples move through the chain of custody, through the process of DNA profiling, they are transformed from mutable, changeable and movable samples to immutable numbers (molecular weights) which can go anywhere, and be instantly exchanged with any crime laboratory in the country.

Geoff Bowker and Leigh Star are among those who took up the challenge to make our invisible categories visible. They note that:

> [S]tandards and classifications, however imbricated in our lives, are ordinarily invisible. The formal, bureaucratic ones trail behind them the entourage of permits, forms, numerals, and the sometimes-visible work of people who adjust them to make organizations run smoothly. In that sense, they may become more visible, especially when they break down or become objects of contention. But what *are* these categories? Who makes them, and who may change them? When and why do they become visible? How do they spread? What, for instance, is the relationship among locally generated categories, tailored to the particular space of a bathroom cabinet, and the commodified, elaborate, expensive ones generated by medical diagnoses, government regulatory bodies, and pharmaceutical firms? (Bowker and Star 1999, 3).

Bowker and Star (1998) identify two types of classification: Aristotelian and prototypical.[38] The third form of classification, considered to be the strongest is

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[37] Bruno Latour argues that one of the things scientific work produces is inscriptions which record the details of a scientific investigation. Once numbers – measurements, readouts, regression results – are transferred to paper, they are mobile and can escape the confines of the laboratory. Once inscribed on paper, he believes they are no longer changeable, and calls them "immutable mobiles." According to Latour, scientific knowledge travels through the circulation of immutable mobiles (1987).

[38] Aristotelian classification proceeds by way of sets of binary characteristics, which the item either does or does not possess. In Bowker and Star's example of the classification of a pen, it would have to exhibit enough characteristics to separate it from a population of pens, balls and bottles. If we wanted to distinguish the item "pen" from other writing implements such as pencils, crayons, fountain pens and felt markers, we would need to add more binary characteristics to our classification scheme, so that ultimately, each item could fall into one,

genetic classification, or classification by origin (not classification by DNA).[39] As noted above, from the moment the possibility of identifying individuals by this penultimate form of classification by origin -- actual DNA in the form of the DNA profile -- the Federal Bureau of Investigation had the classification of "dangerous" individuals on its agenda. CODIS, the Combined DNA Indexing System, which is a computerized, international databank of the DNA profiles of convicted felons, and profiles of unknown DNA from violent crimes, was a gleam in the eye of the bureaucracy before the databank had a name, before DNA profiling procedures were standardized, before controversies over random match probabilities were begun, let alone solved -- indeed almost before the procedure was invented.[40]

Standards are not the same as classifications. Bowker and Star (1999) define standards as a set of agreed upon rules for the production of material or textual objects which span more than one community of practice (or locale). Standards have temporality -- they persist in time, and they are used to make things work together over distance and differences in measurement schemes. Standards are enforced by

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and only one, unique category. They go on to show that there is evidence that in "real life," our classification schemes are not binary, but are quite fuzzy. Prototype theory holds that we have in our minds a broad example of what a chair, or a dog is. We hold these broad examples in our minds while we try to adduce whether the object under consideration has any direct physical or metaphorical links to the image we hold in our minds of "chair," or "dog" (Bowker and Star 1998, 256).

[39] "[R]ocks might be metamorphic or sedimentary, languages might be Indo-European or Nilotic" (Bowker and Star 1999, 276). However, a DNA profile is at present, in our culture, the "ultimate" genetic classification. Sorting people by their DNA -- the key to life itself, the "holy grail" of genetics -- called the "Code of Codes" by molecular biologists Daniel Kevles and Leroy Hood (1992) -- is the penultimate system of ontological classification.

[40] In a personal interview with the head of the FBI's Forensic Science Research and Training Centre, director Dr. Bruce Budowle told me that the agency's interest in DNA profiling pre-dated the discovery of the technique. They had Dr. Carl Merrill out to the agency to speak on DNA classification in 1984, and awarded him a contract before Dr. Alec Jeffreys developed DNA fingerprinting in 1985 (Jeffreys et. al., 1985).

legal bodies (in my terms, social structure) – without a means of legal enforcement, a proclamation of a standard will fail. Bowker and Star caution that the best standard does not always win, and that standards can be difficult to change because they possess a significant amount of "inertia" (13).

Porter (1995) argues that translating properties of nature into numbers is a way to facilitate communication between groups – numbers are a language that many groups share. A DNA profile reduces the biological identity of an individual into a string of numbers: the molecular weights of the bands at each locus, or site along the molecule of DNA. The historical problem addressed in this dissertation is that although no two individuals, except identical twins, share the same pattern of DNA, two people could share the same DNA *profile*, although the probability is very low. The question of how low that probability is was not easy to answer.

Systems of classification, such as the International Classification of Diseases, "provide a stabilizing force between the natural and the social worlds. They hold in place sets of arrangements that allow one to read the natural as stable and the social as tightly linked to it. For the ICD, this means describing disease in a way that folds the socially and legally contingent into the classification system itself and so naturalizes it" (Bowker 1998, 272). Timmermans and Berg (1997) argue that standardization does not always require a central actor, and quite often does not have one – they view the achievement of universality as a "*distributed* activity" (Timmermans and Berg 1997, 275).

In the history of DNA profiling, computerized systems were developed to read the band lengths. These computerized systems were supposed to be more accurate than comparing band lengths by eye. Research on such "decision tools" shows that getting them to work in one place for one person is a huge amount of work. Making

the tool work for everyone in one location, or many people in many locations is even more difficult. Getting tools like this to work usually requires that the work practices within which the decision tool is to be embedded have to be changed to adjust to the limitations and "needs" of the tool (Berg 1997). ACORN is a computerized medical decision tool developed in the UK. Its purpose is to help nurses to decide whether patients with chest pain should be admitted immediately to emergency, referred to their doctors, or sent home. Berg argues that "getting a decision-support tool to function in particular medical practices involves a thorough and specific transformation of these practices" (Berg 1997, 79). On the witness stand in the *Castro* case, lab director Dr. Michael Baird confessed that the Lifecodes laboratory did not use their computerized system to determine whether fragments matched – he said all determinations of a match were made by eye (Lander 1989). One possibility for relying on visual judgments instead of using the computer could have been because people could not, or were not supported in changing their practices to match the requirements of the computerized decision tool. The FBI also developed its own computerized measurement tool, and they had more success using it than did Lifecodes (Budowle et. al. 1991; Budowle et. al. 1992).

Getting techniques such as standard DNA protocols to work in basically the same way for all the practitioners involved usually requires a huge amount of on-site "tinkering." In the case of DNA profiling, some of this tinkering was done at the FBI's Forensic Science Research and Training Center in Quantico, Virginia, and some was done when the members of TWGDAM returned to their home laboratories after meeting in Quantico. In attempting to make the chemical part of the process simpler, cheaper, more robust and less sensitive to individual variation, the members of the FSRTC and TWGDAM "played" with the different parts in the chemical extraction of

DNA from samples. Bruce Budowle, head of the FBI's FSRTC, said that they were able to remove many of the more volatile chemicals and "sensitive" steps in the extraction process. Some of the chemicals dropped from the process were expensive, and so ultimately, simplifying the protocols made the process more accessible to smaller, under-funded laboratories (Budowle 1997, Personal interview). Some of the chemicals that were dropped were extremely sensitive to inter-user variability – they were not very stable, and no amount of local tinkering could make them robust enough to transport between laboratories.

Monica Casper and Adele Clarke (1998) chronicle the huge amount of "tinkering" required to make the Pap smear into a reliable tool for the screening of cervical cancer. For the pap smear, "tinkering" included "gendering the division of labour, juggling costs, exploring alternative screening technologies, pushing for regulation of laboratories, and settling for locally-negotiated orders of clinical accuracy instead of global standardization, still elusive today" (1998, 255). In different ways, each of these steps also occurred in the standardization of DNA profiling. In an attempt to ensure some kind of continuity between laboratories in the interpretation of DNA profiles, TWGDAM laid out a hierarchy of who within a laboratory could "see" and "interpret" the final product. They laid down minimum educational requirements for laboratory technicians, and included a "grandfather" clause which allowed those trained at the FBI, but who lacked the professional credentials required of newer entrants, to interpret and write reports on DNA profiles. In other words, regardless of their education, people who had been exposed to the original "tinkering" and could pass it on were included in the community of those who were allowed to see and interpret autorads. In the guidelines for laboratory proficiency (which were legislated in the DNA Identification Grants Act of 1994), TWGDAM required that the person

doing the reading and interpreting of DNA profiles spend time at the FBI's lab in Quantico, learning firsthand how to conduct the FBI/TWGDAM protocol (TWGDAM 1989; TWGDAM 1991).

Laboratory regulation, particularly with regard to proficiency, was a huge part of the work undertaken by TWGDAM once they had settled on standard interpretations for DNA profiles. In the case of the Pap smear, users eventually settled for "locally negotiated orders of clinical accuracy." With DNA profiles, despite all the work that went into establishing how to "correctly" interpret a profile, there is still variability in interpretation. This is intrinsic to the nature of a DNA profile, which requires a skilled and trained eye to interpret. Pamela Newall, head of one of two of Canada's private forensic laboratories (the Federal Forensic Sciences laboratory in Toronto) said that one of the biggest benefits of belonging to TWGDAM was that it provided a community of practitioners, all trained in the same place with the same methods, that one can send a profile to, then call up on the telephone and say "what do *you* think?" (Newall 1999, Personal interview). What does seem to have been achieved in the case of DNA profiling is that if a laboratory has at least one person who has spent time in Quantico, and the laboratory uses the chemicals in TWGDAM's protocol, uses their restriction enzymes and analyzes the DNA at the same sites, then a DNA profile can be translated into a set of numbers that are transferable between laboratories. These numbers are the molecular weights of the bands at the different allele sites, and they are what is stored in CODIS. So, despite inevitable local variation involved in the production of DNA profiles, the procedure is quite robust. It is important to remember that this "robustness" was the outcome of a lot of work done in the late 1980s, by many individuals in crime laboratories across the country and at the FSRTC.

g) Summary

The analysis you are about to read can be interpreted partly as a story of the power and hegemony of the FBI. However, that institutional power and hegemony is tempered by the actions of individuals from other institutions. Much of the work done to stabilize and standardize DNA profiling was done by TWGDAM, which is a voluntary community of crime laboratory practitioners. However, it is true that TWGDAM would not have existed if the FBI had not poured tremendous material resources into flying people to Quantico four times a year to meet and form a true community.

Virtually every crime laboratory in the United States uses the protocols advanced, advocated and developed by the FBI and TWGDAM. The FBI and TWGDAM were legislated as the "watchdogs" over new developments in DNA profiling.

Could it have been otherwise? Would we want it to be otherwise? Every story which involves institutions, individuals, nature and money could have turned out differently.[41] Early in the history of DNA profiling in the U.S., there was a brief window, when DNA profiling was being offered only by two companies, Lifecodes and Cellmark, that the private corporations may have been able to obtain a monopoly on the technology. But to this day the FBI purchases many of the chemicals in its protocol from Lifecodes, which either developed them or holds the proprietary rights to them. As noted above, the FBI had deep institutional and technical interests in having the history turn out the way it did, and it had deep pockets. To a great extent,

[41] Literature in the area of the social construction of technology emphasizes the contingent nature of technological development. Bijker and Law point out that there is no internal logic that "drives" innovation (1992, 8). When studying innovation, the social world, the path of history, and technology are "treated as rather messy contingencies" (Bijker and Law 1992, 8).

it was through the material resources of the FBI that DNA profiles done in different parts of the country can be compared to each other. Had each crime laboratory developed their own protocols, and done their own testing and validation, each county or city would have its own "metric" for a DNA profile. If this were the case, trying to identify someone by their DNA would be difficult, because DNA profiles in different jurisdictions would be different. The scenario might resemble the example of the cathedral at Chartres, which Bowker and Star point out was built without standard measures. In those days, each cathedral town posted its local metric at the town gate, and traveling builders calibrated their instruments to match the local metric. They also note that many cathedrals fell down (Turnbull 1993, cited in Bowker and Star 1999, 14)! To the extent that we, as members of a society, feel that it is a social "good" to be able to uniquely identify individuals -- particularly those who may have committed crimes, or who have disappeared unexpectedly, or died violently -- then perhaps the story of the power and hegemony of the FBI is not a bad one.

6) Data and Methodology

Primary data for this study were drawn from a wide range of sources. These include in-person and telephone interviews with most of the key individuals involved in the stabilization and standardization of DNA profiling. In-person interviews were conducted in the spring of 1997 in Boston, New Haven, Philadelphia, Baltimore, and Washington, D.C. Following the publication of the second National Research Council report on DNA fingerprinting in 1996, a second round of telephone interviews were conducted with members of the second committee, members of the FBI, and members of the Technical Working Group on DNA Methods.

Other primary sources include the reports of the two NRC Committees on DNA profiling, technical articles from the academic literature, "News and Comment"

articles and letters to the editor from the technical journals *Nature, Science* and *The American Journal of Human Genetics*. Also included are transcripts of two Congressional Hearings (held in 1989 and 1991), copies of the 1994 DNA Identification Grants Act and the Innocence Protection Act of 2001, and videotapes of an early two-week training seminar at the Forensic Science Research and Training Centre at the FBI in Quantico, Virginia. The FBI published extensively on their "validation protocol" in their in-house journal *The Crime Laboratory Digest*.

The analysis was conducted using qualitative sociological and historical methods. Participant observation in the genetics laboratory of Dr. John Bell, at the University of Alberta in the summer of 1993 gave me an introduction to molecular biology, which greatly assisted in understanding the technical issues involved in DNA profiling. I acquired information about DNA, its structure, and the protocols, methods and tools used to extract DNA from biological material. I gained an understanding of the tools used to analyze and manipulate DNA at the molecular level, including restriction enzymes, gel electrophoresis, Southern blotting, making autoradiograms, polymerase chain reaction (PCR), and how to read and interpret an autoradiogram of DNA sequences.

I interpreted the events and the meanings of the events surrounding the DNA Wars, the FBI's validation protocols, the two National Research Council committees and the key legal cases from the perspective of the participants involved. These first-hand accounts of events were a valuable source of information about what occurred, especially in the two National Research Council committees, as these transcripts are forever banned from pubic purview. There was also a huge number of documents available, some primary sources, such as transcripts from court cases, and expert witness reports. I examined these materials as historical documents, using a

historiographical perspective on science informed by a science studies approach (see Shapin 1995 for an example). I have argued that science studies provides new tools with which to look at the relationship between agency and structure. By not defining *a priori* what counts as knowledge, and allowing the people involved to define the terms of engagement, I follow the knowledge making process across many different social worlds. In this history knowledge making activities are found in courtrooms; in committees of experts who meet in person, and correspond through e-mail and snail mail and telephone; in FBI laboratories, in interactions between groups of professionals working out problems of knowledge. In all cases, working out the problems of knowledge resulted in new forms of structure, or social order.

7) Secondary Literature on DNA and Science Studies

There is very little secondary literature in the social sciences on DNA profiling. I have not found an academic history of DNA profiling in the extant literature.[42] In this sense, the recounting of historical events, albeit interpreted from a science studies perspective, provides a contribution to our fount of knowledge about the development of a new tool in forensic science.

Of the secondary literature which does exist, the most important is probably the collection in the October-December 1998 issue of the journal *Social Studies of Science*, in which most of the articles engaged the intersection of DNA profiling, science and the law. The articles tend to focus on the O. J. Simpson trial, which began at the end of January 1995, and lasted for nine months. DNA profiling received a lot of media attention in the case, and editors Michael Lynch and Sheila Jasanoff

[42] There are at least two popular accounts of DNA profiling: *The Blooding,* by journalist Joseph Wambaugh (1989); and *And The Blood Cried Out*, by Manhattan prosecuting attorney Harlan Levy (1996).

admit that the O. J. trial was not the most important, or even the most interesting trial involving DNA evidence. It was, however, the most widely publicized, and as such it provided an extremely well documented body of evidence from which to examine the credibility of scientific evidence.

Writing in 1998, from the vantage point of a stabilized technology, Lynch and Jasanoff say that the probability that a DNA profile would match somebody else's DNA "can be calculated" (677). The subject of much of this dissertation is to show and elaborate on the labor and interactions that went into achieving an agreed upon method for calculating random match probabilities. If Lynch and Jasanoff had been writing near the end of 1992, they would not have been able to say the random match probability "can be calculated." They would have had to say something like "the correct procedure for calculating random match probabilities is in dispute at this time."

Lynch and Jasanoff note that racial themes played a dominant role in the trial while "questions about forensic science took a back seat." They note that "Simpson was an African American, the victims were white, a key Los Angeles Police Department detective was successfully branded with racist motives during the trial (681). Race was one of the important, but slippery issues in the "forensic science" that was backgrounded in the OJ trial. Other ways to divide the American population for the purposes of forming a reference group for calculating a random match probability arose in the controversies surrounding the forensic uses of DNA technology. Suggestions ranged from dividing frequency distributions of VNTR alleles by race, population, language and geography. When the Simpson trial began, Lynch, Jasanoff and their co-authors were already deeply embedded in investigations of the intersection of science and the law, "the credibility of expert testimony, the transformation of laboratory techniques into forensic tools, and historical comparisons

between DNA 'fingerprinting' and its established namesake" (681). The Simpson trial and its documentation on videotape was a "windfall" for the researchers. While they note that the OJ case became a case about race, for the researchers the OJ case was used for the ways in which it could inform the questions they were already investigating. These had to do with the credibility of scientific evidence in the courtroom, not racial issues.

Arthur Daemmrich's (1998) paper comes closest to my own analysis of the relationship between agency and structure in the proliferation of this technology. Daemmrich argues that the credibility of the DNA profile required an extensive infrastructure of law enforcement and forensic institutions and practices. I concur and take Daemmrich's analysis one step further. I argue that the credibility of DNA profiling, and what helped to close the controversies was the entrenchment of knowledge about DNA profiling into the very fabric of our formal social structure – i.e., legislation. As will be explored later in the dissertation, the 1994 DNA Identification Grants Act not only set aside money to be disbursed to create laboratories for the forensic DNA analysis, but it created a DNA Advisory Board, headed by the Director of the FBI, and which included members of the fledgling professional group TWGDAM (Technical Working Group on DNA Methods).

8) Summary

Trust and credibility have emerged as the twin foundations upon which knowledge is based. Steve Shapin claimed that in the 17th century, trust in "gentlemen" was necessary for a claim to attain knowledge status. He claims it is still true in the 20th century, that no knowledge claim "shines by its own light" or stands by itself. *All* knowledge claims still are processed through the testimony of at least one credible person. Further, Shapin claims that in the 17th century and today, solving

problems of knowledge required solving problems of order. This study asks in part, whether this still holds true for the 20th century.

The concepts drawn from science studies help to tell a story about the creation of social structure out of the agency of individuals. Some of these individuals were placed within powerful institutions, which helped them to mobilize resources to fulfill personal and institutional interests. The dynamics of agency show partly in the way that each group created order within itself – that is, how it constituted itself as a community. This was a precursor to the use or creation of knowledge. This was true for individual courtrooms, as well as the National Research Council committees, and the FBI. The reader will see that part of the reason that the first National Research Council failed to create stable knowledge is that they were unable to constitute themselves as a community – until their very last meeting they remained an unruly bunch of individuals, working against each other. Trust, shown in the establishment of credibility, allows us to have and hold knowledge. The deep claim for sociology is that this trust is also the fabric of social order (Shapin 1996).

Social structure is formed over time, in different social worlds where actors dance intricately in and out of each other's home spheres in their attempts to have and to hold knowledge. There is usually some kind of boundary object involved which has enough cohesiveness to maintain its identity in many spaces and in the hands of many actors. Stabilization, standardization – of whatever is the subject matter – is the way "out" of the world in which the knowledge began.

Chapters Three to Six contain the detailed historical analysis which allows me to take the first step in my theoretical project, which is to use a complex scientific controversy to demonstrate a link between agency and the formation of social structure. In the conclusion I will move one step closer to that elusive intersection,

discussing the ways in which knowledge and social order are mutually constitutive. Chapter Three begins with the discovery of DNA profiling, and follows it through the *Castro* case, which severely damaged the scientific credibility of the new technology. At the end of Chapter Three, the social world of the courts have handed academia a new problem to solve. Chapter Four tells the story of how the academic community dealt with solving the problems involved with DNA profiling: basically by declaring war on each other. This period in the history of DNA profiling is known as the "DNA Wars," as scientists met each other in the courtroom and rushed back to their academic offices to write up heated and vitriolic prose, that they then published in peer reviewed journals. Chapter Four also tells the story of the first National Research Council committee, and why it was fraught with such tension, and how that tension impinged on its ability to solve the problem before it. Chapter Five continues with the fallout from the first NRC committee, and tells how the second NRC committee was constituted differently from the first. I also advance some hypotheses about why its suggested solutions to the problems of the random match probability were accepted, while those of the first NRC committee were not. Chapter Six tells of the efforts of the FBI. In time, this chapter runs parallel to all the preceding chapters, chronicling the FBI's efforts from 1985 to the turn of the century. In this chapter we see the formation of the Technical Working Group on DNA Methods, the simplification, stabilization, and standardization of DNA profiling protocols, the working out of standards of interpretation and proficiency, and the formation of strong group identity, leading to professionalization. In Chapter Seven I present the social structures which were formed as manifest or latent results of all the social activity in Chapters Three to Six. But before the story can begin, it would be helpful to know just how we *got* to DNA profiling. How did personal identity become a problem, and why does it continue to be

of interest? The chapter also introduces the technical details of the DNA profiling procedure discussed in this analysis. We turn now to situating DNA profiling from a number of different perspectives, disciplines and time periods.

Chapter Two

DNA Profiling: How Did we Get Here?

1) Historicizing Identification

One way in which this inter-disciplinary work can be viewed is as being about identity.[1] Most societies have had means of marking criminals, dating back to the biblical mark of Cain. The Goths, Lombards, and Visigoths used *décalvation* (baldening) to mark convicts, and in medieval Europe some courts used branding or mutilation. Early modern European and American courts also occasionally branded – or, more rarely, mutilated – convicts. Courts in Amsterdam presciently branded convicts on the ball of the thumb. In some cases a rudimentary system of symbolic brands even communicated the type or severity of the offense, as, for example, in the colonial East Jersey codes of 1668 and 1675, which mandated a letter "T" branded on the hand for burglary and an "R" on the forehead for the second offense, or in the famous scarlet letter "A" for adultery of Puritan New England (Cole 2001, 7). As we move towards the 20th century the identification of individuals becomes ever more entwined with formal classification systems which identify and keep track of convicted criminals, and even those suspected of crimes.

Before formal means of identification such as driver's licenses, passports, birth certificates and social security numbers became common, individual identity was

[1] The very word "identity" is a 20th century word, which became popular in the social sciences in the 1950s. The 1930s edition of the *Encyclopedia of the Social Sciences* does not have an entry for "identity," and the entry on "identification" is linked to fingerprinting and criminal investigations (Gleason 1983, 910). In the social sciences, Erik Erikson "was the key figure in putting the word into circulation" (914), and around 1940, the term was picked up by the school of sociologists known as symbolic interactionists (917). Charles Horton Cooley and George Herbert Mead spoke of the "self," and this term became translated to identity by Erving Goffman and Peter Berger (Gleason 1983, 917).

more fluid. Until the Industrial Revolution and the huge influx of people to cities, most people lived out their lives within a few miles of their birth place. Their identity was stored in the memories and experiences of the individuals with whom the majority of people lived most of their lives. When traveling far away from their places of birth, most people needed letters of identification from credible sources, marked with some reliable mark establishing the identity of the person vouching for the traveler (Cole 2001).

An early case of identity theft is the infamous case of Martin Guerre, which has not fallen out of the collective memory since it occurred in the mid-16th century. It has been the inspiration for a play, two novels, and an operetta, in addition to the 20th century movie *The Return of Martin Guerre*, which was based on Natalie Zemon Davis' 1983 academic historical account of the events which transpired in the mid-sixteenth century. Briefly, the story is that after the peasant Martin Guerre had been absent from his village and his marriage bed for eight years, a man claiming to be Martin Guerre "returned" home. His wife, Bertrande de Rols, accepted Arnauld du Tihl as her husband, and she quickly bore him a child. On the return of the real Martin Guerre, Arnauld du Tihl was twice brought to trial for assuming the identity of Guerre.

Both trial courts struggled with the problem of how to *prove* who a person was – how could they establish unique identity? In the absence of "hard" evidence like photographs and dental x-rays, the only physical evidence irrefutably linked to the body of the "real" Martin Guerre were a pair of wooden shoe lasts made by a village cobbler. Unlike the 20th century O.J. Simpson case (where the shoe fit!), the mold of the wooden shoes did not fit Arnauld du Tihl. However, this did not convince the courts of the fraudulent nature of his claim to Guerre's identity and the property that came with it. Of the 150 people that testified at the trials, about 60 people claimed

that they did not know whether the defendant was the real Martin Guerre. About 45 witnesses claimed that the "real" Martin Guerre had physical features differing from the defendant, and around 36 people who had known Guerre since birth swore that the defendant was indeed Martin Guerre (Davis 1983, 63-68).[2] In 1650 the imposter, Arnauld du Tihl, also known as "Pansette" (the Belly) was executed for his impersonation.

In the late 19th and early 20th century, fingerprints became a reliable means of identifying individuals.[3] They revolutionized the identification of individuals in law enforcement circles.[4] Simon Cole (2001) argues that impetus behind using fingerprints for the identification of criminals was linked with the growth of cities, and the increasing anonymity they provided.

> Business and social transactions, once based on trust, took on a new air of suspicion. People in modern cities might not be who they claimed to be. They could be anyone; they could come from anywhere. Nineteenth-century society shifted from a closely hierarchical society of ranks and orders, in which everyone knew his or her place and the place of others, into what the historian Michael Ignatieff has called a "society of strangers" (Cole, 2001, 9).

As cities grew, and penitentiaries flourished, the identification of repeat offenders became a pressing problem. Written descriptions of facial and bodily features were cumbersome, difficult to standardize, and hard to match to a convict

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[2] For more information on the case of Martin Guerre, and the contentious historiography surrounding Natalie Zemon Davis' account of the case, see the first ever forum in the American Historical Review: *AHR Forum: The Return of Martin Guerre* (May 1988, Volume 93, Issue 3) consisting of a debate between Robert Finlay and Natalie Zemon Davis.

[3] While fingerprints did not take their place as markers of criminal identity until the late 19th and early 20th century, they have a very long history. Fingerprints have been found on the rock paintings and carvings of early humans. In the 700s the Chinese used fingerprints to identify documents and clay sculptures (Inman and Rudin 2000).

[4] It was not until 1996, after the advent of DNA profiling, that the FBI introduced computerized searches of the AFIS fingerprint database (Inman and Rudin 2000).

using a name different than the one under which s/he had first been convicted. Cole argues that early fingerprint examiners, or "dactylographers," did something extraordinary. "They created a link between an individual body and a record held by the state" (2001, 4). In the 20th century, fingerprint identification became one of the most trusted types of forensic evidence in existence. Unlike the complex path which DNA profiling followed to gain credibility as a form of individual identification, Cole argues that there has never been a successful challenge to the fundamental reliability of fingerprinting (2001, 4).

Within the history of identity, the first attempt at classifying fingerprints, and the first suggestion that "finger-marks" could be used for the identification of prisoners, came from British physician Henry Faulds. Faulds published his suggestion in the journal *Nature*, while serving at Tsukiji Hospital in Tokyo during the late 1870s (Cole 2001, 73). The first criminal trial based on forensic fingerprint evidence took place in 1898, in India. The police used a classification system created in 1889 by Darwin's cousin, Sir Francis Galton, whose untested system was used to match a bloody fingerprint left at the scene of a bloody stabbing death. The fingerprint, left on a wooden box from which money had been taken, matched the prints of one of the deceased's servants. The servant had recently been imprisoned on accusations of theft, at which time his fingerprints had been taken. It was determined that the bloody print on the wooden box matched on 18 of Galton's fingerprint characteristics, and the police arrested the servant.[5]

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[5] Academic research on the history of "forensic science" is scarce. There is no academic publication under the "history of forensic science" per se. There is a 1977 doctoral dissertation on the history of "criminalistics" in the United States between 1850 and 1950 (Dillon 1977). It is not clear when the term "science" was applied to criminal investigations, nor when the term "forensic" came into use. A 1950 history of "criminalistics" (Morland 1950) and a 1966 book titled *Crime and Science* (Thorwald 1966) indicate that even in the second half of the 20th

2) A New Technique for Individual Identification

DNA typing, or DNA fingerprinting as it is often called[6], was developed in 1985

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century, "forensic science" was not the term applied to the study of criminal investigations. More recently, DNA experts Keith Inman and Norah Rudin have included a "History of Forensic Science Timeline" in their new book *Principles and Practice of Criminalistics: The Profession of Forensic Science* (2000). The timeline was supposed to be a brief footnote, and turned into three years of intensive research (Rudin, Personal communication, 2002).

The first recorded application of medical knowledge in the solving of a crime occurred in 1248, in a book published by the Chinese *Hsi DuanYu (the Washing Away of Wrongs)* which described how to distinguish drowning from strangulation (Inman and Rudin 2000). In 1784, an Englishman was convicted of murder, based on matching the torn edge of a piece of newspaper in a pistol, that matched a piece left in his pocket. In 1877 Thomas Taylor, a microscopist to U.S. Department of Agriculture advanced the idea that markings of the palms of the hands and the tips of the fingers could be used for identification in criminal cases. His idea was published in the *American Journal of Microscopy*, *Popular Science,* and *Scientific American*, but the forensic utility was not pursued from Taylor's work (Inman and Rudin 2000) In 1888 doctors in London, England examined the wound patterns of Jack the Ripper's victims with the intention of garnering information about the perpetrator of the crimes. In the 19th century tests for the presence of blood were developed; a murderer was caught by the comparison of bullets, and the detection of arsenic (toxicology) was used in a jury trial. The 19th century also saw the development of the first crystal test for hemoglobin using hemin crystals; the development of a presumptive test for blood. Photography was used to identify criminals and document evidence at crime scenes, and the first microscope with a comparison bridge was developed (Inman and Rudin 2000).

Academic forensic science may have its origins in 1902, when Swiss Professor R. A. Reiss, at the University of Lausanne, Switzerland, established a forensic science curriculum (Inman and Rudin 2000). In the early 1930s in the United States some universities began offering courses in "criminalistics" and "police science." One of the first academic departments of criminology/criminalistics was established in 1950 at the University of California at Berkeley. Also in 1950, the American Academy of Forensic Science (AAFS) was formed in Chicago. George Washington University in Washington, D.C. opened a Department of Forensics especially for the purposes of training FBI agents.

In the 20th century there were many technical advances in criminalistics, or what we now refer to as forensic science. In the 1920s, the complex patterns and tracks that gun barrels left on bullets were compared using microscopes, and in 1935 in Holland, physicist Frits Zernicke developed the interference contrast microscope. He received the Nobel Prize for this invention in 1953. Absorption-inhibition ABO blood typing was developed in 1931 and in mid-century the chemiluminescent reagent luminol was developed as a presumptive test for blood. The 20th century also saw investigation into the viability of voiceprint identification, the invention of the "Breathalyzer", used by law enforcement officials for tests of sobriety in the field; and the discovery that red blood cells were polymorphic (varied from person to person). In 1975, the Federal Rules of Evidence were enacted, and in 1985, DNA profiling was discovered by Alec Jeffries in Leceister, England. Shortly thereafter, the polymerase chain reaction (PCR) for the amplification of small amounts of DNA was applied in forensic arenas (Inman and Rudin 2000).

[6] The technique is also known as genetic fingerprinting, DNA printing, DNA profiling, DNA forensic identification testing, and DNA RFLP (restriction fragment length polymorphism) analysis. In the forensic science and population genetics literature, it is referred to as DNA typing, although when initially introduced it was referred to on a widespread basis as DNA

by Dr. Alec Jeffreys, a research fellow in genetics at the Lister Institute of Leicester University in Great Britain. Jeffreys was not looking for a technique to revolutionize forensic science, but was investigating a group of genes which produce myoglobin protein, which carry oxygen in muscle tissue (Conner 1988). While working on this topic, Jeffreys suddenly realized that he had accidentally found a technique capable of establishing unique identity from DNA samples (Connor 1988, 31). In a linguistic stroke of genius, he termed his new technique "DNA fingerprinting".[7] Jeffreys was examining portions of the genome known as "hypervariable regions", which were discovered in the United States in 1980. While most DNA patterns are the same from person to person, these hypervariable regions vary widely between individuals. However, Jeffreys had found short core sequences of DNA within the hypervariable regions that were common to the sequences. These core sequences could be used to find and tag, or "probe" the hypervariable stretches of DNA. The hypervariable regions are inherited, and every person receives half their DNA from each of their parents. By testing his procedure on the DNA of a large family where he already knew the genetic relationships, Jeffreys showed that he had found a technique that could identify who was related to whom, and realized that it could also be used to

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fingerprinting. A survey of article titles in the indexing service Current Contents shows that the biological sciences continue to refer to the technique as DNA fingerprinting, and the term "DNA typing" or "DNA profiling" has become the accepted terminology for forensic and scientific applications.

[7] It would be interesting to examine the extent to which naming the technique a "fingerprint" served to enhance its credibility as a means of establishing unique identity. It is likely that calling the technique a fingerprint from the outset "piggybacked" the credibility of the new technique on the already established credibility of traditional dermal fingerprints. In fact, DNA "fingerprints" do not uniquely identify individuals, as it is possible that other individuals share the same pattern of polymorphisms at the same sites. The technique is only able to establish a probable match between two sources of DNA, and as will be shown, most of the controversies over DNA typing occurred over the proper methods of establishing just what probability should be placed on the match between any two given DNA profiles.

establish individual identity.

Interestingly, Jeffreys was scheduled to give a public talk on his new discovery, but his daughter fell in the playground, gashed her face and had to be taken to the hospital, causing Jeffreys to miss his lecture. Had he given the lecture he would have been unable to patent his invention, because British patent law holds that any public mention of an invention before applying for a patent places it in the public domain, making it unsuitable for patenting. After the Lister Institute applied for a patent, Jeffreys introduced the technique to the scientific community through a letter to the editor of the journal *Nature* (Jeffreys, Wilson and Thein 1985; Gill, Jeffreys and Werrett 1985). Within months the Lister Institute sold the rights to the technique to the large British pharmaceutical company, ICI, which expected "to earn millions of pounds from it over the next few years" (Connor 1988, 31). ICI soon created a new company, called Cellmark Diagnostics, which quickly set up shop to carry out genetic fingerprinting for paying customers (Joyce 1988, 31).

One of the first uses of the technique was to establish the genetic relationship between a young boy living in Ghana who wanted to come to England to live with his mother. The immigration department held that the child was not the woman's son, and denied him entry to the country. Using his new technique, Jeffreys was able to prove the genetic relationship between the two, and the boy was allowed entry to the country (Connor 1988, 32). The first forensic use of the technique was in the case of *Regina vs Pitchfork* in England. Here, the test exonerated an individual who had confessed to the murder of two young girls in the village of Enderby in 1983 and 1986 when it was found that his DNA did not match the semen samples left at both crime scenes. The police in Enderby requested blood samples from 5500 men living in the area. Initially, none of the samples matched the crime scene evidence. However, it

came to light that one of the local residents, Colin Pitchfork, had bribed someone to submit a blood sample for him, using false identification. Pitchfork's DNA proved to be a "perfect match" to that of the crime scene semen. Jeffreys testified that the probability that the DNA could have come from anyone other than Pitchfork was 1 in 30 billion. Pitchfork was sentenced to two life terms in prison (Wambaugh 1989).

3) Restriction Fragment Length Polymorphisms

In the United States, the type of DNA profiling which was most frequently performed is called single locus analysis, and involves the examination of single loci at four or five different sites along an individual's DNA molecule.[8] To do this, a procedure called "restriction fragment length polymorphism" is utilized."[9] In this procedure, restriction enzymes are used to chop up long repeating sequences of DNA. The restriction fragment lengths occur on stretches of DNA that used to be called "junk" DNA, because they do not code for any proteins. Nobody is sure what biological role these long stretches of DNA play. At certain places on this "junk" DNA, there are some regions that are characterized by what are called "variable number of

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[8] DNA, or deoxyribonucleic acid, is the carrier of genetic information. It is a long thread that consists of sequences of nucleotides, which are also called "bases" or "base pairs." There are four different nucleotides, given the initials A, T, C, and G. The molecule looks somewhat like a twisted ladder, as pairs of nucleotides join across its width. Across the "ladder," A is always paired with T, and C is always paired with G. A given sequence of DNA (one strand of the ladder) may look like a four letter alphabet soup: ACCTAAAGGACT. The sequence of the nucleotides is responsible for the specific action of DNA, and it is copied in almost all cells without error (Cavalli-Sforza, Menozzi, Piazza 1994, 5). Relatively short sections of DNA have been identified as genes, and the sections of DNA that are used in DNA profiling are called "junk" DNA, because they do not seem to have any use. However, different alleles (or versions of a "gene") can be identified in these long repeating segments of DNA.

[9] The technique invented by Alec Jeffreys is known as multi-locus analysis. Multi-locus analysis probes for many different unique sequences all at the same time and provides a pattern of dozens of bands. In the United States this type of DNA profiling was offered by Cellmark Diagnostics, of Germantown, Maryland, which was a division of the parent company ICI in England. Cellmark's only competition was Lifecodes Corporation of Valhalla, New York. Lifecodes offered single locus analysis, in which the band lengths are established for one site on the DNA molecule at a time. A DNA "profile" is established by examining and establishing the band lengths at least three different sites along the genome.

tandem repeats (VNTRs). A VNTR is a region in which the same sequence of DNA repeats itself over and over again, and these occur in the special hypervariable regions examined by Alec Jeffreys. The number of tandem repeats in these hypervariable, or polymorphic regions varies widely from person to person. Each person has the same sequence of DNA, but each individual has a different number of copies, or repeats, of the VNTR sequence. The number of different repeats that an individual has is referred to as an allele, which is a version of a gene. What makes something in this region an allele is the number of different copies of the repeating pattern.

Initially, this procedure was offered only by one company, Lifecodes Corporation of Valhalla, New York, although the FBI began dedicated research into the procedure shortly after Alex Jeffreys published his letter in the journal *Nature*. Lifecodes used restriction enzymes to cut up the DNA. A restriction enzyme is a natural enzyme that recognizes, or is attracted to, specific patterns of DNA. If a known pattern of DNA occurs before one of these long repeating stretches of DNA starts, the restriction enzyme that is attracted to that pattern can be used to cut the DNA at that point. "Restriction" means that when the enzyme recognizes the sequence of DNA, it binds to it and cuts it at that point.

For each person's DNA, the restriction enzyme finds the pattern at the beginning of the long repeating stretch, and cuts it there.[10] Some people will have very long repeating stretches, perhaps 100 copies of the segment, and others will have shorter stretches, possibly 50 copies of the segment that repeats, or 10 copies.

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[10] See Joan Fujimura, *Crafting Science. A Sociohistory of the Quest for the Genetics of Cancer*, Cambridge, MA and London: Harvard University Press, 1996, 80-82 for a discussion of how restriction enzymes became easily accessible "tools" to be used in molecular biological work.

These are called restriction fragment length polymorphisms (RFLPs), which means that the lengths of the fragments that are cut (restricted) varies from person to person (polymorphism).

Once the DNA is cut up into fragments, the fragments are separated into lengths, so that they can be measured and recorded. This is done by putting a solution which contains the fragments into an agarose gel to which a difference in electrical potential is applied. DNA fragments are negatively charged, and so once a potential is applied to the gel, the fragments are attracted to the positively charged end (anode) of the gel. Smaller fragments travel further down the gel than larger fragments, and so when the current is stopped, the larger fragments remain closer to the end of the gel where they started from, and the smaller fragments are further away from the starting place.

The DNA fragments are still not visible to the naked eye, and in order to see where they stopped, the entire gel is hybridized with a radioactive marker or probe, which seeks out the particular pattern of DNA of interest (the entire gel is filled with DNA, but we are only interested in particular chunks of it). In a procedure called Southern blotting, the gel is pressed up against a nylon membrane, the DNA is transferred to this membrane, and then the membrane is immersed in a solution that contains radioactive markers, or probes, that go and find their matching bits of DNA on the gel, the bits that repeat themselves over and over (the RFLP's). The radioactive probes are attracted to the repeating lengths of interest, and when that nylon membrane is pressed against a sheet of x-ray film, the radioactive bits leave a visible picture, or "band" at a particular place, which corresponds to a particular length of fragment. Most people have two different lengths of fragments (one from each

Figure 1

The DNA Molecule

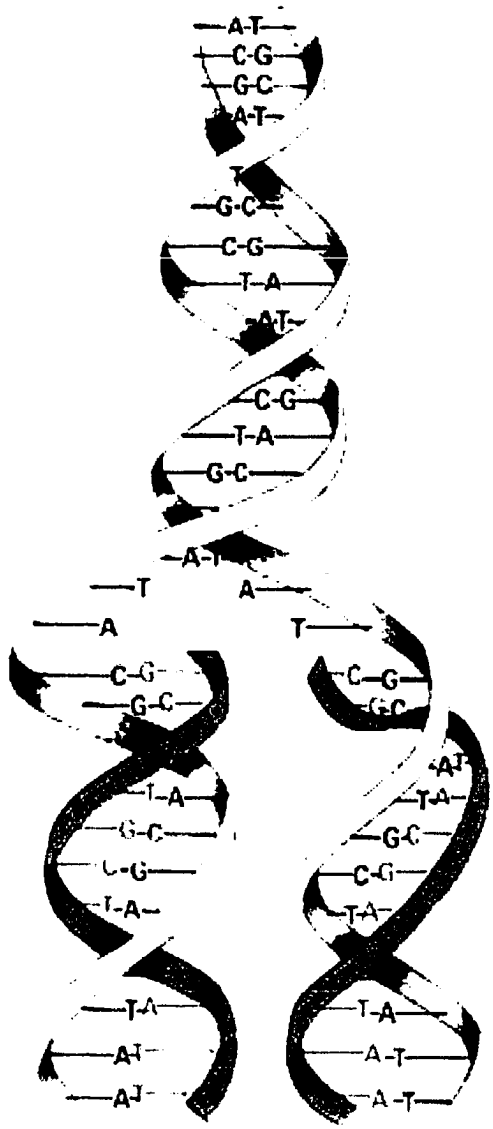

Figure 1: A graphical representation of the double helix structure of the DNA molecule, showing how the molecule can be split (denatured) into two strands, and "new" DNA added to increase the amount of DNA in a sample. This is used in the procedure known as Polymerase Chain Reaction. Figure used with permission of Genelex Corporation.

Figure 2
Example of Restriction Fragment Length Polymorphisms

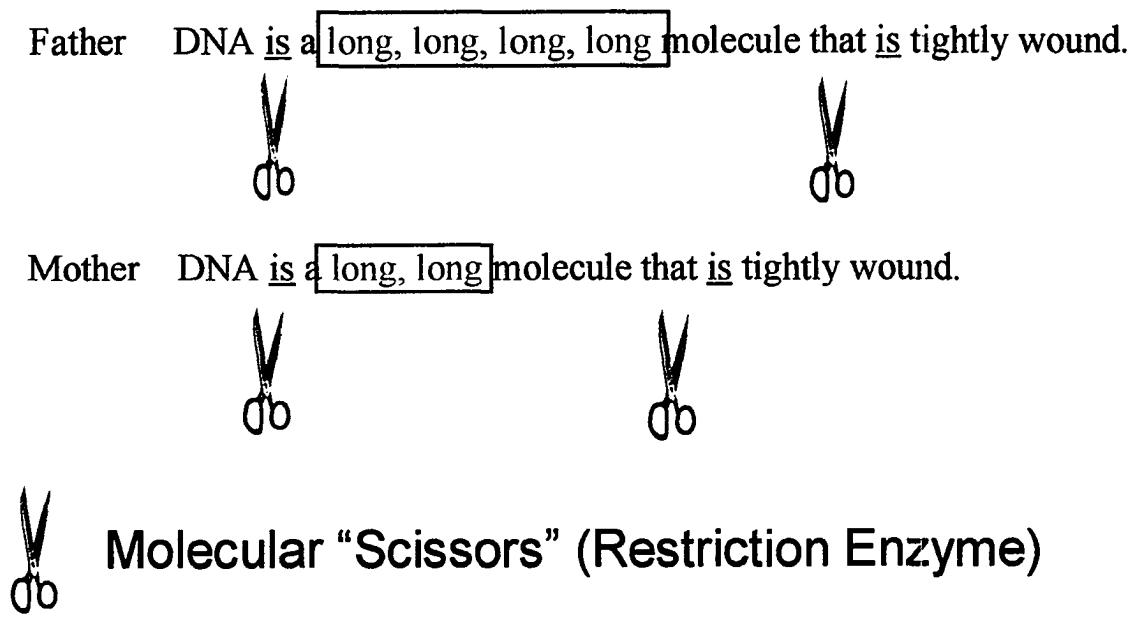

Figure 2: Diagram showing how restriction enzymes cut the DNA when they encounter specific repeating sequences of DNA. Used with permission of Genelex Corporation.

parent). The determination of length is made by looking at where the bands are, relative to the starting point of the gel and to the position of the marker DNA whose lengths are already known. The known lengths come from a "ladder" of DNA whose length measurements are known, which is run in several lanes on the same gel as the fragments of interest. This ladder serves as a ruler by which the lengths of interest can be measured by comparing where they are relative to the position of the known markers. Longer chunks are closer to the top of the gel, and smaller chunks go further down.

Once this procedure has been carried out at four or five different sites, or loci, the question still remains as to how likely it is that someone else could share this particular pattern of DNA fragments. The loci that are most useful for DNA fingerprinting are the loci that are highly polymorphic, that is, the ones that vary a lot from individual to individual.[11] Some people might have 5 repeats of the same sequence, someone else might have 10, someone else 100. And while these vary from individual to individual much more than regular genes, which usually have only one or two alleles across the entire human race, they do not vary infinitely. There are a limited number of sets of repeats across human populations, but the distribution of allele frequencies in racial and ethnic sub-populations is unknown. Because VNTRs are so highly polymorphic, if two different samples of DNA have exactly the same patterns of bands at four or five different loci, it is very likely that they are from the same person. However, in the judicial system, the knowledge that two matching DNA

[11] Scientists at Lifecodes Corporation, of Valhalla, New York, published several articles characterizing different VNTR sites in peer-reviewed journals, long before any of the population genetics controversies began (Baird et al 1986; Balazs et al 1989; Flint et al 1989; Kidd et al 1991). These issues became controversial only when the scientific evidence was challenged in courtrooms, by scientists acting as expert witnesses.

profiles are "very likely" from the same person is not good enough. The courts required that a number assessing the probability that a given match could belong to another person (termed a "random match probability"), be calculated for matches.

These probabilities were initially calculated using distributions of the frequency of VNTR alleles that were constructed by the private companies offering variants of DNA typing. (Lifecodes Corporation, Cellmark Diagnostics and Cetus Corporation). [12] These companies constructed distributions in three populations -- typically Black, Caucasian and Hispanic.[13] The frequency distributions were made by obtaining DNA samples, usually from blood, from a variety of people, and obtaining their DNA profiles, and counting how many people had which alleles, at which site. Once a frequency distribution was obtained, the probability of two people having the same fingerprint was calculated by multiplying the probabilities of having a certain allele for each of the sites. The probability of having an allele was based on how frequently that allele occurred in the frequency distribution the company had constructed, which was taken to be representative of the American population. For example, if 200 people out of 1000 in the sample have Allele A at site 1, then the probability of a person chosen at random from the population having that allele A at site 1 is 200/1000. The probabilities at each site are multiplied together, to obtain the overall probability of a random person having exactly the same alleles at all the sites as the person, or fingerprint, of interest. This is called the multiplication rule, and the probability attained is called a random match probability. It is usually a very tiny number,

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[12] Cetus Corporation offered a slightly different technique, based on PCR (polymerase chain reaction), which can take a very small quantity of DNA, such as a single hair root, and "amplify", or copy, the DNA in the sample millions of times.

[13] For a discussion of the way that racial and ethnic categories were invoked in the development of DNA profiling, see the discussion at the end of this chapter.

indicating that the probability that the DNA profile obtained could belong to anyone other than the suspect is very small.

In 1986 Lifecodes constructed their first VNTR allele frequency distribution from a "random" collection of 700 blood samples from the New York Blood Center, obtained from blood donors and from individuals seeking paternity testing. The first distributions made by Lifecodes were for only two sites, or loci (most DNA profiles were made from information obtained from at least three loci).

The first site was the flanking region of the HRAS-1 gene which had been restricted by using the *Taq*I enzyme. The other site was the *D14S1* locus, restricted with the *Eco*R1 enzyme. Lifecodes found that for all 700 people, the alleles obtained from the HRAS-1 region varied in length from 1.80 kbp (kilo, or thousand, base pairs) to 4.40 kpb. They were able to measure the size to the nearest 100 bp. They decided that at this site (HRAS-1), for this RFLP, that the fragment lengths of the 700 people could be divided into 18 distinct size groups, or bins. They divided the 700 people into three ethnic groups, and then counted up how many of each different size group, or allele, were found in each ethnic group:

> The most important features of these allele distributions are: (1) the 2.59-kbp fragment is the most common in all three groups, (2) the 2.31-kpb, 2.59-kbp, and 3.04-kbp fragments account for the bulk of the alleles in the three groups (Caucasians: 82.4%; Blacks: 68.6%; Hispanics: 76.2%), and (3) These differences in allele frequency distributions, between the various ethnic groups, are all highly significant ($P < .01$) (Baird et al 1986, 493).

Lifecodes found that allele frequencies were statistically different according to race. Some fragment lengths (alleles) were found more often in Blacks than in Caucasians: "the 2.41-kbp, 2.51-kbp, 2.75-kbp, 2.84-kbp, and 2.92-kbp fragments are found five

Figure 3

Autoradiograph of DNA Profile at One Allele

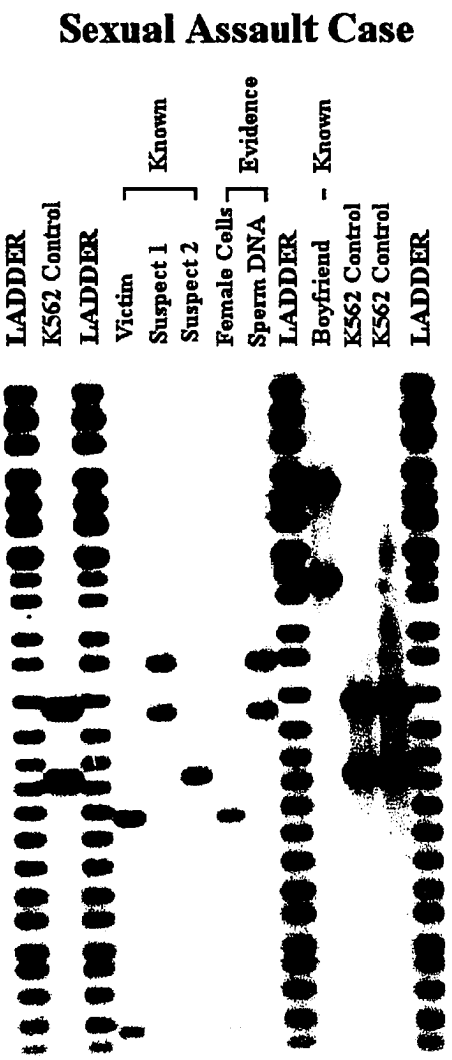

Figure 3: Photograph of an autoradiograph (autorad) from a sexual assault case. The lanes marked "ladder" are used for sizing the bands of DNA. The reader can see that the bands in the "known" column and the "evidence" column appear to match very closely, while those of the "boyfriend" are of a different length, i.e., they do not match the bands in the "evidence" column. Figure used with permission of Genelex Corporation.

Figure 4
Southern Blotting

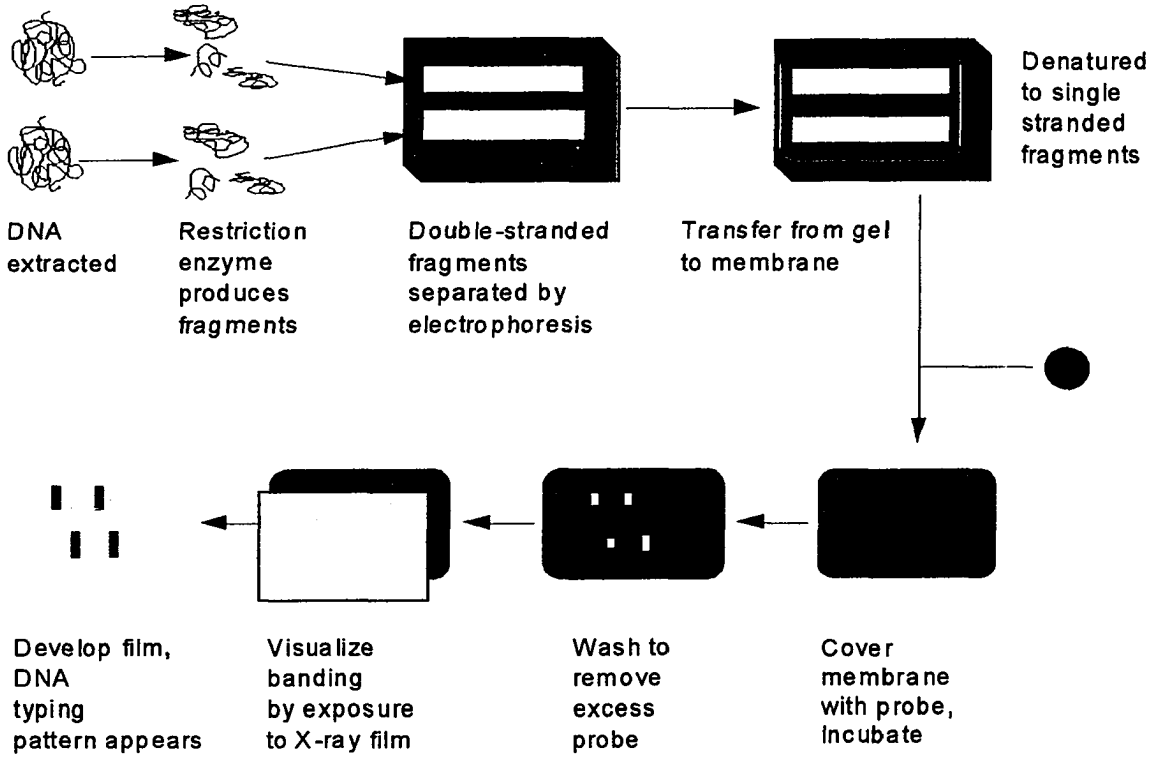

Figure 4: Illustration of the steps involved in the Southern Blotting Procedure. Figure used with permission of Genelex Corporation.

times more frequently in blacks than in Caucasians (Caucasians: 4.1%; blacks: 21.8%). In Hispanics, those alleles were present in 10.2% of the samples" (1986, 493). According to the rules of statistics, it was not just the "luck of the draw" that certain fragment lengths occurred more frequently in the Black group than in the Caucasian group. Their results show that there is only a very small chance that in a different sample of 700 people, Blacks and Whites would come up with the same frequencies of alleles. The important point is that when probabilities of a random match are calculated in a courtroom, incorrect probabilities would be obtained if the Caucasian frequency distribution was used to calculate probabilities for a Black DNA sample. This seemingly simple finding was to become the subject of much contention over the next decade, in the world of the courtroom, in the world of academic and forensic science, and for the national science advisory bodies, the National Academy of Science and the National Research Council.

4) The Puzzling Problem of Race and DNA Profiling

One of the interesting but puzzling aspects of these early debates of DNA fingerprinting procedures is the casual but inevitable invocation of racial categories. The reader will recall (from Chapter One) that because DNA "fingerprints" are not unique to individuals, the question arose as to how to assign a probability to the chance that someone else could also have the same DNA profiles at the loci that were analyzed. In the early days of DNA profiling, race was a naturalized biological category. Vials of blood and semen samples came to the laboratories with the donor's race having been self identified. Knowing the race of the person whose DNA profile they had analyzed, the companies created frequency distributions of VNTR alleles based on the most common "races" in the United States: Black, Hispanic and

Caucasian.[14] In a very taken for granted manner, they utilized these distributions and categories to calculate the random match probabilities demanded by the courts. Early in the history of DNA profiling, the private laboratory Lifecodes published articles showing that VNTR allele distributions varied by racial category (Baird et al 1986; Balazs et al 1989). Based on this evidence, it was sensible for them to calculate random match probabilities using the distribution closest to the suspect's race. With what appears to be little thought, they assumed that if the suspect was an African-American person, then the random match probability should be calculated using the Black database of VNTR allele frequencies, because they had shown that VNTR alleles occurred with different frequencies in different "racial" groups.

Once DNA profiling garnered the attention of the academic community -- particularly Richard Lewontin, Daniel Hartl, Kenneth Kidd and Ranajit Chakraborty – the role of race and which database with which to calculate random match probabilities took on a new twist at the same time as the stakes became higher. Lewontin and Hartl (1991) claimed that the distribution of VNTR alleles in the world was not known, let alone how they varied by racial group. They argued further that the category of Hispanic was a linguistic category and a "biological hodgepodge" (Lewontin and Hartl 1991), and racial distributions labeled "Hispanic" were therefore meaningless. The FBI responded by dividing their frequency distributions of Hispanics into Southwest and Southeast Hispanics (thus dividing a linguistic category by geographical location).

Chakraborty and Kidd (1991) advanced an argument from a totally different perspective. In their *Science* article they suggested that there was no reason to

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[14] It is interesting, and inexplicable, that the racial category of "Asian" is never mentioned in any of the discussions of race or sub-populations.

assume that the "random" person came from the same ethnic or racial background as the accused, and so databases of VNTR allele frequencies should not be constructed by race. They felt that random match probabilities should be calculated from a general database representative of the potential perpetrators, which, they argued, could be anyone in the United States at the time of the crime.[15]

This was the space in which the controversies over the role of race in DNA profiling took place. No one, including the FBI, who used their own agents to create their database, thought deeply about how to classify people by race or ethnicity. As the debate heated up in the academic community, drawing the attention of population geneticists and statisticians, it also garnered the attention of legal scholars (Lempert 1997, Kaye 1993), who kept tabs on the academic debate, courtroom decisions, and the repercussions of the National Research Council's first report (National Research Council 1992).

The first NRC committee tried to provide a method for calculating the random match probability that would circumvent the whole issue of race.[16] The second committee effectively argued away the issue, by showing that no matter which database was used, the random match probabilities obtained were tiny, and the differences between probabilities obtained from different databases were too small to be forensically significant.

Over the course of my research, it became clear that this debate in the forensic, academic and legal arenas interfaced in perplexing ways with the extant

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[15] See also Lempert (1993) who echoed this belief. Richard Lempert was a member of the first NRC Committee on the forensic uses of DNA profiling.

[16] Their solution is called the "ceiling principle." It is explained in detail in Chapter Four and a definition can be found in the Glossary in Appendix A.

definitions and understandings of race. The problem of race as it was raised in the DNA Wars is perhaps most confusing in the informal, taken for granted way in which some actors treated it. Because of the very subtle way in which race was invoked by the private laboratories, and later by the FBI, its importance became clear only near the end of my investigation.

What became clear is that for the scientists involved, race had a biological ontology, and whether that ontology was referred to in terms of "race" or "population" is immaterial – it contradicts definitions of the concept that were in play for other social groups at the time. In most academic disciplines had been accepted for a long time as a social, rather than a biological category. After World War II, the United Nations Educational, Scientific and Cultural Organization (UNESCO) pronounced that there was "no biological evidence to support claims of racial superiority" (Reardon 2001, 362). With this proclamation race lost its status as a *scientific* category, and became a concept of *sociological* interest.[17] Scientists who study genetic diversity now generally refer to "populations," rather than "races," but this switch in terminology did not solve the problems. Jennifer Reardon asks, "[d]oes 'population' refer to a real biological object? Should 'population' be defined by geography, culture, language or biology? Does 'population' also refer to an identifiable social group?" (2001, 362).

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[17] L. Luca Cavalli-Sforza is probably the most eminent population geneticist in the world. In his book *The History and Geography of Human Genes* (1994), he and co-authors Paolo Menozzi and Alberto Piazza discuss the history of the concept of races, and the scientific failure of the concept (19). They note that "[h]uman races are still extremely unstable entities in the hands of modern taxonomists, who define from 3 to 60 or more races" (19), and they show that the categories that they use to provide definitive global allele frequencies by population are "completely arbitrary." They argue that "[a]lthough there is no doubt that there is only one human species, there are clearly no objective reasons for stopping at any particular level of taxonomic splitting" (19). They further note that to some extent, the way populations are defined depends on whether the person doing the classification is a "splitter" or a "lumper" (19).

It is important to note that although race is a social category, it still plays an important role in science and scientific concepts. "We invented race and crime, but we did not invent them out of nothing; our categories have a history and rely on prior practices and understandings" (Root 2000, S630). In the United States, the history of race as a social classification has a peculiarly American trajectory which had its genesis in American social organization and historical bodies of belief:

> Our categories of race are our choice rather than nature's but are not chosen as we might choose a password or select a seat on an empty bus. We divide ourselves by race as part of our speculations about human origins, in light of an African slave trade and as part of a long-standing interest in comparing the worth of different kinds of people (p. S630).

In the article which set off the DNA Wars, Richard Lewontin and Daniel Hartl (1991) suggested that the problem of gaining empirical knowledge of the distribution of VNTR alleles across difference racial and ethnic groups in the United States could be solved by studying 15 to 20 "genetically pure" populations. They never defined how such entities would be defined, let alone found and sampled. In the same vein, scientists studying the global distribution of genetic diversity in a project called the "Diversity Project" encountered the same problem:

> Populations ... did not exist out there in the world for researchers to pick from for their studies, but rather had to be put together for the purpose of answering the Project's particular questions. Further, defining these populations would involve managing the boundaries between 'society', 'culture,' and 'biology'. Like populations, these boundaries did not exist in the world, but would have to be produced together (co-produced) and managed (Reardon 2001, 366).

When research on this dissertation was started, the Human Genome Project was in its heyday. As this investigation proceeded, it became clear that questions about the correct procedures for calculating random match probabilities were interwoven in complex and vexing ways with the concepts of race and population.

However, it also became very clear after many interviews and reading the published literature, that the concepts of race, and perhaps less the concept of population, held an unexamined, naturalized meaning for most of the actors involved. Despite questioning in interviews about how racial categories in allele frequency distributions were allocated, no more deep or meaningful response than "the race was on the vial of blood," or some similar statement could be elicited from the actors. In the following pages, it will become clear that race has a complex ontology for the actors involved – some seek out racial diversity, some claim a lack of diversity, and some said that the extent of diversity was just a big unknown.

It is only one of many deep paradoxes in this history that the same actors who debated bitterly over which allele frequency distribution, categorized by race, ethnicity, linguistics or geography should be used in calculating a random match probability, could not speak more deeply of the formation of the categories than that "they were on the blood vials from the blood bank." However deserving of deeper inquiry the complex role and meanings of race in the history of DNA profiling may be, the data obtained in this study – interviews, publications, news and comment pieces from scientific journals, transcripts of hearings and commissions – did not support a deeper analysis into the concepts of race and population. This is a deep and important issue, and it appears that in scientific circles, the ontology of race is not as "closed" as natural or social scientists would like to believe.

5) Lawyers Intervening in the History of DNA Profiling

There is clearly another story to be told which involves another group of people for whom a *scientific* stamp of approval on the technology was essential. The wars happening in academia did not go unnoticed by judges. Some defense attorneys who were on point, or linked to the DNA Task Force, pointed out the

academic controversies, and called highly credentialed academic witnesses to testify against the DNA evidence. In some cases, judges rejected the technology on the grounds that it had not attained general acceptance in the scientific community.

The interface between the worlds of the law, the courtroom, and science is not the main focus of this dissertation, but it is a thread that weaves the different aspects of the history together. From the perspective of the law, the history of DNA profiling in the courtroom is one part of the larger issue of how to utilize and determine the probative value of complex scientific information in the courts. Judges' expertise does not usually lie in the realm of science, but scientific evidence, often very complex, is playing an increasingly large role in the courts (Berger 1999, Personal interview). Lawyers are finding more and more often that to appropriately defend their clients, they must learn to understand technical scientific information. Judges have few resources with which to determine whether the prosecution's or defense's scientists are more credible or represent good, better or best science. A new trend is that in cases which involve complex or contentious scientific evidence, some judges are appointing a "court's witness," who speaks to the judge and whose role is to act as an unbiased expert interpreter and translator of the scientific evidence presented in the courtroom (Jasanoff 1998, 716).[18] Jasanoff notes that in both the United States and the UK, the practice of appointing an expert to act as an impartial witness for the

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[18] Eric Lander was called as the court's witness in the six-week *Frye* hearing brought against Bonds, Verdi and Yee: see *United States v. Yee,* 129 F.R.D. 629, 631 (N.D. Ohio 1990). The judge's report of the 1993 appeal (*United States v.John Ray Bonds, Mark Verdi and Steven Wayne Yee,* 12 F.3d 540; 1993 U.S. App. Lexis 32574; 1994 FED. App. 0085P.

> "The magistrate judge [in the original court case] then conducted a six-week Frye hearing to determine whether the proposed experts' trial testimony about the DNA evidence was based on principles generally accepted in the scientific community. During the hearing, the Government called six expert witnesses, the defendants called five expert witnesses, and the court called Dr. Eric Lander as the court's witness."

court is becoming more and more common. She believes that "[a]s a neutral 'third eye' in the traditional two-party form of litigation, the court-appointed expert might occupy a quasi-judicial position and would form, together with the party experts, a smaller 'facts' triad (a *de facto* Science Court) within the larger justice 'triad' comprised by the judge and the legal advocates (Jasanoff 1998, 732)."

There is a large body of literature written by lawyers and legal scholars on DNA profiling. This literature begins around 1990, and continues to this day as new forms of DNA profiling and databanking come online. The legal discourse on the technique takes several forms, roughly mirroring the "three waves" conceptualized by David Kaye mentioned earlier. Initially lawyers "in the know" about DNA technology took a pedagogical stance and published articles in law journals which translated the scientific aspects of the procedure for other lawyers, who were completely unfamiliar with DNA (Thompson and Ford 1989; Thompson and Ford 1991).[19] Some articles raised doubts about the utility of the technique for the courts (Shultz 1992), and others summarized the successes and failures of the new technology in the courtroom (Gass and Shultz 1992).

As the DNA Wars progressed, and the National Research Council convened its two committees on the subject, different aspects of the technology became salient for the legal community at different times. For example, after the *Castro* case concerns about the admissibility of the new evidence took center stage (Thompson and Ford 1993). A huge issue for lawyers was the differing standards utilized in different courts and states to determine the scientific validity of "new" scientific

[19] One argument about why the worlds of science and the law had so much trouble talking to each other over DNA profiling was that lawyers self select themselves *away* from careers which involve science and mathematics, towards careers that emphasize logic and rhetoric (Berger 1999, Personal interview).

knowledge. In the early 1990s, most jurisdictions in the United States still followed the 1923 *"Frye Rule"* (293 F. 1013 (D.C. Circuit 1923)), which involved the admissibility of evidence from lie detectors. This venerable rule required that a new scientific technique could only be deemed admissible as evidence if it were "generally accepted" by the relevant scientific community (Thompson and Ford 1989, 53-60). In *Frye v. United States* (1923) the court held that expert testimony which relates to novel scientific evidence must satisfy special foundational conditions which are not applicable to other types of expert testimony. Specifically, in the *Frye* case the judge declared that

> Just when a scientific principle or discovery crosses the line between the experimental and demonstrable stages is difficult to define. Somewhere in the twilight zone the evidential force of the principle must be recognized, and while the courts will go a long way in admitting expert testimony deduced from a well-recognized scientific principle or discovery, *the thing from which the deduction is made must be sufficiently established to have gained general acceptance in the particular field to which it belongs* (Frye v. United States 1923, original emphasis; 1014).

In some states the *Frye* rule was being replaced by the newer "relevancy" standard, based on the Federal Rules of Evidence 702. This rule holds that a novel scientific technique need *not* have gained general acceptance in the relevant scientific community, but only that testimony from an expert would aid the jury in reaching a decision (Hoeffel 1990, 507-19). The relevancy standard gave tremendous discretion to judges to determine who constituted an "expert" in the field, and what counted as "relevant." The strength of the *Frye* rule in courts was further diminished by the 1993 decision in *Daubert v. Merrell Dow Pharmaceuticals, Inc.* (113 S. Ct. 2786 (1993)). In the *Daubert* case it was determined that the *Frye* standard of general acceptance had been superseded by the Federal Rules of Evidence 702. While the *Daubert* case did uphold what seems like the looser standards of the relevancy test, it still required that

while the scientific information need not be "generally accepted" in the scientific community, it must be scientifically valid, and also be of some assistance to the jury. While *Daubert* is binding only on Federal courts, it may influence courts at the state level (Thompson 1993, 31).

6) Science Studies Approaches to "Science and the Law"

In the courtroom it is hoped that science will provide decisions with a firm and authoritative foundation -- one that cannot be questioned. Smith and Wynne note "[t]hat the science often appears equivocal is put down to procedural problems rather than inherent properties of scientific knowledge or methods" (1989, 1). Law professor and member of the second NRC committee Margaret Berger argues that in the criminal justice system there is no mechanism for saying "we don't know what the truth is," and waiting to for it to be determined (Berger 1999, Personal interview). Some determination of truth must be made for that specific time, place and circumstance. The courts look to science and scientific expertise to provide an objective foundation for making these necessary decisions. "Ought" decisions in the courtroom are based on the perceived epistemic status of "is" claims in science. This apparent discrepancy between the logic of truth attainment in the two different social worlds arises many times during the DNA Wars, especially after the *Castro* case and the release of the first NRC report.

It is true that the attainment of truth in a courtroom versus a laboratory is very different. However, I argue that it is not so much the *logic* that is different, but the *practices* and the web of meanings into which evidence falls in each social world that differ. There are boundaries and limitations on the truth seeking process imposed by the needs of the justice system. Despite its special status in the western world, science is like any other activity in life: scientists strive to create adequate

conclusions from inadequate premises (Wynne 1989). This means that ultimately, the attainment of truth in a scientific world is not *logically* different from its attainment in the world of law. Roger Smith and Brian Wynne argue that the courtroom is a much more complex social world than that of science (Smith and Wynne 1989, 7). Courtrooms are obviously constrained by time and the necessity to render a judgment as to the "truth" in the case at hand. In the courtroom for something to count as "truth," it must have a direct witness speaking for it. In the case of scientific evidence, this witness must be someone with expertise in the scientific matter under consideration. In a perverse loop, the authority of the expert witness is constructed through the process of reaching closure in court (Wynne 1989).

Lawyer, law professor, and sociologist of science Sheila Jasanoff argues that in the case of DNA profiling evidence, the expert witness serves as the 'container' or 'concealer' of "much of the behind-the-scenes work of translation, from the investigative site through the forensic laboratory into testimony at trial" (Jasanoff 1998, 716). She also contends that the dynamics of litigation obscure "the complexity of the translations by which samples, artifacts, recordings or pictures become evidence" (Jasanoff 1998, 716). When scientific knowledge is closely scrutinized its "rather informal procedural mechanisms" can be quickly deconstructed, compromising its claims to objectivity (Smith and Wynne 1989, 2). Subjecting scientific claims to inspection highlights their fragility. Jasanoff argues that all scientific claims, especially those that are implicated in legal controversies, are "highly contested, contingent on particular localized circumstances and freighted with buried presumptions about the social world in which they are deployed" (Jasanoff 1995, xiv). Truth in the courtroom is the outcome of skilful negotiation between questioner and witness. Only by being spoken in the courtroom, either in the form of utterances by an

expert witness or by documents entered into evidence, can words come to count as evidence for the truth. The legal process, i.e., the questions asked by the prosecutor or the defense attorney, and not the expert witness define what utterances are proper for the expert to utter. A "good" expert witness is "someone who can subordinate his or her technical view of the relevant question to that defined by the court" (Smith and Wynne 1989, 4). The battle over what the truth will be can depend on the skill of the questioners, or who has the best qualified or the most expert witnesses. Unfortunately, the outcome of this battle -- what is taken as the "truth" -- generally depends on funding. The defense bar is often inadequately funded to challenge DNA evidence and so often fails to have its interpretation of the truth emerge victorious (Berger 1999, Personal interview).

All decisions require the management of uncertainty (Smith and Wynne 1989, 8). Where science and the law differ on the surface is that the law has rigidly defined "rituals of precision" to adjudicate uncertainty, whereas science can sometimes proceed quite informally (Porter 1995). Scientists may work from established protocols, but these protocols are not legislated, they are free to change procedures if something is not working, or develop new protocols that work better. Porter argues that scientists often do not need to make their methods and procedures formal until they are to be used by an outside community, such as in a court case or board of inquiry. When communication between science and another social world arises, Porter contends that scientists translate properties of the natural world into numbers, because numbers are a universal language (Porter 1995, 224-231).[20]

[20] This translation of properties of the natural world into numbers is exactly what happens with a DNA profile – a swab of saliva from inside the cheek or the back of a stamp, a bloodstain on concrete, a hair with a root, are all chemically processed, the DNA extracted, and that DNA turned into a DNA profile which exists, ultimately, as a set of molecular weights, which are the

Perhaps it is most productive to view the difference in truth making practices on a continuum. Science does have formal rules and procedures, some scientists are very free to choose procedures, and live with informal estimates. Law, on the other end of the continuum, has explicitly legislated rules for proceeding to establish the truth, such as rules for cross examination and rules for what can count as evidence. It is also true that the actual *practice* of law does not coincide with the textbook application of the formally legislated rules. However, courtroom law does have clearly articulated procedures for reaching closure, which is defined as truth for that time, place and moment.

Roger Smith and Brian Wynne argue that the relationship between science and the law can be summed up in three ways. First, legal institutions socially construct what is to count as factual scientific knowledge. Second, science has an inherent fragility when it is placed in skeptical contexts like that of the courtroom. Third, the legal system, and specifically judges, "construct authority for judgements or decisions which are reached, and the role of both 'objective science' and 'legal fictions'" (1989, 12).

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numbers associated with the band lengths of the DNA fragments.

Chapter Three

From Order in the Courtroom to Disorder in Science: Forensic DNA typing in the United States, 1987-1991

1) DNA Typing in the United States

a) The First Case

DNA typing was first admitted as evidence in a rape case in the case of *State of Florida v Tommy Lee Andrews* in 1987 (Roberts 1991, 1721). In July of that year, Jeffrey L. Ashton, an assistant state attorney for the state of Florida, was reading a lawyer's magazine and saw an ad for paternity testing. He wondered if this test was the same technique used in England in the Enderby case (Wambaugh 1989). He called the company which had placed the ad, Lifecodes Corporation of Valhalla, New York, found out that it was the same test, and that it could indeed be used for forensic identification. Ashton was excited about testing the admissibility of DNA evidence in court, but

> "[k]new that whatever case we were going to do this in needed to be something significant. It needed to be a case where identification was the only issue and a case where we could convince the court and our own office to spend the money to break into this new area (Ashton 1989, in United States Congress 1990, 76).

Ashton got in touch with Tim Berry, who was prosecuting the case of Tommy Lee Andrews, who was charged with six separate counts of extremely violent rape. Ashton and Berry decided to try the new type of evidence. They could find no indication "that anyone had ever done this before, but we felt it was worth a try" (Committee on the Judiciary 1990, 77). Ashton and Berry sent six sets of samples

from the victims and the accused to Lifecodes. The analysis came back positive on two of the six. "The report was that Tommy Lee Andrews was, in fact, the person that was the source of the semen found during the rape exams of two of the victims. We then set the cases for trial" (Committee on the Judiciary 1990, 77).

In November of 1987, the first of the six cases went to trial in the Circuit Court of Orange County, Florida. This case was a break-in and rape of a young woman as she slept in her bed. Two dermal (traditional) fingerprints taken from a broken screen window on the ground floor were identified as belonging to Tommy Lee Andrews. During the rape her face was covered with a pillow, and there were no lights on, so positive visual identification of the rapist was difficult. The problem with the dermal fingerprints was that there was no way of proving how long they had been on the screen window, and no way of linking them to the time of the rape.

The evidence of DNA identity testing was given to the jury by Dr. Michael Baird, manager of forensic testing at Lifecodes. Baird testified that the semen samples taken from the victim matched the blood samples taken from the accused, and that the probability that the DNA samples could belong to anyone but the accused were 1 in 839,914,540 (Andrews v. State of Florida, 843). The jury also heard testimony from Alan Giusti, the Lifecodes' technician who performed the test, as to the exact procedures used and the quality of the samples. Dr. David Housman, a member of the faculty of the Massachusetts Institute of Technology also testified to the validity of the basic scientific principles behind the accuracy of DNA identity testing.

During the Andrews trial the defense offered no challenge to the DNA identity evidence, and brought forth no expert witnesses. After hearing two days of expert testimony brought forward by the prosecution, the jury returned after two and one-half

hours of deliberation. Tommy Lee Andrews was found guilty of burglary, sexual battery, and aggravated battery. Ashton notes that "From all available counts, this is the first conviction ever obtained anywhere in the world using the results of DNA identity testing. Mr. Andrews was sentenced to 22 years in the Florida Department of Corrections for those offenses" (Andrews v. State of Florida 1988, 843).

In October, 1988 Andrews appealed the conviction. His lawyers did not challenge the admissibility of the DNA identity evidence but did challenge the methods used by Lifecodes on two counts. First, that the internal testing done by Lifecodes on new reagents was unreliable, because "the new gel is only tested to be certain that it works the way the old gel worked and that if the old gel worked improperly, that error would be carried over to the new batch" (Andrews v State of Florida 1988, 849). The judge "found no merit" in this consideration. The defense's second challenge was that the database of 710 samples was "too small to be statistically significant" (Andrews v. State of Florida 1988, 850). The judge accepted expert testimony that a database of two to five hundred samples provided adequate statistical results (Andrews v. State of Florida 1988, 850).

Because DNA typing was a new scientific technique with no history in American courtrooms, and this was the first criminal case using DNA identification evidence at either the trial or appellate level, the judge had to address a variety of factors which bear on the reliability of the evidence,

> Including [the] novelty of [the] new technique, i.e., its relationship to more established modes of scientific analysis; existence of specialized literature dealing with technique; qualifications and professional stature of expert witnesses, and non-judicial uses to which scientific technique are put (Andrews v. State of Florida 1988, 842).

In this case, the judge did not follow the *Frye* standard of general acceptance in the scientific community, but followed another rule called the Arelevancy@ rule, based on the Federal Rules of Evidence 702, following *United States v. Downing* (1985).[1] The relevancy test of *Downing* says that where no established "track record" for a novel scientific procedure exists, the court must look to "other factors" which bear on its reliability. One of these is novelty -- how does the evidence relate to other established modes of scientific analysis. Another is whether the judge feels the evidence presented by the expert would be helpful to the jury, regardless of whether it has attained Ageneral acceptance@ in the scientific community. In applying the relevancy test, the judge found that "it seems clear that the DNA print result would be helpful to the jury", (Andrews v. State of Florida 1988, 849) and that Dr. Housman's testimony assured him that "DNA print identification is predicated on several well accepted scientific principles" (Andrews v. State of Florida 1988, 847).

Although we cannot know exactly what persuaded the judge of the credibility of the experts and the reliability of the technique, his written opinion provides his reconstructed -- and the official -- basis for forming such an opinion. Judge Orfinger seemed to be persuaded of the witnesses' credibility because of their scientific credentials. His written opinion reiterated their professional credentials, stating that Dr. David E. Housman held a bachelor's degree and a PhD in biology, was a professor of molecular genetics at MIT, in which position he dealt with "the structure and function of the DNA molecule" (Andrews v. State of Florida 1988, 847). The judge noted that Housman had taught at several universities since 1973, and published approximately 120 papers on molecular genetics, most of which dealt with DNA. In

[1] According to Judge Orfinger, the state asserted correctly that the evidence would meet the *Frye* standard as well as the relevancy test of *Downing*.

addition, Housman had served on several advisory boards for the National Institutes of Health, which involved DNA, and he had made a personal tour of the Lifecodes facility.

The judge observed that Allan Giusti, the technician who carried out the specific DNA identity tests, held a Bachelor of Science from Yale University, and had published several papers on genetics, "one of which involved his own research on DNA analysis." Additionally, Giusti had performed the test about 200 times (Andrews v. State of Florida 1988, 847). Dr. Michael Baird held a Ph.D. in genetics from the University of Chicago. He had worked in research at both the University of Michigan and Columbia University "at the DNA level", and been manager of forensics for Lifecodes for one and a half years. He had joined Lifecodes at its inception in 1982, and continued to teach graduate courses in DNA technology at New York Medical College and had published "a number" of articles on DNA testing (Andrews v. State of Florida 1988, 847).

> Relying on Dr. Housman's testimony, the judge found that DNA testing has been utilized for approximately ten years and is indicated by the evidence to be a reliable, well established procedure, performed in a number of laboratories around the world. Further, it has been used in the diagnosis, treatment and study of genetically inherited diseases. This extensive non-judicial use of the test is evidence tending to show the reliability of the technique" (Andrews v. State of Florida 1988, 849-850).

The judge's opinion in this appeal affirmed the use of DNA identification in criminal courts in Florida.

b) The First "Wave" of Cases

David Kaye, Professor of Law at Arizona State University, characterizes the history of DNA typing in the courtroom in the United States as occurring in three "waves" (Kaye 1993, 101). In the first wave, beginning with the *Andrews* case in

1987, DNA evidence was accepted unchallenged by defense witnesses and was poorly understood by judges. However, it passed *Frye* hearings and relevancy tests which determined that it was a technique which was generally accepted in the scientific community, and thus admissible as evidence.

This period marks a juncture, where existing institutions in society, here the legal and scientific, were attempting to grapple with the status of new knowledge and a new technology – DNA typing. During this period, the evidence was deemed admissible because the testimony of the experts testifying to its credibility and applicability were deemed to be credible and trustworthy.[2] At this time we also see one of the sites where new social structure is established, from the ground up – as a bridge was built between two social institutions: science and the legal system. During this time a novel scientific technique gained credibility in a different institutional realm than the one in which it was first advanced. Specific individuals acting -- that is, exercising their agency -- took the first steps towards the formation of new structure. In the Andrews case, this first step was established through the labor of the prosecuting attorneys, Tim Berry and Jeffrey Ashton, and the expert witnesses. It is not important that it was Tim Berry who took the first step towards using DNA testing in the forensic arena. Someone in the United States was bound to think up the idea, particularly since it had already been used successfully in the United Kingdom to solve a series of murders of young girls (Wambaugh 1989). A novel form of

[2] Courtrooms are a social space where personal testimony is not only expected, but where it becomes evidence, and as evidence, part of what determines "truth" in that social space. Scientists were called to courtrooms many times to act as expert witnesses, to speak for or against certain practices in DNA profiling. The reader will recall from the first chapter that to be held as knowledge, scientific evidence must still pass through the testimony of one or more credible individuals before it attains the status of "truth" (Shapin 1996). In this sense, having scientists testimony count as evidence for or against DNA profiling in the courtroom, is not so very different from what occurs at conferences or in peer reviewed journal articles.

knowledge (DNA typing) was given the power to bring order within society (stand as evidence to convict offenders) based upon the testimony of institutional (scientific) experts. These kinds of processes illustrate the delicate balance between actors, acting within pre-existing institutions, utilizing the pre-existing credibility of other actors. However, in doing so, they break the ground for the creation of new social structures and new forms of knowledge. Structure is made through the forging (and destruction) of coalitions. This is part of the dynamic relationship between agency and social structure. These events are not interconnected events of little consequence, but in fact important instances which illustrate the necessary tension between agency and structure in social formation.

The virtually unchallenged credibility, and therefore admissibility, of DNA evidence may have been because judges were "overawed by the magnitude of its scientific underpinnings" (Starrs, Subcommittee on the Constitution of the Committee on the Judiciary United States Senate 1990, 11). As an example of how poorly understood the technique was by judges, James E. Starrs, Professor of Law and Forensic Sciences at the George Washington University in Washington, D.C., testifying before a Senate Subcommittee (Subcommittee on the Constitution of the Committee on the Judiciary United States Senate 1990), said that in the Timothy Spencer trial for capital murder and rape, Circuit Court Judge Benjamin Kendrick

> listened attentively ... to the scrupulously precise and detailed testimony of prosecution expert Dr. Michael Baird [of Lifecodes Corp.]. After Dr. Baird had concluded spelling out the intricacies of each of the laboratory steps in DNA matching, Judge Kendrick, in some obvious puzzlement, asked for clarification on one point. The judge had counted only seven steps, but the expert said there were eight in all. Was not the digestion of DNA by a restriction enzyme identical to the process of separation of fragments by gel electrophoresis, the judge inquired in so many words. A muzzled gasp was almost audible in the courtroom from the observers schooled in the rudiments of DNA matching or even in just plain instrumental analysis. ... His honor was

> gently informed that the processes were distinctly different, as distinctly different one might add, as bicycling is from swimming even though in a triathlon one follows the other (Subcommittee on the Constitution of the Committee on the Judiciary United States Senate 1989, 11).

After only one day of expert testimony, and despite his own obvious puzzlement, Judge Kendrick admitted the DNA evidence as being "scientifically well-ensconced." This scientific "ensconcement" was based on the prosecution entering into evidence what was to later become a Lifecodes' publication titled "Human Population Genetic Studies of Five Hypervariable DNA Loci." The article was co-authored by four people, including Lifecodes' Dr. Baird. However, at the time of the pre-trial hearing, the article had not even been submitted for publication, let alone published. However, the defense attorneys did not challenge its admissibility or scientific credibility (Subcommittee on the Constitution of the Committee on the Judiciary United States Senate 1990, 23). Again, in this case testimony was offered only by the prosecution, and not countered by the defense (Subcommittee on the Constitution of the Committee on the Judiciary United States Senate 1990, 12). In fact, at the trial itself, "defense counsel prefaced his cross-examination of the prosecution's chief expert, Dr. Baird, with the apology, 'I feel inadequate to the task of cross-examining you'" (Subcommittee on the Constitution of the Committee on the Judiciary United States Senate 1990, 22).

c) DNA Evidence Challenged in Court for the First Time

In early 1989, two New York defense lawyers, Barry Scheck and Peter Neufeld, mounted the first challenge to DNA evidence in a courtroom. The case of *People v. Castro* (People v. Castro, 1989) involved the murder, on February 5, 1987, of Vilma Ponce, who was seven months pregnant, and her two-year-old daughter. Ponce and her daughter were stabbed to death in the Bronx. Ponce's husband

discovered the bodies, and told police he had seen a man leaving the building with what looked like blood on his hands. He later identified the man as 38 year old Joseph Castro, a janitor's helper (Parloff 1989, 1). During questioning police detectives noticed a small bloodstain on Castro's watch. Blood samples from the victims and the watch were sent to Lifecodes Corporation, of Valhalla, New York, for DNA analysis

On July 22, in a two page report, Lifecodes Corporation reported a match between the blood on the watch and the blood of the adult victim. They reported that the chance of such a match occurring at random was one in 189,200,000 (Parloff 1989, 2). The prosecution attempted to have this result entered into evidence. However, for the first time, the defense challenged the admissibility of the DNA evidence, which led to the longest, most extensive pre-trial hearing on the admissibility of evidence ever held in the United States.

Andrew Rossmer was the court-appointed lawyer in the Castro case but realized before the Frye hearing on the admissibility of the evidence began that he was in "way over his head." He asked Peter Neufeld and Barry Scheck to handle the *Frye* hearing, knowing that at the time Scheck and Neufeld, both criminal defense lawyers, were serving on a New York State panel studying the use of DNA testing in forensic contexts. Scheck and Neufeld had been friends since 1977, when they were both criminal defense lawyers for the Legal Aid Society of the Bronx. When approached by Rossmer, Neufeld was a solo practitioner in New York City, and Scheck was a professor at the Benjamin N. Cardozo School of Law (Parloff 1989, 2).

While preparing for the *Frye* hearing, Scheck and Neufeld concluded that the admissibility of DNA testing in the cases in which it had already been admitted had been based on almost no probing of the techniques. At first, they did not know how to

challenge the results from a scientific standpoint, but they were planning to argue that because Lifecodes was a private and unregulated firm, "there was no accountability, and we were concerned with that as a matter of social policy" (Neufeld, quoted in Parloff 1989, 53). Lifecodes claimed that its procedures were generally accepted by the scientific community, but they were essentially trade secrets. For Scheck and Neufeld, it seemed that Lifecodes functioned like a black box -- evidence went in, and results were spit out. And because of the mathematical certainty of the results, they had been functioning in effect as verdicts (Neufeld, quoted in Parloff 1989, 53). Scheck and Neufeld set out to find someone who could explain to them and to the jury what was happening inside the black box that was Lifecodes Corporation, but found that the experts they approached initially were reluctant to become involved because they did not want to attack what was seen as a valuable technology.

In November of 1988 Scheck, Neufeld and the other members of the state advisory panel were invited to a symposium on DNA evidence at Cold Spring Harbor. Dr. Michael Baird, who was the director of Lifecodes forensic and paternity laboratories, was one of the speakers. He brought with him some forensic autorads, which were the first ones some of the research scientists had ever seen. During his talk, Baird said that sometimes he would declare two samples to be a match, even though the bands did not visually line up on the autorads, because he knew that the misalignment was caused by "bandshifting",[3] and so he didn't have to perform any control testing. Dr. Eric Lander, a mathematician and human geneticist with appointments at the Whitehead Institute for Biomedical Research at MIT and Harvard

[3] Bandshifting occurs when one or more lanes in a gel run at a faster or slower speed than the other lanes, thus physically and visually shifting the bands up or down, displaced horizontally from their potentially "matching" bands in other lanes.

Business School, was "disturbed" by Baird's talk. "'I came away appalled at the lack of controls,' Lander recalls, 'With due respect, it's the sort of thing you don't let your graduate students get away with'" (Lander, quoted in Parloff 1989, 53). Seizing the opportunity, at the symposium Peter Neufeld "cornered" Eric Lander in a kitchen and asked him to look at one of the autorads from Lifecodes that had been obtained through discovery in the *Castro* case.

> As Neufeld tells the story, Lander responded, "Let me show you how we do things in science." Lander then called over several colleagues, slapped the autorad up against a window, and said, "Match, or no match?"
>
> "Garbage," one responded.
>
> "Do it again," said another.
>
> "Garbage," said a third (Parloff 1989, 53).

Scheck and Neufeld asked Lander to testify for the defense in the *Castro* case, but he turned them down due to time commitments and his belief that Lifecodes' procedures could not be seriously flawed. But he did agree to act as an educator for Scheck and Neufeld, so every day after the hearing, the two lawyers sent Lander the day's transcripts. Lander would feed the lawyers questions and he assumed that Baird would "concede there were problems with the data." But, in Lander's words: "I became increasingly distressed and appalled at the answers that came back, they were not defensible answers" (quoted in Parloff 1989, 53).

During December of 1987 and January of 1988 Scheck and Neufeld used discovery demands and subpoenas to get access to Lifecodes data, including laboratory notebooks, autorads and computer printouts. Examining this data, Scheck and Neufeld discovered that for one locus, Lifecodes had reported a single band,

10.25kb long, from the stain on Castro's watch and Ponce's blood -- in other words, Lifecodes had reported that both bands were exactly the same size. However, when Neufeld and Scheck went over the computer printouts, they discovered that the watch stain was 10.16 kb long and the band in Ponce's blood was 10.35 kb. Lifecodes had averaged the two measurements and reported the one number as applying to both. Further, in published papers, Lifecodes had reported that its procedure for dealing with matches was to confirm visual matches by measuring the bands and confirming that they fell within three standard deviations of each other. Neufeld and Sheck determined that 10.16 and 10.35 were outside the three standard deviation limit. In other words, Lifecodes was not following its own matching rules. "Lander confirmed [their] observation, and added that the measurement for a second pair of purportedly matching bands were even further outside of three standard deviations. Under Lifecodes' own published rule, two of its six matching bands weren't matches" (Lander, in Parloff 1989, 54).

Scheck and Neufeld had been given blue film copies of the autorads, and on these they observed that there was a band in Ponce's lane, with no corresponding band in the watch stain lane. Lifecodes had not reported this. In January of 1989, Scheck and Neufeld went to visit Lifecodes laboratory in Valhalla to examine the original autorads. On an autorad which came to be called the "Cooke's probe" autorad[4] "Scheck noticed that while Ponce's lane on this autorad had three bands, the watch stain lane had five, including what he now describes as 'two little fuzzy bands' not visible in the blue film copies turned over in discovery" (Parloff 1989, 54).

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[4] Dr. Cooke, who was a witness for the defense, had provided the probe DXYS14 to Lifecodes. This probe has the property of creating up to eight bands in a lane, instead of the one or two bands that most probes produce (Lander 1989, 502).

When questioned about the two extra bands in the watch lane, Dr. Michael Baird testified that the extra bands were probably due to contaminants that had come from a non-human source of DNA. Baird testified that he knew this because one of the properties of the probe DXYS14 was that it showed bands which decreased in intensity in proportion to their length. By this time, Eric Lander had been persuaded to become a witness, and he countered this, showing that in the published literature, there was no correlation between the length of a fragment and its intensity (Lander 1989, 502). In fact, the person who had published this information was also an expert witness for the defense, Dr. Harold Cooke. Dr. Cooke testified that if no experiments could be done to explain away the extra bands, then the only interpretation for the autoradiogram was to exclude the defendant.[5]

Scheck and Neufeld were able to show that in the Castro case, Lifecodes had not followed its own "matching rule" for determining when two fragments are the same length and thus match each other. Sometimes two fragments may show up in slightly different places on the gel, although the operator has reason to suspect that they may "actually" be the same length because they are believed to come from the same person. When making measurements that need to be precise -- and especially because someone's life and liberty may be on the line, these measurements have to be precise -- some means of expressing accuracy is required. Generally, this takes the form of a number that expresses numerically how big or small measurement differences, or errors, are on average.[6] This number is called the standard deviation,

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[5] "Exclude" and "include" are terms used to describe whether a defendant can be included in the set of people that might have committed the crime, or excluded from that set of people. So, in this case, for this autorad, Cooke testified that the only interpretation possible, in the absence of qualifying experiments, was to conclude that the autorad excluded the defendant from the set of people that could have committed the crime.

[6] An easy interpretation of a standard error, or standard deviation is as a "typical", or

or the standard error, and Lifecodes had published papers indicating that their standard error was 0.6% of a fragment's length (Balazs et al 1989, 183). Lifecodes created a formal "matching" rule to use when two fragments appear to be close together. The reader will recall that Lifecodes had stated that two fragments would be said to match if their lengths differed by less than three standard errors (Lander 1989, 502). In the Castro case, the fragments at the sites D2S44 and D17S79 did not appear to match although Lifecodes had declared that they did match. As it turned out after Scheck and Neufeld examined Lifecodes data, the bands at site D2S44 differed by 3.06 standard errors, and the bands at D17S79 differed by 3.66 standard errors. Both measurements were outside Lifecodes own self-declared matching rule. As a result, if Lifecodes had been following its own rule, the bands should have been declared *not* to be a match. When Scheck and Neufeld were able to cross-examine Baird he admitted that "Lifecodes did not use any objective standard to declare or confirm a match; its matches were purely visual" (Parloff 1989, 54). This was an extremely important admission, because Lifecodes had published that they used the three-standard deviation rule not only to declare matches, but that the three standard-deviation rule was also used in the frequency distributions used to calculate the probability of a random match. If they were using only subjective criteria to determine matches, the validity of their probability calculations was compromised.

Scheck and Neufeld were able to point out other problems with Lifecodes' data, including Lifecodes' inability to identify the DNA in a control lane, which is

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"average" error or deviation from the mean value. If you don't know anything about a distribution, your best guess as to what value a member of that distribution has is the mean, or average value. The standard deviation quantifies what a "typical" deviation from that mean, or average value is. By the rules of statistics, for normally distributed populations, 95% of all the cases fall within two standard deviations of the mean, and 99% of the cases fall within three standard deviations of the mean. The standard error is a number that expresses the range of variation one can normally expect in measuring fragment lengths.

supposed to be DNA of known origin, acting as a control to make sure the probes are working properly. Originally, a Lifecodes technician and Dr. Baird testified that the DNA in the control lane was from a male Lifecodes' scientist, Dr. Arthur Eisenberg. Baird felt he could recognize Eisenberg's DNA from the patterns it produced, since they frequently used his blood as a control.

> When Scheck asked why the control DNA had shown no reaction to the sex identification test, Baird responded blithely that 'Dr. Eisenberg has an interesting characteristic in that his male DNA does not react with this repeat sequence on the Y chromosome. Scheck vividly recalls Lander's reaction to this exchange: 'This man's DNA is publishable!' [Lander] added that it did not make sense to use as a control the DNA of a genetic mutant" (Parloff 1989, 55).

However, in late April, Baird changed his mind and testified that the DNA in the control lane had not come from Eisenberg, but from a female lab technician, Elia Meade, and that he had actually never seen a test of Eisenberg's DNA which had been run with a sex identification probe (Parloff 1989, 55).

With Lander's help, Scheck and Neufeld were also able to attack Lifecodes statistical calculations. After a match is declared, laboratories go through three steps to calculate the probability that the match might have arisen by chance. First, for each allele, the frequency with which the matching bands occur in the relevant population is counted (in this case, Lifecodes used a Hispanic database). Second, each locus is tested to see if it is in Hardy-Weinberg equilibrium, making the assumption that the population is mating at random and does not contain any subgroups. Third, for the complete pattern, using all of the loci tested, the single-locus probabilities are multiplied together, again assuming that the genotypes are independent, or uncorrelated. This is called being in "linkage equilibrium." Lander reports that in the *Castro* case, none of these steps held up to scientific scrutiny (Lander 1989, 503).

Lifecodes used different matching rules for determining whether the two bands

matched, and counting the frequency of the bands in the population. "It is axiomatic that the same matching rule must be used for counting the matches occurring in the population database" (Lander 1989, 503). The method used by Lifecodes severely underestimated the probability of a random match. As Lander puts it, "Such a statistical procedure is like catching a match with a 10-foot-wide butterfly net, but then attempting to show how hard it is to catch matches with a 6-inch-wide butterfly net" (Lander 1989, 503).

Eventually, the witnesses for the prosecution and the defense decided to meet together, outside the courtroom, with no lawyers present, to see if they could determine what was going on with "the science" at hand. This came about because Lander had written a 50 page report critiquing the methods used by Lifecodes, but the judge had ruled it as inadmissible hearsay. Dr. Richard Roberts of Cold Spring Harbor was a witness for the prosecution, and Lander gave him a copy of his report.

> Roberts was so upset to discover from Lander's report that the company did not follow its published matching rule that he proposed a mini-summit conference: a meeting of the experts who had testified in Castro to see whether, as scientists, they could come to some consensus (Parloff 1989, 55).

All of the expert witnesses called in the case, except for Baird, reached the consensus that the tests as performed by Lifecodes were

> 'not scientifically reliable enough to support the assertion that the samples ... do or do not match'. They agreed that Lifecodes had failed to account for the non-matching bands produced by Cooke's probe or to justify deviating from the three-standard-deviation rule, and that its reported statistical probability of a random match 'understates the actual probability' (Parloff 1989, 55).

In the words of Eric Lander, Lifecodes' laboratory procedures were "incredibly sloppy"

(Lander 1997, Personal interview).

The case of *People v. Castro*, marked by the longest, most extensive pre-trial Frye hearing ever held in the United States, was the first case in the United States to declare DNA evidence to be inadmissible. After 15 weeks of testimony by the expert witnesses, Justice Gerald Sheindlin ruled that the theory underlying DNA typing was generally accepted in the scientific community and that the technique could produce reliable results. Further, he recommended that pre-trial hearings should generally be conducted to determine if in the specific case under examination the "testing laboratory substantially performed the scientifically accepted tests and techniques, yielding sufficiently reliable results to be admissible as a question of fact for the jury." He also ruled that in this specific case, the testing laboratory, Lifecodes Corporation, had failed to "substantially perform the scientifically accepted tests thereby obtaining sufficiently reliable results" (People v. Castro 1989, 985, 999). In his opinion, Lifecodes had failed to use generally accepted scientific techniques and experiments. The *Castro* case never came to trial. In late 1989, Castro pleaded guilty to the murders.

The *Castro* case was an important case for several reasons. It was the first time that a court ruled that DNA evidence was inadmissible for any reason. However, the case is perhaps most important because it made DNA typing an issue of some controversy for the scientific community. Again we see the interplay between individual agency and social structure. Through the *Castro* case the defense lawyers and expert witnesses were successful in exposing the inadequacies of the technology as it was produced in private laboratories. Scheck and Neufeld exposed the fact that the technology was still extremely dependent on local practices which were subject to huge variation (Jasanoff 1995).

The *Castro* case brought to the attention of the academic community that there was a problem with DNA profiling that required academic attention. The actions of the lawyers and the scientists made the DNA profile cross social worlds (Clarke 1991; Star 1991). The DNA profile was the boundary object (Star and Griesemer 1989) which had enough coherence in both domains to sustain this transference. Eric Lander's 1989 *Nature* article ("DNA fingerprinting on trial") was the first article published by an academic scientist pointing out potentially severe problems with the technique. After the *Castro* case, all the expert witnesses on the case lobbied for the National Academy of Sciences to appoint a committee to articulate standards for DNA testing, which did happen. Eric Lander was appointed to that committee.

As a boundary object which crossed the divide between the courtroom and academia, the DNA profile was embedded in a slightly different set of meanings, interests and goals in each social world. In the courtroom, a defense lawyer presented with a profile said to be a "match" would be concerned with issues that are more salient in the courtroom than in other contexts. The defense attorney would be particularly concerned with the chain of custody – how had the evidence been collected and handled? Had the testing laboratory done the procedure correctly? Can the defense attorney show a disruption or fracture in the chain of custody? Is there evidence to show the DNA might have been contaminated? She or he would want to know if there was enough evidence left over for the defense to perform their own set of DNA tests, given that they had adequate funding.

The random match probability would be of great interest to both the prosecutor and defense attorneys, and it is a particularly good example of how the DNA profile functioned as a boundary object, existing in several different social worlds simultaneously, while being serving a different set of needs and sometimes

controversies in each of them. In the courtroom, once it was established that a DNA profile did not necessarily uniquely identify a single human being, there was a need to establish the degree of certainty that could be placed in the statement that the DNA from the crime scene matched the DNA from the suspect. Thus, the need for a random match probability arose because of the court's interest in certainty of identification. It took some time to establish which professional community had jurisdiction over that type of question. The first National Research Committee did not have a statistician or a population geneticist on the committee. After release of the first report it was argued that intellectual jurisdiction over the random match probability properly belonged to the fields of population genetics and to statistics. At first, population geneticists believed it was an easy problem to solve. However, it turned out to be a knotty problem that resisted "perfect" solution even in the second National Research Council committee on the topic (Hartl 1997, Personal interview). For the academic community, the problem was how to assign the probability that the courtroom needed. One of the questions became how to situate a given DNA profile with respect to a "population." The concept of population became became entwined with issues of race and ethnicity, and in the case of Hispanics, linguistics. The population geneticists had to solve the problems of how different alleles were distributed across different populations, what counted as a population, and what kinds of evidence would be counted for or against claims that a population was "stable" in its mating patterns. So, while the DNA profile was the same entity for the laboratory, the lawyer in the courtroom, and the population geneticist, it took on a different focus for each of them.

d) The Second "Wave" of Court Cases

The Castro case marks the beginning of the second "wave" of court cases

using DNA evidence, which covers the period from late 1989 to 1991. In *Castro*, Justice Sheindlin ruled that DNA typing was generally admissible, that is, he found it to be generally accepted within the scientific community, but in this specific case he ruled that the testing laboratory had failed to perform the techniques according to accepted scientific procedures, and thus the evidence was inadmissible. After the Castro case, and in the face of organized challenges by defense attorneys, more and more courts ruled that DNA evidence was inadmissible. However, David Kaye argues that despite these challenges, the evidence continued to be accepted in the majority of courtrooms across the nation (Kaye 1993). As of January, 1990, the Office of Technology Assessment reported that DNA testing had been admitted as evidence in at least 185 cases in 38 states (Office of Technology Assessment, 1990).

The effect of the second wave of cases on the *scientific* community, however, was very great. In *Castro*, Scheck and Neufeld successfully exposed how dependent on local practice and personal idiosyncrasies the application of some of the "scientifically accepted" procedures could be. In challenging the methods used to calculate the probability that a specific DNA sample could match someone else in the population chosen at random, Scheck and Neufeld exposed the fact that the methods and frequency distributions being used to calculate these probabilities were being created *ad hoc* by the private, unregulated laboratories conducting the DNA tests.

After *Castro*, Scheck, Neufeld and other defense lawyers who were part of the "DNA Task Force" continued to assault both the specific application of DNA typing procedures, and the methods used to calculate random match probabilities. Prosecution and defense attorneys enlisted highly credentialed members of the population genetics community to testify for or against the procedures, and as a result of the controversy between expert witnesses in courtrooms around the country, the

scientific literature exploded with articles either defending or critiquing the technique. The initial pieces were written by people who had been expert witnesses, but it did not take long for the debate to spread to other members of the scientific community. What seemed on the face of it to be a simple problem in the methods of calculating probabilities, turned out to be a knotty problem which had never been faced by population geneticists before, and for which they, and other scientists, would have no easy answer.[7]

In subsequent cases in which Scheck and Neufeld challenged DNA evidence, they brought together highly credible experts in the fields of population genetics and molecular genetics, such as Eric Lander from MIT, Richard Lewontin from Harvard and Daniel Hartl from Washington University. Scheck and Neufeld went to the top of the hierarchy of molecular biology and population genetics and enrolled these experts in their struggle to challenge DNA evidence.

Doubts about the technology began to emerge in the courtroom. But it is important to note that these "doubts" did not just appear by accident, they were created by a team of dedicated defense lawyers determined to find a way to make DNA evidence at least a little wobbly. In March of 1990, the National Association of Criminal Defense Lawyers (NACDL) held its First National DNA Symposium and created a "DNA Task Force" to aid defense lawyers in attacking the technique. Barry Scheck and Peter Neufeld were the chairs of this taskforce (National Association of Defence Lawyers, *The Champion*, June 1991, 17). Neufeld and Sheck reported that

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[7] Sociologist of science Sheila Jasanoff argues that the process of deconstruction of the scientific claims made by the private laboratories was set in motion by the "legal system's normative commitment to finding two sides in every case [which] led in *Castro* to a confrontation among experts who finally questioned some of the methodological premises of DNA testing and testimony" (Jasanoff 1995, 57).

the cost and the complexity of this type of litigation was enormous, and the brunt of the burden was being born by public defenders and assigned counsel. In November of 1990 the NACDL held its Second National DNA Defense Symposium in Los Angeles. The DNA Task Force was organized to the level of having regional contact people. At the Los Angeles Symposium, they brought in experts to teach the defense counsel how to challenge DNA evidence, including the "Irvine Mafia" -- William Thompson, Simon Ford and Laurence Mueller -- so called because they are all faculty members at UC Irvine.

The formation of the DNA Task Force is another instance where the agency of individual lawyers brought into existence a new social structure -- the DNA Task Force. Only time would tell how solid, enduring, meaningful and effective this Task Force would be, but here is another moment where a new structure comes into being through the concerted efforts of a number of individual lawyers. The challenges to DNA evidence in the courtroom were organized and spearheaded by a highly dedicated, select group of people. They sought out, created, and nurtured a community of scientists who were opposed to some aspects of DNA typing. In their written accounts, they use the term "engineered" to describe their efforts to attack DNA evidence in the courtroom. For example, in the case of *People v Despain* (No. 155-89 (Yuma Co., Ariz. 2/12/91)), after a very thorough hearing, all the DNA evidence was excluded on *Frye* grounds (Sheck and Neufeld, 1991, 19). In the Despain case, the court held that "the FBI's methods for estimating probabilities were not generally accepted in the field of population genetics" (Scheck and Neufeld 1991, 19). Sheck and Neufeld, in speaking about Despain, say that

> *Despain* was a major effort of the DNA Taskforce, brilliantly engineered by Jo Sotello, with great assistance from Drs. Simon Ford and Bill Thompson. *Despain* illustrates the great advantage we have in

> being able to track the FBI's agent examiners as they bounce around the country. Jo Sotello ingeniously used all the work and discovery being generated simultaneously in the *Yee* case ... to expose contradictions in the FBI's positions (Scheck and Neufeld 1991, 19).

It can be argued, however, that despite serious challenges to its credibility, during this second wave period, "in the majority of cases, the courts continued to hold DNA matches and probabilities admissible even in the face of conflicting expert testimony" (Kaye 1993, 12). For example, in *United States v Jakobetz* (United States v. Jakobetz 1990) despite testimony by Richard Lewontin as to population substructure[8] problems and the inability of the FBI to make two samples of its own allele distributions match, Judge Billings stated that "the [government] has sufficiently established that the current reliability and accuracy of DNA profiling justifies an aura of amazement" (United States v. Jakobetz 1990, 282). However, some high ranking courts, including the Supreme courts of Georgia, Massachusetts and Minnesota excluded at least some DNA evidence. In *Commonwealth v. Curnin* (Commonwealth v. Curnin 1991, 565), the Supreme Court of Massachusetts found that Cellmark's DNA evidence in a rape case had been erroneously admitted due to the failure to show that there was general acceptance of the process leading to the calculation of the random match probability. The trial court held that

> there was no demonstrated general acceptance or inherent rationality of the process by which the laboratory that conducted the tests arrived at its conclusion that one Caucasian in 59,000,000 would have the DNA components disclosed by the tests that showed an identity between the defendant's DNA and that found on the nightgown (Commonwealth v. Robert W. Curnin 1991b, 48).

The court found further that

> Evidence of this nature, based on the scientific principle that every

[8] "Substructure" means that a population is made up of several separate, distinct populations which within themselves may have different allele frequencies than when they are all lumped together into one large population.

> human has unique genetic characteristics and having an aura of infallibility, must have a strong impact on a jury. The erroneous admission of such evidence would undoubtedly be prejudicial in any case where, as here, the identification of the person who committed the crime is in serious dispute. We conclude that the results of DNA testing were improperly admitted in this case. The convictions must be reversed, and the case retried (Commonwealth v. Robert W. Curnin 1991b).

The court relied on testimony by Dr. Laurence Mueller, and also on the expert witness reports by Drs. Richard Lewontin of Harvard and Daniel Hartl (University of Washington, St. Louis) from *United States v Yee*. The *Curnin* case was important because it was a high ranking court, and because the judge's opinion was that challenges to DNA evidence should focus both on general acceptance, and that the evidence should be suppressed if it could not be shown that the test procedures were properly performed in the specific case before the court.

In *State v. Schwartz* (State v. Schwartz 1989), the Supreme Court of Minnesota found that DNA typing had gained general acceptance, but that in this case the laboratory (Cellmark) did not meet appropriate standards. The court also ruled the statistical evidence to be inadmissible, even if it had been accurately reported, because it felt that juries might give undue weight to the statistical evidence. In *Caldwell v. State* (1990), the Supreme Court of Georgia required Lifecodes to amend (upwards) their calculation of a random match probability because Lifecodes had assumed Hardy-Weinberg equilibrium in their database, when this was in fact shown to be inconsistent with the data.

e) The Yee Case

An important case in the Task Force's mission to destabilize DNA evidence was the case of *United States v. Steven Wayne Yee et al* (United States v. Steven Wayne Yee et al 1993; United States v. Steven Wayne Yee et al 1994). This case

was an appeal by Steven Yee, John Ray Bonds and Mark Verdi of their convictions for murder, claiming that the district court erred in admitting expert testimony concerning DNA evidence.

Yee, Verdi and Bonds were members of the Hells Angels motorcycle gang, accused of gunning down music store manager David Hartlaub as he stopped after work on February 27, 1988 to make a night deposit. The motive was not robbery -- the money was left on the seat of the van. Apparently, the murder was a case of mistaken identity. Mr. Hartlaub drove a yellow van identical to that of a local member of the Outlaws, a rival motorcycle gang. Yee, Verdi and Bonds had apparently planned to "hit" this member to retaliate for the shooting of a Hell's Angel member the previous year in Jolliet, Illinois (United States v. Steven Wayne Yee et al 1993, 546). The police found the van used in the murder abandoned behind a nearby hotel. There was a MAC-11 9-mm semi-automatic pistol in the van which was spattered with blood. The carpet in the van was also splattered with blood. Blood tests showed that the blood did not belong to Mr. Hartlaub, but matched that of John Ray Bonds, who had apparently been wounded by a ricochet from the gun.

The *Yee* appeal addressed the issue of whether the district court "committed reversible error" in admitting expert testimony support the DNA evidence submitted by the FBI. The defense took issue with the Magistrate's Report and Recommendation in United States v. Yee 1991, on three counts: first, consensus and general acceptance of the evidence in the scientific community; second, reliability of the evidence, and third, Federal Rule of Evidence 403. The DNA typing conducted by the FBI was the restriction fragment length polymorphism type discussed above in connection with Lifecodes. To calculate the probability that the blood found at the crime scene could have come from anyone other than the defendant, the FBI

conducted DNA studies on FBI agents and compiled this information into a table of allele frequencies. They developed frequency distributions for Caucasian, Black and Hispanic racial groups (United States v. Steven Wayne Yee et al 1990, 550). The distribution for Caucasians was developed from the DNA profiles of 225 FBI agents. To estimate the frequency of the suspect's DNA sample in the general population, the FBI used the product-rule procedure, where the individual allele frequencies are multiplied together. On April 7, 1989, the FBI's DNA laboratory reported a match between blood found in the victim's car (Hartlaub) and the blood of Bonds. The FBI calculated the probability that this blood could have come from some other unrelated member of the population other than Bonds at 1 in 270,000. In May of 1990 the FBI revised its probability estimate to 1 in 35,000. No information is provided as to how or why they revised this estimate.

In a previous trial, the defense had moved to have the DNA evidence suppressed, because at the time the FBI had not published in a peer reviewed journal any of their methods or data to support their results (United States v. Steven Wayne Yee et al 1991, 550). The defendants requested that they be granted access to the FBI's data underlying the DNA tests, and also access to the results of internal proficiency tests. In *United States v Yee* (1990), these requests for discovery were granted. At that time, the magistrate called for a six-week *Frye* hearing held from June 26 to September 12, 1990, to determine the admissibility of the DNA evidence. In the end, in a 120 page Report and Recommendation, the Magistrate recommended that the Government's motion to admit the DNA evidence be granted and the defendant's motion to suppress be denied (see United States v. Yee 1991). During the hearing, the government called six expert witnesses, the defense called five, and the court called Dr. Eric Lander as the court's own witness.

The lineup of witnesses called by both the prosecution and defense read like a "Who's Who" in population and molecular genetics. In addition to two FBI employees -- Dwight Adams, examiner, and Bruce Budowle, head of the FBI's Forensic Science Research and Training Center -- the Government called Dr. Patrick Conneally, a Distinguished Professor of Medicine at the Indiana University School of Medicine, Dr. Stephen Daiger, a Professor at the Graduate School of Biomedical Sciences at the University of Texas Health Science Center, and Dr. C. Thomas Caskey, holder of the Henry and Emma Meyer Chair in Molecular Genetics at the Baylor College of Medicine. Dr. Caskey had been the Chair of the Advisory Panel to the Office of Technology Assessment's 1990 Report on genetic testing, of which Dr. Eric Lander, the court's witness, was also a member. The Government also called Dr. Kenneth Kidd, Professor of Human Genetics, Psychiatry and Biology, of Yale University School of Medicine.

The defense called Dr. Peter D'Eustachio, Associate Professor, Department of Biochemistry, New York University Medical Center; Dr. Paul J. Hagerman, Associate Professor of Biochemistry, Biophysics and Genetics, University of Colorado Health Sciences Center; Dr. Richard C. Lewontin, Alexander Aggassiz Professor of Zoology and Professor of Population Sciences, Harvard University; Dr. T. Conrad Gilliam, Assistant Professor of Neurogenetics, Department of Genetics and Development, College of Physicians and Surgeons, Columbia University, and Dr. Daniel L. Hartl, James S. McDonnell Professor and Head, Department of Genetics, Washington University School of Medicine.

The first defense challenge was to the FBI's protocols and laboratory procedures, calling into question the extent to which these were generally accepted in the scientific community. In order for the evidence to be admissible, the government

had to show that there was general acceptance of its protocols and laboratory procedures. In regard to this matter, the defense challenged the design of the FBI's standards for declaring a match, as well as questioning the quality of the FBI's basic work and its reliability and reproducibility. They also questioned the FBI's research into the effect of environmental insults and degradation on DNA, and claimed that the FBI had failed to implement a regular program of proficiency testing (United States v. Bonds et al 1993. Their major criticism was that when the FBI performed a second round of DNA tests on the same 225 agents they had used to construct the first distribution, they were unable to match the profiles of the two sets of data. On the two separate occasions, identical bands were classified into different bins, resulting in two different frequency distributions from the same individuals. Because the FBI "were unable to identify their own agents as being themselves"[9] the defense witnesses testified that the DNA profiling procedure could not be deemed to be reliable, and therefore it could not be generally accepted in the scientific community. Under these conditions, they requested that the evidence be ruled as inadmissible. The question before the court was whether the FBI had established a reliable and generally accepted procedure for DNA typing. If they had, the DNA evidence would be admissible in court.

The second defense challenge was about the representativeness of the sample of 225 Caucasian agents, and kinds of subpopulations that might be represented in the backgrounds of these 225 individuals. Questioning the reliability of the database means that the defense was questioning the ability of the FBI to reliably

[9] See transcripts on file at courthouse in Toledo, Ohio for United States v. John Ray Bonds, Mark Verdi and Steven Wayne Yee. Page citations are to 12 F. 3d 540; 1993 U.S. ApLexis 32574; 1994 Fed Ap0085P.

and accurately estimate a random match probability once a match had been declared. The defense experts, as well as the Court's witness, Dr. Eric Lander,

> contended that the basic design of the FBI Caucasian database was flawed because it failed to take into account the likelihood that there is no such thing as an American Caucasian population. Instead, in the view of the defense experts, there was a significant likelihood of 'substructure' whereby the frequency of particular alleles might vary on the basis of the ethnic ancestry of particular subpopulations within the overall American Caucasian population" (United States v. John Ray Bonds, Mark Verdi and Steven Wayne Yee, 12 F. 3d 540; 1993 U.S. ApLexis 32574; 1994 Fed Ap0085P. 174).

The defense witnesses testified further that because the magnitude and frequency of substructure in the FBI's population database was unknown, the probability estimates calculated from that distribution were so speculative that they were unacceptable to the scientific community (United States v. John Ray Bonds, Mark Verdi and Steven Wayne Yee, 175). The government's witnesses rebutted that even if population substructure did exist within the database, it was infrequent, and of such a small magnitude that it was meaningless, and that if it did occur, it was as likely to bias the statistics in favor of the defendant as against him. Dr. Kidd acknowledged the existence of substructure in the North American Caucasian population, but believed its effect on the calculations would be insignificant. All the witnesses agreed that the extent to which the European and North American Caucasian population is substructured by ethnic groups is unknown (United States v. John Ray Bonds, Mark Verdi and Steven Wayne Yee 1993, 181). Drs. Lewontin, Hartl and Lander all agreed that it was very likely that the frequency of some of the alleles in the FBI Caucasian database differed by ethnic group. Dr. Lewontin testified "that no scientifically acceptable compensation factor has been or could be built into the FBI's Caucasian database that would adequately respond to and ameliorate the potential effects of possible substructuring" (United States v. John Ray Bonds, Mark Verdi and Steven

Wayne Yee 1993, 181). Dr. Lander agreed that there was no compensation factor that could be applied to the calculation to correct for the unknown substructuring.

The government's witnesses stressed the "conservative" nature of the FBI's protocols and methods of calculating the random match probability, asserting that this compensated for any bias introduced by possible (but unknown) population substructure. Among the conservative aspects of the FBI's method were the use of "fixed bins" for determining allele frequencies, which overestimates the true frequency of any allele (United States v. John Ray Bonds, Mark Verdi and Steven Wayne Yee 1993, 182), the use of bins that are wider than the match window, the doubling up of bins when allele frequency in a bin is below five, the allocation of "borderline" alleles to the bin with the larger frequency, and using 2P instead of P-squared when only one band appears. The government's witnesses testified that all these things singly and together resulted in the overestimation of allele frequencies, which acts in the defendant's favor when the probability calculation is made.

Dr. Lewontin was not persuaded by the claims of conservativeness, focusing instead on the fact that "because of the failure to take substructure into account, no scientifically acceptable estimate of probability can be made on the basis of the FBI's database" (United States v. John Ray Bonds, Mark Verdi and Steven Wayne Yee 1993, 182). For Dr. Lewontin, it was scientifically unacceptable to place a numerical value on the significance of a match when the extent of population substructure was unknown. He stated that it was "an unacceptable procedure in science to float numbers for which we have such uncertainty, any number you give is of unknown relationship to the correct value, when you don't know the range of uncertainty and there is no way to quantify that uncertainty, it is scientifically unacceptable even to give an estimate" (United States v. John Ray Bonds, Mark Verdi and Steven Wayne

Yee 1993, 182).[10]

Dr. Lander was equally convinced that substructure existed, and that the FBI's method of calculating the random match probabilities was not "generally accepted in the scientific community." For Lander, "you have a consensus amongst those people who have now seen the facts that we are deeply in doubt about what is and is not defensible" (United States v. John Ray Bonds, Mark Verdi and Steven Wayne Yee, 182). Lander did not believe the FBI's contention that their procedures were "conservative", that is, biased in favor of the defendant, and pointed out that all the procedures cited as being conservative for the calculation of random match probabilities were in actuality instituted because of the inability to accurately measure discrete alleles, not to calculate probabilities.

> Dr. Lander pointed out the difference between the purpose of the FBI's fixed bin approach and the problem created by the possibility of insufficiently unacknowledged substructure: "I've never understood the reason for a larger bin ... to be the ability to multiply ... I've understood the reason for a larger bin to be a desire to be cautious, careful... But not to guarantee multiplication" (United States v. John Ray Bonds, Mark Verdi and Steven Wayne Yee 1993, 182).

In response to the suggestion that failing to take substructure into account may produce a probability estimate that is not at all what it would be if substructure were accounted for, Dr. Lander replied: "That's the nature of the concern as it affects lawyers and Courts. The nature of the concern as it affects scientists, I think I'd put

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[10] It is important to recognize that there is more going on than a debate over the existence or relevance of subpopulations. Richard Lewontin is deeply concerned with the status of what we know to be true about the natural world -- truth, with a capital "T." He is an empiricist, and when he spoke of being on the witness stand, he was vehement about what he could and could not, under oath, claim to "know" as truth (Lewontin 1997, Personal interview). At this stage, there was very little known about the distribution of VNTR alleles in *any* population, let alone their distribution by racial division. Lewontin was concerned about social justice, and about using "scientific evidence" which had not been proven to be true – i.e., had not achieved the status of knowledge – to convict people.

more basically. It's that we do not have the proofs that would allow us to know what procedure to use" (United States v. John Ray Bonds, Mark Verdi and Steven Wayne Yee, 183).

All of the witnesses agreed that population substructuring existed, but they differed in their interpretation of its effects on the probability calculations. Dr. Kidd admitted that it was not possible to obtain an absolutely true estimate of the random match probability, but testified that the methods used by the FBI allowed one to say with conviction that a match was "robustly uncommon" (United States v. John Ray Bonds, Mark Verdi and Steven Wayne Yee 1993, 184).

. In the end, the judge was persuaded by the government's witnesses, citing the fact that Dr. Caskey had adopted the FBI's protocol in his own laboratory as evidence that the FBI's method was generally accepted in the scientific community.

The reports submitted by Dr. Lewontin and Dr. Hartl in the *Yee* case were immediately faxed about the country to other defense lawyers, and were submitted as evidence in other ongoing trials, such as the *Despain* case mentioned above. In two cases in Chicago in March of 1991, a DNA Taskforce member "engineered" another victory by bringing in a new expert from the area (Dr. Jerry Coyne from the University of Chicago), as well as Dr. Laurence Mueller of UC Irvine. "These experts were able to introduce into evidence the expert reports from *Yee*, thereby enlarging, for purposes of that record, the community of scientists opposed to the FBI's forensic DNA method" (Scheck and Neufeld 1991, 19).

These activities were the beginning of a new community: defense lawyers who were attempting to undermine the credibility of DNA testing. They quickly obtained and used a report which supported their case that DNA profiling was not yet "generally accepted" in the scientific community. By copying the reports from the *Yee*

case, they were able to use Richard Lewontin and Daniel Hartl's credibility in a virtual manner. By these actions, the social worlds of science and law were entwined again. The agency of individual lawyers in obtaining and utilizing the report added to permanent court records, which became precedents which could be cited in later cases. Once again, DNA profiles were the boundary objects which were embedded in a web of shared meanings that was large enough to encompass the worlds of science and the law.

The adversarial process in general, and the Yee case specifically, had a direct effect on the academic publications of those who had been appearing as expert witnesses. Events in individual courtrooms had an effect on academia – one social world (the courts) changed the trajectory of the other (the academy). As a result of meeting their peers in the adversarial setting of the courtroom and debating scientific issues, the scientists who were called as expert witnesses for both the prosecution and the defense began to publish academic articles which reflected their opinion of the technique. The first of these articles was by Eric Lander who set his views to print in the front-line journal *Nature* (Lander 1989) after the *Castro* case. Lander pointed out the paucity of scientific research on both the theoretical and technical issues grounding claims made in DNA typing and argued that the correct place to work out the controversies over techniques was in the academic literature, not in the courtroom. He argued that there were severe problems with the interpretation of DNA typing results, due to a lack of consensus and standards in the scientific community (Lander 1989, 501). For Lander, what had happened was simple. The perceived power of DNA typing to uniquely identify individuals was so great that it had been rushed into court by private laboratories before the scientific research necessary to ground the technique had been conducted, and before the necessary standards had

been set. Lander said that the courts were premature in ruling that DNA typing was "generally accepted in the scientific community", stating that "the scientific community has not yet agreed on the standards that ensure the reliability of the evidence" (Lander 1989, 501). Sociologist of science, Sheila Jasanoff, presents a somewhat different picture, arguing that in the case of DNA typing, the adversarial process was successful in "exposing the unacknowledged and untested presumptions concealed within a seemingly robust scientific claim" (Jasanoff 1995, 55).

Perhaps responding to challenges by defense attorneys in some cases that the assumptions grounding their techniques had neither been published nor peer reviewed, the FBI began to publish articles in academic, peer-reviewed journals.[11] Lewontin and Hartl rewrote their expert witness reports for the *Yee* case and submitted an article critical of DNA typing to the journal *Science*. Daniel Hartl had been invited by Editor-in-Chief Daniel Koshland to submit a piece on a subject of his choosing. Instead, he and Lewontin submitted the article for peer review (Hartl 1997, Personal interview). Again, there is a direct effect between the events in the social world of the courtroom on the social world of academia. In our society, academia is the social world to which we look to tell us what is the "truth" about the natural world.

The actions of Lewontin and Hartl did not go unnoticed. Powerful members of the legal profession attempted to influence the content of this particular scientific publication. James Wooley, the prosecutor in the *Yee* case, saying he was acting in

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[11] Speaking to the issue of the effects of population substructure on the probability calculations, in *State v. Jakobetz*, both Dr. Kidd (Yale) and Dr. Budowle (FBI) conceded "that some substructure exist, but contend that the frequency differences for VNTRs between subgroups are insubstantial and thus are more than offset by the FBI's conservative fixed bin procedures" (747 F. Sup250, 1990 U.S. Dist. Lexis 12714, 260). The judge found it "of some concern that neither Dr. Kidd nor Dr. Budowle cite to any published studies to support these conclusions", but found their testimony credible and convincing anyway. See also *United States v. Yee et al*, 12 F.3d 540, 1993 U.S. ApLexis 32574, 550.

the interests of "justice", asked Hartl not to publish the article, which challenged the fundamental assumptions of the technique. "Wooley warned [Hartl] of the 'political consequences' of publishing and asked him to reconsider because of the possibly disastrous consequences for future DNA fingerprint-based prosecutions" (Anderson 1991, 500). Daniel Hartl says that

> Jim Wooley ... called me basically to say, you know, this would be a terrible thing for justice, you're going to lead to all kinds of horrible things, criminals going free, etc., etc. He had been told about the paper, and obviously was using his position, his ability to argue, to try to get me to withdraw the paper or at least disassociate myself from it. He said he was calling as a private citizen. Now that's hard -- you can't be called by a public prosecutor and he said this is a call from a private citizen. ... And he finally said 'what do you think I'm going to do to you? Look at your tax returns or something?' And I thought 'oh Jesus' I don't need this. And you know, he claimed later to have completely mis-spoke, but let me say that if you're at the other end of the line, you can easily take that as a threat (Hartl 1997, Personal interview).

A little later, at the 1991 meetings of the International Congress of Human Genetics, Dr. Kenneth Kidd approached a senior editor of the journal *Science*, with whom he was acquainted. He told her

> I don't know how this [the Lewontin and Hartl article] got accepted in *Science*, but something went really wrong. ... And I said this is going to be a major national problem. This is science and society, and that article is going to have a horrendous impact on the court system, an unwarranted impact, and a huge financial impact (Kidd 1997, Personal interview).

The senior editor went to Editor-in-Chief Daniel Koshland, who was also approached independently by several members of the population genetics community, at least one of whom, Dr. Tom Caskey, was a witness in the Yee case. Koshland was also contacted by officials from the FBI, who told him the article was "devastating" (Kidd

1997, Personal interview). Following this, Daniel Koshland asked to see the paper again. "Deciding that the 'discussion part went beyond the results part', he asked Lewontin to 'tone it down', but Lewontin refused, threatening to accuse Science publicly of trying to suppress the paper" (Anderson 1991, 500).

In response to this pressure from members of the population genetics community (Kidd 1997, Personal interview, Hartl 1997, Personal interview), Koshland decided to publish a rebuttal to Lewontin and Hartl's article in the same issue that their original article appeared. The rebuttal was co-authored by Kenneth Kidd, prosecution witness in the Yee case. These two articles set off what have come to be called the "DNA Wars" (Lander and Budowle 1994, 735).

3) Summary

In this first and second wave of cases, covering the period from 1987 to 1991, we see the destabilization of an initially uncontested knowledge claim. The adversarial setting of the courtroom allowed defense lawyers to expose the often local and idiosyncratic methods by which the private testing laboratories conducted DNA typing. They deliberately sought out and found a number of scientists willing to testify that the scientific knowledge which would support the private laboratories' claims either did not exist or was questionable. In other words, these scientists testified that there was no scientific grounding for the claims made by the testing laboratories. Richard Lewontin, Daniel Hartl and Eric Lander argued that much more research was needed before the claims could be said to be "true", particularly regarding the claims related to statements of probability associated with random matches. They were challenged by their equally esteemed peers such as Kenneth Kidd, Thomas Caskey and Bruce Weir, who argued that the population differences which Hartl, Lewontin and Lander saw as large and problematic were really small and

meaningless in practice. These debates will be explored in detail in the following two chapters.

After these high profile, very senior members of the population genetics community were brought together in the courtroom setting, they began to publish scientific peer-reviewed papers and letters to the editors of *Science, Nature* and *The American Journal of Human Genetics* voicing their concerns, both for and against the state of the art in DNA typing. In 1989, Eric Lander slightly pried open the lid of "Pandora's box", putting his concerns in print (Lander 1989). The lid really came off the box with the publication of Lewontin and Hartl's critical article in the December 21, 1991 issue of the journal *Science* (Lewontin and Hartl 1991), and the pro-status-quo rebuttal by Chakraborty and Kidd published in the same issue (Chakraborty and Kidd 1991). The concurrent publication of these two articles set off a long and heated controversy within the population genetics community. The concerns of these prominent scientists, and the FBI's increasing difficulty in having DNA evidence declared admissible in court, led the National Academy of Sciences to establish a committee to look into the forensic uses of DNA typing and the controversies associated with it.

At the beginning of this period, when the technology was first introduced to the forensic community and the legal system, "order" existed within the scientific community about knowledge on DNA typing. There was no controversy. As a result of its apparent acceptance within the scientific community and the fact that defense attorneys did not bring forth any expert witnesses to challenge the technology, judges found the evidence to be admissible, allowing it to help to establish order within the legal community. By the end of the period, after defense attorneys and prosecutors had brought the field's most prominent scientists into contact with each other in the

courtroom and asked them to establish the facts, the scientists were at each other's throats, at conferences and in print. The journal articles became weapons in the courtroom battles, as defense attorneys cited the articles critical of the technology, and prosecutors paraded the articles which said all was well. Judges went both ways in their decisions. The process of re-establishing scientific order would take the next five years, two National Academy of Science (NAS) committees, and countless articles in a number of prominent journals.

It is important to stop and take stock here: The events that unfolded over the next five years, in the two NAS committees, and the debates in the scientific journals were the direct outcome of actions by individual lawyers and scientists, transiting the nexus of science and the law. The scientists were introduced to a scientific problem, with which they had not before been confronted. Defense lawyers banded together and formed the DNA Task Force. Prosecution lawyers looked to the FBI and its expert witnesses. The expertise of many individuals was mobilized, and the effect of their actions reverberated across the domains of the law, science, and law enforcement. How order was attained and stable knowledge produced is the subject of the next two chapters.

Chapter Four

The DNA Wars: When Social Worlds Collide

1) Introduction

In late 1990 the National Academy of Science commissioned a report from the National Research Council's Board on Biology, titled "DNA in Forensic Science" (NRC 1992). This report was hastily rushed to print overnight and released on April 16, 1992. Why the rush? Because on April 14, 1992, two days prior to the publication of the NRC's report, Gina Kolata – a science writer for *The New York Times* – working from pre-publication pre-prints published a splashy front page article claiming that the long-awaited NRC report said that DNA evidence "should not be allowed in court in the future unless a more scientific basis is established" (Kolata 1992). Committee Chair Victor McKusick was upset and enraged when he heard of the article, and immediately called an emergency press conference to try to squelch any belief that the NRC's report was against the use of DNA typing in the courts (McKusick 1997, Personal interview).

The first NRC report was released in the middle of what have come to be known as the "DNA Wars" -- arguments among scientists, mostly about population genetics and the correct procedures for calculating random match probabilities. The NRC report was meant to solve problems which were seen as potentially damaging to the technology after DNA evidence was first declared inadmissible in 1989 in the case of *U.S. vs Castro*. This chapter details the dynamics of the DNA Wars, the efforts of the first National Research Committee to solve the problems, and the reaction of individuals in many different social worlds to the first NRC report: "DNA

Typing in Forensic Science" (NRC 1992).

2) The DNA Wars Prior to the First National Research Council Report

As discussed in Chapter Three, the DNA Wars began in the courtroom as prominent scientists were brought together in adversarial situations in the courtroom. Most of the initial arguments against DNA profiling were advanced in pre-trial *Frye* hearings intended to determine the admissibility of DNA evidence. The first criticism of DNA typing came out of the 1989 *Castro* case, in which both defense and prosecution witnesses banded together to declare that the DNA evidence submitted by Lifecodes Corporation did not meet the standards of adequate scientific evidence. After acting as a consultant and then an expert witness in the *Castro* case, Dr. Eric Lander published the first article critical of DNA typing in the journal *Nature* (Lander 1989). Lander focused mostly on revealing Lifecodes' "incredibly sloppy" laboratory procedures. He described DNA profiling as a new technology with tremendous power, but one which was lacking formal scientific credentials.

This first article critical of DNA profiling was a "wake up call" to the private laboratories which had been conducting DNA typing. The private laboratories had introduced a new technology, but had not published peer reviewed papers detailing the scientific foundations of the new procedures. And, because Lander's article appeared in the prestigious academic journal *Nature*, the academic community could not ignore his call for action. The article also set the legal community on edge, as *Castro* was the first time that DNA evidence had been declared inadmissible in a U.S. courtroom. The FBI was afraid that *Castro* would set a precedent which would mean that increasing numbers of judges would disallow DNA evidence.

In the wake of *Castro* and in the face of sustained efforts from the FBI, funding was finally found for the National Research Council to put together a committee to

look into the problems surrounding DNA profiling. In January of 1990, the FBI, together with the National Institutes of Justice, the National Institutes of Health National Center for Human Genome Research, the National Science Foundation, the Alfred Sloan Foundation, and the State Justice Institute provided the National Academy of Science (NAS) with funding for a National Research Council (NRC) committee called the "Committee on DNA Technology in Forensic Science." The NRC was delighted, as this first committee (hereafter referred to as NRC1) had been proposed by the NRC's Board on Biology several years before funding was found. Since the discovery of DNA fingerprinting in 1985, the Board on Biology had wanted to do something on DNA fingerprinting, but being dependent on soft money they had lacked the requisite funding.

It is important to understand that the National Academy of Science and the National Research Council are among the most prestigious and credible scientific institutions in the United States. The National Academy of Science gained its imprimatur during the American Civil War in 1863 from President Abraham Lincoln. Its purpose was to "investigate, examine, experiment, and report upon any subject of science or art" whenever needed to do so by any branch of the government (National Academy of Science 2000). One of the purposes of the National Academy was to guide public, or state action, in matters related to science. As such, the NAS has tremendous prestige, power and credibility, to speak about matters concerning the state of knowledge on a given subject. It is a powerful institution which can bestow credibility on individuals just by giving them the right to speak for the Academy.[1]

[1] Currently, the National Academy of Science has two functions. The first is to be a self-perpetuating honorific society: prominent scientists are elected to the NAS because of their accomplishments. The second mandate is to respond to requests from the government for research in science and the "useful arts," or technology (Fischer 1997).

The National Research Council is an institution of at least equal credibility and power. It was formed in 1916 during the First World War to be a more active arm of the then somewhat inert National Academy of Science. At the time there were concerns about the United States' preparedness to go to war, and the NRC was formed to aid in the "coordination of scientific and technological research and development." The initial mandate of the NRC was to bring together government, educational, industrial and other research organizations with the intent of investigating natural phenomena, in aid of national security. After the war, it was felt that the NRC had proved its value to the American people, and so President Wilson made it a permanent organization on May 11, 1918. The NRC differs from the NAS in that the advice from the NRC comes from the broader expert community in the country, and not just NAS members.[2] By forming an NRC committee to look into and pronounce judgment on scientific controversies surrounding the production and use of DNA profiling, the National Academy of Science was bestowing tremendous freedom, power and credibility on the individuals chosen to be on that first (and any) NRC committee. Insofar as this investigation is about the relationship between agency and structure, the NAS and the NRC have to be seen as very powerful, pre-existing social structures. They are not the dependent variables in this particular analysis, but social structures which pre-date these events, and which had a causal effect on events. The NAS and the NRC had not-so-vested interests in maintaining their credibility as arbiters for the state on matters of science. And so, when the first NRC report met with such heated criticism, in some ways, they had no choice but to convene another committee, to try to sort out what had gone so awry.

[2] Organizationally, the President of the NAS is also the Chair of the NRC.

Shortly before the first NRC committee put out its final report on April 16, 1992, there was a major move – actually a real beginning – in the DNA Wars. The expert witness reports submitted by Richard Lewontin and Daniel Hartl in the 1991 *Yee* case had been re-written as an article critical of population genetics issues in forensic DNA typing, and published in the premiere scientific journal *Science* in December, 1991 (Lewontin and Hartl 1991). This was done partly at the request of defense lawyers Barry Scheck and Peter Neufeld, who told Lewontin and Hartl that having a peer reviewed, published article to enter as evidence would strengthen the position of defense attorneys because they would have something of unqualified credibility to present in court (Lewontin 1997, Personal interview). As the reader will recall, a very controversial rebuttal to Lewontin and Hartl's article, written by Ranajit Chakraborty and Kenneth Kidd was also published in that same issue of *Science* (Chakraborty and Kidd 1991).

The publication of these articles in *Science* completed the transition of competition for intellectual authority over DNA typing from the commercial biotechnology community to the social world of academia. These two articles -- added to the concerns already stirred up two years earlier by Lander's *Nature* article -- set off a heated debate which took place in the pages of *Science, Nature*, the *American Journal of Human Genetics*, and the *Journal of Forensic Science*. Unlike most academic prose, the articles and letters which followed were worded in very strong, partisan language. As noted by Barnes, "it is true that scientists count and others do not, and ... it is equally true that some scientists count more than others" (Barnes 1985, 56). Prominent and prestigious scientists have a very strong influence on the acceptance and dissemination of new scientific knowledge "precisely because everything they say is taken seriously and evaluated with proper thoroughness[.] ...

[T]hey are able to act as sponsors of important new ideas and innovations and to use their standing to secure a fair hearing for them" (Barnes 1985, 56).

3) Lewontin and Hartl's Controversial Science Article

Since the articles by Lewontin and Hartl, and Chakraborty and Kidd had such an impact in scientific and legal[3] circles, it is worth going into their arguments in some detail here. These articles are an excellent example of how scientific knowledge is still transmitted through personal testimony, which is a form of action that rests almost solely on credibility (Shapin 1995). In the pages of *Science*, one of the most prestigious scientific journals, four very eminent scientists took positions, and used the full weight of their knowledge, eminence, credibility, and testimony to convince readers that their version of the truth was the correct one. Here we see the dynamics of agency in action – these scientists had tremendous credibility, the scientific establishment had great faith in their opinions, and in that venue they presented their opinions, and left the scientific community to choose the winners.

Lewontin and Hartl (1991) challenged the scientific validity of the product rule for calculating random match frequencies. Their argument was that the probabilities were calculated as if no population substructure existed, and independence of all alleles was assumed. "Population substructure" means that the population is not homogeneous, but is composed of separate groups which mate with each other. In other words, the groups do not mate at random with any member from any group. The problem is that the allele frequencies of the genes could be different in these sub-groups than in the large population. If this is the case, the population is not in

[3] Weir (1992: 11654) notes that in the case of *People v. Barney*, 8 Cal. App. 4th 798 (1992), the articles by Lewontin and Hartl and Chakraborty and Kidd were used to deny admissibility to DNA evidence.

Hardy-Weinberg equilibrium. If Hardy-Weinberg equilibrium holds true, then the gene frequencies in a given population are stable from generation to generation. If there are subpopulations that are not mating at random, then the gene frequencies are going to change from generation to generation. More pertinent is the fact that the frequencies in the subpopulations will differ from those in the overall population. The problem is not unlike the analysis of variance test in statistics, where by comparing variances of the groups, the researcher attempts to decide if there are separate groups in the distribution or if the observed variation is due simply to random sampling fluctuations. Lewontin and Hartl (1991) argued that there is indeed substructuring, and that what little data existed did support the theory that VNTR alleles were distributed differently in different subpopulations. This meant that probability calculations using the multiplication rule and based on an assumption of Hardy-Weinberg equilibrium would be incorrect. While Chakraborty and Kidd (1991) conceded this point, that there were different allele distributions among subpopulations, they argued that the 'problems' this caused were too small to take notice of.

Lewontin and Hartl correctly contended that there is no data to address the questions of whether population substructure exists in the distribution of these alleles, and that to determine whether it does or not, extensive empirical sampling of the population is necessary to obtain the true distribution of VNTR alleles.[4] Their concern

[4] The issue of potential effects of subpopulations on allele frequencies was first raised by Joel E. Cohen (1991). Although the abstract begins with "Some methods of statistical analysis of data on DNA fingerprinting suffer serious weaknesses," the article was not cited by Lewontin and Hartl (1991), nor by Chakraborty and Kidd (1991). This issue was also brought up even earlier, in Devlin, Risch and Roeder (1990b). In an age of increasing information and differentiation between disciplinary specialties, it is not uncommon for important contributions by relatively unknown scientists to go unnoticed, and for more famous scientists to receive tremendous attention when they take up the same topic. The history of science is riddled with stories of people who were not attributed with great discoveries, simply because eminent

was with the 40 to 50% of criminal cases in which a suspect's VNTR profile did match that of a forensic sample. The question was whether a valid and reliable estimate of the probability of matching between 'random' individuals could be obtained with the use of the then current method of multiplying together the estimated frequencies with which each of the individual VNTR pattern occurs in a reference database (Lewontin and Hartl 1991, 1746). They asked two questions. First, what was the correct reference population for calculating the random match probability, and second, how should the data from different VNTR loci be combined to get an overall probability (in other words, what method should be used to calculate the random match probability).

At the time, with regard to the question of reference population, the FBI was using only Caucasians and Blacks (Budowle *et al* 1991, 899). The FBI claimed that "each of these groups constitute a homogeneous population undergoing random mating within itself, so that reliable probability statements can be made based on the reference samples" (Lewontin and Hartl 1991, 1746). To answer the second question, because the FBI assumed that Caucasians and Blacks were selecting mates at random with respect to VNTR alleles within their own populations, they felt that they could obtain correct probabilities by multiplying the frequencies for each of the VNTR loci separately (the multiplication rule). The rationale that made it all right to multiply the different frequencies at different loci was the assumption that the groups were undergoing random mating, that the loci were on different chromosomes, that the chromosomes were assorting at random, and that they were "independent in a

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natural philosophers of their day, such as Darwin and Gauss, failed to read their letters (Barnes 1985).

During its history, DNA profiling became entwined in several different systems of classification (Bowker and Star 1999). What began as the most contentious classification schema (and ultimately became the least important) was that of how to divide the VNTR alleles of individuals by racial group for the calculation of a random match probability of a DNA profile.

statistical sense." This was the assumption that the loci were in linkage equilibrium (Lewontin and Hartl 1991: 1746).

Lewontin and Hartl disputed both of these claims and argued that the FBI was ignoring a "considerable body of evidence" that there was genetic substructure within what were called Caucasian, Black and Hispanic populations. They argued:

> The census populations designated 'Caucasian', 'Black', and 'Hispanic' are actually each made up of multiple subpopulations that are genetically diverse. Consequently, with currently available data, the current method of estimating the probability of a match by multiplying together the frequencies with which each of the individual VNTR pattern occurs in a reference database is unjustified. Furthermore, the magnitude and direction of the error depends on the particular VNTR locus, the bands observed, and the reference database. Hence, it cannot be ascertained whether the estimates as currently calculated are biased for or against any particular defendant. On the other hand, although the current method is flawed, it is not irretrievable, and suitable data could be gathered that would allow acceptable probability estimates to be made (Lewontin and Hartl 1991, 1746).

Lewontin and Hartl were arguing that census categories had been reified into racial categories, and that untested assumptions had been made about these racial categories. They examined the makeup of the categories of Caucasian, Black and Hispanic, and found that there was evidence that these populations were not undergoing random mating. While some educated guesses could be made as to the extent of genetic polymorphisms among Caucasians of European descent, because the political and migratory history of these subpopulations was known, almost no reliable data existed on the genetic heritage of African-Americans brought to the United States as slaves. This population has undergone varying degrees of mixture with American Indian and European populations, and some alleles which exist in the Caucasian population have been found not to exist in some Black populations. In addition to mixture with American populations, African populations experienced

significant migration in Western Africa, particularly with respect to the slave trade, and also with respect to the spread of Islam (Lampland 2002).

At the time, there were no extant data on VNTR distributions among this group. With regard to Hispanics, Lewontin and Hartl called this a census category a "biological hodgepodge" (Lewontin and Hart 1991, 1748) which was without biological meaning. They stressed that no meaningful reference population could be put together for this group. They made the very strong claim that the data on VNTR allele distributions did not exist to determine the extent of substructuring within *any* population.

In population genetics, "random mating" has two different meanings. The first is that a particular genetic characteristic does not directly determine a person's choice of a mate. The second, which Lewontin and Hartl took issue with, is that "individuals choose their mates without regard to region, ethnicity, geography, and so on" (Lewontin and Hartl 1991, 1746). Lewontin cited sociological evidence to show that people still tend to marry within a like group, and so the second sense of random mating could not be true. They said that "[f]rom the standpoint of the entire census population, when people choose their mates endogamously, they are unconsciously making a choice among blood groups and other traits correlated with ethnicity" (Lewontin and Hartl 1991, 1747).

By late December of 1991, courtroom debates about population genetics had focused mainly on statistical tests of Hardy-Weinberg equilibrium. However, statistical tests for Hardy-Weinberg equilibrium have very little power to indicate population substructure, because even very large genetic differences between subgroups result in such small deviations from Hardy-Weinberg equilibrium that they are virtually undetectable by statistical tests. Lewontin and Hartl argued that *not* finding deviations from Hardy-Weinberg equilibrium did not indicate a lack of population substructure:

> Statistical tests for Hardy-Weinberg equilibrium are so lacking in power that they are probably the worst way to look for genetic differentiation between subgroups in a population. The proper approach is the straightforward one of sampling the individual subgroups and examining the differences in the genotype frequencies among them (Lewontin and Hartl 1991, 1747).

The consequence of finding subpopulations was that there was no single homogeneous reference group which could be used for estimating the probability of a random match. In this case, each individual may require their own reference group "composed of appropriate ethnic or geographic subpopulations." What upset the FBI the most was that Lewontin and Hartl claimed that because of the potential for population substructuring, the multiplication rule could not be used legitimately to calculate random match probabilities across multiple loci: "[t]he implication for DNA typing is that American ethnic groups may have substantial differences in the frequencies of multi-locus genotypes. Therefore, it is inappropriate to use a general multiplication rule and an arbitrary 'Caucasian' database to calculate the probability of a multi-locus VNTR match" (Lewontin and Hartl 1991, 1748).

4) Chakraborty and Kidd's Rebuttal

Ranajit Chakraborty and Kenneth Kidd claimed that their rebuttal to Lewontin and Hartl was situated "at the nexus of science and the law," and they said that "[t]he significance of a DNA match should be evaluated in a legal setting" (Chakraborty and Kidd: 1735). In portraying the debate this way, they were saying that this scientific debate would be subject to different rules that those that held in academic science, because of the connection to the practical world of the courtroom. They were attempting to move the site of adjudication from the social world of science to the social world of the courtroom. Rather than referring the issues to population geneticists, for whom the terms "population" and "sub-population" are ordinary vernacular, they tried to ground the ontology – or rather the *lack* of a meaningful

ontology -- of populations, reference groups and subpopulations in legal principles: "in reality no precise genetic definition of either population [reference or subpopulation] emerges from any *legal* principle" (Chakraborty and Kidd 1991, 1735, emphasis added). They tried to draw a distinction between the empirical scientific question of 'what is the case in the world' and the more pragmatic question of 'what is adequate science for legal use.' They claimed that the courtroom did not need *exact* measures, but only *meaningful* estimates:

> In the context of courtroom applications of DNA typing, it is necessary to *draw the distinction between exact values and valid estimates*. The issue under debate is whether, when a match occurs, a meaningful estimate can be obtained for the frequency of the DNA pattern (Chakraborty and Kidd 1991, 1735, emphasis added).

Chakraborty and Kidd were attempting to transfer their scientific credibility to some unknown entity which they called a "meaningful estimate." However, given that the *Frye* standard asserts that the truth of scientific claims in the courtroom depends on their "general acceptance" by the community of scientists, in retrospect this attempt seems naïve, and almost desperate. That sense of desperation came across in personal communication with Kenneth Kidd – he was absolutely convinced that any criticism of DNA profiling would jeopardize the status of the technology and halt its use in the courtrooms of America.

Chakraborty and Kidd clearly took a pragmatist position, and attempted to shift the debate to what would count as a "meaningful estimate" adequate for use in the courtroom. Unlike Lewontin and Hartl, they did not take a strongly empiricist position, and they *did not* believe it was necessary to find any "exact values," which would be difficult, expensive, and time consuming to attain. They argued that following this

empirical approach would needlessly delay use of DNA profiling in the courtroom.

Chakraborty and Kidd's secondary argument was that the markers used in DNA profiling were not essentially different from other genetic markers which had been in use for many years. They claimed that VNTRs differed from older markers only in that VNTRs had more alleles, and that present technology did not allow exact measurement of VNTR lengths. Thus the number of alleles at any given site on the DNA molecule could not be exactly established.

Chakraborty and Kidd's third argument involved the role of race in DNA profiling. They argued that the relevant reference population for a random match probability is that of all potential perpetrators, which should be determined by geographical location and time of occurrence of the crime, not by the race or ethnicity of the accused. Their "reference population" was a theoretical construct composed of mixed races and ethnicities, defined mostly by geographical criteria. In contrast to Lewontin and Hartl, they discounted the possible effect of subpopulations on the random match probability calculations:

> no assumptions regarding 'random mating' or 'population substructure' are needed in [random match] computations. In fact, classic population genetic principles show that even if the reference population was a mixed one, these 'binned allele frequencies'[5] are unbiased estimates, of the averages of all underlying ethnic or endogamous subgroups contained within the reference population (Chakraborty and Kidd 1991, 1736).

Chakraborty and Kidd said that it was legitimate to assume Hardy-Weinberg equilibrium and linkage equilibrium even if the reference population was of mixed ethnic and racial origin. They argued that "even if the subgroups contained in the

[5] VNTR lengths cannot be measured exactly, because existing agarose gel and electrophoretic technology lack the ability to distinguish between alleles of slightly different sizes. Therefore, VNTR allele lengths are placed into "bins," rather than being recorded as discrete lengths, much as we code age in years, or groups of years, rather than exact years, months and days since birth. The difference is that with age, we can usually establish the exact age if we want to.

reference database have significantly different allele frequencies, their effect on deviation from Hardy-Weinberg equilibrium and linkage equilibrium is so small that the effect cannot be detected in practice" (Chakraborty and Kidd 1991, 1736).

Chakraborty and Kidd framed the issue in terms of "how big is big"? They gave the example of a crime committed in an Italian and Polish neighborhood, and used as an example the assumption that the perpetrator was a child of an Italian and a Pole. Using blood group data, and first calculating the probabilities separately for Italians and Poles, they then averaged the probability and reported that an estimate of the probability of a random match in this population would be 3.69×10^{-5}. Then they asked the reader to pretend that only pooled population data were available. Under this scenario, the mixed probability comes out to be 1.19×10^{-5}, which is "only 3.1 fold smaller than the best estimate." Chakraborty and Kidd argued that forensically, the difference between these two probabilities was meaningless, because the difference in probabilities of a random match are 12 versus 37 in a million, which may be *statistically* different, but not *forensically* meaningful (Chakraborty and Kidd 1991, 1736). For Lewontin and Hartl, a statistically significant difference represented an ontological difference in the natural world. However, for Chakraborty and Kidd the multiplication rule was "robust, even when the allele frequencies are chosen to indicate that the subgroups are genetically well differentiated" (1991, 1736).

Chakraborty and Kidd challenged the relevance of sociological data which suggested that people marry within like groups who are in close proximity. They then argued that even if people were mating in a non-random way, it would have almost no effect on deviations from Hardy-Weinberg equilibrium, and thus statistically, the presence of sub-populations would be undetectable.

The reality of human evolution shows that even though marital

> preference is nonrandom at every level at which one can define populations, its effect on deviation from HWE of genotypic frequencies or linkage equilibrium is minimal. No new population genetic principles are needed to apply this thesis to forensic DNA typing (Chakraborty and Kidd 1991, 1737).

To complicate their argument further, Chakraborty and Kidd conceded that although substructure might be present in a population, that the "component subpopulations are genetically similar," thus the presence of subpopulations would have no effect on the population as a whole. They argued that for VNTR alleles, there was more variation between racial groups overall than there was between subpopulations within a population -- thus making it legitimate to treat "Caucasians" or "Blacks" as a population, and not worry about substructure. For Hispanics, who are not a racial group, they suggested that the U.S. Hispanic database from the Southwest would be adequate (Chakraborty and Kidd 1991, 1738). They concluded that Lewontin and Hartl's "concern about the inappropriateness of the HWE [Hardy-Weinberg Equilibrium] and multiplication rules has no basis" (Chakraborty and Kidd 1991, 1738).

Chakraborty and Kidd went on to examine empirical VNTR allele data for two loci. They presented a "worst case scenario" and calculated the random match probability from distributions which deviated from Hardy-Weinberg equilibrium, and in which alleles differed drastically in their frequency between Caucasians and Blacks. They argued that the probabilities obtained when treating the groups individually or as a mixed population did not "differ in any meaningful way." The meaning attributed to the difference in random match probabilities obtained under the "worst case scenario" was that the difference was forensically insignificant, or "small is small."

Chakraborty and Kidd concluded that

> we are not concerned with estimating the frequency of a DNA profile among individuals who have the same ethnic ancestry as a defendant, for example, one-eighth Irish, one-fourth Italian, one-eighth French,

> one-fourth Polish, and one fourth Amerindian; no such database will ever exist, nor is it necessary. ...[T]he U.S. Caucasian database and the U.S. Hispanic database from the Southwest will provide conservative estimates that indicate the degree of uncertainty that might exist (1991, 1738).

They claimed that uncertainty in estimates is common in statistics, and that they had shown that the uncertainty in random match probabilities was far less than that predicted by Lewontin and Hartl. What was important was the VNTR allele frequencies in the general population, not the frequency in any particular subgroup to which the suspect or defendant may or may not belong. The practice of using binned allelle frequencies rather than exact allele frequencies "assures that most of the frequencies used are overestimates," and thus produced conservative random match probabilities.

Lewontin and Hartl suggested three solutions to the problem. The first was not to use the multiplication rule, but to use the frequency of the multi-locus profile in the available database (Lewontin and Hartl 1991, 1749). Every new profile that did not exist in the database would have a frequency of less than 1/N (where N is the total number in the database). Chakraborty and Kidd agreed that this would be a conservative solution, but argued that it was too conservative and that it was not realistic because there were so many more multi-locus profiles possible than could be attained in any one sample that comprises a database. "The use of the 'don't multiply' approach with a database of a few hundred samples fails to convey adequately the true significance of a match" (Chakraborty and Kidd: 1738).

Lewontin and Hartl's second solution was to set "ceiling" frequencies for a number of different ethnic subpopulations within the major racial groups (Lewontin and Hartl 1991). Under this method, for each locus, the maximum frequency of *any* VNTR allele from all of the ethnic subgroups would be used in the multiplication rule. Eric Lander (1991) noted that this method should be "robust" even when the

defendant's ethnic background was not known, because it will take the maximum frequency from a wide selection of databases that capture the range of variation in allele frequencies among subpopulations. Chakraborty and Kidd argued that this method was essentially the one already in use in forensic laboratories:

> the assumption is now made by some forensic laboratories that the source of the evidentiary sample is from a specific racial group (say, Caucasian, Black, or Hispanics), and respective databases are used for estimating probabilities. The use of the largest of the compared values provides an additional safeguard beyond those in place for each database alone (Chakraborty and Kidd: 1738-39).

The claim that something akin to ceiling principles were already in general use in forensic laboratories is extremely interesting. If this was indeed the case, then it is difficult to understand both the FBI's reaction and the reaction of population geneticists and statisticians to the method of calculating random match probabilities recommended by the first National Research Council Committee on forensic uses of DNA, known as the "ceiling principle." The ceiling principle that the first NRC committee proposed is essentially the same as that advanced by Lewontin, and endorsed by Lander. The ceiling principle is a method for calculating random match probabilities that used the highest appearing frequency in *any* racial database, and so gave the most conservative estimate of a random match probability that was possible. If, as Chakraborty and Kidd claim, some version of the ceiling principle was already in use in most laboratories, then why was the FBI so opposed to the first NRC report and the proposed use of the ceiling principle. The FBI was so unhappy that FSRTC director Bruce Budowle felt the ceiling principle would "tie the FBI's hand in court" (Budowle 1997, Personal interview). The FBI Director requested a second National Reseach Council committee on forensic uses of DNA? During the course of this study, no evidence surfaced to indicate that the FBI or any other forensic laboratory

used any version of any form of a "ceiling principle" prior to its appearance in the first NRC report (NRC 1992). Instead, they reported using the simple multiplication method which assumes independence of alleles across multiple loci and uses actual frequencies from existing databases. The first NRC committee and the ceiling principle are discussed at length in the next chapter.

Chakraborty and Kidd did not address Lewontin and Hartl's third proposal which was to "fix the current method." To do this, Lewontin and Hartl suggested collecting empirical data on VNTR frequency distributions in the many subpopulations that make up the American population at large. Lewontin and Hartl note that:

> Some data of this sort has begun to appear for Amerindians, Hispanics, and American blacks. Not surprisingly, differences that are highly statistically significant are found among subgroups, confirming the current understanding of human population substructure and genetic differentiation inferred from extensive anthropological sampling of blood groups and enzymes (1991, 1749-50).

For their part, Chakraborty and Kidd argued that they had shown both theoretically and empirically that the current method "does not require 'fixing' for it to be used in courts." Further, they argued that no matter which data was used to estimate the probability of a random match, no meaningful difference in the probability resulted: "[r]eal examples given above indicate that even if the defendant belongs to a small endogamous subgroup, no meaningful change in the interpretation of a DNA match occurs by using the current data" (Chakraborty and Kidd 1991, 1739).

The argument between the opponents boiled down to issues of pragmatism versus empiricism in the calculation of an estimate in a certain context. Lewontin and Hartl took the hard-line empiricist position, that the extent of population substructuring was unknown, it was not known how different the distribution of VNTR alleles was between different ethnic groups, and because that was not known, it could not be

determined whether the errors made in using the multiplication rule were conservative or not. Chakraborty and Kidd's position garnered the support of the FBI, prosecuting attorneys, and many other scientists, including Thomas Caskey of Baylor University's College of Medicine, who sat on the first NRC committee on DNA typing. Kenneth Kidd's summed up his position when he said that "[i]t makes absolutely no difference to me if the number is 1 in 800,000 or 1 in 5 million, [and] it probably doesn't matter to a jury either" (Roberts 1991, 1721).

5) Response to the *Science* articles

After the publication of these two articles, the responses came rapidly in the form of letters to the editor of *Science*. The first series of letters appeared in *Science* on February 28, 1992 (Wills 1992; Austad 1992; Bever *et al* 1992; Yarbrough, 1992; Cleveland 1992; Koshland 1992; Chakraborty and Kidd 1992; Lewontin and Hartl 1992). Writers had varying concerns, including incredulity about the sequence of events surrounding the publication of Lewontin and Hartl's article (Yarbrough 1992; Cleveland 1992). These letters, published in one of the most prestigious academic journals in the world, show members of the academic community mobilizing to respond to what was for them a new problem of knowledge. Lander's 1989 article in *Nature* had introduced the topic, but in the interim no one of equal prominence and credibility had taken up the issue in print. Letters to the editor are not peer reviewed, and so writers are free to express their personal opinions. This is one of the scientific spaces where individual agency prevails (although it was controlled by the editor of the journal, at that time, Dr. Daniel Koshland). Letters to the editor are an important arena in which the "DNA Wars" were fought. Crucial issues, such as the role of race in DNA profiling, and the ontology of race, which included questions about the existence of subpopulations, were explored here. These first letters were focused mainly on population genetics issues, but there were varied concepts of "race" and

"population" at play in the discourse. It appears that for all writers, "race" had a biological ontology of some form, although they did not make this clear or explicit.

The ontological, epistemological, and cultural confusion that reigned in the academic community over concepts of race and population are clear in the following letters. For example, one letter claimed that Lewontin and Hartl used "old and inappropriate blood group data" to bolster their claims (Willls 1992). The writer then denied that there were meaningful differences between racial groups, and that it was probably impossible to collect the kind of data (blood from 15 to 20 "pure" populations) that Lewontin and Hartl claimed was required for sound empirical knowledge. He then went on to claim that it was not necessary to gather this data because the "churning process has homogenized racial groups to a remarkable degree" (Wills 1992). The "churning process" would be interpreted by social scientists as marriage between racial/ethnic/population groups.

Another group claim was that no "pure" groups can be isolated in the American population to act as reference groups for calculations of random match probabilities, because of the "heterogeneity that exists in most second- and third-generation American Caucasians and blacks" (Bever *et al* 1992). Without defining how they conceptualize race, nor how this might be different from a "pure group," these researchers go on to claim that *their* databases are accumulated by the racial groups of Caucasian, Black and Hispanic, not by ethnic origin (Bever *et al* 1992).

On this issue Chakraborty and Kidd had argued that not only was Lewontin and Hartl's data old, but that their claim that Polish and Italian immigrants to the United States constituted a homogeneous group was false, because these immigrants "did not come from any single subgroup in these countries" (Chakraborty and Kidd 1992). They suggested that in the absence of knowledge of ethnic origin, that the national average (in respective countries) for allele frequencies should be

used. Chakraborty and Kidd appeared to make dual assumptions about other countries: that their populations differed significantly enough by region so that Lewontin and Hartl could not legitimately use data on immigrants, but also that these populations were homogeneous enough that a "national average" of allele frequencies was somehow meaningfully representative of genetic variation in the country. They seemed to assume that while the United States was a melting pot, the populations of other countries were "purer."

These letters represent the openness of the scientific domain to input from other actors. They are not just expressions of opinions of members of the scientific community, but represent part of the field on which the dynamics of agency and structure in DNA profiling were played out. That people from different disciplines responded illustrates the complexity of the issues involved, and possibly the importance of the topic.

In their response to the letters Lewontin and Hartl denied claims that they were "against" DNA typing (1992). In their 1992 letter, they reiterated their belief that "DNA typing is possibly the most powerful innovation in forensics since the development of fingerprinting in the last part of the 19th century" (Lewontin and Hartl 1991, 1746; Lewontin and Hartl 1992). Lewontin and Hartl concluded their letter with a political challenge, asking whether the push to calculate probabilities in the absence of adequate data was because "the organizations whose interests are served by numerical exaggeration have also been in charge of choosing the statistical procedures" (Lewontin and Hartl 1992, 1055).

Interestingly, the findings of the first NRC committee on DNA technology were more in line with Lewontin and Hartl's viewpoint, that the extent of substructuring was unknown and needed to determined. Lewontin and Hartl's views were carried to the

first NRC committee in written form by Richard Lewontin, and verbally by Eric Lander. Their views were deemed to be anti-DNA profiling because their objections were perceived to put unnecessary roadblocks in the way of a perfectly good technology. The empirical studies they called for were considered by some to be impossible -- how would random samples be obtained from 100 people in 15 to 20 different populations? The second committee decided that it was an unnecessary, impossible task (Reilly 1997, Personal interview). The second NRC committee came out with a verdict much more congruent with Chakraborty and Kidd's belief that no matter which database was used to calculate the probabilities, they were always small. Interview evidence showed that many people involved in the DNA Wars felt that the reason for the second committee was that the first committee had made many mistakes, it is also possible that the position of the first committee was too close to Lewontin and Hartl's politically incorrect call for empirical research, while the second committee came down with a verdict much more in line with the mainstream justice system. That is, no more work needed to be done and random match probabilities could be calculated in the manner in which the FBI had begun doing them in the late 1980s.

One of the issues during the early stages of this debate was about what constitutes "reasonable agreement" between theory and data. Hence the arguments about how "big" deviations need to be to indicate the presence of substructure, or how irrelevant several orders of magnitude are in a small probability -- "small is small." In this stage, this controversy can be seen as a normal process of science, where scientists attempt to establish a common order of agreement when they are attempting to measure, or apply numbers to attributes of the physical world in areas where that has not been done before. That consensus was achieved socially, and because it is supposed to bind the actions of individuals, it had a moral component.

Consensus is normative and provides a foundation for the evaluation of what counts as criteria of reasonable agreement (Derksen 2000). This consensus arises out of the practice of science. Its attainment is one of the crucial junctures, required for objective knowledge.[6]

What is defined as "reasonable" is the outcome of a social process of boundary work (Gieryn 1983) where each side tries to define its own viewpoint as "inside" the boundaries of reasonable, and those of the opponents as outside the boundaries. For example, Chakraborty and Kidd defined Lewontin and Hartl's viewpoint as unreasonable:

> [Chakraborty and Kidd] contend that Lewontin and Hartl are counting angels on the head of a pin; engaging in a fascinating if esoteric academic debate that has almost zero relevance to the use of DNA fingerprinting in court. Similarly, one of their staunch supporters in the FBI, John Hicks, director of the Crime Laboratory, calls the whole dispute 'much ado about not very much' (Roberts, 1991, 1723).

Boundary work also involved determining how big a deviation was big enough to count for or against a particular viewpoint:

> Sure, Chakraborty and Kidd concede, there are genetic differences among subgroups, but they are not as great as Lewontin and Hartl make out. 'There is overwhelming evidence that no genotype is common', says Kidd. And even if huge undetected genetic differences do exist, they say, the procedures used by the FBI and the testing companies are robust and conservative enough to compensate for them (Roberts 1991, 1723).

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[6] For a discussion on how laboratory "techniques" like PCR or DNA fingerprinting can be seen as social, see Jordan and Lynch (1996), "The dissemination, standardization, and routinization of a molecular biological technique." Here, Jordan and Lynch argue that techniques like PCR are social in at least two senses. First, "a technique is an artifact 'in' a social context, like a raisin in a pudding. According to this conception, the relevant way to reveal the culture of an artifact is to specify how the context determines, shapes, affects, impacts, sets conditions for the use of, provides resistance to, or sustains the technical object in question. Variations in the design of the artifact can thus be indexed to group interests, communities of users, modernizing influences and modernity itself, and commercial appropriation and control" (manuscript, p. 6). The technique is social in a second sense, in that its existence and production are not guaranteed by instructions or definitions, but "are accomplished through laborious struggles and negotiations" (manuscript, p. 6). See also Jordan and Lynch, "The Mainstreaming of a Molecular Biological Tool" (1992).

Lewontin and Hartl consistently tried to make the boundaries of the debate be based on empirical evidence, not models or hypotheses:

> [T]hey ask, how can Chakraborty and Kidd say the current procedures are conservative enough when there are simply no data that would allow them to estimate the magnitude of the error? (Roberts 1991, 1723)

From Lewontin and Hartl's viewpoint, data speaks for itself, and at this point they believed there was a paucity of data – thus nature could not speak and there was no empirical support for using random match probabilities in court. Chakraborty and Kidd saw the question as one of the meaning attributed to numbers. They conceded that differences might be statistically significant, but when the size of the difference was examined, they felt that it did not make any *meaningful* difference. For them, "small is small," and any match and its associated random match probability should count as evidence. The order of magnitude of the random match probability was of little meaning to Chakraborty and Kidd, and of great meaning to Lewontin and Hartl.

6) Hardy-Weinberg Equilibrium and Coalescence: How 'Big' is 'Big'?

Another case of boundary work involving the issue of "how big is big" was the debate about coalescence versus homozygosity. In 1990, long before Lewontin and Hartl published their article, and in the early stages of the first NRC committee, a debate had begun in print between academic scientists about Hardy-Weinberg equilibrium, and the effect of deviations from it on calculations of random match probabilities. Interestingly, neither Lewontin and Hartl's article, nor Chakraborty and Kidd referenced any of these articles in their own publications, although they are directly related. Ror *Science* readers, or perhaps the *Science* editor, the debate did not begin until scientists with the credibility and high profile Richard Lewontin and Kenneth Kidd joined the game. The previous articles were not referenced in all the

subsequent debates.

Cohen (1990), and Devlin, Risch and Roeder (1990) do reference Lander's 1989 article in *Nature.* This lends support to my argument that it was the fallout from the *Castro* case and Lander's *Nature* article that caught the attention of high profile members of the academic community. They began to appear as expert witnesses, and then began to publish the results of their reports to the court, thus instigating the "DNA Wars."

The debates in the literature before the DNA Wars were about measurement, and how "big" a deviation had to be to indicate the presence of subpopulations. The arguments also focused on technical measurement issues such as how to determine the difference between coalescence versus an excess of homozygosity. These prior debates took place in articles, 'Technical Comments' and letters to the editor. The content began with the premise that at each locus on a DNA profile, two bands should appear on the autorad, since one band is inherited from each parent. Coalescence is a phenomenon that appears when the two bands are of *almost* equal length, and so they appear on the autorad as one band, sometimes quite blurry. It is impossible to tell if these bands represent real coalescence (two bands appearing together, thus indicating a normal heterozygote -- a person with two bands), or if the presence of one band represents "homozygosity" (a rare individual with only one band is called a homozygote). Joel Cohen (1990) argued that the issue of an excess of homozygosity in some databases was evidence for deviations from Hardy-Weinberg equilibrium and thus evidence for the presence of subpopulations. He argued that these subpopulations would then throw off the calculations of random match probabilities.

In population genetics, it is generally assumed that an excess of homozygotes means that the database includes two or more populations which are not mating

together. Devlin, Risch and Roeder (1990) argue that technical limitations prevented these claims from being tested. The technical limitations involved the measurement error intrinsic to agarose gel electrophoresis, preventing the determination of fragment lengths of two bands of very similar size. Devlin, Risch and Roeder examined Lifecodes' database and determined that what appeared to be an excess of homozygotes, thus evidence for subpopulations, was in fact just many coalesced bands misclassified as homozygotes. Devlin, Risch and Roeder's goals were to

> (i) show that there is an apparent but not real excess of homozygotes at VNTR loci, making previous tests of H-W [Hardy-Weinberg Equilibrium] invalid; (ii) develop an appropriate method of testing H-W for VNTR loci; and (iii) demonstrate that there is no evidence that H-W is violated for three VNTR loci commonly used in forensics (1990, 1417).

Although they were not acknowledged, the issue of Hardy-Weinberg equilbrium and the effect on random match probabilities of deviations from that equilibrium were already on the academic table when Lewontin and Hartl's (1991) controversial article hit the pages of *Science*. Lewontin and Hartl contended that if sub-groups existed, then there should be higher frequencies of certain single and multi-locus genotypes observed *than would be expected if the sites were independent*, that is, if the groups were undergoing random mating. In other words, if subpopulations existed, then one would *expect* to see an excess of homozygotes.

The debate about coalescence continued over the period over the publication of the two major articles in *Science*, and in the aftermath, became intertwined with those issues. People took partisan positions, and split into two camps which roughly coincided with the pro-DNA typing and con-DNA typing people. On the pro side, Chakraborty and Kidd (1991), Devlin, Risch and Roeder (1990a, 1039; 1990b, 1416),

and FBI scientist Bruce Budowle (Budowle *et al* 1991; Budowle 1992) all argued that in order for these deviations from what is expected under independence to be meaningful, there had to be fairly large variations in allele frequencies between the sub-populations. They argued that this large amount of variation would require very extreme demographic and genetic conditions, which, they did not believe existed. Thus, they concluded that there were no subgroups, that the assumption of independence between alleles held, and that it was appropriate to calculate random match probabilities using the multiplication rule. Bruce Budowle of the FBI argued that "at present, it is not possible to assess whether, for the alleles at a particular VNTR locus analyzed by southern blotting a population sample is in Hardy-Weinberg equilibrium." Nevertheless, he went on to assert that "a reasonable, empirical assumption of random association of alleles can be made" (Budowle *et al* 1991, 841).

Eric Lander (1991), Philip Green (1992), and Lewontin and Hartl (1991) all disagreed, claiming that an excess of homozygotes was empirical evidence for deviation from Hardy-Weinberg equilibrium, and that this deviation was due to population substructure. However, the other side countered -- arguing that the excess of homozygotes was attributable to the technical factor of coalescence, where the fragments are not "really" the same length -- they are just a tiny bit different in length -- but that small difference is too small to be resolved on a gel, and so it only appears that there is an excess of homozygotes (Devlin, Risch and Roeder 1990b; Devlin and Risch 1992). They argued that the appearance of too many homozygotes was essentially a function of measurement error.

Lewontin and Hartl claimed that if probabilities were not calculated on the correct reference group, the probabilities would end up being artificially tiny, with a power to convict all out of proportion to what they should be. Lewontin, Hartl, Lander,

Green, and Cohen all argued that the chances of this were higher in some ethnic and racial subgroups than in others. They claimed that if the incorrect frequency distribution was used to calculate the probabilities, they would come out too low -- giving the probability too much power to establish unique identity, and the court would therefore place too much weight on it. Lewontin's concern was with the potential probative power of DNA profiling, and his goal was to prevent the possibility that convictions would be obtained on the basis of very small random match probabilities, calculated from incorrect databases. He felt that before DNA profiling was used widely in the courts that random match probabilities should be calculated on empirically known information (Lewontin 1997, Personal interview).

For the analyst, the complex issue of the ontology of race is a thread running throughout the history of DNA profiling. Richard Lewontin has argued that variation within races exceeds variation between races, and therefore, race does not have a biological existence. Lewontin's entry into the DNA profiling fray as a radical empiricist on the ontology of sub-populations seems at first a paradox, but he couches all his language in terms of populations and sub-populations, not race.

In an attempt to address the extent to which population differences were a problem for the calculation of random match probabilities, Bernard Devlin, Neil Risch and Kathleen Roeder analyzed the two databases in existence in the late 1980's -- one compiled by Lifecodes and one by the FBI. Both were supposed to be representative of the "American" population, but both were actually samples of convenience, taken from blood banks, paternity testing laboratories, or in the case of the FBI from their own agents (Devlin, Risch and Roeder 1990a; 1990b). They found that there were statistically significant differences in allele frequencies among some of the racial groups. However, the *meaning* attributed to these differences was

downplayed. The FBI's Bruce Budowle (Budowle *et al* 1991), and Chakraborty and Kidd (1991) said that the differences between databases were not large enough to be evidence for deviation from Hardy-Weinberg equilibrium, and claimed that they were the result of the DNA profiling production process: the technical artifact of coalescence. Their position was that no *forensically significant meaning* need be attributed to the statistically significant differences in allele frequencies between the groups.

7) The First National Research Council Committee: "DNA Technology in Forensic Science"

In late 1989, the National Research Council's Board on Biology began to select members for their Committee on "DNA Technology in Forensic Science." The committee began to meet in January of 1990, its last meeting was on the day of the now infamous *Science* publications, and its report was issued on April 16, 1992. In selecting members for the Committee, the Board on Biology sought to strike a balance between genetics, forensic scientists, and representatives of the legal community. They did not anticipate that issues of population genetics or statistics would be of major concern, and so no "card-carrying" statisticians or population geneticists were invited to sit on the committee (Fischer 1997, Personal interview). The people with the most expertise in these areas were mathematician and geneticist Eric Lander, who held appointments at MIT and Harvard and geneticist Mary-Claire King, who was then at the University of California at Berkeley. Eric Lander, key defense witness in *Castro* and the head of his own Human Genome Project laboratory was trained in mathematics, and very comfortable with statistics, but was not a statistician or a population geneticist. In the same fashion, Mary-Claire King was an excellent geneticist, but not a *population* geneticist. The relevance of population

genetics and statistics to the committee did not become apparent until near the end of the committee's tenure (Lempert, Personal interview, 1997; Reilly, Personal interview, 1997). While in retrospect it appears obvious that population geneticists and statisticians were the "correct" kinds of scientists to address issues surrounding the random match probability, this was not obvious at the outset. The division of scientific labor has become so specialized that

> [p]articular scientists, even quite eminent ones, find that their claims and judgments are taken seriously over a narrower and narrower range of topics. On any given topic those who count are all members of the relevant discipline, or specialty, or even some smaller *ad hoc* group recognized as possessing the relevant experience and expertise (Barnes 1985, 57-58).

The NRC committees were the kind of "ad hoc groups" which were recognized as possessing the appropriate expertise and experience to establish knowledge in this area. These committees were another part of the field on which the dynamics of agency and structure in the stabilization of DNA profiling were played out. The National Research Council is a social institution which pre-dates the DNA Wars, and one with an enormous amount of credibility and prestige. The NRC is the body to which the government looks for the "final word" on scientific issues. When asked by the National Academy of Science to investigate a particular issue, the NRC appoints a study director, and begins the process of selecting members to sit on the committee. These committees are quasi-independent, and committee members are blue-ribbon members of their individual scientific fields. In this way, the NRC uses its resources and credibility to confer agency on the individuals who form committees.

To be asked to serve on an NRC committee is time consuming, it is labor without monetary compensation, and it is also extremely prestigious and indicates that the member has both attained credibility, and will gain further prominence and

credibility by serving on the committee – *if* the committee is successful.

Each NRC committee has its own personality, resulting from the dynamic between the individual personalities on each committee. The first NRC committee had a weak chairperson, and two very strong, deeply opposed members with equally high academic prestige. This particular committee was characterized by a number of breaches of confidentiality and backstage manoeuverings. At one point the chapter on population genetics was leaked to the FBI. There was so much controversy over that chapter, that its original draft was subpoenaed in an Oregon court case, after the chapter had be re-written. The entire report was leaked to the press before it was printed, resulting in an overnight rush to print the document.

The first committee had an extremely broad mandate. It was to cover the technical considerations in DNA typing, the statistical interpretation of DNA profiles, standards, quality assurance and reliability, the possibilities inherent in DNA databanks and associated privacy issues, the use of DNA information in the legal system, and its broader ramifications for society. Although the report became best known for the much maligned "ceiling principle" for calculating random match probabilities, that portion of the report is only a tiny section. For this reason, many people continue to say the first report was an excellent report in many respects, particularly for the contributions it made in areas other than statistics and population genetics (Fischer 1997, Personal interview; Reilly 1997, Personal interview).

Victor McKusick, a geneticist at John's Hopkins Hospital in Baltimore, Maryland was appointed as Chairperson of the Committee. Also appointed were Paul B. Ferrara, Division of Forensic Science, Department of General Services, Richmond, Virginia, and Haig H. Kazazian, then of The Johns Hopkins Hospital (now in the Genetics Department at the University of Philadelphia). Dr. Henry C. Lee, a forensic

pathologist with the Connecticut State Police and highly esteemed prosecution witness, brought slides and other artifacts to help the committee learn what was involved with collecting evidence from a crime scene (Reilly 1997, Personal interview).[7] Expert on evidence Richard O. Lempert, of the University of Michigan Law School in Ann Arbor, Michigan sat on the committee, as did Ruth Macklin, of the Albert Einstein College of Medicine in the Bronx, New York; Thomas G. Marr, of Cold Spring Harbor Laboratory, New York and Philip R. Reilly, physician and lawyer from the Shriver Center for Mental Retardation, Waltham, Massachusetts. Being both a medical doctor and a member of the Masachussetts Bar Association, Reilly was able to bridge the gap between the worlds of genetics and the law, and translate information in both directions (Reilly 1997, Personal interview, Personal interview). Rounding out the committee were George F. Sensabaugh, Professor of Criminology and Forensic Science at UC Berkeley; and District Court Judge Jack B. Weinstein, a prominent expert on evidence, Judge Weinstein was very important in making the scientists on the committee understand what judges would need in the courtroom: clarity, simplicity and conservativeness (Reilly 1997, Personal interview). Dr. C. Thomas Caskey, of Baylor College of Medicine in Houston, Texas, sat on the committee until its last meeting, when he resigned due to a perceived conflict of interest (Anderson 1992). Another member who resigned early in the committee's tenure was Michael W. Hunkapiller, of Applied Biosystems Inc., of Foster City,

[7] Henry Lee simultaneously holds the titles of state police commissioner, chief state fire marshal, chief building inspector, director of the state forensic laboratory and university professor. Lee completed his Ph.D. in human genetics in 1965 at New York University Medical Center. As Director of Connecticut's forensic laboratory, Lee has "discovered a new method to extract DNA from evidentiary samples, a technique to enhance bloody fingerprints, images and procedures for estimating the volume of blood found at a crime scene, and a method to develop footprints. The latter method ... revealed a set of shoeprints at the Nicole Simpson homicide that were not made by the Bruno Magli shoes that the prosecution claimed O. J. Simpson was wearing at the time of his ex-wife's death" (Watanabe 2000, 2).

California. Oscar Zaborsky of the National Research Council acted as the Study Director for the project.[8]

After interviewing many members of the committee, it is apparent that this first NRC Committee on DNA Technology in Forensic Science was a highly politicized, highly polarized and deeply troubled committee. Committee member Dr. Richard Lempert said "It [NRC1] was the most politicized thing I've been involved in, because of this great concern that it might lead to criminals being freed, or on the other hand that it violates due process or some such" (Lempert 1997, Personal interview).

Most members had strong opinions about DNA technology before they joined the committee. Committee member Richard Lempert, for example, noted:

> [Y]ou can't say that people don't come there [to the committee] with their own commitments and their own values, particularly on a committee like this in which so many people were involved in one way or another. I mean, one of the interesting things about this committee is if you look at their background, is that you'll find that Eric Lander, for this, was known as someone really suspicious of DNA. You'll find on the other hand probably five or six committee members are people who have used it, who are promoting it, who were some way involved in it, and for a committee with that composition to come out with something that got so slammed by the same user community, I think it says something about the process of the committee (Lempert 1997, Personal interview).

Harvard population geneticist Richard Lewontin agreed about the inevitable politicization of the first and second NRC committees on DNA profiling. He said that the committee had to ask everyone who had been vocal in the disputes to either appear before them or provide a report, to give the

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[8] Mr. Zaborsky had left the National Research Council when I contacted them, and they did not know of his whereabouts. I was unable to set up an interview with him. Given that the first committee's tenure was characterized by strong personalities and strongly polarized positions, an interview with the study director would probably have been very informative, if he were willing to share information. I found the members of the first committee, for the most part, to be very unwilling to speak about the factions and fractures, the problems and disagreements. They said that in the end, they had "all signed the document", and that they therefore had to stand by it, even years after the fact.

appearance that the committee had polled the variation in the academic community. But when he spoke about the make up of the committee, it seems he felt that it was not possible to form an unbiased committee with the necessary expertise. Lewontin refused to appear before the committee because the committee gave Dr. Bruce Weir, who acted exclusively as a prosecution witness, several days of their attention (Lewontin 1997, Personal interview). He felt they could not put Bruce on the committee, because of his obvious partisan position, but they drew all the knowledge they could from him. Lewontin felt that any committee which fairly represented the variation of opinion in the population genetics committee "would never reach consensus."

> They couldn't put me on the committee because I wouldn't agree with the consensus. They couldn't put Bruce [Weir] on the committee because he's too strongly identified as a constant prosecution witness. That's what Bruce spends all his time doing. His presence on a committee would impeach the credibility of the committee. In that sense you can't make the committee out of people who make a living at it. But it's easy to find people who make a living out of it.

Lewontin says he wrote to the President of the National Academy of Science and said that he would write the entire report for them without a committee! He felt that it was not possible to get an unbiased committee:

> There's no such a thing as a person who is knowledgeable in the subject who doesn't have a pre-formed opinion. I've got one, Bruce has one, everyone's got one. Therefore, the final report will reflect whatever the membership of the committee is about.

When questioned, Dr. Lempert refused to talk about the schisms and factions and problems on the committee. He felt that the committee had done their best, with a very difficult task, a constantly changing scientific target, and people who had strongly partisan positions about DNA profiling when they came into the committee process. However, he stressed that the fact that in the end, each of the members had signed the report, and they were honor bound to stand behind it, despite the criticism

which it received. In his mind, the fact that they did eventually reach a consensus meant "that there is openness of people despite preconceptions" (Lempert 1997, Personal interview), and that the National Academy did a good job of vetting people for conflicts of interest (despite Dr. Thomas Caskey's last minute resignation). He pointed out that the committee was under tremendous pressure to reach consensus:

> Here we are, supposed to produce a report that will guide courts, well, if we come out seven to five, that's not going to guide anybody, and even though it's somewhat frustrating that all our work will have gone for naught, we don't want that to happen, so there's tremendous pressure in these settings to achieve consensus (Lempert 1997, Personal interview).

Lempert talked about the committee process being very much influenced by the interaction among people. Some people were very strong personalities (Eric Lander and Tom Caskey), and some people barely spoke at all (Mary-Claire King). Lempert felt that the people who emerged as leaders played a "disproportionately powerful role" in the conclusions of the committee:

> The values and other commitments of people who play a powerful role can affect what is supposed to be objective judgment of science. Usually the more powerful people are very good people, they have quite good reputations quite a bit beyond there, but nonetheless, science is a human process. ... If it had been up to me, personally, by the time the report was going to come out, I probably would have said 'Look, why don't we not issue the report and wait for a year, and see what, because things are changing so fast, and next year, let's meet again'. Well, the first thing is we had no money to meet again. The second answer would be our sponsors have put in half a million dollars and they expect a product. The Academy, the research process would not exist if it does not produce products (Lempert 1997, Personal interview).

It was difficult for the first committee to reach consensus for many reasons. The main "players" on the committee were Eric Lander, Thomas Caskey and Judge Weinstein. There were deep cleavages on the committee between Eric Lander and Thomas Caskey, who each advocated different methods for calculating the random match probability (Reilly 1997, Personal interview). Caskey was a strong supporter of

the FBI's method for calculating probabilities, and had adopted the FBI's protocol for making DNA profiles in his own laboratory. Lander was concerned, and became increasingly concerned over the committee's tenure, with the potential effects of population sub-structuring on random match probability calculations. He felt the committee had to come up with a method of calculating probabilities that either took the possibility of population substructure into account, or bypassed it altogether. Lander carried the concerns of Richard Lewontin to the committee, although Lewontin himself did submit a written presentation to the first Committee (Lewontin 1997, Personal interview).[9]

Philip Reilly agreed that the Committee was constrained in its ability to answer the questions before them. He said that if the report had been a grant application, they could have gone out and done the required population studies that would answer the questions about population substructure. However, the mandate of NRC committees is not to do original research, but to synthesize existing research and make policy recommendations.

One of the pressures on the committee was to stay on time, on budget, and reach consensus. The legal community especially exerted strong pressure on the committee to reach consensus, despite their differing points of view. Reaching consensus about the best science, the best way to calculate the random match

[9] Officially, the *Science* articles by Lewontin and Hartl and Chakraborty and Kidd came out too late (the day before the Committee's last meeting) to have any effect on the recommendations of the first NRC Committee. However, members of the first committee were "well aware" of Lewontin and Hartl's views (Kazazian 1997). In addition to Lewontin's written presentation to the Committee, many of them received pre-prints of the *Science* article. Committee member Haig Kazazian said "there was an effect of that paper I would say, in the sense that we were compromising between various positions" (1997). Eric Fischer, study director of the second NRC committee felt that Eric Lander would have been very aware of Lewontin's concerns. He said: "[y]ou know how science is, the relevant people were probably seeing pre-prints. Lander and Lewontin were both in Boston, and presumably they were talking to each other about this stuff" (Fischer 1997).

probabilities, and the best way for the forensic community to proceed to use the DNA technology was an irreducibly social process, involving extended negotiations. The committee's work was not a simple matter of sorting through known knowledge on DNA typing and summarizing the literature, because it had to come up with a solution that would be workable for the courts. The Committee had to walk the fine line between an empirical literature that was constantly shifting, and the pragmatic needs of the courts to have a "way to go on" to use the valuable new technology.

A big problem for the committee was that during its tenure (meeting approximately every three months between January 1990 and December 1991), the "science" was constantly changing, giving the committee what many members called a "moving target" (Lempert 1997, Personal interview). The committee wanted to utilize the "best" science in making their recommendations, but every time the committee met, something new would have been published, often contradicting evidence presented only three months before. Committee member Richard Lempert recalls:

> The other thing about this committee that made it unusual compared to other committees I've been on ... [was that] the science was changing so rapidly. ... It's very hard to keep up with the science, because you want the best science. The best science is often the latest science, but it was dangerous to rely on the latest science. If you look at the DNA area, particularly in the time the committee was operating ... you would have a blockbuster article about some point come out in *Nature* or the *American Journal of Human Genetics* or some other of that sort, and then, three months later, you would have another article disputing that, and so the question is "what was the good science?" It couldn't be the latest science, because the latest science would have been peer reviewed in the process of publication, but had not been reviewed by necessarily all the right people (Lempert 1997, Personal interview).

The "moving target" gradually took shape most importantly as issues surrounding population genetics. Specifically, how to determine the effect of sub-populations on the correct allele frequency distributions to be used to calculate

random match probabilities.

The needs of the courtroom were a particularly salient issue on the first NRC committee, whose members all said in retrospect that they were not searching for a "scientific" solution to the problem of how to calculate a random match probability, but were looking for a method that would work for the courts. Although NRC committees are quasi-independent scientific bodies, we can see here that the pre-existing structure of the judicial system exerted a not-so-subtle force on the members of the committee. The interests of the judicial system were carried to the committee partly through the voices of Judge Weinstein, Dr. Richard Lempert, and Dr. Philip Reilly, who were all members of the bar, and thus familiar with courtroom culture. They knew that judges and juries could be convinced to accept a less accurate number if it was conservative (biased in favor of the defendant).

Philip Reilly says that they spent their time talking practically about the use of scientific evidence in the courtroom, looking for something to satisfy the *Frye* standard of "general acceptance." The Committee was working to bring the worlds of law and science together. The task was to "abstract the scientific questions and present them in a way that was very simplified but fair." This was made more difficult because as Reilly put it, "When law and science come together, it's like trying to hire a simultaneous interpreter for the United Nations -- it's two different languages" (Reilly 1997, Personal interview). Judge Weinstein had made it clear that the justice system needed a method that was easy to understand, conservative (biased in favor of the defendant), and avoided the issue of racial and ethnic background altogether (Reilly 1997, Personal interview).

The committee chose a pragmatic short term solution to the problem of how to calculate a random match probability, and suggested empirical investigation as the

long term solution. In the short run, they felt the courts would be more than happy with their proposed ceiling principle and the interim ceiling principle. And they advocated that empirical studies of different populations be done after their report's publication, to provide the empirical basis for precise estimates of random match probabilities.

Haig Kazazian, member of both NRC committees, feels that the members of NRC1 knew they were not proposing a scientific solution. In his eyes, the ceiling principle was a reasonable compromise:

> We realized that some parts of it [the ceiling principle] did not have scientific validity but they were reasonable estimates on an upper boundary, and on as low as you should go in terms of the frequency. We all realized that pulling those fractions for an allele of 'don't go below 5% and don't go below 10%' in the modified ceiling principle was pulling the numbers right out of the air. We all realized that. ... Nobody said it was a specific scientific estimate, everybody on the first committee said it was a reasonable approach to the issue for the purpose of the courts. We were not trying to promote science with it (Kazazian 1997, Personal interview).

Kazazian went on to state that the really key thing about the ceiling principle for the first committee was that the numbers could be used no matter what ethnic group was being examined, thus rendering the issue of race irrelevant.

As it turned out, because many sharp defense lawyers argued that the NRC's proposed solution of the ceiling principle had failed to attain general acceptance within the scientific community, the pragmatic answer that was supposed to be "good enough" for the courts was *not* good enough for many scientists. In a backlash effect the ceiling principle then became unacceptable in some courts *because* it was unacceptable to many scientists. Some defense lawyers were able to show that general consensus on the issue did not exist in the scientific community. This left the problem of how to correctly calculate a random match probability up in the air again.

The NRC Committee was not the only body to struggle with the issue of the "best science." In a few cases, for those clients with the good fortune of having counsel educated in issues of DNA profiling, the lack of consensus in the scientific community had become an issue big enough for some judges to declare DNA evidence to be inadmissible. In January of 1991, in the case of *Commonwealth v. Cumin* the Supreme Judicial Court of Massachusetts rejected DNA evidence, pending a resolution of population genetic issues. Several weeks later, courts reached the same conclusion in *Arizona v. Despain* and *Illinois v. Fleming*" (Lander 1991, 820). On NRC1, Judge Weinstein, Dr. Lempert, and Dr. Philip Reilly knew that the courts needed a solution that was simple and conservative. By late 1991 it was becoming increasingly apparent that although it was not included in their committee mandate, and they lacked the specific expertise to address the question, the committee was going to have to address issues of population genetics. Dr. Mary-Claire King and Dr. Philip Reilly became very concerned during the last third of the committee's tenure at the lack of a "card-carrying" population geneticist on the committee (Reilly 1997, Personal interview). During the last six months, between June and December of 1991, as Eric Lander and Thomas Caskey engaged in increasingly heated debates about the product-rule, it became clear that no one on the committee had the needed expertise in both population genetics and statistics. Thomas Caskey was a firm believer in the existing product-rule method. Lewontin's argument, carried to the committee by Eric Lander, that there was not sufficient knowledge about population substructure to reach a conclusion about using the product rule, was a conclusion reached during the last few months of the committee's tenure. However, Philip Reilly commented that they still did not see the population genetics issues as "dramatic" (Reilly 1997, Personal interview). That issue became

much more dramatic *after* the report was issued.

At the last meeting, Tom Caskey resigned and the committee, forced by time and outside pressure into achieving consensus, reluctantly accepted Lander's ceiling principle as an interim solution to the problem of how to calculate random match probabilities while taking into account the possible effects of population substructure. At this point the report was late, and the committee decided that there was not sufficient knowledge about population substructure to reach a conclusion about using the product rule, although in a vast underestimation of the furor to come, they did not feel this was a large issue (Reilly 1997, Personal interview). They were under extreme pressure to produce a report, and population substructure and the product rule were felt to be only a tiny portion of what the report covered. The ceiling principle was viewed as a solution that "bent over backwards" in favor of the accused, and so it was anticipated that it would be appropriate and adequate for the needs of the criminal justice system. In retrospect Dr. Philip Reilly framed the decision this way:

> In the end we were willing to sacrifice absolute scientific rigor to adopt a very conservative stance that would allow DNA evidence to go forward in the courtroom in a way that would protect the interests of the defense bar. ... We weren't looking for scientific rigor, we were looking for something a judge could understand (Reilly 1997, Personal interview).

From an evidentiary point of view, it was felt that the existing product rule would not satisfy *Frye* hearings because it had not gained general acceptance in the scientific community. The Committee believed that the ceiling principle was more likely to be declared admissible in a *Frye* hearing because it was a simple "counting rule." For example:

> If you have an evidentiary sample that consists of a red bean, and a jar full of beans over there, you count how many red beans are in the jar, and if there is only one or no red beans in the jar, it tells you that red beans are probably in general pretty uncommon. That says something simple about a counting rule. It says to the judge that we really do not know how many red beans are in the population, but among these 300 beans, there was one or no red beans (Reilly 1997, Personal interview).

This "counting principle" was the grounding of the ceiling principle. The committee felt the ceiling principle would definitely meet the *Frye* standard because of its simplicity and conservativeness, and that there was nothing in it to arouse the ire of anyone, except someone who might think it was a little too conservative.

Since the ceiling principle was the subject of so much controversy, it is worth going into some detail as to how it works. In interviews, members repeatedly referred to it as a "reasonable solution." When they made their report, the committee did not know what the effects of population substructure were -- they said the data did not exist.[10] The committee's recommendation for settling the issue of how much population substructure existed was to take random samples of 100 people drawn from 15 to 20 homogeneous populations. What counts as a "homogeneous population" is left vaguely defined. In each of these populations, at each locus, the largest allele frequency, or 5%, whichever was larger, would be used in the multiplication rule (NRC 1992, 83). The following table is excerpted from the report to demonstrate how the ceiling principle would guide the calculation of the random

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[10] There is disagreement about whether the data needed to answer the questions the committee had about whether population substructure existed. When asked to comment on the lack of data on population substructure available to the first NRC committee, Bruce Budowle of the FBI said: "In 1992 when the National Academy of Science was coming out with their first report, and it came to our attention that they claimed that there was ... insufficient data to render an opinion or interpretation about the population genetics, that was just either being uninformed or lazy, and I think it was lazy. I said all you have to do is use an amazing device called the telephone and the fax machine and you can get these data, because if you attend the science meetings, you see these results, people present them. So, the first thing to demonstrate is that these data exist. The NRC group said there is insufficient data -- they didn't do the work."

match probability for a two locus DNA profile:[11]

Table 1
Example of Allele Frequency Distributions
at Two Loci in Three Populations

	Population 1	Population 2	Population 3
Locus 1			
Allele a	1%	5%	11%
Allele b	5%	8%	10%
Locus 2			
Allele c	3%	4%	4%
Allele d	2%	15%	7%

The example from the report is as follows:

> For the genotype consisting of a/b at locus 1 and c/d at locus 2, the ceiling principle would assign ceiling values of 11% for allele a, 10% for allele b, 5% for allele c, and 15% for allele d and would apply the multiplication rule to yield a genotype frequency of [2(0.11)(0.10)][2(0.05)(0.15)] = 0.00033 or about 1 in 3,000. Note that the frequency used for allele c is 5% rather than 4%, to reflect the recommended lower bound [or ceiling] of 5% on allele frequencies. Because the calculation uses an upper bound for each allele frequency, it is believed to be conservative given the available data, even if there are correlations among alleles because of population substructure and even for persons of mixed or unknown ancestry. This is more conservative, and preferable, to taking the highest frequency calculated for any of the three populations (NRC 1992, 83).

Although in retrospect committee members said they had not been seeking a scientific solution, the report says that the ceiling principle reflects "a number of important scientific and policy considerations" (NRC 1992, 83). The reason for sampling many different populations was to determine if some sub-populations had much higher allele frequencies at some loci than the general population (1992, 83).

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[11] In practice, the smallest number of loci used would be four, and probably the largest would be six. In the new DNA profiles made from Short Tandem Repeats, probabilities are calculated at thirteen loci. These thirteen loci are used by all crime laboratory practitioners in North America.

The sample size of 100 was determined to be reasonable since 100 people would provide a sample of 200 alleles, which was considered adequate for estimating allele frequencies. The committee recommended that "genetically homogeneous populations from various regions of the world" should be examined (1992, 85). If blood banks in various countries could not supply adequate samples, it was suggested that immigrants to America be sampled. The goal was not to sample every sub-population in the world, but to get enough of a range to determine what the probable range of allele frequencies "really" was.

The justification for the 5% floor placed on allele frequencies was that the problem when dealing with unknown populations is the occurrence of rare alleles -- alleles that are rare in the general population might have a higher frequency in a sub-population. The committee felt that allele frequencies of 1% were rare, and by placing a lower bound on the estimate of 5% they were in effect being extremely conservative. Under the ceiling principle, the lowest frequency obtainable at any allele was 1/400 -- (1/20 x 1/20 – or 5% x 5%) (1992, 85).

Because the ceiling principle rendered the same random match probability for any suspect, it was felt that it circumvented the problems of race and ethnicity. By using the highest frequency found in *any* racial or ethnic group (sub-population) for the calculation, and making the lowest possible frequency be 1/20 (5%), the reported frequency "represents a maximum for any possible ethnic heritage" (1992, 85). The Committee felt the calculation was fair to suspects, because the estimated probabilities were likely to be conservative in their incriminating power.

> Although the ceiling principle is a conservative approach, we feel that it is appropriate, because DNA typing is unique in that the forensic analyst has an essentially unlimited ability to adduce additional evidence. Whatever power is sacrificed by requiring conservative estimates can be regained by examining additional loci. (Although there could be cases in which the DNA sample is insufficient for typing

> additional loci with RFLPs, this limitation is likely to disappear with the eventual use of PCR.[12]) A conservative approach imposes no fundamental limitation on the power of the technique (NRC 1992, 85).

On an NRC committee, there is a certain amount of structure provided by the parent organization. NRC provides a study director, and staff to facilitate the work of the committee. The Board on Biology set the mandate for the Committee, but within these guidelines, the Committee had to determine how to work together to fulfill that mandate. Victor McKusick was a soft-spoken, gentle chairperson, and Eric Lander and Thomas Caskey were strong personalities on opposite sides of the fence. If Caskey did not directly represent the interests of the FBI, then he certainly was an advocate of their methodology, having adopted it in his own laboratory. On the other hand, Lander was strongly supported Lewontin.

From what I have gleaned, committee meetings were almost a state of "war of all against all." They were not able to establish order amongst themselves. Stephen Hilgartner argues that successful NRC committees establish and maintain their credibility partly by keeping their in-fighting and "dirty laundry" behind the scenes, and out of the view of the public (2000). The NRC contributes to this by keeping all documents and meeting minutes strictly confidential. However, this first committee failed to keep its backstage squabbles behind the scenes. Their failure to get along contributed to their failure to produce what was seen as valid and reliable knowledge. The Committee achieved a forced consensus on the ceiling principle, and in the end, they all signed their agreement to it. However, by the time the report was produced, the world had already seen two years of in-fighting and squabbling. The outside world

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[12] PCR stands for polymerase chain reaction, which is a technique by which very small amounts of DNA are copied and amplified millions of times, making it possible to create a DNA profile from a sample as small as the follicle from a single hair. For information on the history of PCR, see Rabinow 1996.

was primed to jump on the Committee's findings before they were even published.

8) Reaction to the Report on DNA Technology in Forensic Science

Of all the reactions to the NRC's long awaited report *DNA Technology in Forensic Science* (NRC 1992), none was so dramatic, or so misguided, as the front page article in the *New York Times* by Gina Kolata (1992a). When Kolata published her article the NRC report had not even gone to press. She was working from a pre-press manuscript. As a result of Kolata's article, McKusick rushed the report to press on Wednesday, April 15, for release on Thursday. The report includes a prominent refutation of the first *New York Times* article. To its credit, *The New York Times* printed its first-ever front page retraction the next day (Kolata 1992b). The corrected article said that while the NRC committee called for higher standards in laboratories conducting DNA tests, that it did *not* recommend a moratorium on the use of the technology in the courts. The article reports that in the press conference, Victor McKusick said that "The present methods are good, but this doesn't mean they can't be better." Dr. Haig Kazazian, a member of NRC1, said that "Right now, DNA typing is being done by a relatively small number of laboratories, and it's being done quite well in those laboratories. but in the future, the use of DNA testing might be carried out in a very large number of laboratories, and we thought it very important that a national quality assurance program be set up" (Kolata 1992b).

The press conference and the paragraph in the front of the report disclaiming the *Times'* first article worked -- for some groups. The popular press reported that the new NRC report on DNA testing said that the new technology had now been approved for use in the courts. *The Washington Post* reported that "a panel of experts assembled by the National Research Council has concluded that courts should accept the reliability of the technology and the fundamental soundness of

current laboratory techniques" (Washington Post 1992). The *Chicago Tribune* said that the report recommended national testing standards, federal supervision of laboratories conducting the tests and a different method of analysis than the one currently used by the FBI.

However, the ceiling principle was soon to become the center of another heated controversy. It would be called "unscientific," "ad hoc," and "lacking in scientific grounding." Ultimately, the controversy over the ceiling principle led the National Academy of Science to convene a second committee on DNA technology, this one focused almost completely on statistical and population genetic issues. The report from this second NRC committee said in print several different ways that it could find no scientific justification whatsoever for the ceiling principle (NRC 1996, 35). However, the controversy about the ceiling principle and whether or not it was scientifically grounded stayed within the scientific and forensic community, and were not reported on in the popular press.

a) Reaction in the Scientific Press

The scientific press reacted more cautiously to the release of the first report than the lay press. They were cautious, in that the writers seemed to sense that although the report endorsed DNA typing, they knew that there had already been controversy over the third chapter on population genetics, and that the report was unlikely to solve all the issues. But the scientific press was not cautious, in that even the prestigious journal *Nature* published an article one day before the report was actually released. The headline reads "Academy approves, critics still cry foul" (Anderson 1992). The *Nature* article accurately reported that the NRC committee found the technique of DNA typing reliable for use in the courtroom, but correctly predicted that this report would not end debate on the issue. It predicted the

population genetics would be the site for controversy, based partly on reports that the FBI and the US Department of Justice had been leaked a copy of the population genetics chapter in late 1991, and had protested so vociferously that the chapter was re-written to be more favorable to courtroom use of DNA typing. The FBI's John Hicks submitted a long letter to the committee critiquing the chapter, but Chairman Victor McKusick insists that the changes came about as a result of the peer review process, not FBI input. However, the California Court of Appeals, the Massachusetts Supreme Judicial Court and the US District Court of Guam all ruled that DNA evidence was inadmissible, citing the NRC report and the potential controversy over population substructure. These courts felt the unknown effects of population substructure to be controversial enough that DNA evidence could not pass a *Frye* hearing, which would indicate general acceptance in the scientific community.

The first mention that the ceiling principle was "unscientific" came in a *Science* article of February 5, 1993, where statistician Neil Risch of Stanford University is quoted as saying "If I were asked if there is any scientific justification to the ceiling principles I'd have to say no" (Aldhous 1998). This kind of critique of an NRC report is unusual, given the high status of the National Academy in scientific circles and the credibility given to NRC reports on the whole.

b) The FBI's reaction:

In their in-house journal *The Crime Laboratory Digest* (FBI 1992), the FBI responded point by point to the recommendations of the NRC1 committee. In regard to the recommendations about the technical issues related to DNA typing, the FBI's published a list of their own published articles which they felt addressed the technical concerns raised in the report (FBI 1992, 49-56). On the issue of the ceiling principle, the FBI said that it would continue to use the product rule in its court cases, but would

calculate probabilities according to the more conservative ceiling principle if requested to do so by a court (FBI 1992, 58).[13]

Complying with a judge's request was not the problem for the FBI. There was fear that calculating random match probabilities using the ceiling principle would result in probabilities too low to be convincing evidence, thus "tying the hands" of the FBI in court (Budowle 1997, Personal interview). Regarding the recommendation of sampling 15-20 homogeneous populations to determine the extent of population substructuring, the FBI reports that it had already begun gathering data from forensic laboratories in other countries, with the intent of "determining the range of variability for respective indigenous populations -- the central issue underlying the Committee's recommendation" (FBI 1992, 57). At this time (July of 1992), the FBI said it would take no position on the advisability of conducting the recommended population studies until it had its own data in hand to analyze (FBI 1992, 57). On the issue of ensuring high standards for DNA typing, the FBI referred the Committee to the work of the Technical Working Group on DNA Methods (TWGDAM), which had already published the requested formal and detailed quality assurance procedures (TWGDAM 1989; TWGDAM 1990; TWGDAM 1991). The response is measured and detailed, showing in the case of each recommendation how the FBI had already met the recommendation, was taking steps to meet it, or had no comment.

However cool the print response to the report was, the response of people in

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[13] Much later, in 1994, the Technical Working Group on DNA Methods (TWGDAM) published its own response to the ceiling principle, stating that "The Technical Working Group on DNA Methods cannot recommend the application of the ceiling principle. The basis for the need for a ceiling principle approach is flawed. ... The need for the ceiling principle is based upon the faulty premise that there is more genetic variation among subgroups within a major population group than between major population groups[;] the extant data demonstrate the opposite and that the application of the ceiling principle is unnecessary. The current methods employed by forensic scientists have been demonstrated to be robust scientifically"(Technical Working Group on DNA Methods, 1994a, 21).

the institution was much more heated. Bruce Budowle, head of the FBI's Forensic Science Research and Training Center at Quantico, Virginia, said that the ceiling principle "was an absurd ad hoc procedure that had no scientific basis" (Budowle 1997, Personal interview). In his view, it was ultra-conservative, like answering the question "how tall is the tallest person in the world?" with the answer "under a mile tall." The FBI was concerned that courts were going to start throwing out DNA evidence because the professional community was saying that the ceiling principle was not scientifically valid, in other words, DNA evidence would not pass *Frye* hearings as the NRC committee had expected. The FBI's two main concerns were that the power of the technique was being artificially decreased because of the conservativeness of the ceiling approach, and that the NRC report had caused confusion in the courts (Fischer 1997, Personal interview).

Bruce Budowle said that the first report was a very poorly done job in that there were a lot of flaws in it scientifically and statistically. He felt the report missed the mark in the arena of population genetics, as he felt there were a lot of population genetics data that were available and the committee either was unaware of it or chose not to consider it. He felt it was evident that the report was poorly done or the NRC would never have done a second report, and that the reaction of the FBI as a whole to the report was that "overwhelmingly there was criticism of the report as not being very good" (Budowle 1997, Personal interview). In fact, the FBI felt so strongly that the report, and specifically the recommendations on the ceiling principle would limit the effectiveness of DNA profiling in court, that in April of 1993 Judge William Sessions, the Director of the FBI, asked the NRC to do another study to resolve the controversy (NRC 1996, vi).

The Technical Working Group on DNA Methods felt that the whole

consideration of population substructure was based on a hypothetical construction that it might exist. They took the position that TWGDAM could not "recommend the application of the ceiling principle" because the basic premise that population substructure exists and affects probabilities estimates is "flawed" (Technical Working Group on DNA Methods 1994b).

c) Reaction of the Scientific Community:

The professional community of scientists, especially population geneticists and statisticians, and surprisingly, even Richard Lewontin, were, almost to a person, dissatisfied -- and in some cases incensed -- with the NRC1 report. Eric Fischer, study director of NRC2 said that there were statisticians, population geneticists, and forensic scientists who were saying 'this doesn't make any sense' (Fischer 1997, Personal interview). Almost all the attention was focused on the third chapter of the report which dealt with population genetics and the proposed, newly controversial ceiling principle.

Lewontin's criticism was that the NRC report did not provide enough guidance for judges in how to use the ceiling principle and VNTR databases. He wrote to *Science* claiming that this lack of guidance had led one judge, District Court Judge Edward Lynch in Minnesota to refuse to allow numbers to be presented in court. He says the judge was told that one locus in each of two databases held by the Minnesota Bureau of Criminal Apprehension showed some departures from Hardy-Weinberg equilibrium. The judge then ruled, in what he thought was accordance with the NRC committee's recommendations, that the entire database should not be used. Lewontin points out that it is always possible to find at least one human locus that is not in equilibrium, but that this does not mean that loci that are in equilibrium cannot be used in probability calculations. Lewontin faulted the report for "a lack of guidance

... as to the appropriate course of action when some disequilibrium is found, and this finding will be very common when as many as 20 databases are tested for equilibrium at each locus, as required by the report[.]" (Lewontin 1993).

Leading the bandwagon of protests in print were statisticians Bernie Devlin, Neil Risch and Kathryn Roeder, who wrote a critique of the NRC's first report which criticized many of the population genetics and statistical issues. They argued that "there is little scientific basis for their [NRC1] method of forensic inference -- the ceiling principle," because the panel did not investigate what was currently known about the population genetics of the U.S. population (Devlin, Risch and Roeder 1993, 837).[14] Engaging in boundary work by defining who constitutes a minority and who a majority, and what is scientific versus unscientific, Devlin, Risch and Roeder argued that they could understand why courts would take the findings of the NRC panel and the article by Lewontin and Hartl as evidence of a lack of consensus in the scientific community on population genetic issues. However, they contended that the NRC panel and Lewontin and Hartl were a small minority among what was "indeed a consensus supporting the reliability of estimates of genotype probability" (Devlin *et al*, 1993, 748). On the issue of the effects of subpopulations on probability estimates, they said it was small, and that the research programme proposed by both Lewontin and Hartl and the NRC panel would not "resolve the population genetics debate." They felt that with the small sample size of 100 people endorsed by Lewontin and Hartl, and NRC1, most of the variance between groups would be due to sampling error, which would artificially exaggerate differences between subpopulations (Devlin, Risch and Roeder 1993, 748). On the issue of the ceiling principle, they argued that

[14] For other articles critical of the report, see Cohen 1992; Weir 1992; Weir 1993; Devlin, Risch and Roeder 1994.

the upper and lower bounds on allele frequencies of 10% and 5% were completely arbitrary, chosen from the air, as it were. In the end, their argument boiled down to saying that the subpopulation issue was a non-issue:

> [C]ontrary to the NRC panel's assertion, U.S. populations fit the forensic paradigm of reference populations rather well: (i) most genetic diversity is among individuals; (ii) as the number of markers that comprise the multi-locus genotype increases, the probability of randomly choosing two individuals with identical genotypes becomes remote; and (iii) genetic diversity among subpopulations of an ethnic group is less extreme than the differences among ethnic groups. The VNTR markers are not exceptions to these observations" (Devlin, Risch and Roeder 1993, 749).

Predictably, Lewontin and Hartl disagreed. In a letter to *Science* in response to Devlin, Risch and Roeder, they examined a number of databases using the FBI's product rule, and showed evidence that for some databases that show no statistical evidence of being out of Hardy-Weinberg equilibrium, the estimates obtained by the multiplication rule were "artificially small." Devlin, Risch and Roeder had argued that even with sub-structuring, the multiplication rule yielded adequate results that were conservative. Lewontin and Hartl disagreed, arguing that "the new data demonstrate that the methods currently used in court are not conservative -- they are systematically prejudiced against the defendant -- and no amount of argument will make them conservative" (Lewontin and Hartl 1993).

Lewontin and Hartl agreed that the 10% upper bound for the ceiling principle was arbitrary and conservative, but stuck to their guns in claiming that the only way to settle the argument was to go out and conduct population studies of allele frequencies, of the very kind that Devlin, Risch and Roeder claimed would be ineffective, and the FBI said were unnecessary. Devlin, Risch and Roeder got out their calculators and argued that Hartl and Lewontin's data in their April 23, 1993 letter to *Science* did *not* show that probability estimates were biased against the

defendant, and instead claimed that "the new data demonstrate nothing of the kind. In fact, they could be used to argue for the opposite conclusion" (Devlin, Risch and Roeder 1993b). In this letter, Devlin, Risch and Roeder began a subtle campaign to circumvent the issue of race by arguing that in the ethnically mixed U.S. population, under a presumption of innocence, there was no reason to believe that the suspect and the actual perpetrator came from the same ethnic or racial background (1993b, 1058). They argued that the database used for random match probability calculations should be an ethnically and racially mixed database reflective of the makeup of the U.S. population. This claim returned the debate back to square one, where Lewontin and Hartl had argued that the U.S. population was *not* homogeneous, that subgroups do not undergo random mating, and that alleles in some subpopulations might not be independent, thus compromising probability calculations. Devlin, Risch and Roeder argued that

> population substructure would have to be unrealistically large (contrary to evidence) for standard forensic calculations to be seriously in error. Thus, we see no scientific justification for adopting even more conservative methods, as Hartl and Lewontin and the NRC report advocate (Devlin, Risch and Roeder 1993b, 1058).

Ranajit Chakraborty worked closely with the FBI on issues of DNA profiling, and also held large National Institutes of Justice grants to study the distribution of VNTRs. Chakraborty believed that Lewontin and Hartl had reversed their opinion that "there is approximately as much variation among ethnic groups within major races as there is among the races" (Lewontin and Hartl, quoted in Chakraborty 1993, 1059). He claimed that Lewontin and Hartl now felt there was about one-third more variation among races than among ethnic groups within major races. He stated that "this

reversal of opinion ... should be sufficient to illustrate that the effect of population substructuring has little impact on the significance attached to DNA profile match found in forensic case analyses" (Chakraborty 1993, 1059). Revisiting the issue of "how big is big?," which had pre-dated the DNA Wars, Chakraborty went on to argue that

> the hyper-variability of inter-individual DNA profiles is so great that it dwarfs any inter-populational difference, no matter how crudely or finely populations are defined. As a result, each multilocus DNA genotype is so rare that its forensic significance virtually eliminates the possibility of miscarriage of justice when a match is observed over three or more loci (Chakraborty 1993, 1059).

Eric Lander attempted to bring some clarity to the squabble by reminding the warring factions that the NRC committee had had a practical goal of creating a method of calculating random match probabilities that would be admissible in court. Lander said that the committee became aware that a controversy was indeed brewing over how to calculate random match probabilities, and so it sought to provide a method that was very conservative, and one that eliminated all considerations of race and ethnicity, so that there could be "no serious scientific argument that the evidence could be said to *overstate* the case against a defendant" (Lander 1993, 1221). On the issue of the perceived arbitrariness of the 10% and 5% levels, he argued that

> all margins of safety involve some element of judgment, but this does not render them 'illogical' or 'arbitrary'. In this case, the NRC committee simply concluded that the chosen upper bound sufficed to eliminate serious scientific objections to the population genetic statistics while still allowing odds of up to 6,250,000:1 for a match at four genetic loci (Lander 1993, 1221).

Lander believed that probability calculations done according to the ceiling principle would meet the admissibility criteria of "general acceptance" because no one would argue that they were biased against the defendant, even though some might argue that they were too conservative. However, in his letter of April 23, 1993, this is exactly

what Richard Lewontin argued does indeed happen with some databases.

d) Reaction in the Courts

Despite the war that was raging in the scientific community, over the next few years most judges continued to find DNA evidence admissible, and accepted probability calculations conducted according to the ceiling principle. However, by 1994, the controversy surrounding the first NRC report was beginning to diffuse into case law, and more and more courts began to use it as evidence that there was no "general acceptance" of *any* method of calculating random match probabilities (Kaye 1994). David Kaye notes that in the courts, the NRC report cemented the belief that a controversy existed. This idea was already promulgated by eager defense lawyers:

> If there was any doubt left in early 1992 that prominent scientists were divided over the adequacy of the forensic computations, the NRC committee put it to rest. Starting with *People v. Barney*, court after court has noted the committee's report of 'considerable dispute' and a 'substantial controversy'" (Kaye 1994, 373).

David Kaye describes the period after the report came out as the "third wave" in DNA typing in the courts where DNA evidence met with "mixed outcomes and opinions expressing grave concern over certain aspects of DNA evidence" (Kaye 1994, 370). Almost always arguments centered around the proper way to calculate a random match probability. However, courts went both ways with the NRC report -- it was not always found to indicate evidence of a controversy. Many courts accepted the ceiling principle as valid (Kaye 1994, 376).

In November of 1994, Washington, D.C. Superior Court Judge Henry H. Kennedy Jr., who had in 1990 rejected the use of DNA evidence, now declared that the evidence was admissible as long as the prosecution used the methods outlined in

the NRC report (Torry 1994). For the prosecution, this meant lower odds of a random match. For example, in the case above tried by Judge Kennedy, using the FBI's multiplication rule, the odds of a random match were calculated to be 1 in 15.6 billion. Calculated using the ceiling principle, those odds came down to 1 in 58,000. It is a huge understatement when the reporter says that these differences in calculation methods "are important to scientists" (Torry 1994). Paradoxically, such seemingly huge differences in random match probabilities would eventually be erased as the second NRC committee decided that differences in probabilities calculated from different databases were too small to take notice of -- in other words, they were to decide that "small is small."

The interplay between the controversy surrounding an NRC report produced by blue-ribbon members of the academic and legal communities, the various courtrooms in which random match probabilities were presented as evidence, the lawyers who continued to band together to defeat the technology, the scientists who spoke for and against DNA profiling in general, and the ceiling principle in particular, show keenly in how many different domains knowledge about DNA profiling was utilized and stabilized. Although we tend to give special credence to the scientific community in solving problems of knowledge, the fact that in this third wave period, some courts went one way, and other went another, indicates a relative freedom from the domain of science in solving problems of knowledge *in the courtroom*. This period shows keenly the tension between individual agency and the genesis of social structure. There was a looseness to structure, and knowledge, yet people went on in the courtroom, despite the scientific wars raging. Scientists went on in their publications, and they continued to fight with one another in court. In the act of going on, many, many individual acts and court cases began to form more and more stable

structures. The DNA Task Force solidified, and a stable community of scientists who would testify against DNA testing gained cohesiveness. Conversely, prosecutors had the FBI at their side, who were continually working with the community of crime laboratory practitioners in TWGDAM to refine DNA testing techniques, and to develop standards for quality assurance and laboratory proficiency. The prosecutors also amassed a wide range of equally prestigious scientists to testify *for* the stability of DNA profiling. In these intertwined yet separate social worlds, the actions of individual lawyers, scientists, judges, FBI agents, crime laboratory directors and law enforcement personnel were forming structures that were beginning to take shape. TWGDAM had pretty much settled on standards for quality assurance and laboratory proficiency (TWGDAM 1989, 1991). By 1994, which David Kaye marks as the third wave period in the history of DNA profiling, the second NRC committee was beginning to meet. The point is that there were a lot of individual people, going about their business, pursuing their interests, acting on behalf of their clients, making decisions that seemed to best fit the evidence. This flurry of activity across many social worlds is the mixture of agency that results in new social structures, some of while will be outlined in Chapter Six.

Law Professor Margaret Berger, member of the second NRC committee on DNA typing, felt that the fact that DNA evidence was first admitted and continued to be admitted was because it was never really challenged in any meaningful way, because the defense bar is generally under-funded and often "incompetent about all kinds of issues" (Berger 1999). She felt the first NRC report was useful because it established an informed vocabulary for the courts to use, and established links between people in the different communities. In her opinion, the report led to more uniformity and more understanding between social worlds. A major, ongoing problem

in her view is that scientists and others do not understand how difficult it is to convey scientific information to a judge or jury. She believes that people who go to law school self-select themselves *out* of the world of science and mathematics -- that they "go to law school to avoid the sciences," which exacerbates the differences between the law and science, and results in the two worlds having totally different ways of thinking.[15] She believes that it is a very rare scientist that is able to communicate scientific information in a way that makes sense to the non-scientist, and that problems for the legal community are often not issues for the scientific community.

For example, Professor Berger mentioned that in court, it is a real and valid question whether DNA evidence that has been buried in dirt for years can yield useful identity information, whereas for the pure scientist, she says this "struck them as a totally non-scientific question... it's just not a question that would come up in the laboratory if you were doing theoretical DNA work." For a lawyer, (and for the forensic scientist) this was a very significant question. Berger says there were many questions like that, that the scientific community really had no interest in.[16] Berger commented that on the second NRC committee the people who were best at straddling the worlds of science and the law were the statisticians. She said that they knew that the ceiling principle was a statistical convention and that there are lots of other statistical conventions that have nothing to do with "the real truth" (Berger 1999). In her

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[15] In an interview on April 21, 2001, Peter Neufeld, one of the defense attorneys in *Castro*, founder of the DNA Task Force and the Innocence Project, said "I have no background in science. My partner, Barry Scheck, has no background in science. In fact, like a lot of other lawyers, it was the difficulty in comprehending chemistry that moved us to law school in the first place. The last thing we ever wanted to see, as lawyers, was any kind of physics or chemical equation" (Kreisler, 2001).

[16] Many of these issues which were not of interest to pure or academic scientists were addressed in deep detail by the FBI in their validation protocol studies, which are discussed in Chapter Four.

experience, the statisticians, at least the ones on the second NRC committee, were more comfortable living with pragmatism over empiricism.

When asked about the different standards for what counts as evidence in the courtroom versus in science, Berger said that was a difficult question to answer. It is difficult in the sense that the two fields are engaged in a totally different search for the truth.

> The court has to deal with a question that has to be answered almost immediately -- there really are no mechanisms in the law for saying "we do not have enough information, come back in 20 years." So for better or for worse, the court has to decide on what is available to it at the moment, and it's not that it's saying that that's the truth, but that the party with the burden of proof has or has not met its burden of proof, whereas in science really if you get an inconclusive result but its sort of an interesting field, you get more funding and you do more research. The two fields have very different timelines (Berger 1999).

These different timelines mean different interpretations of "the truth." Berger felt that on the first committee, as they neared their last meeting with great pressure to publish the report, that it "was impossible to arrive at a definitive scientific answer because things kept changing" (Berger 1999). She felt that the more pragmatic people on the committee, including the legal people, felt that some sort of practical solution was required. The ceiling principle eliminated a stalemate and seemed to be fair to the defense bar. It was "fair" in the context of the legal system, where the ceiling principle was being advanced to assist the courts. "[T]his was not a group of scientists meeting to present a paper at a scientific convention on what the latest word was on probabilities" (Berger 1999).

The evidence points to the finding that the legal members of both NRC committees were not seeking a "scientific" answer, because the justice system did not require one. However, the National Academy of Science and the National Research Commission are supposed to be independent arbiters of truth for the government.

Publishing a report that raised such an uproar in the scientific community, particularly the claims that the findings of an NRC committee lacked a sound scientific foundation, put the National Academy in an untenable position (Fischer 1997, Personal interview).

9) Summary: Science and the Law

On April 16, 1992, the first NRC committee on DNA Technology in Forensic Science released its report recommending the ceiling principle as the most pragmatic means to arrive at a fair, conservative of a random match probability. It also had the added advantage of bypassing the issue of race -- the race of the perpetrator and the race of the reference group were irrelevant when using the ceiling principle. Reaction to the first NRC report took three forms. The FBI cried out loudly and strongly that the ceiling principle was too conservative and would incapacitate them in the courtroom. They feared that probabilities would be too low to attain convictions. The second, and stronger reaction came from the scientific community. They contended that the assumption made by NRC1 that population substructure was a problem was based on faulty assumptions. Their second, stronger concern was that there was absolutely no scientific grounding for the ceiling principle. Given that many courts were still following the *Frye* rule which requires general acceptance of a scientific technique before it can be declared as admissible evidence, the claim by some scientists that recommendations of the first NRC committee lacked scientific validity created doubts in the minds of some judges, and led them to declare DNA evidence as inadmissible. This and the FBI's strong concerns led to the formation of a second NRC committee in 1993.

Sociologist of science Brian Wynne observes that "[s]cience, like life in general, involves creating adequate conclusions from inadequate premises" (Wynne

1989). If this is true, then the only "unscientific" thing about the ceiling principle was that many people felt it was not an adequate conclusion, given the premises. All the members of the first NRC committee, when interviewed *after* the fact, said words to the effect of: "we were not trying to be scientific, we were trying to produce a number for the courts to use." However, I believe that during the tenure of the committee, the members *did* feel that they were coming up with the best scientific conclusions possible, given the data available to them. It is unlikely that the National Academy of Science would commission a report from a blue ribbon committee under the rubric of the National Research Council to reach a *non*-scientific conclusion. Defining the ceiling principle as unscientific was the result of post-hoc boundary work by population geneticists and statisticians who had not been represented on the first committee. The ceiling principle had been created without input from "card-carrying" population geneticists and statisticians, and therefore, in the eyes of those communities, it lacked the certification necessary for it to be "scientific."

Sociologist Stephen Hilgartner (2000) has examined NRC committees. His research shows that one thing that successful NRC committees do is to keep *everything* except the final report behind closed doors -- or to use Goffman's terms, in the backstage, out of the eyes of the prying public. The first NRC committee failed to keep their deliberations within the confines of the committee. The reader will recall that third chapter of the report was leaked to the FBI, and its the conclusions on population genetics were so controversial that the original, unedited version of the chapter was subpoenaed in an Oregon court case. The report itself was also leaked to the lay press, leading to Gina Kolata's ill-fated report on the front page of the *New York Times.*

For an NRC report to be viewed as having produced credible knowledge,

dissent and confrontation must be kept to the backstage. The public must see only unity and consensus. Although members of the first NRC committee were reluctant to divulge specifics, all of those that I interviewed admitted that the committee was fraught with discontent, in-fighting, and tugs-of-war between powerful personalities. When the NRC committee process is successful, the panel of experts and the NRC are perceived as competent, credible, knowledgeable and trustworthy. When this presentation of the corporate "self" is successful, the contents of the report are viewed as credible scientific knowledge. The first NRC committee failed miserably at keeping their corporate self hidden from view, at keeping their strong personalities under control and within the confines of the group, and at representing themselves and their deliberations as credible. This is one of the reasons that their solution to the problem of the random match probability problem, the ceiling principle, was seen as being non-scientific. They failed utterly at maintaining the credibility which they carried to the process as individuals, and the credibility given to the simply by the fact that they were speaking for the National Research Council.

One of the main issues to emerge from this analysis of the DNA Wars is that "truth" carries different meanings in the social worlds of science and the law, and it is also attained by somewhat different means in the two spheres. In the world of science the time frame to determine "what is the case" or "what is the truth" is usually long, or at least until the next grant is due. Even then, the need for solving the problem can become the justification for future funding. Scientists are comfortable with saying "we don't know" and allowing more time for research. They believe that eventually they will crack the code and find an answer. Within their own communities, they are also quite comfortable with informal and fairly imprecise ways of going on. However, informality and lack of precision are not tolerated in the courtroom.

Recall that during the DNA Wars there was a battle between academic disciplines for epistemic jurisdiction over knowledge relating to DNA profiling. The battle involved two main points of contention. The first was the opposing positions taken by Lewontin and Hartl, and Chakraborty and Kidd in their December 1991 publications in *Science*. Chakraborty and Kidd took a pragmatic position, and suggested that a solution be found which fit the needs of the criminal justice system, not the needs of science. They interpreted these needs as being fulfilled by a scientifically sanctioned "adequate estimate" of random match probabilities. On the other hand, Lewontin and Hartl advocated empirical research to establish the ontological distribution of VNTR allele frequencies in the American population. This information would provide an empirical foundation for the calculation of random match probabilities.

The second point of contention pre-dated the DNA Wars. The pre-existing debates were about coalescence and fragment size. The debate about coalescence was about how to interpret autorads that had only one band showing for a given allele. The two possible interpretations were that a person was a homozygote (both bands were the same length and thus showed up as one band), or that one band was so short it had run off the bottom of the gel. The scientists became involved in debates about how big or small deviations had to be from Hardy-Weinberg equilibrium to count as evidence *for* population substructure and *against* the truth value of estimates attained from samples of convenience. These "how big is big?" debates were curious in that most parties involved agreed that statistical methods for analyzing population structure had very little power to determine if substructure existed, and yet they continued to resort to these methods to attempt to settle the dispute.

The questions about how big deviations had to be to count *as* deviations can be interpreted as a form of boundary work. The scientists engaged in turf wars, variously claiming the territory for the field of population genetics or statistics. In the next chapter we will see that the whole issue of "how big is big" faded from the academic literature when the second NRC committee announced that "small is small." The second NRC committee calculated random match probabilities with many different databases, and decided that it did not matter if the correct database was used or not – the random match probability attained was always a small one. They used this as justification for not conducting empirical studies to resolve the population substructure issue.

Even after the publication of the *second* NRC report, Lewontin and Hartl continued to argue that empirical research was required to resolve the issues to find the "real" or "absolute" truth about population substructure and allele frequency distribution among the population (Lewontin 1997, Personal interview). However, after two NRC committees and countless academic articles, no one was listening to them anymore. Small was small, the multiplication rule was reinstated, and the ceiling principle became a matter for the history books.

The sub-theme of race did not receive as much attention in the scientific literature. Chakraborty and Kidd (1991) had argued that racial issues were irrelevant, because there was no reason to assume that a perpetrator was of the same race as the suspect. Some argument took place as to the correct reference population to use in calculating the random match probabilities. Chakraborty and Kidd (1991, 1735) argued that there was no legal principle that gave rise to a precise genetic definition of population, inferring from this that the reference population should match the makeup of the general American population. However, they did admit that in some

cases it might make sense to ask if there could be other individuals in a particular subpopulation with the same DNA profile. Statisticians also sidestepped the issue of race, saying that it did not matter if the frequency distribution used to calculate the probability matched that of the defendant's genetic makeup (Devlin, Risch and Roeder, 1993b). Eric Lander spoke for the entire first NRC committee when he said that the whole point of developing the ceiling principle was to a) be conservative and b) rule race and ethnicity out as an issue to be considered when making random match calculations (1993, 1221).

10) Summary

This chapter has highlighted the scientific debates called the DNA Wars, and has touched upon the differences between the attainment of truth in science versus a courtroom. It has opened a window into the processes of negotiation and social dynamics by which procedures for calculating random match probabilities were established. These processes of negotiation necessarily involved the exercise of agency on the part of scientists and other players, within pre-existing institutional structures. Bourdieu would say that during this period scientists began to mobilize from their positions in their fields – those with high credibility and prestige had immense amounts of power, or agency, to change the status quo.

The agency/structure dynamic is evident in the many spaces in which the DNA Wars played themselves out. On an institutional, or structural level, there was interaction between the social worlds of the courtroom and science – but this always took place through individual action. The DNA Wars were started by lawyers who enrolled a scientist, and presented evidence in a courtroom. One scientists' convinced the rest of the expert witnesses that the evidence lacked a scientific grounding, and the credibility of DNA typing disintegrated after the 1989 *Castro* case.

Scientists began to work on the problems, which turned out to be knottier than expected. Indeed, if the criminal justice system had not needed to place a number, or probability on a DNA match, the issue of how to calculate random match probabilities would never have arisen for scientists (Hartl 1997, Personal interview). This controversy highlights how changes were brought about in two macro level institutions, "science" and "the law," through the actions of individuals.

This chapter has also shown how the establishing new procedures and norms of behavior is the outcome of a process. Partly because of the rapidity with which inventions in molecular biology appear and are disseminated, the controversy over DNA typing occurred in a liminal space. A liminal space is an in-between state in which the values and norms of one stage have been left behind and the values and norms of the later stage have not yet been established.[17] After the *Castro* case it was clear that private DNA typing companies had not upheld the values and norms of the scientific community. It is also clear by the reaction on all fronts to the first NRC committee's report that the NRC's attempt to establish new rules, norms, and values had failed. This failure in turn meant that a second NRC committee was required to bring epistemic closure to issues pertaining to the random match probability, and to provide the criminal justice community with the correct "way of going on." The second committee's goal was to establish that DNA profiling was credible because it had solid epistemic foundations, and its job was to establish and demonstrate those foundations. However, the second committee did not release its report until 1996, and between 1992 and 1996 the criminal justice system did "go on." The story of how they

[17] See Knorr Cetina (1999, 63) for a discussion of liminal spaces in molecular biology and high energy physics. I am indebted to Dr. Martha Lampland for the original reference to the explication of the liminal. It is from van Gennep's *The Rites of Passage*. She notes that Victor Turner's analysis (1976, 59-92) focuses only on this specific moment in rites of passage. It is

"muddled through" – despite the lack of solid epistemic foundations and agreement in the scientific community is the subject of the next two chapters.

Before DNA profiling procedures could be used in the courts, they had to be stabilized and transferred to the relevant community of crime laboratory practitioners. Kathleen Jordan argues that before DNA profiling could be used in the courts it had to go through a process of "credibility crafting." This crafting of credibility was necessary for the individuals speaking and writing for the technology, and also for the technical procedures (Jordan 1997). However, Jordan does not note the local and contingent nature of establishing credibility around DNA in the courtroom. In the criminal justice system, establishing the credibility of individual and procedures was dependent on the skill, knowledge, and financial resources of lawyers in each and every courtroom in which DNA evidence entered. Because the courts sometimes work on precedent, the more times that DNA evidence was accepted, the more acceptable and credible it became. Also, because each expert witness had to submit reports to the judge, under oath, the reports from very prestigious scientists could be used by lawyers, especially under-funded defense lawyers -- to act as "virtual" expert witnesses who had extremely high credibility.

DNA profiling also lacked "disciplinary objectivity," which is the kind of objectivity which does not assume that *all* scientists agree about a particular phenomenon, but "instead takes consensus among the members of particular research communities as its standard of objectivity" (Megill 1991, 301). DNA typing was also a new technology and there were no existing norms for its application. Not only was there no consensus among the members of the research community as to a

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important to note that a liminal state is not simply a transitional phase, but also a transformative phase (Lampland 2002).

standard of adequate procedure, there was no consensus as to which research community had jurisdiction over DNA profiling. Until the *Castro* case, commercial laboratories and their scientists proceeded in an *ad hoc* fashion, creating what they needed as they went along. The controversy began when the adversarial setting of the courtroom was used to expose how dependent on local practices and trade secrets the procedure really was. This chapter has opened a window into the processes of negotiation and social dynamics by which procedures for calculating random match probabilities were established. These processes of negotiation necessarily involved the exercise of agency on the part of scientists and other players, within the pre-existing structures. Bourdieu would say that scientists began to mobilize in their positions in their fields – those with high credibility and prestige had immense amounts of power, or agency, to change the status quo, or at least shake it up a little.

Chapter Five

Movement Towards Closure: The Second National Research Council Committee on DNA Technology

1) Premature Closure

On the eve of the O.J. Simpson murder trial, two of the main adversaries in the DNA Wars declared that "[t]he DNA fingerprinting wars are over" (Lander and Budowle 1994, 735). They published their jointly authored paper in the prestigious journal *Nature*. Lander and Budowle "could identify no remaining problem that should prevent the full use of DNA evidence in any court" (735). This position was a radical about face for the FBI's Budowle. In the article he claimed the ceiling principle was an acceptable, if "unnecessarily conservative" means of calculating random match probabilities. More than one person involved in the DNA Wars felt that the article was a political ploy to make it appear that there was consensus among scientists on DNA profiling. This would mean that in the O.J. Simpson trial, DNA evidence would pass a California *Kelly-Frye* hearing. Lander and Budowle reassured the public that there was no scientific reason to doubt DNA profiling results if the testing laboratory had done the tests correctly. They felt that a positive outcome of the DNA Wars was that it had helped to professionalized the field of crime laboratory personnel, and made clear the need for standards and quality assurance procedures, but that "now it is time to move on" (Lander and Budowle 1994, 738).

This article represents two of the highest profile players in the DNA Wars attempting to act by mobilizing their credibility. Their goal was to change the way DNA profiling was perceived in the academic and judicial communities. They probably hoped that if two such credible, visible, and vastly opposed players in the DNA wars could agree, that they could close the controversy. The article can be seen as an

attempt by two individuals to change beliefs. To the extent that beliefs and knowledge are incorporated into some forms of social structure, their actions were intended to have a large, sweeping effect on the credibility of DNA profiling.

The article was published in October of 1994. The Simpson trial was scheduled to begin that November, and the second NRC committee on forensic DNA typing had just begun to meet. Its report was not expected until late 1995, but in fact it did not come out until mid-1996. If, as Lander and Budowle claimed, there were "no problems" with DNA profiling, why was the NRC funding another committee, *at the FBI's request*, to solve controversial statistical problems with the procedure? Lander and Budowle attribute the second committee to the "urging" of the National Institutes of Justice. However, the second report's preface says that the report was spawned by an April 1993 letter from the then director of the FBI, Judge William Sessions (NRC 1996, v-vi), who requested that the NRC do a follow-up study to resolve the statistical controversies which erupted after the publication of the first report. It was also no secret that the FBI found the ceiling principle to be too conservative, and had anticipated that it would handicap them in the courtroom. And what is particularly confusing is that in 1997, three years after the handshake with Eric Lander, Bruce Budowle said that there was no scientific justification for the ceiling principle, and that it was "ad hoc" and "absurd" (Budowle 1997).

Richard Lewontin was sceptical of Lander and Budowle's conclusions and motives, and denied their claim that closure had been reached and all problems with the technology solved. He questioned Lander and Budowle's motives, asking

> Why did Lander and Budowle choose to embrace in the pages of a leading journal of science, just before Budowle is scheduled to appear before tens of millions on television as a witness for the prosecution in

what is surely the most publicized crime since the assassination of John Kennedy? (Lewontin 1994, 398)

The complexities of "closure" in the DNA Wars are highlighted by this convergence of events. Lander and Budowle's declaration that the DNA Wars were over, at precisely the same time a second prestigious NRC committee had begun to meet, points to a lack of consensus between social worlds on what constituted knowledge, and what parts of the controversy were "closed."

Richard Lewontin's perspective is that the article can only be understood in the context of Eric Lander's professional life. Remember that Lander was originally the person who represented Lewontin's anti-DNA typing views to the first NRC committee between 1989 and 1991. In a personal interview conducted in the spring of 1997, Lewontin said that there were two "different" Eric Landers. The Lander who published the 1994 paper with Budowle was a "socially developed" Eric Lander, who was extremely careerist and ambitious. Lander had become "Mr. Big" in the Human Genome Project with his completely roboticized laboratory, and he desperately wanted to be elected to the National Academy of Science. Lewontin suggested that to get in with the "right" people, Lander had to eliminate his reputation of being opposed to DNA profiling, and the article with Budowle was exactly the right vehicle. Lewontin stated that:

> He [Lander] had gone out of his way to praise publicly Tom Caskey, a person he was originally testifying against. Why? Because Tom Caskey is really at the political center of all this, his influence is very important[.] The declaration that the "war is over," the whole metaphor of a "war" and so on, was Eric coming in from out of the cold because that's the one thing that he needed to purge from his past -- his negative attitudes towards DNA, his testimony against the use of forensic DNA. That had to go in order for him to be in with the "in" people. He wants to be part of the National Academy -- Caskey, Koshland -- he wants to be at the center of the establishment science

> in America, because he wants to be somebody. He had to purge himself of his former dissidence (Lewontin 1997, Personal interview).

This may be a partial explanation for Lander's motivation to write the article with Budowle. Budowle's abrupt about-face on the ceiling principle remains confusing in light of personal interview data three years later, where Budowle had nothing good to say about the ceiling principle. Just before the article was published Budowle was, as Lewontin noted, about to go on national television as a prosecution witness in the most publicized murder trial of the century, defending a controversial technique upon which a verdict of guilt or innocence could hinge. When interviewed, Budowle said that if courts wanted him to calculate the random match probability following the ceiling principle, he could and would do that (Budowle 1997). The article gives the impression that other than being "unnecessarily conservative," in Budowle's eyes there really was no problem with the ceiling principle, that there never had been, and that any questions about DNA profiling had been "fully resolved by the NRC report, the TWGDAM guidelines and the extensive scientific literature" (Lander and Budowle 1994, 735). The article not only defends the ceiling principle, but counters six different criticisms of the method. Budowle gave his name to an article which provides extremely strong advocacy and support for a principle that Budowle later called "ad hoc" and "absurd." Given this, it appears that the article represents what several people I interviewed suggested it was -- a last minute, last ditch attempt to shore up the credibility of DNA profiling so that Simpson's "dream team" of defense lawyers could not challenge it.[1]

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[1] For more on O.J. Simpson's defense and its relationship to science studies, see Michael Lynch, "The Discursive Production of Uncertainty: The OJ Simpson 'Dream Team' and the Sociology of Knowledge Machine," *Social Studies of Science*, Vol. 28, Nos. 5-6 (October-December 1998, 829-68. In this paper Lynch shows how prosecutors took a "realist" position towards the technology, and defense lawyers wrote a motion which took a constructivist approach, showing the extent to which quantitative estimates of random matches were

2) Why a Second National Research Council Committee on DNA Technology?

On August 30, 1993, Dr. Jim Crow was asked to chair the second NRC committee on DNA technology (NRC 1996, vi). A one day planning meeting had been held in June to determine if it another NRC study on DNA technology was truly needed. The decision committee included some members of the first committee, and possible members for the second committee. Eric Lander, Haig Kazazian, David Kaye, George Sensabaugh and Victor McKusick represented the first committee. Also in attendance were two statisticians who did not end up being on the second committee. Eric Fischer, who was later chosen to be the study director for the second committee, was involved at the time in extensive discussions with the FBI, the Commission on Life Sciences and the Chairman of the NRC on whether or not a second study should be done (Fischer 1997, Personal interview). The *ad hoc* planning committee put together a "wish list" for a second committee, which included a strong representation from the fields of population genetics and statistics (Fischer 1997, Personal interview).

Aside from the controversy following the release of the first NRC report on DNA profiling, and the move from statisticians and population geneticists to claim epistemic jurisdiction in these areas, another reason for the strong push to include population geneticists and statisticians is that the random match probability contains two kinds of uncertainty. The first type of uncertainty stems from the fact that the U.S. population is not homogeneous: it is made up of many, many sub-groups which have not yet formed a "melting pot." Among other things, the extent of population

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dependent on local laboratory practices and interested judgments.

subdivision is what population geneticists study. The second kind of uncertainty is statistical and surrounds the databases used to calculate the random match probabilities. The question had been raised as to how representative these databases were of the U.S. population as a whole, as well as how well they represented any particular subgroup. Assigning a probability to the numbers calculated on these databases is the territory of statisticians, but the distributions of allele frequencies are the territory of population geneticists. Hence, population geneticists and statisticians were deemed to be the experts to speak with authority on what was the correct answer to how to calculate random match probabilities (National Research Council 1996, 10-11).

It was determined that a second committee was needed for several reasons. First, the 1992 report had obviously rankled a large number of population geneticists and statisticians because they had been excluded from decisions about how computations for the random match probability should be made. As George Sensabaugh, member of both committees, puts it, "[i]f one is going to have a committee of national stature that makes statements in an area in which it has no particular expertise, ... then probably that national body has an obligation to come back and do it right" (Sensabaugh 1997). Haig Kazazian, also a member of both committees, said that a second committee was needed because the statisticians did not feel that the statistical issues in the first report had been handled very competently. He felt that the second committee was definitely a response to the academic community's criticism of the ceiling principle:

> The second committee was convened to really concentrate on the statistical issues. We did not worry about the quality control in the lab, we did not worry about how to do the test, but really focused on the

statistical aspects. ... We had a more limited scope and we had experts in that limited scope area (Kazazian 1997, Personal interview).

The second reason for commissioning a second committee was that the first report might have been creating confusion in the courts. Defense lawyers were able to find several expert witnesses, particularly Laurence Mueller of the University of California at Irvine. Mueller exploited poorly worded sections of the first report which said to use all available racial databases. He then sought out very unusual populations to calculate the random match probability and show that it did not always provide a conservative estimate. Sensabaugh's example of this non-conservativeness is as follows:

> [Some experts] said that to apply the ceiling approach we should draw upon data from all available genetic populations and I happen to have an American Indian population where this one particular allele is present in 50 per cent of the population, so despite the fact that it is only present in 1 per cent of any other population anyone has ever looked at, I am going to use that 50 per cent for the frequency. And then there is a Hindu sect that has the other allele at 80 per cent, that is also only at 1 per cent in all other populations, I'm going to use that 80 per cent and so now I'll apply the ceiling approach and lo and behold I end up with a frequency value greater than 1 which I round off to 1 and this genetic typing has no meaning at all (Sensabaugh 1997).

This imaginative exploitation of the ceiling principle by using small, little known populations from which to draw the frequencies was being presented to the courts, and the ad hoc committee felt it would continue unless somebody could come forward and say "this is the correct way to calculate a random match probability." The first committee had failed to do this.

One member of the second committee (who preferred to remain anonymous) went so far as to say that the reason for the second committee was that the first report was a huge mistake. The first committee had made errors, including simple ones in the rules of statistics, which detracted from the credibility of the report as a

whole. In the member's opinion the NRC had sanctioned and published a poor, error-filled report, and to maintain its credibility, it needed a second committee to revisit the controversial issues. This member said that the first report was awful, and that although the Academy would not admit it, that the report was scientifically dead wrong and that this put the Academy in a difficult position. The Academy had lost credibility by publishing a report that was so severely criticized in the scientific literature, and they needed to correct it. In this person's opinion the first committee was dominated by Eric Lander's strong personality, aided by him being the only member with strong quantitative skills. Given this, Lander's opinion carried undue weight on the committee. The first committee had also chosen to ignore some peer reviewer's comments, not making the suggested changes in the areas of statistics and population genetics, and so had published a report with statistical mistakes in it. In the member's opinion the second report was badly needed because the first report was doing damage to the reputation of the Academy and to DNA forensics. While this is the opinion of one person, it fits with Hilgartner's (2000) analysis of how NAS and NRC committees work. Hilgartner would probably agree that the National Academy of Sciences had been unable to control the "spin" from the first committee's report, that too much "backstage" conflict had leaked out to the "frontstage." Faced with the uproar in the scientific community, and the obvious discontent of the very powerful FBI, they had no choice but to commission a second report and convene a second committee.

However George Sensabaugh, Professor of Forensic Sciences at UC Berkeley and a member of both committees, did not feel that the first committee had in any way committed a "mistake" or had done a bad job. He felt the first committee addressed an important need, in the sense that NRC committees are convened to

address a particular problem. In his opinion, the only way the committee could have been a mistake is if there had not been a problem. "In this case there was a problem. In reality, this technology was being applied and brought into practice more rapidly than it should have been" (Sensabaugh 1999, Personal interview).

There are three important issues arising out of the second NRC committee. The first is their strong reaction to the first committee's report. Second is the conclusion reached by NRC2 that "small is small." In other words, that no matter whether the correct or incorrect database was used for calculations, tiny probabilities were obtained, and the difference in magnitude between a probability obtained from a correct database and one from an incorrect database was not forensically significant. The committee concluded that an order of magnitude of "10 fold either way" was not meaningfully different. The third and perhaps the most important contribution of NRC2, is that it served to codify and formalize rules of procedure -- ways of "going on" that over time, would become taken for granted as the correct way to proceed.

Perhaps the National Academy of Sciences and the National Research Council had been naïve -- or perhaps uninformed -- with the first committee, assuming that the problem of the random match probability and VNTR allele frequency distributions had simple solutions. When Dr. Daniel Hartl, now at Harvard University, was asked about whether this problem would have come up for the population genetics community without the demands for the knowledge arising from the justice system, he responded:

> No. That's why it was a challenge to population genetics. Nobody knew how to think about this kind of problem, and when in the earliest stages population geneticists made statements that would lead you to think this was a trivial problem, that was not correct. It's a very difficult problem, how you do this, and even the second Crow committee [NRC2, led by Dr. James Crow] set up a number of scenarios that they didn't know how to do the calculations, and still don't know how to do

> the calculations. So this is not a trivial problem in population genetics (Hartl 1997, Personal interview).

George Sensabaugh said that in the beginning the people who were doing RFLP work in the forensic domain just assumed that the classical rules of population genetics would be simply applied, and they did not worry about trying to test them. He says they realized there were no available statistical tests that would allow them to test the procedures. It was at this point that the critical population geneticists came on board, arguing that forensic scientists were basing life and death decisions on calculations the basis of which had not been subjected to testing (Sensabaugh 1999, Personal interview).

3) The Second National Research Council on DNA Forensic Science: An Update

One year after appointing population geneticist Jim Crow as chair of the second NRC committee, the rest of the committee members were named in August of 1994. The second NRC committee held its first meeting in September of 1994. That interim year had been spent searching for funding. The second report was funded by the National Institute of Justice, the State Justice Institute, the National Science Foundation, the National Institutes of Health, and the Department of Energy (NRC 1996).

For the second committee, the NRC was very careful to put together a committee that would pass muster in the areas that they were specifically supposed to work in, and study director Eric Fischer feels that this is what led to more confidence in the second committee's report. Regarding the membership of NRC1, Fischer says:

> [Eric] Lander was trained as a mathematician, and he's obviously brilliant, he's a guy who works in both fields and everything, but he's not a statistician. A statistician is a special breed of mathematician. Now I'm sure that he's very comfortable with and understands a great

> deal about statistics -- so do I -- but no statistician would ever consider me dust beneath their feet. And Mary-Claire King is an excellent geneticist, but she's not a card-carrying population geneticist, and so when the committee came out with this recommendation -- the ceiling principle -- that had population genetics and statistics combined, card-carrying members of both communities could not really support it, especially because the scientific grounding for it was never clear in the report (1997).[2]

To avoid the problems of expertise that plagued NRC1, three population geneticists and two statisticians were appointed to NRC2. The mandate of the report was to address issues of population genetics and statistical questions, and to clear up "unintended consequences" arising from the previous report. Committee member George Sensabaugh says the committee members were told to focus only on matters of population genetics and statistics (Sensabaugh 1999, Personal interview). The report outlines its mandate as to:

> perform a study updating the previous NRC report, *DNA Technology in Forensic Science*. The study will emphasize statistical and population genetics issues in the use of DNA evidence. ... Among the issues examined will be the extent of population subdivision and the degree to which this information can or should be taken into account in the calculation of probabilities or likelihood ratios. The committee will review and explain the major alternative approaches to statistical evaluation of DNA evidence, along with their assumptions, merits, and limitations. It will also specifically rectify those statements regarding statistical and population genetics issues in the previous report that have been seriously misinterpreted or led to unintended consequences (NRC 1996, 49).

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[2] When interviewed, Eric Fischer stated many times that the ceiling principle had no scientific grounding. The second NRC report echoes this belief. However, following Barnes (1977, 1), it is possible that the problem with the ceiling principle was not that it was not scientifically grounded, but that it failed to gain credibility and thus become an accepted belief, which is the definition of "knowledge." The ceiling principle was the "solution" advanced to a problem of knowledge (how to correctly calculate a random match probability) that was puzzling the law enforcement and forensic communities at the time. If this solution had been accepted by all interested parties, it would have become "scientific knowledge." Instead, members of the knowledge communities of population genetics and statistics said that the ceiling principle produced by the first NRC committee could not possibly be valid scientific knowledge, as the committee did not contain any population geneticists or statisticians. Thus, the ceiling principle failed to attain the status of accepted belief in *any* community, let alone a community powerful enough to enforce its status as scientific knowledge.

The justice department wanted an extremely narrow mandate, suggesting that NRC2 focus only on the ceiling principle (Fischer 1997, Personal interview). However, in the eyes of the NRC there was more context at stake, including issues of how to evaluate evidence. They felt that if the sole focus was on whether or not the ceiling principle should be thrown out, then the question of how to correctly calculate a random match probability would still not be answered. Whatever the explicit mandate, the *implicit* mandate was to secure the ontological and epistemological foundations of DNA profiling so that it could be used unproblematically in the justice system.

These different goals meant that some negotiation was necessary with the funding bodies. The National Science Foundation (NSF) was more interested in the statistical procedures and to some extent the social implications of the technology. The National Institute of Health was interested in the population genetics questions. The Department of Energy was interested in the ethical, legal and social aspects. Across all the different sponsors, it turned out that their different requests meshed fairly well with what the NRC thought should be done on the second committee (Fischer 1997, Personal interview). In effect, the focus of the committee on statistical and population genetic issues made the *de facto* audience for the second committee be the scientific community, whereas the audience for the first committee was ostensibly the courts.

> The [second] report had to convince the expert community. And to convince the expert community it had to be technical. If you look at what happened to the first report, it was the experts [that criticized it]. If you get a report out there, you might be able to get away without having strong grounding within your report if you have a general agreement within the technical community. That is, you might be able to get away without strong grounding if you produce a report that the professional community is going to stand behind. In the case of [NRC2] we didn't know, we knew there were people that would disagree with what the report said. And so it was especially important that the report was able to demonstrate the reasoning for its

conclusions (Fischer 1997, Personal interview).

In the end, NRC2 reinstated the product rule as the correct method for calculating random match probabilities, but this time with a correction factor for possible population subdivision called *theta* (NRC 1996, 102-106). The second committee reports that "Our approach is not to assume HW [Hardy-Weinberg] proportions, but to use procedures that take deviations from HW into account. To do that, we return to discussions of population structure as measured by 2 (theta)" (1996, 104). The interesting thing is that the value for *theta* was determined by using existing databases, which were all drawn from samples of convenience (NRC 1996, 104). In fact, all the conclusions of NRC2 were based on convenience samples, never on random samples. The committee concluded that for their purposes, a convenience sample of at least several hundred people acted, for genetic purposes, in the same way as a random sample (NRC 1996, 32), and so there was no need for the kind of population studies proposed by Lewontin and Hartl in 1991 and in the first NRC committee's report.

4) Reaction of NRC2 to the NRC1 Report

Perhaps the most striking finding of the second committee's report is that it rejected out of hand that there was *any* scientific grounding for the ceiling principle, and so rejected the ceiling principle itself (NRC 1996, 35; 157). The first committee's report described the ceiling principle as being a method grounded in frequency distributions which would result from the sampling of 100 people from 15-20 different homogeneous populations which make up the U.S. general population. The highest frequency found in any of these databases was to be used for the multiplication rule. The interim ceiling principle, to be used until the 15-20 populations were sampled, was to use the highest frequency observed in any database, or 10%, whichever was

larger. Regarding this procedure, the second committee noted that as of 1993, the sampling of the 15-20 populations had not occurred, and that

> The necessary ground work for applying the ceiling principle has not been done, and there have been few attempts to apply it. We share the view of many experts who have criticized it on practical and statistical grounds and who see no scientific justification for its use (NRC 1996, 157).

Member of both committees Haig Kazazian said that "nobody said it was a specific scientific estimate, everybody [on the first committee] said it was a reasonable approach to the issue for the purposes of the courts. We were not trying to promote science with that" (Kazazian 1997, Personal interview). Chair of NRC1 Victor McKusick said that the scientific justification for the ceiling principle was the observation that population stratification does occur (McKusick 1997). Member of the second committee and Professor of Law Margaret Berger agreed with Kazazian, and felt that the scientific community completely misunderstood the ceiling principle. She says that the scientific community looked upon the ceiling principle as a scientific answer, which it was not, so of course it had to be "wrong." Her belief is that the scientists did not understand that the legal profession was interested in having a conservative probability that would not be detrimental to the criminal defendant and that this was a stop-gap measure until more research had been done, which could provide "true" probabilities. In her opinion, the ceiling principle was intended as a way of safeguarding defendants in a criminal system when adequate information was lacking. She said that she "did not think that the scientists ever really understood that science and the law are not necessarily seeking the same answers" (Berger 1999, Personal interview). In her opinion, had NRC1 worded their recommendation

something like: 'from a scientific point of view, we're not sure yet how to calculate the probabilities, but this method does not harm the defendant,' there would not have been such an uproar. Berger stressed that the question in front of the courts is never "is this the ultimate scientific answer," but rather, "would it be fair to give this kind of evidence this kind of weight?" (Berger 1999, Personal interview).

Science and the law ask two different questions of the same number. Scientists ask "is it true" or "is it a good estimate?" In the courtroom the question is, as Berger said, "how much weight should be attributed to the DNA evidence, given that there is a match?" George Sensabaugh said "retrospectively the ceiling approach was not good science, but sometimes you have to adopt an *ad hoc* solution if you have to do something right now" (Sensabaugh 1999, Personal interview).

Given the unequivocal way in which the second committee pronounced the ceiling principle as scientifically ungrounded, it seems fair to say that even if the intended audience was the courts, the members of NRC2 were not the only scientific body which expected a "scientific" answer from the first NRC committee. The very fact that the National Academy appointed a second committee to resolve the statistical issues indicates that more than the population geneticists and statisticians who protested were looking for a scientific answer. The FBI was the strongest lobbyist for a second committee (NRC 1996, vi).

The second committee also felt that in the intervening period between the two reports, enough population data had been collected, mostly in the form of a 4-volume series of population databases collected by Bruce Budowle of the FBI, so that "neither ceiling principle is needed. We have given alternative procedures, all of which are conservative but less arbitrary" (NRC 1996, 35). The second committee sided with TWGDAM members who found that "the current methods employed by

forensic scientists have been demonstrated to be robust scientifically" (NRC 1996, 159).

Dr. Eric Fischer, study director for the second NRC report, felt that the ceiling principle was a reasonable suggestion, but that it was not grounded in any science and felt as others did that it was *ad hoc* and arbitrary. In his view this arbitrariness was the fatal flaw of the ceiling principle, and that to have "a scientific committee coming up with an admittedly non-scientific and rather arbitrary proposal, you're just laying yourself open. In a controversial area you're just saying 'bring on the sword'" (Fischer 1997, Personal interview).

When asked how a group of such bright, professional people could come up with a solution with "no scientific grounding whatsoever," Dr. Fischer speculated that being "experts" in the group situation of the NRC committee allowed people to shed the normal constraints placed on them by the scientific community. He felt that in their everyday lives the committee members, were very used to having to ground everything they did. Then, once they were placed in a room and asked for their opinions,

> They'll start giving their views, and its like they're being released from all the constraints of the scientific method, because now they've got their chance to say things that they really believe, but the constraints have been removed that are self-imposed constraints of the scientific community which govern their behavior, and what you get is somebody that is going to spout their views (Fischer 1997, Personal interview).

Fischer felt that on the first committee, things were not set up in a way in which the committee members were constantly reminded that they had to use the same rules of evidence and the same rules of logic with respect to their discussions in the committee that they used in their scientific work. The first committee was dominated by a few strong personalities and a weak chairman, who was not able to

keep the group working *as* a group. The chapters of the first report were each written by different people, and so in committee meetings it was easy for people to absolve themselves of responsibility for a certain section of the report by saying "I didn't write that."

Fischer felt that the much maligned ceiling principle came out of an attempt to find a middle ground between warring factions on the first committee. The ceiling principle was an attempt to compromise between the opposite poles of extreme empiricism, which would have required counting and empirical evidence, and the statistical modelling used by population geneticists, which is grounded in a null hypothesis which may or may not be rejected in any given case.

> The difficulty in the case of the ceiling principle is that you have a group of scientists who came up with an ingenious and potentially very useful -- even some of the critics say yes, it can be useful -- but not scientific, approach. Some people said it was a political compromise if you will. It was an attempt to say 'OK, this is a middle ground, yes it's not scientifically valid but at least it gives you a way of using the frequency data, it's more powerful than counting, it's less powerful than straight population genetics, but it gives you a way of doing it' (Fischer 1997, Personal interview).

He agrees that there was a logic to the ceiling principle, but that it was not a *scientific* logic. However, here lies one of the many paradoxes of this story. The first committee felt they were not asked to provide a *scientific* solution to the problem of random match calculations, but a solution that would produce numbers that were adequate for the courts. In Fischer's view, the problem is that the credibility of any National Academy of Science committee lies in its scientificity. In the end, it is the arbiter for the government on scientific matters, and so to have a National Academy committee produce a non-scientific answer to a scientific problem created a new problem all its own.

According to Fischer, the way the National Academy ensures that its reports

will have credibility is first, that they have the right mix of expertise on the committee. The first committee obviously lacked the expertise in population genetics and statistics required to convince those relevant communities that the ceiling principle was valid and grounded. Second is the name of the National Research Council, which is itself imbued with so much status and credibility that any report coming out under its name has instant credibility, much more so than would be given the same group of experts, working on the same problem, but on their own, not under the aegis of the National Research Council. In Fischer's words,

> An Academy report's strength is in taking things that have been proposed in the scientific community for which there is some grounding, looking at the evidence that's available, and coming up with judgments that you can ground, that are grounded. ... It's not enough to have the name of the Academy. At one time it might have been enough to have the name of the Academy and the expertise. At one time it might have been enough just to have the name of the Academy. Then you had to have the right experts on the committee. But now people aren't going to believe that, they're too sophisticated, they want to know the reasoning. It has to be grounded (Fischer 1997, Personal interview).

According to Fischer and many other critics, the ceiling principle was not grounded in anything but a logic of conservativeness. The limits for the ceilings (5% and 10%) were admittedly arbitrary, and the whole thing was fashioned in an *ad hoc* way. It did not come out of existing research, and repeated attempts to gain from Eric Lander and the rest of the committee the foundations of the ceiling principle met with only silence.[3] In Fischer's words,

> No one was able to come up with a mechanism themselves. We were

[3] While committee members were reticent to say just who on the committee "came up" with the idea of the ceiling principle, it is in essence the same procedure suggested by Lewontin and Hartl in the 1991 *Science* publication. Recall that Lander and Lewontin were in close contact, and that Lewontin presented a written presentation to the first committee, so they were familiar with the concepts of the ceiling principle as advocated by Lewontin and Hartl, even though the publication did not come out until their last meeting on December 21, 1991.

> told at one point that yes it was justified, but we never saw the evidence for it. There just simply wasn't any. And the thing is that a number of people associated with the earlier report, committee members, said that it was clearly arbitrary, that it was an attempt at a compromise between two extremes (Fischer 1997, Personal interview).

The first report itself has very little technical information on the background to the ceiling principle, which is one of the loopholes that allowed clever defense lawyers to use it to their advantage (Reilly 1997, Personal interview). There was also no published literature justifying the ceiling principle. Fischer notes that "the [second] committee talked with the principals involved, and they talked with them about the justification for the ceiling principle, and weren't happy with the answers that they got. I never saw anything that I could consider a scientific justification" (Fischer 1997, Personal interview).

In Fischer's view, the third thing which ensures credibility for National Academy reports is the quality control process, which involves extensive peer review to make sure that the report has kept to its mandate, and that the committee has not gone beyond either their expertise or the scope of what it was supposed to do. This step may not have failed in the first NRC report, as there was some evidence that the members did not follow all the recommendations of the peer reviewers. Whether this was a matter of time, or choice is not known. In speaking of the role of scientific advisory bodies such as the NRC in the role of regulatory science, Sheila Jasanoff says that groups such as the Food and Drug Administration use these bodies to "harness the authority of science in favor of its own policy preferences (Jasanoff 1990, 178)." She goes on to note that the individuals on these panels seem "at times painfully aware that what they are doing is not 'science' in any ordinary sense, but a hybrid activity that combines elements of scientific evidence and reasoning with large

doses of social and political judgment" (Jasanoff 1990, 229). If this is the case, then there is every reason to think that NRC1 could have solved the problem before them, *if* they had been able to solve the problem of how to work together as a group. Like other NRC committees, they had the brain power, they had access to the finest scientific reasoning, and the balance to provide large doses of social and political judgment. However, NRC1 was not able to solve its internal problems of order, and this was central to its inability to produce a product which would be recognized by other scientists and the forensic community as a legitimate scientific solution.

As noted in the previous chapter, there was a fair amount of resistance in the professional community to the ceiling principle. The lack of qualified population geneticists and statisticians on NRC1 perhaps had led to the ceiling principle being doomed, but Fischer felt that people would have been willing to accept it if there had been any grounding shown for it. He felt that "the ceiling principle had population genetics and statistics combined, and so members of both communities could not really support it, especially because the sort of scientific grounding for it was never clear in the report" (Fisher 1997).

Forensic scientist George Sensabaugh, who sat on both NRC1 and NRC2 felt that NRC1 had not done a good job of thinking through the ramifications of the ceiling principle when applied to very common alleles. The focus of the committee had been on how to treat very rare alleles. Part of the problem was with the newness of the restriction fragment length polymorphism (RFLP) markers. Until the early 1980s and the discovery of RFLP's most genetic markers had two or three alleles. When there are only two or three alleles, a sample of one or two hundred people provides an adequate distribution of all alleles. However, when considering RFLP's, which often had hundreds of alleles,

> We suddenly moved into an entirely different domain of genetic markers. These were polymorphisms where there were 50 or 100 or 100s of different alleles, and even if we could not resolve every allele ... nevertheless there were still enough alleles that if we tried to assess the frequency of all possible genotypes at every locus, we might have to test several thousand individuals, and even then we would have a number of genotypes that would have zeroes or ones in them and you cannot really do significant statistics when you have cells that are occupied by a single individual. And so with RFLP markers not only were we possessed with markers that had a huge discrimination power, but we did not know how to assess whether they followed the conventional population structure of population genetics. We did not have the statistical tools (Sensabaugh 1999, Personal interview).

The first NRC committee came up with a compromise method that seemed to fit the needs of the justice system, and in their eyes, would do no harm. However, given the lack of credible expertise, were its recommendations doomed from the outset to failure? Haig Kazazian, member of both NRC1 and NRC2, felt the criticisms of the ceiling principle were unfair and were asking for something NRC1 had not been mandated to deliver:

> We were not saying it was a scientific estimate. They [the scientific critics] wanted to have a more precise estimate based on population genetics, they felt that these loci should have been independent. ... Well who was trying to be scientific? We were trying to be fair. We decided to come up with a reasonable compromise ... realizing that some parts of it did not have scientific validity but they were reasonable estimates on an upper boundary, on as low as you should go in terms of the frequency, and that was the modified ceiling principle. We all realized that pulling those fractions for an allele of do not go below 5 percent and do not go below 10 per cent was pulling the numbers right out of the air. We all realized that (Kazazian 1997, Personal interview).

Kazazian's comments seem almost shocking, coming from a member of a prestigious NRC committee, but the issue of audience for each report surfaces here. The first committee clearly felt that their audience was the courts and judges, and their mandate was to come up with a pragmatic, fair and reasonable solution to the random match probability calculation problem. And they came up with a solution.

Given the make-up of the committee, this, and any proposed solution was almost guaranteed to be called "unscientific." What would have counted as appropriate "grounding" for population geneticists and statisticians would have been having the ceiling principle based on a logic or methodology taken from one of, or shared by, the two fields. In other words, grounded in a pool of logic or theory on which the community of professionals had already reached consensus. The ceiling principle was *called* unscientific because it was not seen as "going on in the same way" (Collins 1985, 13) or being based on the same rules of logic that population geneticists or statisticians took for granted. It would not have mattered if the ceiling principle were an "ingenious solution," as Fischer called it. It would have been labelled "unscientific" because it did not follow the rules of the game of science as laid out by population geneticists and statisticians.

Sociologist of science Harry Collins argues that rules of logic, or taken for granted ways of going on are not private, but are the property of a social group. In science, the rules for going on in the right way vary from group to group (Collins 1985, 15), and between sciences (Knorr Cetina 1999). There is no one epistemic standard or anything called a "scientific method" which applies to all of the sciences (Knorr Cetina 1999). In the case of DNA profiling, when the ceiling principle was proposed, it was seen as belonging to the "turf" of population geneticists and statisticians. Hence, a solution to the problem of calculating a random match probability would have to fit with the logics in use in both these disciplines. The ceiling principle and the interim ceiling principle were methods that had a logic, but not a logic endorsed by either of these expert groups, and because of this the principles were called unscientific.

5) How Big is Big? Small is Small...

It was the task of the second committee to determine how to "go on in the right way" with regard to calculating the random match probability. They had to determine the rules and procedures, provide the formulas, for people to follow that would fit into the logic of population genetics and the statistically acceptable ways of answering the question. The second committee agreed that ideally, the reference data set for a random match probability would be a simple or scientifically structured random sample from the relevant population (NRC 1996, 30). However, here the picture becomes muddy as they abandoned random samples not for scientific reasons, but because of the difficulty and expense of attaining them. NRC2 notes that it is not always clear what the relevant population is, whether the sample should include males and females, or be local or national (1996, 30). What took the question of random sampling out of the picture is that the committee agreed that random sampling was "difficult, expensive and impractical" (1996, 30). Margaret Berger commented that "there were so many of them [convenience samples] around and that it would be so impossible to randomly sample that it was okay. Random sampling seemed impossible in terms of cost and also unnecessary" (Berger 1999, Personal interview).

Eric Fischer admitted that the issue of random versus convenience sampling was an issue for the committee, especially for the frequentists who wanted empirical counts of allele frequencies to get the distributions to calculate the random match probabilities. Fischer says that they asked themselves that if they were dealing with a convenience sample that acted like a random sample, could they treat it like a random sample? About convenience samples, Fischer commented:

Basically they act like random samples. ... Essentially you've got for

> all intents and purposes, random samples, unless some purist is going to say no, you don't have a random sample because you don't have a random sample. You're not randomly sampling the underlying thing directly. Well frankly, you can't randomly sample. It's not possible to randomly sample. You can just be less biased than right, but it's one of those goals like zero fatalities for the FAA, it's an ideal that you'll never reach. So in the end the committee concluded that based on the evidence, from the genetic perspective, [when using convenience samples] you're essentially dealing with random samples (Fischer 1997, Personal interview).

Given this consensus about samples of convenience acting like random samples, NRC2 then used existing databases constructed from DNA from blood banks, paternity-testing laboratories, laboratory personnel, clients in genetic-counselling centers, law enforcement officers, and people charged with crimes (NRC 1996, 30). In addition, they gratefully turned to the four volume dataset of convenience samples provided by Bruce Budowle of the FBI, titled "VNTR Population Data: A Worldwide Study" (Laboratory Division, Federal Bureau of Investigation 1993). An interesting thing about this compilation of datasets is that Budowle collected it by getting on the telephone and fax machine and calling people around North America and Europe, and asking for faxes of their databases (Budowle 1997). No information is given on each database as to where it is originally from, how it was collected, who is included, and how decisions on race were made (Laboratory Division, Federal Bureau of Investigation 1993).

The members of NRC2, left with samples of convenience which they decided were essentially random and therefore adequately representative of the American population, began examining how different the random match probabilities would be if they deliberately used an incorrect database to calculate the probability. For example, using a Black database when a Caucasian one was called for. In the end they agreed that no egregious errors were made if an incorrect database was used, and that in

any case the "values that are possibly incorrectly estimated lie within 10-fold above and below the 'correct' value. We conclude that it is reasonable to regard calculated multi-locus match probabilities as accurate within a factor of 10 either way" (NRC 1996, 35). Haig Kazazian summed up their position:

> Forensically, and as far as the FBI told us, getting the one in 20,000 answer did not differ from getting the one in 200 million answer. That's another reason we weren't so worried about getting the modified ceiling principle -- 1 in 20,000 -- who cares? Maybe its not one in 200 million which scientifically you say it might well be, but it's reasonable for all populations and that's why it's fine (Kazazian 1997, Personal interview).

The previous questions of "how big is big" enough of a deviation to count as evidence for subpopulations, were translated to a consensus that "small is small." No matter how the probabilities were calculated, they were tiny numbers, and the committee did not feel that a difference of "10 either way" was a substantively meaningful difference. Small was small when comparing trillionths to billionths. Study director Eric Fischer comments:

> So you talk about the difference. It might be a statistically significant difference at a probability level of 10 decimal points, in the billionths or trillionths -- a statistically significant difference which is not meaningful. ... If you're talking about a difference of only one or two decimal places ... between .1 and .15 or between .1 and .2, versus the probability of 1 trillionth and one billionth (Fischer 1997, Personal interview).

Paradoxically, the same reasoning applies to both of the ceiling principles, and yet the FBI was sure that its "conservativeness" would impair its ability to obtain convictions based on DNA evidence. George Sensabaugh, who sat on both committees, comments on the modified ceiling principle:

> Lots of people felt it was too conservative, but if you did the calculations, it might change by a factor of 10 or 100 -- the biggest one I saw changed from 1 in a trillion to 1 in a billion. Not a big deal. It's

> worth sacrificing a little bit of discrimination power in order to make the problem go away (Sensabaugh 1999, Personal interview).

Law professor and member of the second NRC committee Margaret Berger believes that in the shuffle between the two committees, the fact that what method (or database) was used for calculating the random match probability actually made very little difference. She said that putting the evidence before a jury,

> Once you have the evidence in a case, and whether it is 1 in a billion or 1 in 10,000 or 1 in 100,000, none of it is going to make terribly much difference. It's going to be convincing to a jury. The difference in the probabilities is totally meaningless, and when you compare it to the ordinary evidence in a case, that the defendant was seen wearing a red jacket and a red jacket was found at the perpetrator's home -- what kind of odds do you have there that that's the same red jacket? (Berger 1999, Personal interview).

Although very detailed and filled with formulas and mathematical equations, the conclusion of NRC2 that "small is small" remains somewhat confusing because they do admit to large differences between racial databases:

> Extensive studies from a wide variety of databases show that there are indeed substantial frequency differences among the major racial and linguistic groups (Black, Hispanic, American Indian, East Asian, and White). And within these groups, there is often a statistically significant departure from random proportions. ... [T]hose departures are usually small, and formulae based on random mating assumptions are usually quite accurate. So, the product rule, although certainly not exact for real populations, is often a very good approximation (NRC 1996, 33).

The second NRC committee handled the potential population subdivision by coming up with a correction factor they called *theta* (θ). They made the assumption that there was undetected substructure in the population, and adjusted the product rule to compensate for the unknown substructure (1996, 33). Again, they came to the same conclusion that the first committee did -- that population subdivision exists and affects the calculations of random match probabilities. The difference is that this time population geneticists and statisticians were making the rules. Acknowledging the

existence of some undetected substructure, but minimizing its possible effects, and coming up with a correction factor to be added to the already well entrenched product rule formula was considered by NRC2 to be "going on" in an acceptable way. The solution fit into the rules of both the academic worlds of population genetics and statistics. The committee suggests that *theta* be usually less than .01 for U.S. populations, and that when dealing with a very small database or population, its value should be increased to .03 (NRC 1996, 29-30). There is no explanation given for why .01 and .03 are less arbitrary numbers than the ceiling frequencies of 5 and 10 percent chosen by NRC1, or the .05 and .01 level of significance conventionally followed in most statistical procedures. However, *this* particular correction factor was the outcome of group consensus. The ceiling principle was not the result of group consensus, but a solution forced upon NRC1 in their last meeting, and a solution whose merits they had fought over for two years. The second NRC committee not only achieved consensus, it was a group filled to the brim with experts qualified to make rules for population genetics and statistics. The publication of NRC2's report in 1996 reinstated the product rule, and forensic laboratories across the United States now routinely use the theta correction factor when calculating random match probabilities (Kahn 1999, Personal interview).

Why was one set of arbitrary numbers accepted as a "scientifically valid" correction factor which provides "adequate estimates," while another set were seen as "ad hoc" and "unscientific"? Both of these conclusions were reached through a group process of discussion and consensus. Perhaps NRC1 was hindered in their ability to come up with a better solution because they were unable to establish social order within their own committee. Right up until the last meeting and the resignation of Thomas Caskey, the first committee was divided and dominated by a few strong

personalities. The first committee and fought and squabbled and debated and left the key statistical issues until the very last minute.

> We knew we had a big conflict. We had people on both sides of the fence. There were people that wanted to go for multiplying, others said some modification of multiplying frequencies, and others said no, it's got to be no worse than 1/N, the database size (Kazazian 1997, Personal interview).

Committee chair Victor McKusick put in a lot of time to try to get the committee members to reach a compromise. This was only achieved at the last meeting, after Caskey's resignation.[4] The solution the committee accepted was the ceiling principle, put forth by Eric Lander (Kazazian 1997, Personal interview). Kazazian indicates that NRC2 was a much more convivial committee which functioned much more smoothly:

> [There were] a lot of differences [between the two committees]. [NRC2] was more focused, we had the statistical group, and because we had good data there was less controversy. The chair could really lead the committee – Jim [Crow] had a lot of time to do it. It went very smoothly. From a sociological point of view it was a much smoother process -- but a lot of that depended on the composition of the committee. I would say also that the committee really got along quite well. People got along much better than on the first committee (Kazazian 1997, Personal interview).

As Eric Fischer said, he was determined that the lack of consensus, and the outright cleavages which became intensely personal were absolutely "not going to happen on

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[4] A member of the second NRC committee told me that Thomas Caskey was forced off NRC1 because he did not agree with Eric Lander's viewpoint. Caskey and Lander were on opposite poles of a proposed solution to the population genetics problem. Caskey preferred the existing multiplication rule, in use by the FBI. Lander lobbied for his "ceiling principle." However, in print, Caskey's resignation was reported as being due to a conflict of interest between his acting on the committee and the fact he had substantial financial interests in a large DNA fingerprinting company (Anderson 1992, 753). In addition, his laboratory had received a very large grant from the National Institutes of Justice to work on DNA profiling. Also of interest is that in the *Yee* case, part of what persuaded the judge to admit the DNA evidence was the fact that Thomas Caskey had adopted the FBI's procedures and protocols for use in his own laboratory (U.S. v Bonds *et al*, 1993, 38).

my committee" (Fischer 1997, Personal interview). The second committee has been described as a polite, collegial community. It did much of its work by e-mail and snail mail, as drafts were mailed back and forth across the country between members. Not meeting as often in person cut down the potential for interpersonal strife. The second committee had a strong chair, retired population geneticist Jim Crow from the University of Wisconsin at Madison, who had accepted the position knowing that it would take a lot of time, and he put that time in. He was also a strong personality and able to control and override other strong personalities when necessary.

There are other interpretations of the second report. Richard Lewontin's reaction was sceptical.

> It's simple. The second report is a very silenced report. What it does is it bears down a lot on the issues on which there is some genuine difference of opinion and tension -- which are population genetic issues, the adequacy or non-adequacy of databases, which we can argue from here to eternity.
>
> If the NRC were serious about advising the courts, they'd say you want to use DNA evidence. Fine. It's a form of identification. It's a form of identification like eyeglasses and height and skin color and everything else. Say to the court "we did a DNA test, we did it in a laboratory which follows the following national guidelines, and we got a match." And forget the whole probability thing. We got a match. That's evidence. It's what is called probative.
>
> So, there we are, those are the contradictions, and that's the politics behind the second NRC report. The politics is the report was bought and paid for by the Justice Department because it didn't like what it had before, and the committee was guaranteed to begin with to contain those people who would give them what they did want (Lewontin 1997, Personal interview).

If an expert, and insider like Richard Lewontin can say that the population genetic issues surrounding allele frequency databases could be argued "from here to eternity," it highlights even more than these are deep, complex issues, that were not so much settled by the first or second NRC committees, but in the end, simply

bypassed. The first attempt at bypassing racial issues -- the ceiling principle, did not work. It did not achieve credibility among the academic community. The second NRC report met with little fanfare.

6) Making the Rules of the Game

For most people in the academic community, the second NRC committee brought closure to the DNA Wars. From the perspective of stabilizing and standardizing knowledge, the most important contribution of the second committee was that it provided a set of rules for "going on in the right way" that were acceptable to the disciplines of population genetics and statistics. The committee provided mathematical rules which were seen as a solid epistemic foundation for calculating random match probabilities. These rules were also stated in the scientific community's favorite language: mathematics (Porter 1997). However, expert population geneticist Daniel Hartl said there were still statistical formulas which the second committee was not sure what to do with, and which were not understandable, in the report (Hartl 1997, Personal interview). After the publication of the report there was still some scientific controversy -- Richard Lewontin complained that the required empirical studies had never been undertaken. The second committee had dodged the question of random sampling and representativeness by accepting samples of convenience. In Lewontin's view it had taken an easy way out and not done the hard empirical research required to "truthfully" answer the questions (Lewontin 1997, Personal interview).

However, the majority of the scientific community had quit complaining before the second committee published its report. This meant that defense lawyers had less ammunition with which to claim that there was a controversy in the scientific community. This may have contributed to the fact that by 1996, random match

probabilities had become less of a target in the courtroom. Some members of the FBI that had been acting as expert witnesses throughout the period of the controversy said that they did not know why one committee, let alone two had been required. In their eyes, they were always able to get their evidence accepted (Adams 1999; Deadman 1999). If the *Yee* case is an example, this may have been due to precedent and the tremendous institutional credibility of the FBI. And, the FBI was happy with the second report because it reinstated the basic multiplication rule, albeit with the *theta* correction factor – but the method was essentially the same as they had been using since the late 1980's. However, most of this calming down of the controversy took place before 1996, before the second committee had published its report.

Margaret Berger believes that the second committee's report established an informed vocabulary and that it created links between people in the different communities – or different social worlds -- so that there was more uniformity and more understanding. She said "this was a very useful exercise in getting a lot of people up to snuff and also sort of presenting a way in which a new scientific development could be handled jointly by the scientific and the legal communities" (Berger 1999, Personal interview).

The second NRC committee did not so much close the controversy provide reassurance that there were sound reasons and procedures for believing in the probabilities produced by forensic laboratories. They legitimated what had already become stabilized and standardized practice. The second NRC committee began meeting while the nation was spellbound by the OJ Simpson trial, in which DNA evidence, and particularly the "chain of custody" figured prominently.[5] And yet, the

[5] The reader wishing more information on the O.J. Simpson trial from a Science Studies perspective is directed to the special issue of *Social Studies of Science* on DNA

report was released with little media attention. *The New York Times* announced the release on page 21, and its headline read "Expert Panel Calls Evidence From DNA Fully Reliable" (Leary 1996). The article reports that the NRC had developed "a new combination of formulas to calculate the likelihood that a DNA match between evidence and a suspect could be explained by mere coincidence" (Leary 1996). This is in strong contrast to the first report – whose recommendations were erroneously reported on the *front page* of the New York Times even before the report had been printed!

To see the second NRC report as solely responsible for closing the controversy is compelling, but overly simplistic. Closing the controversy required much more than the "right mix" of experts producing procedures and rules in the language of mathematics. The controversy was also closed because the knowledge became more stable, partly through the standardization of protocols for producing DNA profiles. Members of the FBI and TWGDAM did this work. While the second NRC committee was dithering about correction factors and creating ever more complex statistical formulae, the technical and interpretive work done by the FBI and TWGDAM provided the forensic and law enforcement community with ways to shape their practices – their work -- in the absence of guidance from the NRC and the academic community. Social structures began to grow, despite the lack of "closure" which the report of the second NRC committee was to provide. In 1994, as NRC2 began to meet, legislation was passed providing for the disbursement of funds for DNA testing laboratories across the country, as long as the laboratories followed the TWGDAM guidelines for quality assurance and proficiency, and the FBI protocols for

producing DNA profiles. While the NRC committees were trying to establish an epistemic foundation for the calculation of random match probabilities, the forensic community itself became educated and proficient in the conducting of DNA profiling.

In a sense then it is not surprising that NRC2's report came out with little fanfare or attention. It reinstated the product-rule (which had been in use in the late 1980s) as the method for calculating random match probabilities. It also provided the rather arbitrary correction factor called *theta* to correct for possible population subdivision. In the meantime, the community of practitioners had quietly worked out standards for procedure and interpretation, and did not find it difficult to incorporate NRC2's guidelines into their own now taken-for-granted forms of practice. In the absence of guidance from the NRC, individuals and groups acting in the different social worlds created social structures which stabilized the technology. It would be a huge error to see the stabilization of DNA profiling as happening within one social world – within academia, within the NRC, within the courtroom or within the forensic community. The action, over time, of individuals and groups in all these social worlds played different roles in stabilizing and standardizing, and ultimately closing the DNA Wars.

For all its mathematical formulas and statistical equations, NRC2 came up with some very simple answers for the two kinds of uncertainty addressed at the outset of this chapter. Recall that they had been asked to address issues of population subdivision, and statistical issues about the representativeness of databases of the American population. As did the first NRC committee, NRC2 assumed that hidden population substructure existed which needed to be compensated for. However, unlike NRC1, they kept the much-used multiplication rule and added the small correction factor called *theta*, whose value varies from

population to population. *Theta* was calculated using the solution to the second kind of uncertainty about the representativeness of samples of convenience. The committee decided that samples of convenience acted as random samples, since it could not be determined how representative these samples were, and random sampling was deemed impractical, expensive and impossible, *theta* was calculated using samples of convenience.

There are three major conclusions resulting from the second NRC committee's process. First, that samples of convenience act as random samples, and from a genetic point of view, are as good as random samples. Second, small probabilities are small probabilities, and any statistical differences attained in using incorrect databases do not have a *forensic* significance. Third, and most importantly, they codified a set of rules and procedures, including a correction factor for population subdivision, which the community of forensic practitioners could easily incorporate into their already existing practices.

As mentioned above, sociologists of science Harry Collins and Karin Knorr Cetina have shown that each science, or indeed each individual laboratory, can have its own taken-for-granted rules for "going on in the right way." The field of DNA profiling lacked these rules, and NRC1 failed to provide rules which connected with the already existing rules in population genetics and statistics. The first NRC committee also failed to connect to the ways in which the forensic community was beginning to stabilize practices around the production of DNA profiles. More than anything else, the report of the second NRC committee on DNA technology was an attempt to reinstate the credibility of the National Research Council *vis a vis* knowledge on DNA profiling. Credibility which had been lost in the debacle of the 1992 report.

As far as the FBI's Bruce Budowle was concerned, NRC2's report served as a vindication that the FBI had been right all along in using the product-rule method to calculate random match probabilities (1997). Perhaps the most important and lasting contribution of NRC2 was to set out acceptable rules, perceived to be grounded in "hard" science (mathematics), for calculating random match probabilities. The second NRC committee legitimated procedures for calculating random matches to the scientists and the government. It set down rules for going on in the "right way." These mathematical rules and slight correction factors were adopted by groups like TWGDAM, the FBI, and prosecutors across the country and once again made part of their everyday routine in calculating random match probabilities.

7) Agency and Structure in the NRC Committees

Recalling Harry Collins' argument that rules of logic are the property of a social group, both NRC Committees were given the task of coming up with rules of logic that would serve not only their private, NRC Committee, but a logic that would serve the wider scientific community. The NRC Committees stand in an interesting position to the theoretical concepts surrounding issues of agency and structure. The committees were convened by a pre-existing social structure with tremendous credibility – the National Academy of Sciences. The National Research Council is also vested with that same credibility, and provided the quasi-independent blue-ribbon committees with the institutional support of a study director and a staff to facilitate meetings and the formation of group consensus in the form of a report. The committees left the starting gate with the credibility and prestige of the National Research Council. It was up to them, as a group, to organize themselves in such a way to state the "state of the art" knowledge on DNA profiling. Thinking theoretically for a moment, the institutional structure, and the pre-existing credibility of the NAS

and the NRC greatly enhanced the probability that a group of very smart people could exercise their individual agency to provide a stamp of approval on a new technology. For this to happen, the group had to establish a social order within the group. They had to constitute themselves as a community before they could go on to produce knowledge. To do this, they had to establish working relationships with people they may or may not have known, so that their intelligence and creativity could be freed to fulfill their mandate.

The first NRC committee to look into problems with DNA profiling failed to achieve this kind of social order within the committee. The cleavages, strong personalities, the in-fighting and lobbying for the Lewontin/Lander ceiling principle versus Caskey's support of the FBI's multiplication rule, all converged to make it impossible for the first NRC committee to achieve the kind of environment within the group which would allow each of the members to exercise their agency, even within that powerful, prestigious, credible structure which was *set up* to enable the agency of individuals. It is possible that because the first NRC committee did not have any population geneticists or statisticians on it, that no solution that it came up with would satisfy scientists. However, the members of NRC1 were as smart, as credible, and as prestigious as those of the second committee. However, the second committee was constituted differently than the first, and amongst themselves, successfully formed a working community.

The first, and most obvious difference between the two committees is that the second committee had population geneticists and statisticians on the committee. In the eyes of the academic community, it had the people who were accredited to produce knowledge in those domains. However, in terms of enabling the knowledge production process, the second NRC committee was put together differently than

NRC1. As study director Eric Fischer indicated, he was absolutely determined that the fighting and cleavages which had rocked NRC1 would not characterize "his" committee. To ensure continuity, some members, such as George Sensabaugh and Haig Kazazian, sat on both committees. The rest of the members were top-notch, blue-ribbon professionals in their domains, but they were not "players" in the DNA Wars. The members of the second NRC committee may or may not have entered the committee with opinions about DNA profiling, but they did not have the high profiles that Eric Lander and Thomas Caskey had. Caskey had already adopted the FBI's protocols in his own laboratory, and had extensive funding for his laboratory from the National Institutes of Justice. Lander was involved with issues surrounding DNA profiling before the 1989 *Castro* case. After appearing as an expert witness in that case, he had been instrumental in introducing DNA profiling as a problem with which the academic community should become concerned. Caskey and Lander were not merely intelligent academicians, they both had an interest in the outcome of the committee. For the most part, members of the first NRC committee respect the "backstage" rule, and are reluctant to talk about the skirmishes and cleavages on the committee. What is clear, however, is that despite the strong structural predisposition to an ideal knowledge creation environment, the first NRC committee could not solve its own internal problems of order and agree upon a solution to the random match probability problem. The ceiling principle was presented by Lander, and appears in Lewontin and Hartl's (1991) *Science* article, although not by that name. Richard Lewontin also made a written submission to the committee, which included his recommendations for solutions to the random match probability problem (Lewontin 1997).

In sum, the National Research Council sets up its committees to maximize the

probability that committee members will be able to mobilize their individual resources (agency) and create social order among themselves conducive to solving knotty scientific problems. This failed to occur with the first NRC committee, and was successful in the second. However, by the time the second NRC committee published its report in 1996, the other social worlds had grown weary of waiting for the blue ribbon panel to tell them what to do, and had formed their own communities and stable forms of practice.

Chapter Six

The FBI: Stabilization, Standardization, and Classification through Community Formation

1) The FBI: Building Community, Knowledge and Social Structure

Whether by mandate or ambition, almost from the time forensic DNA typing was introduced to the criminal justice world, the Federal Bureau of Investigation (FBI) played a central role in the development, stabilization, and standardization of the new forensic technology. By the early 1990s they had become *the* central clearing house for the training of new personnel, research on DNA techniques, quality assurance, standard setting, protocol development, expert witnesses, training of visiting scientists, and information dissemination. In addition, their laboratory had laid the groundwork for a national DNA databank which would allow state and federal agencies to exchange DNA profiles to aid criminal investigations which crossed state lines (Miller 1990, 3).

In retrospect there are two things that stand out as being the most important contributions of the FBI to a history about DNA typing. First, the FBI simplified, validated and standardized the techniques and protocols involved in DNA profiling.[1] Second, they poured tremendous material and human resources into creating a community of practice, which facilitated the quick dissemination of the FBI's protocols across the United States and Canada. Had this community not been created, it is unlikely standardization could have occurred. DNA profiling technology would have disseminated much more slowly, and probably not in a standardized form (Newall

[1] This chapter illustrates many of the key issues discussed by Geof Bowker and Leigh Star in their book *Sorting Things Out: Classification and its Consequences* (1999). The FBI was engaged in exactly a process of classification, stabilization and standardization. This chapter outlines the procedures the FBI used to bring stability and standardization to what was, in 1985, an unknown and extremely complex procedure.

1999, Personal interview; Kahn 1999, Personal interview; Deadman 1999, Personal interview). Because different laboratories would have developed different protocols, used different reagents, and examined different loci, DNA profiles results from different laboratories would not be comparable. This would have made a DNA profile of the same person taken in New York and Los Angeles incomparable. Had this occurred, a national DNA databank would have been an impossibility.

> One of the reasons for the FBI's success in the DNA profiling arena was their total commitment right at the very beginning. They saw it, they recognized it, they dropped everything else and threw 99% of their resources behind the development of forensic DNA analysis. So they did a superb job. Without the FBI and without Bruce Budowle and Jim Mudd and Dwight Adams and Randy Murch and all the guys who were there at the very beginning, it would not have happened in North America, I am absolutely sure of that (Newall 1999, Personal interview).

The FBI's interest in DNA technology for forensic uses actually predates the "discovery" of DNA fingerprinting by Alec Jeffries in 1985. In late 1984 Dr. Carl Merrill of the National Institutes of Health in Bethesda, Maryland gave a presentation on mitochondrial DNA to the FBI Laboratories, and was awarded a contract to work with the agency "long before this became an in vogue methodology" (Budowle 1997, Personal interview). The entrance of private sector companies into the forensic arena also spurred the FBI to quickly develop simplified protocols and standardized procedures for the technique (Hicks 1990).[2] The reader will recall that in 1987 only two private companies were offering DNA typing for forensic purposes: Lifecodes Laboratory of Valhalla, New York, and Cellmark Diagnostics of Germantown, Maryland, a subsidiary of the British pharmaceutical giant, ICI. In 1985 the FBI's FSRTC began to research and evaluate possible forensic applications of the new DNA technology

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[2] Testifying before a Senate Subcommittee in July of 1991, on "Genetic Testing as a Means of Criminal Investigation," Deputy Director of the FBI Laboratories John Hicks stated that the FBI was spurred in its research by developments in the private sector (Hicks, Subcommittee on the

(Hicks 1990, 3). In 1986, FBI scientists travelled to several private DNA testing companies in the United States and the United Kingdom as well as to other institutions engaged in DNA research (Hicks 1990, 65-66). In July of 1987 the FSRTC created a research team dedicated to developing a DNA analysis method which could be used by the FBI Laboratory in forensic cases (Miller 1990, 3).

It is in the creation and the meetings of the Technical Working Group on DNA Methods that the dynamics of agency and structure at the FBI become particularly clear. TWGDAM began as a loosely knit organization of crime laboratory directors who did not know each, who knew nothing about DNA, and who were invited to Quantico to learn about forensic DNA technology, at the expense of the FBI. Over the next decade and a half, they became a tightly knit, highly knowledgeable professional community, whose standards would become entrenched in legislation. The growth of TWGDAM is a story of agency to structure which is apparent in the haggling between individuals over the proper interpretation of DNA profiles, in the actions of a group laying down educational qualifications, proficiency standards, and quality assurance standards for all crime laboratories in the United States, to being legislated as *the* body to oversee the future of DNA profiling. In the beginning, there were individual people, going about their business, enabled by the powerful resources of the FBI, a pre-existing institution. At the end, there was not only a powerful organization, renamed the *Scientific* Working Group on DNA Methods, but their dictums had become part of the formal structure by which we bring order to society – formal legislation.

Beginning in early 1988, the FBI invited members of the crime laboratory community to seminars, training sessions and courses, and formally created the

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Constitution of the Committee on the Judiciary United States Senate 1991, 65).

Technical Working Group on DNA Methods, which would meet at Quantico four times a year. The FBI funding allowed members from small county and state laboratories to participate on an equal footing with the larger, well funded laboratories such as Los Angeles, Illinois and Miami. These large laboratories had the resources to develop their own protocols and methods for obtaining DNA profiles if they had wished to. However, as one director of a small crime laboratory commented, the FBI had already begun the work of validation and standardization, and were publishing the results in peer reviewed journals, and they made this knowledge free for the taking. Although the crime laboratory community had some experience in working with blood, except for a very few individuals, nobody had knowledge of the DNA molecule or the techniques of molecular biology needed to carry out DNA profiling. The FBI's extensive and comprehensive validation process made it hard for any laboratory to justify starting out on their own from square one with new, different and untested materials, and to develop protocols which would have to undergo extensive validation and standardization procedures to be admissible in courts of law.

The FBI provided both the material base and the formal organizational structure which created conditions which favored the formation of social structures which supported the FBI's own DNA typing methods and procedures. The FBI helped to create a community that was so strong that very few, if any, crime laboratories in the United States chose to "go it alone" and develop their own protocols. The community created by the FBI also increased the probability of creating successful knowledge, or accepted belief. Recall that Barnes (1977) argues that knowledge is created by people who are engaged in streams of practical activity. Lave and Wenger (1997) argue that before knowledge can be created or diffused, communities of practice must be formed. TWGDAM was just such a community of practice.

The FBI contributed a dedicated physical space which provided for all the needs of the participants, including meals, sleeping accommodations and recreational needs. This physical structure also helped to increase the probability that knowledge would be created by creating many different social contexts in which a community could be formed. They provided a stable source of material support (funding) throughout the entire period of stabilization, and their support continues to this day.

The FBI did not assume this central role by accident. Their legislated mandate is to investigate violent crimes. Even before Alec Jeffries' discovery in 1985, some forensic people felt that DNA identification could possibly provide a powerful and revolutionary new tool to help fulfill this mandate.[3]

One of the powerful aspects for forensic uses of DNA profiling lies in its unique capacity to type tissues that are not easily typed. A large proportion of the FBI's cases are rape cases, which means that a majority of their samples involve semen. For the protein marker systems available in the mid to late 1980s, semen was not really a good tissue type because it did not provide a lot of information. However, DNA had the potential to provide much more information, making it a better tissue to analyze (Budowle 1999).

All of the validation research was conducted at the FBI's Forensic Science Research and Training Center (FSRTC) in Quantico, Virginia. The Training Center has a mandate to "develop methods that can help resolve or define or characterize

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[3] "The Mission of the FBI is to uphold the law through the investigation of violations of federal criminal law; to protect the United States from foreign intelligence and terrorist activities; to provide leadership and law enforcement assistance to federal, state, local, and international agencies; and to perform these responsibilities in a manner that is responsive to the needs of the public and is faithful to the Constitution of the United States." Responsibility for investigating "violent crimes" came to the FBI relatively late in its history, during the 1970s and 1980s, along with the mandate to investigate civil rights violations, organized crime, counter-terrorism, financial crime and drugs. (Internet document, March 7, 1999, located at http://www.warez.nbase.com/over/over.htm).

evidence found in crime scenes" (Budowle 1997, Personal interview). The FSRTC also has a mandate for training, education, and acting as an information source for the crime laboratory community. The FBI had the financial, physical, human and organizational resources required to create new knowledge quickly. It also had its own in-house publication to disseminate information across the entire forensic laboratory community, the *Crime Laboratory Digest*, published in affiliation with the forensic community's professional association, the American Society of Crime Laboratory Directors (ASCLD).[4]

As early as 1988 the FBI was aware that the new techniques they were developing at the FSRTC would come under intense legal scrutiny when actual casework reached the courts. However, they did not anticipate any major problems with establishing the scientific validity of the technique to the satisfaction of the courts. They felt that they were using techniques which were already "in widespread use by diverse groups of scientists" (Budowle, Deadman, Murch and Baechtel 1988, 18). As one of the first examiners puts it, when they took the new technology into the courtroom:

> We were relying on the fact that we were using standard procedures that had been used by a lot of people, that the RFLP approach was an accepted approach, and all the procedures were standard procedures that everybody and his dog used in the molecular biology laboratory, and so the development of the autorad wasn't really that much of an issue, we thought (Deadman, 1999).

Even outside the crime laboratory community, in the arena where the social worlds of traditional academia, law, forensic science and the criminal justice system mingled, there did not appear to be any concerns about the validity of the new technology. In

[4] In his statement before the Joint Hearing on Forensic DNA Analysis June 13, 1991, John Hicks identifies the *Crime Laboratory Digest* as "an FBI Laboratory publication" (Hicks 1991).

October of 1988 a conference on DNA typing technology was held at the Banbury Center at Cold Spring Harbor Laboratory in New York. Participants included various members of the non-forensic scientific, forensic scientific, and legal communities. While great emphasis was placed on the need for quality control procedures and standardization, participants did not voice any concerns with regard to the soundness of the technology itself (Ballantyne, Sensabaugh and Witkowski 1989). However, the FBI knew that as prosecutors, the burden of proof in the courtroom would fall on their shoulders. In order to meet the anticipated legal scrutiny the FSRTC developed a "validation protocol" with fourteen steps. They felt that if the steps were followed the validation protocol

> should enable research scientists to establish the scientific validity of DNA typing methods as they are applied to the examination of evidentiary materials and directly address the Frye standard ... for admissibility of evidence. The Frye standard will be satisfied if the methodology and validation test results are subjected to scrutiny by the scientific community and found to be generally accepted as reliable to a clear majority of this group (Budowle et al, 1988).

2) A Plan for Research and Validation

In 1985, research at the FBI's Forensic Science Research and Training Center began in earnest. They had first heard about the technique from the British Home Office, which had used the DNA typing procedure developed by Alec Jeffries to solve a case (Budowle 1997, Personal interview). In 1983 Dr. Bruce Budowle had applied for a position at the FSRTC, which was looking for someone to do research in the area of genetic markers. Budowle had a Ph.D. in Human Genetics, and had done post-doctoral research on genetic risk factors that might help predict people at high risk for particular diseases. He was hired, and began research on genetic markers found in blood and semen and other body fluids. His involvement with forensic DNA technology was not plannend. It began in the mid-1980s, as he puts it: "the DNA thing took off, and I was

just there at the right time and started the project, and the rest is history" (Budowle 1997, Personal interview).

At this point, Budowle did not have the skills in molecular biology to conduct DNA typing. To learn them, he travelled to the Salt Lake City, Utah, laboratory of Dr. Raymond White, an renowned expert in cancer research at the Howard Hughes Medical Institute at the University of Utah. Dr. White had been partly responsible for developing the RFLP technology that was so key to DNA profiling, and later characterized one of the most common "workhorse" loci and probes: D2S44 (also called by White YNH24). At White's laboratory Budowle learned to work with DNA and acquired the skills in molecular biology needed for doing DNA profiles (White 1999, Personal interview). He also went to Lifecodes and Cellmark Diagnostics, as well as the facilities at the British Home Office in the United Kingdom (Hicks 1990, 65-66). Armed with this new knowledge, in July of 1987 the FSRTC began to create its own research team headed by Budowle, which took on the task of developing a simple and robust method for creating DNA profiles, one which could be performed by novices and easily shared with crime laboratories across the country (Miller 1990).

The research team was composed of six people with Ph.D.'s and three or four biological science technicians. Other than Budowle, members of the team had to be trained in the required molecular biology techniques. One of the members was Dr. Dwight Adams who transferred to the FSRTC at Quantico in mid-1987. Dr. Hal Deadman was also one of the original researchers. He needed to learn DNA technology. He was concerned primarily with obtaining DNA from hairs, and trying to generate enough DNA from hair samples to go through some kind of typing process. He says his skills with DNA "grew up alongside RFLP," although during the research project he was working mostly with PCR (Deadman 1999, Personal interview).

3) Simplification, Validation and Standardization

The FSRTC's fourteen-step validation protocol, published in the January 1988 edition of the *Crime Laboratory Digest* was included in the initial article introducing DNA analysis to the crime laboratory community. It was written by the key members of the FSRTC's DNA research team: Bruce Budowle, Harold Deadman, Randall Murch and Samuel Baechtel (Budowle et al 1988). This protocol was not a protocol in the ordinarily accepted scientific usage of the word, where it is used to mean something like a recipe, guide, or the exact steps for performing a particular procedure. Instead, the validation protocol was a plan for research. It was believed that successful completion of these steps would make DNA typing hold up to scrutiny in the courtroom.

The first step was to determine how to retrieve DNA from different types of samples. The research protocol spelled out a plan to extract DNA from increasingly difficult media, starting with pristine liquid blood samples, and then moving on to samples more like those found at crime scenes -- dried stains and degraded samples. The exploratory nature of the validation protocol is further evidence that the procedures for conducting DNA typing were very open and non-standardized at this time. Each step of the protocol is itself a research project designed to provide information about how to retrieve DNA from a particular type of sample, not a step in a chemical recipe. In these early days the FSRTC team functioned as a "test kitchen," with the goal of developing their own simple, robust recipes, or protocols.

The team began to examine "fresh body tissues and liquids obtained and stored in a controlled manner" (Budowle et al 1988, 19). In the forensic world, this type of specimen is absolutely ideal. Once they had determined how to extract DNA from ideal specimens, they needed to know if the protocol they had developed was replicable: could other people, in other spaces, obtain the same results? To accomplish this, they

enlisted the members of TWGDAM, who took protocols home, and then returned three months later to discuss their results and negotiate the steps of the protocol. Dr. Bruce Budowle of the FSRTC says this process of making a "robust" protocol was one of the first challenges in working with DNA.

The forensic setting of the research, coupled with the need to use of DNA profiles in the courtroom, provided challenges that were different from those experienced by academic researchers. For example, in the academic world, sensitivity and specificity are not usually problems because there are large, if not unlimited amounts of DNA samples to work from. However, in forensic work, sample sizes are often very tiny and degraded. Therefore, tests must be extremely sensitive and specific because there is often not enough DNA in a sample to run a test again. Tests which would only work on a "bucket of blood ... would not be useful" to the FBI (Budowle 1997, Personal interview). Fortunately, the FBI and TWGDAM determined that DNA profiling could work with less than a bucket of blood.

From a videotaped presentation of one of the FBI's early training meetings, it is clear that there were many challenges which had to be overcome. An example is the use of fluorimeters to measure the quantity of DNA in a sample. On the video, Dr. Samuel Baechtel says that measuring DNA quantity by fluorimeter was unsatisfactory for several reasons. First, it measured the presence of *any* type of DNA in the sample, including, for example, human, dog, bacteria or yeast. Having the fluorimeter indicate a high yield of DNA would sometimes lead the researchers astray, because the actual quantity of *human* DNA in the sample was very small. This meant they had to develop other tests that could discriminate between the different sources of DNA. In the end they settled on a hybridization probe assay that was specific to primates, so that interpretation of results was not affected by the presence of DNA from non-primates.

For Budowle and his team, from the very beginning is was important to simplify and streamline the technique so that the same results were achieved, no matter who conducted the procedure. Most of the users would have little or no experience with DNA or molecular biology techniques. Budowle said that when they first started working with DNA protocols, many of the suggested procedures and reagents used were not present because they were the optimal procedure or had been tested, but because they had "always been used" in a particular laboratory or by a particular researcher. He noted that there is a tendency to keep using what "works," even if it is not necessarily the most efficient method. This is another difference between academic science and the forensic context of the FSRTC: at the FBI they had to be concerned with the efficiency of tests, they could not afford to have difficult, unnecessary reagents or extra steps in a procedure simply because "that is how it has always been done." Much of the work done by Budowle, the FSRTC and later on by TWGDAM consisted of testing each reagent in the protocol to see if it was an essential part of the chemical reaction. They were able to eliminate many of the chemicals which were "difficult to handle," creating a simpler, streamlined, much cheaper procedure.

The elimination of volatile reagents – those that were difficult to handle or mix properly -- decreased the person-to-person variation in the DNA profiling procedure. By eliminating several reagents which varied a lot from batch to batch, because different people measured and mixed them up slightly differently, they reduced another source of variability in the finished product. These steps increased the probability that results produced by different people and in different labs would be comparable. The end result was a robust protocol which varies slightly by how someone uses it, as all protocols do, but does not tend to vary so much that it cannot be used for comparisons of data

(Budowle 1997, Personal interview).[5]

Members of the crime laboratory community have a high degree of admiration for the simplified protocol produced by the FSRTC. One person said that the FBI and TWGDAM had pulled off a "feat of simplification" which aided in disseminating and standardizing the procedure by eliminating many steps and chemicals that could be tricky or problematic to work with. Speaking about one of the simplifications in the process made by the FBI, Dr. Roger Kahn (who was one of the few with molecular biology experience) said:

> Their hybridization solution was absolutely amazing. I think when I first left Yale, the hybridization solution I had must have had 10 or 12 ingredients in it, and the one we used from Budowle's methodology was three or four. There were also different sources of expertise. One fellow that I know you've talked to, Ron Fourney [of Canada's Royal Canadian Mounted Police], knew a great deal about the nylon membranes that you use for Southern Blotting. They weren't as reliable in the beginning as they became. Ron alerted us to the idea that we should look at the thing and see if there are pinholes in it for God's sake -- I mean this is something that can't leak, and a pinhole could be a problem, and you can see it if you look at it and just stare at this sort of paper-like material in advance. And when a better material came along, one of the manufacturers developed something that worked much more consistently and didn't have these problems, and we were able to know about this immediately because of his participation in that group [TWGDAM]. That was great. And we also developed -- some of it was developed and some of it was just given to us by the FBI Lab by Bruce Budowle and his colleagues -- a series of tests that were like critical reagent tests to do before subjecting these materials to real evidence (Kahn 1999, Personal interview).[6]

Once the protocol and techniques had been perfected with ideal and known samples, the next step was to move into analyzing samples characteristic of those

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[5] See Marc Berg (1999) for empirical examples of how difficult it is to get different people all doing something in essentially the same way.

[6] In 1988 Dr. Kahn was one of two members of Florida's Dade Country DNA Lab. He had a PhD in Human Genetics from Yale. The members from the Dade County lab were in the forefront of local laboratories doing DNA analysis work. Dr. Kahn was one of the few members of the crime laboratory community who knew a lot about molecular biology at the beginning. However, he knew very little about criminalistics, or forensics, and learned as he went along.

found in the forensic arena. One of the big questions was whether DNA obtained from liquid, pristine samples would be the same as the DNA obtained from dried samples. By doing the tests on the same DNA, once in liquid form and then in dried form, researchers were able to establish that the DNA profiles were identical, no matter what the source of the stain was (Adams 1999, Personal interview). This also held true for degraded samples, which are commonly found in forensic evidence. This is another difference between the academic analysis of DNA and forensic analysis -- forensic DNA evidence has often been stored in less than ideal conditions for a long time, whereas laboratories in universities, or in the private sector, have high-tech freezers that keep samples in pristine condition for years. Next, they researchers wanted to establish whether DNA from two different sources within the same person, i.e., blood and semen, would result in identical profiles. This would be important if, for example, evidence from a rape case was in the form of a dried semen stain, and the sample taken from the accused was in the form of blood or saliva.

The members of the research team stressed over and over how at that time, very little was known about DNA, especially in the forensic arena. For example, Dr. Hal Deadman was working in hair and fibre analysis, trying to find out how much DNA could be retrieved from human hairs. Then, nobody knew that hair has almost no DNA in it, unless it has a root attached to it. He worked diligently for several years -- and says his research efforts were "frustrating," because no matter what he did, he was unable to retrieve DNA from hairs shed at crime scenes. As they learned, this is because most hairs left at crime scenes are hairs that fall out naturally, leaving the root behind in the scalp, therefore they have very little nuclear DNA (Deadman 1999, Personal interview).

4) Measurement Error and Matching Windows

In the January, 1991 issue of the *American Journal of Human Genetics*, an article appeared authored by the researchers from the FBI's FSRTC, and Ron Fourney and John Waye, from the RCMP Central Forensic Laboratory in Ottawa, Canada (Budowle et al 1991). This article was the first publication that characterized the VNTR alleles analyzed by the FBI. It detailed their measurement procedures, how they derived their measurement error and match window. It also covered problems they had encountered with the technology, such as gel resolution, and provided a matching rule for use in the courtrooms. This article has become known in the field as the FBI's "Fixed Bin" article, and is widely and routinely cited by both prosecution and defense counsel and expert witnesses in courtrooms.

One of the most important contributions of this article is the FBI's discussion of how it determined its measurement error. Many things can affect the measurement of a DNA fragment length (Thompson and Ford 1991). These include the width of the band, the resolution obtained on the electrophoresis gel, and the mobility of the actual fragments themselves in the gel. To take into account this imprecision in measurement, the FBI researchers did studies to calculate a standard error of measurement. Their estimate of measurement error was based on 111 samples comparing actual forensic sexual assault victim's blood to vaginal epithelial cells. This analysis resulted in a total of 200 band comparisons. The researchers found that

> [E]ighty-three percent of the band comparisons were within [plus or minus] 1.0% (i.e., the categories 0-0.0200) and that 17.0% of comparisons varied more than [plus or minus] 1.0%. No comparison exceeded the 0.0451-0.0500 category. The data suggest that, in a forensic context, size measurements of some DNA fragments from the same source can vary as much as [plus or minus] 2.5% (or approximately a total of 5%). Thus, comparisons varying as much as [plus or minus] 2.5% may constitute an inclusion, and, beyond [plus or minus] 2.5%, interpretations are deemed inconclusive or an exclusion

(Budowle et al 1991, 842).

It is interesting to note that of all the laboratories that published measurement error and match window criteria, only the FBI determined their measurement error and consequent match window on *forensic* samples. The FBI used samples from actual sexual assault cases to determine their 5% match window. This is important because forensic samples are usually at least slightly degraded, and will tend to migrate further down the gel than bands in a lane without degraded DNA (Budowle et al 1991, 844).

One of the problems with matching bands in different lanes, even when you are dealing with two samples whose source is known, is that if one of the samples is degraded, that degraded sample will run faster on the gel than the less degraded sample. Because of the nature of blood and its treatment during the "chain of custody," sometimes the known sample is more degraded than the crime scene sample. In studies with known samples from blood and vaginal epithelial cells from rape cases, the FBI determined that even with band shifting, there was never a case where two bands from the same source were separated by a size difference of more than 5% (Deadman 1999, Personal interview).

Commercial laboratories like Lifecodes used pristine DNA to calculate their estimates of measurement error, and because of this, had smaller match windows. The size of the match window, which is determined by the measurement error, is called the "matching rule." David Kaye notes that variability between laboratories in the size of measurement error leads to potentially confusing discourse in the courtroom: "The prosecution says to the defendant, 'under our match rule, you match'. The defendant replies, 'That's your rule. Under a different rule, I don't match'" (Kaye 1993, 113).

5) Structuring Knowledge: Population Statistics and Allele Frequency Distributions

Once the FBI determined that they could obtain stable profiles from different laboratories and practitioners, utilizing liquid, dried stains or degraded stains, the researchers turned their attention to developing population distribution data for each allele, divided by racial group. This seemed simple and unproblematic at the time. For their Caucasian sample, the FBI used 225 of their own agents! However, the proper way to collect allele frequencies and to calculate match probabilities was to become the subject of very heated controversy, and one of the major obstacles to establishing the reliability with which DNA profiles could be believed to "individuate," or uniquely identify individuals.

In January of 1991, the FBI published results from their first attempt at establishing population distributions of allele frequencies in *Crime Laboratory Digest.* These allele frequency distributions for the purposes of calculating match probabilities were not the first such distributions -- Lifecodes Corporation made the first distributions -- but these were the first the FBI constructed themselves. The multiple authorship of the article (it has 24 authors, from different police departments and crime laboratories across the United States) indicate that the FBI gathered data for their racial distributions of allele frequencies from a wide variety of sources. Budowle himself said that he had gathered the population frequency data by "getting on the phone" and using an "amazing device called the fax machine" (Budowle 1997, Personal Interview). The size standards for the lengths of alleles came from Lifecodes Corporation, and the probes for the loci D2S44, D14S13 and D10S28 came from Raymond White at the Howard Hughes Medical Institute in Salt Lake City, Utah and the Promega Corporation of Madison, Wisconsin. The probes for D17S79, D1S7 and D4S139 were purchased

from Lifecodes, Cellmark Diagnostics (Germantown, Maryland) and Genelex Corporation of Seatlle, Washington, respectively (Budowle et al 1991b, 10).

The selection of data for allele frequency distributions was to become a source of heated debate in several contexts. As pointed out in Chapter Three, in the *Yee* case, the FBI's use of 225 of their own agents to construct the Caucasian database became an issue. The FBI analyzed the data from these agents twice, and could not get the two sets of profiles to agree. Apparently they lost the first set of data, re-did the analysis, found the original data, and in short, they were unable to identify their own agents as themselves (Deadman 1999, Personal interview).[7]

Using samples of convenience as representative of the U.S. population as a whole was also to become a subject of concern for the two National Academy of Science committees convened to investigate the status of DNA profiling (NRC 1994, NRC 1996). Both the FBI and private corporations used samples of convenience – nobody attempted to collect any kind of random samples.

Once the issue of whether two samples match had been determined, the problem was to decide on how to calculate the probability that a person chosen at random could share the same DNA profile as in the match. In their peer-reviewed "Fixed Bin" article (Budowle et al 1991a), the FSRTC researchers attempted to provide a solution to this problem, or a means "to assign suitable weight to the putative match" in a courtroom. They argued that this method must be conservative (biased in favor of the defendant), and must take into account the "interpretive ambiguity inherent to the technology." These inherent interpretive ambiguities include technical limitations and

[7] See transcripts on file at courthouse in Toledo, Ohio for United States v. John Ray Bonds, Mark Verdi and Steven Wayne Yee. Page citations are to 12 F. 3d 540; 1993 U.S. ApLexis 32574; 1994 Fed Ap0085P.

variability in conducting the procedure and producing autorads, and "limited sample population data, possible subpopulation differences, and potential sampling error" (Budowle et al 1991a, 841).

Because of their inability to measure small differences in fragment lengths, and the inevitable measurement error, the FBI decided to treat allele fragment lengths as quasi-continuous variables, and "bin," or categorize the DNA fragments by arbitrarily chosen lengths. The size categories chosen for the bins were taken from the Lifecodes' sizing ladder. Using the binning procedure, the researchers developed allele frequency distributions for four populations. The Caucasian sample was constructed from the FBI sample of its own agents, and samples from J. Bashinski of the California Department of Justice, Roger Kahn of the Metro-Dade Police Department in Miami, Florida, and Thomas Caskey of Baylor College in Houston, Texas (Budowle et al 1991b, 10). The Southeastern Hispanics were drawn from Kahn's Florida data; and the Southwestern Hispanics were drawn from Caskey's Texas sample. The reason for dividing Hispanics geographically was that the FBI also felt that Hispanics represented more of a "geo-political" grouping than a "racial/ethnic category" (Budowle et al 1991b, 10). To make frequency calculations for the general category of "Hispanics," the FBI made a composite of the data, choosing the larger bin frequency (i.e., more conservative) from both Hispanic distributions.

The Black and Caucasian databases were made from a variety of sources, pooled into one for each race (Budowle et al 1991b, 10). To construct these distributions, they used data generated by their own laboratory, by the laboratory of Dr. Raymond White of the Howard Hughes Medical Institute at the University of Utah in Salt Lake City, Lifecodes Corporation, Cellmark Diagnostics, and Y. Nakamura, who

also worked with Raymond White.[8] In constructing the distributions the alleles were binned so that no single bin had a frequency of less than five.

It was clear to the FBI that the distributions were different for the different ethnic groups. However, they felt that binning the alleles into discrete size categories was a conservative measure, which decreased the likelihood of two DNA profiles being the same no matter what ethnic group was studied:

> It is evident that the data for the different sample populations are not the same for each of the VNTR loci. However, it is obvious that all VNTR loci for the four general population samples are highly polymorphic [vary highly from person to person]. Thus, the frequency of occurrence of any array of DNA profiles would be an unlikely event (even with the conservative statistical approach of binning) in any population sampled (Budowle et al 1991b, 10).

With this logic in mind, the FBI felt assured that they had adequately accounted for differences between ethnic groups, and the possibility of population-substructure. Time would show how wrong that assumption was, once the allele distributions were brought into the bright lights of the courtroom and the harsh criticism of the academic world.

6) Continuing the Validation Protocol -- Investigating Forensic "Contamination"

Forensic evidence is often mixed with a variety of other things that may or may not have contaminated it. An important part of the FBI's validation protocol was to subject DNA stains prepared from liquid specimens to a variety of substances, called substrates, commonly encountered in the forensic arena. They wanted to see if the presence of these substances affected the DNA profile. Up to this point in the validation process there had been no analysis of "actual" forensic evidence. Dr. Dwight Adams, now Chief of the Scientific Analysis Section of the FBI, was the researcher

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[8] Data for characterization of the D14S13 locus is cited as being found in Nakamura, Y., Culver M., Gill J., O'Connell P., Leppert M., Lathrop G., Lalouel J.-M., et al. "Isolation and mapping of a polymorphic DNA sequence pMLJ14 on chromosome 14 (D14S13). *Nucleic Acids Research*, 16:381, 1988.

responsible for conducting the validation studies with contaminants and "environmental insults" (Adams 1999, Personal interview). He looked at DNA profiles obtained from stains prepared from actual forensic cases which were assumed to have been subjected to a wide variety of substances and extremes of climate. Here, the protocol states that it is not possible to address experimentally the possible effects of all the different contaminants and sub-strata (which range from floor tile to the dye in denim blue jeans) to which evidentiary DNA is subjected. So, the researchers focused on comparing stains made from victim's liquid blood samples versus victim's blood deposited on the usual crime scene substrata. Dr. Samuel Baechtel remarked to one audience that denim blue jeans are a "serologist's nightmare." Once this was completed, tests were done to see if the techniques developed on human DNA specimens could also detect DNA profiles from non-human sources.

At this point it was felt that the profiling procedures had been tested enough to move to Washington, D.C. and begin work with actual cases. Concurrently, members of the research team began to publish the results of the previous experimental validation studies in peer-reviewed journals and make presentations at scientific meetings. The FSRTC felt this external exposure would allow the relevant scientific community to review the methodology and validation data and "pass muster" on it. The FBI felt that their validation studies, public presentations and peer reviewed publications would be more than ample evidence to demonstrate "general acceptance" of their techniques in the scientific community. Publications in peer-reviewed journals, presentations at scientific conferences, workshops and laboratory courses, were considered to be "excellent forums for the exchange of ideas and opinions regarding the merit of DNA typing procedures. During such colloquia the relevant community will be in a position to decide if the validation data is exact and worthy of acceptance, or if

the supporting data is flawed and should be rejected" (Budowle et al 1991b, 19-20). However, "general acceptance" and scientific validity were not so easily or passively achieved. The seemingly simple extension of methods "in widespread use" to the task of forensic identification created a controversy which impacted defendants, forensic practitioners, the courtrooms, lawyers and academic scientists.

The last steps of the validation protocol involved training and dissemination of the new techniques to personnel in crime laboratories across the country. The validation protocols had been conducted at the FSRTC at Quantico, Virginia, and in the laboratories of the TWGDAM members. Moving to actual casework meant transferring the knowledge from Quantico to the FBI's active casework laboratory in Washington, D.C. Formal training sessions for scientists in state and local forensic science laboratories were offered. It was felt that offering formal training would not only increase the numbers of practitioners able to do DNA analysis, but the widespread diffusion of the techniques would also demonstrate that they were technologically stable (Budowle et al 1991b, 19).

The validation protocol discussed here did not proceed in a linear fashion. The FBI began to offer seminars in the new technology to members of the crime laboratory community in 1988, the same year in which they were training their own case-work personnel and opening the FBI DNA Analysis Unit and sending their examiners out on the road to act as expert witnesses. However, it is clear that the FBI did not move forward with the presentation of their own protocol to the crime laboratory community until they had simplified it, tested it and validated it in a wide variety of ways. The hard work done by members of the FSRTC research team meant that once the technique was presented to the crime laboratory community there was a huge incentive for other crime laboratories to adopt the FBI's protocols, since they had been so thoroughly

tested and validated within the Bureau. If a laboratory did not adopt some version or adaptation of the FBI's protocol, each crime laboratory was faced with the necessity of doing all of its own testing and validation research. This was a lengthy and expensive process – and basically involved replicating all the work, publications and presentations which the FBI had already done. The FBI gave away their technology to any laboratory willing to use it. The community was delighted with the gift, given their low level of knowledge and expertise in the DNA area. Dr. Pamela Newall say its was "pure luxury" to be in the position of receiving the FBI's protocols and benefiting from their experience in the courtroom.

> TWGDAM had no power to force anybody to do anything, it had no legislative or administrative power, everything that you did, you did voluntarily. So I could go back to my lab and say, this is what I've learned, and this is what we're going to do in my lab now. And in fact, there were things in our protocols that didn't exist in anybody else's protocols, and those of us who came after the FBI, had the pure luxury of learning from them. The guy who goes first puts his neck on the line. So they had their protocol etched in stone, subject to extensive disclosure, extensive discovery, extensive criticism, and we sat on the sidelines and watched them survive or not survive and learned from how they had survived, and altered our methods and protocols accordingly. I mean, it was just pure luxury to be in that position. Pure luxury (Newall 1999, Personal interview).

The FBI's interest in TWGDAM was to increase the probability of standardization among crime laboratories.[9] Standardization would mean comparability of results, which would make the possibility of forming a national DNA databank of convicted felons very real. This had been a goal of the FBI from the outset, but it depended on every crime laboratory in the United States, or every contributing crime laboratory, using the same protocol to produce DNA profiles. By inviting people to attend meetings, by offering for free a tested a reliable technique, by offering their researchers to testify as expert

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[9] Again see Timmermans and Berg (1997) and Berg (1997) for examples of the difficulties involved in getting many people in different places to "do things the same way."

witnesses, the researchers at the FSRTC almost assured that DNA profiling in the United States would be done in a standard fashion. This was a huge and important accomplishment.

It is good to remember, however, that it was an accomplishment made possible only through the virtually unlimited material resources of the FBI. Intellectual resources were not in great supply when DNA profiling came on the scene – but the individual learned. They learned by forming a community of practice, and establishing shared norms of practice (Lave and Wenger 1997). First, Budowle travelled to Dr. Ray White's lab in Salt Lake City. The he and a few members of his team went to the private laboratories, and to the United Kingdom (to the Home Office). The story, therefore, is not one of pure talent, skill or intellect driving an accomplishment – it involves the power and hegemony of the FBI. There are crime laboratories in the United States that are large enough and have enough resources to "go it alone" – Miami, Virginia, Illinois, Los Angeles. However, at that time even these large laboratories lacked the intellectual expertise to produce DNA profiles, and the FBI offered the training for free. They flew people to Quantico, housed them, fed them, trained them -- even entertained them. While providing an environment for leisure activities may seem like a non-essential thing, it is very probable that it is in the times of relaxation that people "let their hair down" and bonded with each other, sharing their experiences, frustrations, insecurities and hopes for the technology.

The FBI also had a "visiting scientist" program in which people from other crime laboratories came for four months at a time to learn the techniques "hands on." When they arrived, people did not know what the structure of a DNA molecule was. In a very short period of time they learned to extract DNA from a variety of samples, and manipulate it to form DNA profiles.

The benefits of the FBI's program and TWGDAM are clear for smaller jurisdictions, who had incentive to participate because they lacked both the intellectual capital, and the material resources to do what the FBI had done. However, one potential benefit of participating for the larger laboratories was that these large jurisdictions would be the very jurisdictions that would have higher crime rates, and thus have a larger interest in storing and trading DNA profile information with the FBI and other databanks across the country.

7) Knowledge and Technology Transfer, and Community Building

For the FBI's dream of a national DNA databank to become a reality, each and every state and local laboratory contributing DNA profiles would have to use the same restriction enzymes, examine the same set of loci, and follow basically the same protocols. In other words, not only did members of crime laboratories have to learn the techniques, but the techniques also had to be standardized and practiced in an almost identical way in each laboratory that wanted to participate in the national DNA databank. To this end a major goal of the FSRTC was transferring the new technology, including the FBI's own newly developed, simplified protocols, to as many state and local crime personnel as possible.

Early in 1988, the FBI began the process of building a community of practitioners who were well informed and trained in the methods necessary for successful DNA typing. From May 31 to June 2 the FBI Laboratory Division in Quantico, Virginia hosted a seminar called "DNA Technology in Forensic Science" (Murch 1988, 79). The principle historical importance of this first seminar may be that it brought together for the first time many members of what was to become an international "core set" of players on the "pro" DNA side in the "DNA Wars" (Collins

1985, 142-147).[10] Approximately 115 forensic scientists, molecular biologists, human geneticists and others attended from the United States, Canada and Great Britain. The attendees represented 24 forensic laboratories, 6 universities, 11 private companies, two national laboratories and two district attorney's offices (Murch 1988, 79). People who were to play prominent roles in the DNA Wars and who were present at this conference include Bruce Budowle of the FSRTC; Samuel Baechtel, FBI Laboratory; Ian W. Evett, of the Home Office Central Research Establishment, Aldermaston, United Kingdom; Ivan Balazs of Lifecodes Corporation, Valhalla, New York; and Robin Cotton of Cellmark Diagnostics in Germantown, Maryland (Murch 1988).

Another technique of knowledge and technology transfer utilized by the FBI was face-to-face in-house training. In April of 1988, prior to opening its own unit, the FBI invited directors of crime laboratories across the United States to nominate individuals to come to the FSRTC for a four month long training program in Quantico, Virginia, called the Visiting Scientists Training Program. This program began in July of 1988 (Hicks 1990, 69). By March of 1989 there were sixteen crime laboratories scheduled for participation. The four month program was designed to "provide the technical resources to address the validity and reliability issues associated with DNA testing as quickly as possible" (Budowle et al 1988, 1). The need for face-to-face, hands-on training is not unusual, as new scientific knowledge is rarely successfully transmitted and diffused without personal contact among practitioners (Collins 1974; Collins 1985).

An additional component of the FBI's face-to-face technology transfer was two

[10] "Core set" is a term coined by Harry Collins (1985, 142-147) to refer to the participants in a scientific controversy. The members may have no sense of group identity or of belonging to a group -- they are bound together only by their "close, if differing, interests in the controversy's outcome" (142). Collins argues that core sets "certify new knowledge" and that from the outside they simply appear to be the group of scientists working on whatever issue is under investigation.

technical training courses. The first was held in January of 1989, with about 40 people from State and local crime labs in attendance. The idea was that through in-person, face-to-face contact, the FBI's DNA protocols could be transferred to the crime laboratory community, and they found that after in-person training, the technique did transfer very easily. Participants in the technical training course were able to successfully perform the tests and get the proper results (Hicks 1990). The second technical training course was scheduled for June, 1989.

While all these activities were important, the single most important act that the FBI did to standardize and stabilize DNA typing was to create and fund the Technical Working Group on DNA Methods (TWGDAM) in November of 1988. This was to become a very important professional group and it is hard to overstate the importance of the role TWGDAM played in standardizing and disseminating DNA typing technology. The group was formed so that standards of protocol, technique, organization, interpretation and training could be worked out by the relevant community, not imposed on them "from above." Forming this group meant that the potential for common standards would exist for the new technology from the very beginning. An unintended effect of bringing this group together so frequently was the creation of a new professional group that became self-policing. By funding the group and bringing it together at least four times a year, the FBI created a cohesive community of practice from the United States and Canada where none had existed before. It was during these sessions, by sharing their experiences, that the group decided on correct procedures for protocols, on quality assurance guidelines, on how to interpret autorads, and the minimum material and intellectual requirements to set up an adequate DNA typing laboratory. Through group interaction, and as the Technical Working Group on DNA Methods became a solid community, they created formal and

informal social structures which supported the fledgling technology.

In October of 1988 the FBI opened its own DNA analysis unit in Washington, D.C., and in December of that year it began taking cases from State and local crime laboratories.[11] Special Agent Dr. Dwight Adams had been working on the research team at the FSRTC since 1987, and once that research was "finished," he was the one responsible for transferring the methods and protocols to the Washington, D.C. Laboratory, which opened in October of 1988 (Adams 1999, Personal interview).

Opening its own unit would seem to indicate that the FBI felt that it had met all the requirements of its validation protocol and was now ready to back the procedure up in the courts. John W. Hicks, Deputy Assistant Director of the FBI Laboratories, referred to the validation procedure as a "shake-down process," and testified that the validation process was completed in December of 1988, at which time the FBI began casework analysis (Hicks 1990, 67). At that time, only Maryland, Virginia, North Carolina and New York had set up their own DNA analysis laboratories.

Dr. Dwight Adams and Dr. Lawrence Presley were the first Examiners to start working cases in the new DNA Analysis Unit of the FBI Laboratory in Washington, D.C. The first case came to trial in April of 1989 in Hawaii, on Maui. Dr. Hal Deadman came on board as an Examiner and started working cases in March of 1989. During the first year Dwight Adams did most of the expert witnessing required. During 1989, Hal Deadman testified 10 or 11 times, and the next year about 35 times. Adams, Presley and Deadman spent much of their time on the road. Their expertise for testifying came from "growing up with the procedure for 20 months" (Deadman 1999, Personal

[11]At the time of the first Joint Senate and House hearing on March 15, 1989 there were about 130 cases in the laboratory (Hicks 1990). By the time of the second Senate hearing on June 13, 1991, the FBI had completed testing on over 3,000 individual cases (Hicks 1992, 16),

interview) and some additional training. For example, a professor came in from the University of Virginia to teach the examiners a credit course. Deadman also took a course at the NIH in molecular biology, and the FBI began teaching a four week course to 40 students in 1988. Deadman was involved in instruction and lecturing, but also attended the course. The training of these first three examiners was informal and hands-on. Current examiners undergo a much more formalized and extensive, year long training process. To this point Cellmark and Lifecodes had been the only ones presenting DNA evidence in courts. By December of 1990 FBI examiners had testified in about 60 admissibility hearings for DNA evidence.

8) The Technical Working Group on DNA Methods

At the first meeting of TWGDAM, the members of FSRTC presented the TWGDAM invitees with their newly simplified protocol, and asked them to take the protocol back to their own labs and report back to the group at the next TWGDAM meeting on what had and had not worked (Newall 1999, Personal interview). TWGDAM met four times per year in the first few years. Had the FBI *not* brought this group together, it is possible that standardization in protocol, technique, interpretation, quality assurance and proficiency might never have occurred, or occurred much more slowly. When asked what the situation would have been without TWGDAM, Dr. Newall commented:

> Well, it [disseminating DNA typing] would have been far, far slower, we would be far far behind where we are now if there hadn't been a TWGDAM. And, I think that Lifecodes got us into trouble with *Castro* in the very beginning, and it took years to get beyond that (Newall 1999, Personal interview).

Such an organization had never been tried before in the forensic community.[12] There had never been such a "coming together" of quality assurance preparations and methodology. Prior to TWGDAM, the transmission of forensic technology was based on informal "collegial relationships between certain people" (Kahn 1999, Personal interview) and participation in some regional organizations. TWGDAM became the first formal organization by which personnel working with DNA analysis techniques in state and local crime laboratories could exchange technical information on DNA testing. Over the years, the members formed strong personal bonds of trust and friendship with each other which to this day support them in using and interpreting DNA technology in the forensic arena (Newall 1999, Personal interview; Kahn 1999, Personal interview).

From a theoretical point of view, TWGDAM's success was in creating social structures that guided the interpretation and supported the practice of DNA profiling across the United States and Canada. The FBI supplied the economic and physical infrastructures necessary to create a community of interested professional practitioners who were normally widely separated by geography and institutional affiliation. Dr. Roger Kahn states that

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[12] A latent effect of the success of TWGDAM, now called SWGDAM (Scientific Working Group on DNA Methods) has been the formation of this type of practitioner-based scientific working group in other areas of forensics. This type of organization has changed the face of forensics. The importance of DNA profiling

> "is that DNA changed the way crime laboratories operated in several ways. And I think TWGDAM was the vehicle for the change. I mean, the emphasis on quality assurance, was strictly an outgrowth of DNA. And the explosion in emphasis on accreditation and maybe even proficiency testing, all this probably happened more quickly because of DNA's influence, kind of pushed along by the TWGDAM group. The proliferation of Technical Working Groups is evidence of that. I mean, every discipline has one now, there's lot of them. There's TWGDRUG and TWGFAST and TWGMAT and TWGBOMB, or something -- there's an explosive -- I can't remember what it's called, I kind of wish it was called TWGBOMB. And now these SWG's for example, there's actually more than one organizing entity these organizations, these working groups" (Kahn 1999, Personal interview).

> [I]t always is interesting to be at Quantico because there is so little distraction, and you're really there for a purpose. You're treated very well, your needs are taken care of. I mean, you can walk to your room, the meals are there, and it's good, it's comfortable in there, the classrooms are appropriate, and the group wasn't very large as I recall, so there was plenty of opportunity to say what you needed to say and to hear what you needed to hear. So we've -- I think, even from the very beginning there was a desire on the part of the FBI to establish standardized methods. And I had assumed that that would be what we would try to do without really talking to anyone, I just saw that there was so much value in trying to do that, so I was glad. When I got there I saw that we had common plans (Kahn 1999, Personal interview).

The virtually unlimited resources of the FBI played a huge part in creating the TWGDAM community. Kahn notes

> The fact that it was free is not a joke. They made each lab equal by doing that. Everyone who participated could get there, to *every* meeting. And that's not true at any other meeting. If you want, if you ask about the American Academy of Forensic Sciences for example, it meets once a year, and maybe you get to go, and maybe you don't. And maybe someone else goes. It depends, who knows what the politics are (Kahn 1999, Personal interview).

9) Forming Normative Structures for the Interpretation of DNA Profiles: "The Good, the Bad, and The Ugly"

TWGDAM was a very important resource for practitioners in working out the details of exactly how to do and interpret DNA typing. One of the events held routinely in early TWGDAM meetings was called "the good, the bad and the ugly." The FBI would put up their worst and most difficult autorads for the group to look at. People would try to figure out what the problem was, where in the process it had occurred, and what the correct interpretation of the autorad was.

> They put up autorads from cases that were difficult, that were ugly autorads, where there was partial digestion, where there was incomplete removal of the bands from the previous hybridization in the most recent hybridization, and ... you had to have techniques and ways of assessing faint bands in the background and trying to establish whether or not that was partial digestion, or whether it was incomplete scrubbing -- incomplete removal. We'd all look at them, we'd all discuss them, we'd all learn from them, we'd all contribute. So we'd all discuss them, and we'd all put in our two cents, and maybe we'd agree with what

> the FBI said, and maybe we wouldn't agree with what the FBI would say, and maybe we would sort of add additional points to their point of view, so we all learned from it, including them (Newall1999).

In the first few meetings, members of the group had very little experience, and they certainly had no common standards by which to judge or interpret autorads. Through "show and tell," events such as "the good, the bad and the ugly" helped to teach the members of the crime laboratory community how to diagnose problems with autorads. In these early meetings in 1988 and 1989, people from different labs were very likely to interpret the same autorad in different ways and argue about the correct interpretation.

> Yes, in the very beginning that's correct, it wasn't always the case [that people interpreted autorads in the same way]. ... You need a lot of training to interpret them [autorads]. A lot of these skills were hammered out in TWGDAM meetings at the very beginning (Newall 1999, Personal interview).

In the early meetings, the FSRTC members, the two RCMP members (John Waye and Ron Fourney), and Dade County's Roger Kahn[13] were the only ones with the molecular biology expertise to "correctly" interpret the autorads. As the group hammered things out, it became apparent that "correct" interpretation was dependent on ready with the FBI's validation studies, as well as the knowledge and ability to recognize all of the things that can go wrong with an autorad. Knowledge had to be transmitted about how one decides whether something is truly a band, and not an artefact of the electrophoresis process. Members had to learn what bandshifting looked like -- indeed they had to learn to look for it, to understand why it occurred, and how to determine if it

[13] Dr. Kahn took great pains to point out that while he did indeed have training in molecular biology, he was totally new to the world of forensics, so he had to learn different things than the rest of the group (Personal interview, June 29, 1999).

had occurred on a given autorad. Size ladders are routinely run every few lanes on an autorad, and members had to become familiar with how those size ladders behaved across a gel, and from gel to gel, and under different laboratory conditions (such as temperature of the lab, humidity in the lab). Members also had to learn what things were not important, and thus would not affect their final reports. Interpretation was complex, and required attention to a myriad of details – interpretation of autorads required:

> That you know what the monomorphic results mean and how to interpret them. That you know how to look for and either include or exclude things like bandshifting, and why that happens, and how you would interpret that finding. That you can read the gender results, which are also an internal monomorphic control. That you understand how the ladder behaves, which ladder you're using, whether there are any pitfalls in interpretation in looking at the ladder and sizing that ladder. How often the ladder needs to be loaded on the analytical gel.
>
> Interpretation is complex. Interpretation includes not only interpreting from a scientific point of view, but being able to interpret for and support that interpretation in a court of law for a lay population. So being able to communicate the significant differences from one autorad to another, from one lane to another, and the differences that may exist that aren't going to matter to the final outcome of your report. Or if they do differ, then to what extent do they differ, do they influence it?
>
> At the beginning, at the very beginning when we were scared stiff and inching along, we had every autorad sized by a second person. So we not only had re-interpretation, we went right back to square one, right back to the raw autorad and had the second person re-size that film (Newall 1999, Personal interview).

By 1990, after about eight group meetings, TWGDAM members had developed normative standards about issues of interpretation. Testing protocols in their home laboratories and events such as "The Good, the Bad, and the Ugly" allowed the group to replicate both the physical and interpretive technologies involved in DNA profiling. The reader will recall from Chapter One that replication is "the set of technologies which transforms what counts as belief into what counts as knowledge" (Shapin and

Schaffer 1985, 225). Knowledge about DNA profiling was created through the constant replication and group interaction.

Once interpretation of DNA profiles was less problematic, TWGDAM shifted its attention to the important issues of quality assurance and proficiency testing. From the outset, these issues had been a huge concern of the members of TWGDAM, as well as the FBI and people outside the forensic world. When DNA profiling had been successfully attacked in court, most notably in the *Castro* case, it was often because the testing laboratory had failed to competently carry out the procedures correctly. This meant that as practitioners, who would be witnessing for the validity of their interpretations, it was important to assure that quality remained consistent within a laboratory, and that there be some way to determine if someone was proficient in doing DNA profiling. These issues were so important that during its first meeting the group formed a subcommittee to create Quality Assurance Guidelines for laboratories conducting RFLP analysis.

10) Setting Quality Assurance Guidelines

The first set of TWGDAM guidelines for a Quality Assurance Program for DNA RFLP Analysis were published as the feature article in the April-July 1989 issue of *Crime Laboratory Digest,* and were updated in April of 1991. These first guidelines include suggestions and directions for *every* aspect of setting up a DNA laboratory -- from space, to funding, to personnel, education, reagents, forms, chain of custody, etc. The subjects are headed "Planning and Organization," "Authority and Accountability," and "Job Descriptions for Personnel." The published "Guidelines" include suggestions for how laboratories should handle documentation, materials and equipment required, validation of analytical procedures, in-house validation of established procedures, evidence handling procedures, internal controls and standards, data analysis and

reporting, proficiency testing, audits and safety procedures.

The interpretive flexibility which time has shown to be intrinsic to DNA analysis was obvious to these practitioners eager to establish standards, although it is never phrased in terms of "interpretive flexibility" or "subjectivity." For example, the guidelines for Quality Assurance recommend structuring a laboratory with a built-in hierarchy of credibility as to who can 'see', who can 'interpret,' who can 'report' and who can appear as a 'witness' (or speak in a legal setting about the interpretation of results). The first set of guidelines identify specifically who in a laboratory is, but more importantly, who is *not* qualified to interpret the results of DNA analysis:

> Technicians involved in performing analytical techniques related to DNA analysis should have a minimum of a BS/BA degree and receive on-the-job training by a qualified analyst. It is understood that *technicians will not have the responsibility for the interpretation of results, preparation of reports, or providing testimony concerning such* (TWGDAM 1989, 45; emphasis added).

Many small laboratories do not have both technicians and scientists -- there is just one person, perhaps with a PhD, that fulfills both functions. However, for larger labs, the lab is divided hierarchically into technicians, who may or may not hold a PhD, and who do the actual labour of running the DNA profiles, and scientists (at the FBI they were called examiners), who do the interpretation of the autorads, write the reports, and perform as expert witnesses in court.[14]

> There never was any question about who was going to look at the autorad and write the report -- the tech or the scientist. I mean, that

[14] Dr. Harold Deadman, retired FBI Special Agent reported that at the FBI, cases were worked by an examiner and a technician. The technician actually does all the hands on lab work. They specialize in taking the samples, and going through all the different steps to generate the autorads, or DNA profiles. They are not involved in the interpretation. They do nothing but "follow the protocol." He said that if there were any reason to want to change or modify the profile, it would have to be okayed by an examiner. Changes are "not something they do on their own" (Deadman 1999, Personal interview).

> would be something that we would have strong input to for the TWGDAM quality assurance guidelines (Newall 1999, Personal interview).

When asked whether the technicians have the expertise to read and interpret the autorads, Dr. Pamela Newall replied that "the tech doesn't have access to the sizing equipment that would allow him to do anything more than to interpret it visually. Does the tech have the ability to interpret? Probably. Does the tech have the training in interpretation? No" (Newall 1999, Personal interview).

In setting guidelines for quality assurance, TWGDAM members allowed each laboratory the freedom to determine the appropriate amount of education required for each job position within a laboratory but they did publish what they called "minimum" criteria for the education, training and experience of the different members of a DNA analysis team.[15] Most importantly, for those people at the top who were to interpret, write reports and witness on the DNA profiles, a period of time spent learning techniques *in another laboratory* was required. This laboratory must be one which had an established training program which met the American Society of Crime Laboratory Directors (ASCLD) accreditation standards, which had been established in 1985. The professional community itself recognized that DNA typing technology was not transmissible simply via written protocols. In-person, hands-on training, in a laboratory other than their "home" laboratory, was required for *anyone* who was to be in a position of interpreting, reporting or witnessing on case work samples.

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[15] These educational requirements are in addition to the minimum educational requirement of a BA/BS degree in a biological, chemical or forensic science. Supervisors should have at least two years experience as a forensic science analyst/examiner, and at least six months of DNA laboratory experience. It was recommended that Examiner/Analysts have one year of forensic laboratory experience, and that prior to any DNA typing or reporting on actual case work samples, the Examiner/Analyst should have at least six months of DNA laboratory experience (TWGDAM 1989).

These recommendations were probably made in the spirit of ensuring that autorads would not be interpreted by anyone who had not had "adequate" training and submersion in the culture of interpretation. Secondarily, the guidelines served to ensure that the first generations of DNA interpreters would have authority, and a sort of hegemony over interpretation in their home laboratories. This probably contributed to the attainment of professional status by crime laboratory directors in the late 1990s.

The recommendations also make excellent sense given relevant findings in the sociology of science. Karin Knorr Cetina's recent research into how molecular biologists acquire knowledge shows that the epistemic culture in molecular biology is such that molecular biologists have to become measurement instruments. Their ability to interpret resides in the training of their bodies and eyes. They become "repositories of unconscious experience" and individual scientists have to develop an embodied sense of a reasonable response to different situations (Knorr Cetina 1992, 119; Knorr Cetina 1999). The practicing molecular biologist literally *becomes* a measurement instrument, they become highly skilled at seeing things others cannot see, and their bodies learn to perform delicate operations in loading gels and manipulating DNA that cannot be taught, only learned through watching, trying, and erring. In their scientific work, individual molecular biologists often have to *guess* as to what procedure is best in a given situation. For this reason the sense of what counts as a successful procedure depends heavily on an individual's experience and upon the predictive ability "which individuals must somehow synthesize from features of their previous experience, and which remains implicit, embodied, and encapsulated within the person" (Knorr Cetina 1992, 121). What counts as a successful procedure or as proper scientific method is implicit -- it is a blend of the individual's experience and the culture in the laboratory. Knorr Cetina calls this kind of reasoning "biographical," because "it is

sustained by a scientist's biographical archive and the store of his or her professional experience" (1991, 115).[16] Knorr Cetina (1999) has shown that in high energy physics, forming consensus about what counts as adequate scientific knowledge and the proper application of scientific method is very much a group process. In molecular biology, the group is involved, in the culture of the laboratory, but each individual scientist is themselves a highly skilled measuring instrument that makes most procedural decisions on their own.

Further evidence supporting the embodied nature of molecular biological "knowing" was provided by a survey conducted by the FBI in June of 1990. By mid-1990, the FBI had already established training programs, but their survey results indicated that these training programs were not "enough." Even after a four week hands-on face to face training course, more than 95% of the participants felt the need for more help in the area of data interpretation -- indicating that reading an autoradiogram is *not* like reading a supermarket bar code, as it was commonly portrayed in the popular press. At this time (June 1990) the FBI offered a four week

[16] In contrast to the highly individual and personalized culture of knowing in a molecular biology laboratory, Knorr Cetina (*Epistemic Cultures*, 1999) also studied high energy physics laboratories and found them to have very different epistemic cultures. Their organization is best compared to a superorganism, such as highly organized colonies of bees, ants or termites. High energy physics involves more circularities and contingencies than molecular biology, experiments are long term and "supra-individual." To further demonstrate the epistemic contrast, in high energy physics (HEP) experiments, the work of producing knowledge is detached from the individual scientist and is shifted on to the group. High energy physics experiments can involve from 200 to 2000 individuals from 200 different institutions around the world, all focused on one common goal, for up to twenty years (1999, 160). Authorship belongs to the experiment as a whole, individual scientists feel that they are representatives of the whole, and there is a sense of collective responsibility among them. Unlike the highly trained body and eyes of a molecular biologist, data interpretation in high energy physics is not made by individual scientists, but by computers. In fact, individual scientists cannot run experiments. HEP experiments are huge, they take many years to run, and each experiment seeds new generations of experiments. High energy physicists do not think in terms of individual achievements in months, but of group successes over years and decades. By examining the organization of the laboratories and the working practices of the scientists in these two domains, Knorr Cetina has challenged head on the philosophical assumption of a unitary scientific method or epistemology.

course in DNA analysis methods. When asked to rate various aspects of the program, only 58% were "very satisfied" with the laboratory work experience provided during the course, and only 53% were "very satisfied" with the analysis of DNA results provided during the course. Overall, 88% of the participants felt that the FBI should offer a follow up course. Ninety-four percent thought this course should focus on refreshing the conceptual and theoretical background of DNA analysis; 98% felt further training in the analysis and interpretation of DNA analysis results was required, including the use of statistics; and 94% felt further training on acting as expert witnesses in the courtroom setting was necessary (Miller 1990, 13). Given this understanding, it is clear that even after a four week training course, participants did not feel they had become proficient either "with" or "as" instruments.[17]

11) Setting Standards for Quality Assurance and Proficiency

In June of 1990 the FBI Laboratory began a telephone survey of crime laboratory directors, using the FBI's "Directory of Crime Laboratories" as the sampling frame. Early in 1989 the Office of Technology Assessment (OTA) had also conducted a mail survey using the same sample. The OTA sample obtained a response rate of 85%, while the FBI's survey had a response rate of 100% (Miller 1990, 1). The FBI's survey results showed that by the middle of 1990, on the whole crime laboratories in the United States had "embraced DNA typing as an important additional forensic analysis tool in their fight against violent and sex-related crime" (Miller 1990, 1).

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[17] In addition to Knorr-Cetina's findings, sociologist of science Harry Collins has demonstrated repeatedly that scientific knowledge cannot be transmitted from written instructions alone, but requires face-to-face interaction. Using the example of the development of the TEA Laser, he shows that scientific developments often cannot be replicated unless there is direct, personal contact between the original researcher and the people attempting to do the replication. Few replications are possible using published results and procedures, and successful replication often rests on the tacit knowledge of the original researcher, knowledge that is not easily transferable (Collins 1974; Collins 1985).

Although only 5% of the crime laboratories had their own DNA analysis units, 76% of them utilized the technology by sending samples to outside laboratories for analysis. Sixty-one percent of the laboratories that conducted DNA analysis were sending samples to the FBI. Forty-nine percent of the laboratories sent cases to Lifecodes Corporation of Valhalla, New York, 52% to Cellmark Diagnostics of Germantown, Maryland, 5% to Forensic Science Associates of Richmond, California, 5% to Cetus Corporation of Emeryville, California, and 13% to other laboratories (Miller 1990, 7, Table 4).

Even at this early date and despite practitioners' lack of confidence in their own skills, the FBI's attempts at standardizing and transferring protocols and educating practitioners were extremely successful. A member of the crime laboratory community and a founding TWGDAM member said that the way the FBI went about introducing the technique to the crime laboratory community was "very smart." If the FBI had *told* people they had to do things a certain way, there would have been balking, but what the FBI did was say "hey, we have this new technology," and they *invited* people to participate actively in its development and dissemination, *at the FBI's cost*, thus making them feel part of something new and exciting (Kahn 1999, Personal interview). I think Dr. Kahn's point is that the FBI did not obviously force their technology down anyone's throats. However, the carrot they held out was so big that even the large crime laboratories could not afford to refuse it. This was possible only through the FBI's access to money, institutional resources, and the physical infrastructure to house and train people.

In order to get everybody "doing it their way," (to meet their eventual goal of establishing a national DNA databank), FBI Examiner Dr. Dwight Adams said that the agency attacked the problem from several fronts. The first was to offer training in the

method to anyone who would come. The second was to offer the potential of belonging to the CODIS databank. In his words, "In being able to get States and local laboratories to do it in a uniform way, one of the best ways to do that is to provide the software free of charge, and so that's what was done" (Adams 1999, Personal interview). The FBI provided training, protocols, and a big database of convicted offenders, which functioned as "big carrots" for State and local crime laboratories. For smaller labs with extremely limited resources how could they refuse when the FBI did the research, aided in transferring the technology, developed software for the databank, and they shared this knowledge, software, training and information to all labs, at no cost to the participating laboratory.

By mid-1990 twenty-five of the crime laboratories the FBI surveyed were at that time either conducting DNA analysis, or they were completing the steps necessary before accepting forensic DNA cases. All but three of these laboratories were using the FBI method for DNA analysis to the extent that was necessary to allow sharing of DNA profiles in a national database (Miller 1990, 2).

By 1990, with the work done by the members of TWGDAM, the FBI already had considerable momentum in the arena of standard setting. In the 1990 Office of Technology (OTA) report (United States Congress, Office of Technology Assessment 1990), the FBI reported that it would facilitate the establishment of standards through the consensus building process of TWGDAM, which published its first set of Quality Assurance Guidelines in 1989. The 1991 revised Guidelines addressed the emerging technology of producing profiles by polymerase chain reaction (PCR), while the 1989 Guidelines addressed only RFLP technology (Kearney 1989).

These guidelines were taken seriously by both the crime laboratory community and the government, which may serve as an indication of increasing professional

status for the crime laboratory community. On January 3, 1991, Representative Frank Horton (R-NY) introduced bill H.R. 339 with input from FBI Assistant Director, Laboratory Division John Hicks and the American Association of Crime Laboratory Directors (ASCLD) president Richard Tanton, titled the "DNA Proficiency Testing Act of 1991." This act proposed to set aside $5 million for the purchase of DNA related equipment in State and local crime laboratories, *providing that* the lab requesting the funds would perform DNA analyses which would meet or exceed the Quality Assurance standards set by TWGDAM in 1989.

This is the most important part of this first effort at legislating forensic DNA technology -- that it attempted to tie material support (funding) to the adoption of the FBI's standards, guidelines and protocols.[18] The relationship between formal legislation and the stability of DNA profiling will be explored further in Chapter Six of this dissertation. At this point, it is important only to know that in the final legislation that was passed, the 1995 DNA Identification Act, TWGDAM's Quality Assurance Standards were legislated as the quality assurance standards for the community, and the Director of the FBI was appointed as head of the "DNA Advisory Board" which was to oversee DNA profiling across the country. The methods for DNA analysis developed by the FSRTC at the FBI were beginning to become entrenched in formal social structure. Legislation was on the table that made the FBI an integral part of funding for DNA laboratories, and for overseeing quality of practice across the country. Knowledge, the knowledge produced by the FBI, was becoming a part of formal order.

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[18] Tying funds to some sort of regulatory compliance was not unprecedented. The Office of Technology Assessment noted in its 1990 report that Congress had frequently used incentives "to encourage certain results from States." The OTA report suggested that the government could speed the diffusion of DNA testing technology throughout the country by tying grants to quality assurance standards and the use of specific materials, protocols and computer software. (U.S. Congress, Office of Technology Assessment, *Genetic Witness: Forensic Uses of DNA Tests*, OTA-BA-438. Washington, D.C.: U.S. Government Printing Office, July 1990, 35).

The previous chapters showed that although the controversy over DNA fingerprinting raised important questions, that the solutions to these questions were not all found in the two National Academy of Science committees, nor in the debate between scientists carried out on the pages of Nature and Science. The solutions to the problems of knowledge began to come about when the FBI successfully established a professional community intent on finding solutions to the problems.

Theorizing about the relationship between social structure and individual action, Jeffrey Alexander (1987) argues that social environments exert a "determinate force" over participants "precisely because they demand so much time and energy if they are to be changed." For example, he argues that it is not the case that an individual worker cannot change his or her class position, but that the time and energy required to change the environment enough to do so make the probabilities very small that any given worker will do so. In this way the worker's class position becomes objective

I use this argument about the effect of social structure in a parallel way by suggesting that the FBI invested such huge amounts of money, time, personnel, energy, equipment not only into the technical processes required to conduct DNA typing, but also into creating a *social* structure, through building the community of TWGDAM. As TWGDAM gained cohesion and solidarity, the probability increased that given members would be able to act in certain ways with respect to DNA typing (i.e., transfer the FBI protocols to their own laboratories), and the probabilities of other actions decreased (such as coming up with their own protocols). The probability that private laboratories could establish and maintain a monopoly was also made very unlikely. The FBI had access to material, intellectual and physical infrastructures that no private company had. From the very beginning, the FBI created the conditions which favoured the development of a strong social bond between actors, and provided

the material environments for them to learn and work in. The outcome was that the crime laboratory community became a more professional (self-policing) community, and TWGDAM's Quality Assurance Guidelines became officially entrenched into formal social structure through incorporation in the1994 DNA Identification Act, which was passed into law.

Chapter Seven

Social Structure and DNA Profiling: From Practice to Structure

1) Structure in Action:

Oklahoma City, Oklahoma, February, 1997: In 1992 five women were bound, gagged and stabbed in a drug house in Oklahoma City. The Oklahoma State Bureau of Investigation developed a DNA profile for the killer in 1995, based on evidence found at the crime scene. In 1997, the California Department of Justice used CODIS [the national DNA databank][1] to match the evidence profile against Danny Keith Hooks, who was convicted of rape, kidnapping and assault in California in 1997 (Niezgoda 1997).

Tallahassee, Florida, February 1995: The Florida Department of Law Enforcement linked semen found on a Jane Doe rape-homicide victim to a convicted offender's DNA profile. The suspect's DNA was collected, analyzed and stored in a CODIS database while he was incarcerated for another rape. The match was timely: it prevented the suspect/offender's release on parole, which had been scheduled to occur eight days later (Niezgoda 1997).

Florida and Iowa, February 2000: In 1995, an unidentified woman's body was found on an off-ramp along an interstate in Des Moines, Iowa. After identifying the victim, police began looking at truck drivers as suspects, due to the location of the body. The Iowa Department of Public Safety sent biological evidence left at the crime scene to the FBI Laboratory for DNA analysis. The FBI Lab analyzed the evidence, and developed a DNA profile of the perpetrator. The profile was uploaded to CODIS, where [it was] matched to the Florida offender. At the time of the hit, the offender was incarcerated in a Florida prison for a sexual assault conviction in early 1999. After identifying the offender, police discovered that he possessed a commercial trucking license (Federal Bureau of Investigation, 2000).

These stories are not presented to be "feel good" stories that the law enforcement system in the United States sometimes gets lucky and solves difficult

[1] CODIS is an acronym for "Combined DNA Indexing System." It is a national databank of DNA profiles of convicted felons, and also contains DNA from crime scenes, whose source is unknown. CODIS operates as a three level system, with DNA profiles being stored at the local, municipal or county level, the state level, and at the national level by the FBI (Federal Bureau of Investigation 2000).

cases. They are presented to illustrate existence, power and magnitude of a social structure that did not, and could not have existed before the forensic uses of DNA were discovered. There are many more stories like this, and even more heartening stories where DNA is used to exonerate people on death row – particularly where those people were normal, everyday people without prior criminal records.

2) Agency, Structure, and the History of DNA Profiling

In Chapter One I argued that contemporary social theorists have focused primarily on the reproduction of structure, not on the genesis of new structure. One problem is that aside from historical sociologists, or those doing time series or working with panel data, "time" is not included in most sociological analyses. By conducting analyses at one point in time, events which are the outcome of processes over time become invisible to sociological analysis. The genesis of new structures happens over time, and cannot be seen in a single cross sectional analysis. Here, by following the actions of individuals and social events over time, I have been able to show how certain social structures came into being. I have use the history of DNA profiling as a case study to provide data for my analysis.

Scientific knowledge is also a product of social action over time. There is a mystery to science as it is usually presented to the public, one which disappears when when the mechanisms by which knowledge and structure are made visible to the analytic eye. Pulling the analytic lens back and viewing scientific action over period of time allows us to see the labor which goes into the production of new knowledge, which becomes incorporated into new social structures.

The reader will recall that for my purposes, the concept of social structure encompasses informal agreements, tacit knowledge, legal standards, legislation and social institutions. For my purposes social structure include agreements the rest on

group consensus on things like the correct interpretation of DNA autorads, on norms of professional behavior, and standards of proficiency in a laboratory. Social structure is laws and legislation which provide material resources, establish governing and advisory bodies, and allocate authority for the legitimate use of force. Those governing and advisory bodies probably belong in the middle of the informal-formal continuum. The important point is that social structures are the *outcome* of situated human labor which takes place over time. As discussed in Chapter One, Anthony Giddens refers to social structure as "rules and regulations" which exist in people's heads. Giddens is not naïve, he recognizes that social structures can be much more obdurate than "rules and regulations" in people's heads, but he does not define social structure to account for the obdurate existence of some kinds of social structure.

One of my goals has been to write a history – by following the reported actions and written documents of groups and individuals – of the genesis of new social structures. In the case of DNA profiling, two main dynamics were at work. Social structures such as legislation, and standards that are enforced by law, rest on the epistemic -- or truth -- status of the knowledge which they are attempting to control and use. However, embedding knowledge about DNA profiling in the formal social order also stabilizes the epistemic status of that same knowledge. In the case of DNA profiling, solving a problem of knowledge involved solving problems of social order in several different domains: academia, the NRC committees, the crime laboratory profession, the FBI and the judicial system. Aside from the Constitution, laws and legislation are perhaps our most formal structures. They are created in assemblies of elected officials, they are enforced in courts of law, and they are partial solutions to the problem of order in a society.

The purpose of this chapter is to show that the stabilization of knowledge about DNA profiling was intimately intertwined with the creation of new social structures. In the early stages, most of the structures were informal, in the form of agreements amongst groups. As the knowledge became more stable, more formal agreements and social structures came into being. And importantly, as more structures were formed which utilized DNA profiling, the more stable the epistemic and ontological status of the DNA "fingerprint" became. The more entrenched knowledge about DNA profiling became in more and more social structures, the more the knowledge claims based on DNA profiles attained the status of "generally accepted belief," or knowledge.

The creation of new social structures, and the institutionalization of new forms of knowledge, is particularly important because once institutions are formed, they tend to persist. Institutions are always tied to human interests, but once formed, there is nothing to ensure that they remain tied to the original goals which brought them into being. Additionally, it is important to understand how activities and beliefs become institutionalized, because once they become part of the fabric of our social order, they have a kind of inertia that makes it difficult to dismantle them.

> [T]he processes whereby activities become institutionalized, and those whereby they cease to be institutionalized, are very different, and although the former may be difficult enough to implement, the latter are nearly always very much more so. Once some activity or practice is generally perceived as a routine and continuing part of society, i.e., as an institution, people plan on its continued existence, and having laid their plans they tend to lose if that continued existence is not forthcoming. By their planning, people acquire a vested interest in the continuation of that institution, and hence, without any particular regard to rights and wrongs, become its supporters (Barnes 1985, 11).

People who have invested a great deal of time in becoming scientists have an interest in the institution of science continuing – and the same is true for medicine

and doctors, law and lawyers – the FBI and its identification and surveillance technologies, such as DNA profiling and CODIS.

One of the complexities of this study has been that the process of stabilizing and standardizing knowledge about DNA profiling led to the creation many different social structures between 1985 and 2000. These include, but are not limited to standards of interpretation, professional communities, communities of practice, laboratories, databases, political projects and legislation. It is important to note that the histories of these social structures are intertwined with each other and the knowledge of DNA profiling. Most of the structures highlighted in this chapter are ones for which I been able to trace some of the processes by which the social and material relations of which they are constituted were made invisible. When these relations become invisible – a social structure takes on a form of "objective" existence as a social institution.

For example, CODIS is a system of classification, made possible by the stabilization and diffusion of standardized DNA profiling techniques. The storage of DNA profiles as molecular weights in the CODIS database represents the creation of a new system of classification which was the outcome of years of hard work by individuals working within specific institutional contexts. In this case, the practical politics of classification and standardization (Bowker and Star 1999) dissolved[2] within CODIS result in a system of surveillance and control, justified to the public as a something which could help to protect them from violent criminals. Some of the structures are meant to be permanent, such as CODIS and TWGDAM; and some

[2] Bowker and Start (1999) argue that all systems of classification have the material and social relations from which they were created dissolved within them.

played a transitory, but necessary role in the stabilization process, such as the DNA Advisory Board and the Committee on the Future of DNA Evidence.

In the process of stabilizing knowledge about DNA profiling, the crime laboratory community became a professionalized group, with the jurisdiction to self-police through the American Association of Crime Laboratory Directors (ASCLD). Prior to the discovery of DNA profiling, this group was not a particularly cohesive or professionalized community. They also had virtually no knowledge of DNA, molecular biology, or the techniques by which DNA is extracted and manipulated. About ten years after DNA profiling was discovered, aside from having attained the professional status and consequent right to self-police, this community of crime laboratory practitioners, known as TWGDAM, had been legislated as the group with the right and obligation to police future developments of forensic DNA technology (DNA Identification Grants Act of 1994). The same act also authorized another crime laboratory body, the Laboratory Division of the American Association of Crime Laboratory Directors (ASCLD-LAB) which was charged with the accreditation of DNA laboratories across the country, *including the FBI's laboratory*. In this chapter I cover the most important, and perhaps the most prominent structures, formal and informal, which were created throughout the history of DNA profiling.

3) Structural Outcomes

a) Disciplining Vision, Stabilizing Practice, Standardizing Knowledge

In 1995, only ten years after Alec Jeffreys developed the first DNA "fingerprint" in his laboratory in Leicester, England, 50,000 RFLP DNA profiles of convicted offenders were stored in the CODIS database in the United States. This number is very small compared to the number that will eventually be stored there. It is, however, quite a large number considering the stabilization and standardization of vision and

practice that had to occur for the database to exist. The database is an outcome of the production of a form of social structure. Social structure can be an intended or an unintended effect of interaction, because interaction creates "institutionalized patterns of behavior" (1984, 110-44) which become social structure. The stabilization and standardization of the DNA profiling process required to be able to store profiles in a national database, and exchange them between distant laboratories, means that that process of institutionalization of behavior occurred.

Disciplined vision: One type of social structure is the consensus on how DNA autorads should be interpreted. This consensus was achieved through interaction, negotiation, and comparing results to the work of others. During these kinds of interactions, the FBI members and the community of crime laboratory practitioners that made up TWGDAM came to agree on how to interpret autoradiograms. As one TWGDAM member said, at the beginning, there was widespread disagreement about how to interpret an autorad, and a year later, and after a lot of interaction and discussion and hands-on work, most of the members of the community interpreted the same autorad in the same way. A standard way of "seeing" had been established. The individual members then went back to their distant laboratories, and transferred their newly acquired skills of seeing to qualified members of their individual laboratories. This disciplined way of seeing and interpreting autorads is a social structure which arose out of group interaction. It is important to remember that the agency of the individuals, and the group were facilitated by the resources (pre-existing structure) of the FBI.

Stabilized practice and Standardized Knowledge: For DNA profiles to be stored in a national database, and for them to be transferable between distant laboratories, each technician, in each individual laboratory, had to conduct the DNA

profiling protocol in essentially the same way. This is notoriously difficult, if not impossible, because there are always local contingencies and variations in how people conduct protocols.[3] The trick was to develop a protocol that was simple, and extremely robust to inter-user variation (Budowle 1997, Personal interview).

The members of TWGDAM and the FSRTC went through an extensive period of testing the protocol for producing DNA profiles. Through extensive testing and communication between laboratories and individuals, many volatile chemicals and tricky, difficult to perform techniques were eliminated from the DNA profiling protocol. The end result was a protocol that was "robust" to inter-user variation (Budowle 1997) and could be transferred fairly easily from professional to neophyte, from lab director to technician. This stabilization of practice was important for a number of reasons. The CODIS database was dependent on DNA profiles that could be compared to each other. This meant they all had all to have been created using the same chemicals, the same protocol, the same sizing ladders and standards had to be applied, and each DNA profile had to have been conducted at the same sites along the DNA molecule.

The work done by the members of TWGDAM and the FSRTC helped to produce stable protocols and standardized DNA profiles. Standardization is a form of social structure. Remember Giddens' dictum that social structure exists as "rules and resources inside people's heads" (Giddens 1984; 1979). Social structure exerts an effect when individuals use those rules and resources to pattern their behavior. The standardization of a molecular biological technique such as producing DNA profiles fits this description aptly. People had to learn to pattern their behavior when following

[3] Berg (1997) and Timmermans and Berg (1997), discussed in Chapter 1, point to the incredible difficulty and the local changes that have to be made to work practices to "discipline" a protocol to work in different environments.

a DNA profiling protocol to a working pattern. People taught each other, at the FBI base in Quantico, and then took those techniques home to their laboratories. Establishing robust, transferable protocols took the work of many people, and transferring the protocols required hands-on training from someone who already knew it.[4]

This hands-on aspect of DNA profiling is a part of the epistemic culture of molecular biology. Karin Knorr Cetina argues that acquiring skill in molecular biology requires that the body of the scientist is transformed into a highly trained instrument (1999). Practitioners' minds and bodies learned and internalized how to extract DNA from all types of samples, how to load gels, do Southern blotting, and how to interpret autorads. The goal was to get the same procedures internalized and embodied in enough peoples' bodies and heads that the DNA profiles produced could be said to be stable, or stabilized. This happened through shared interaction, as members struggled to produce profiles in a consistent and comparable manner. The inter-laboratory comparability of DNA profiles could not have happened without the face to face interaction at Quantico.[5]

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[4] Getting most of the people in most of the laboratories in the United States and Canada to produce DNA profiles using the same protocols, in a similar enough way that these profiles were comparable was a major accomplishment of the FBI and TWGDAM. There are many different ways of producing a DNA profile, there are different sizing standards that can be used, and different reagents, probes and enzymes. Each of these different procedures results in a valid DNA profile – in that it represents the DNA of the individual at the alleles analyzed. However, unless the procedure is done with the same reagents, probes and enzymes; with the same steps in the same order; and the same loci analyzed on the DNA molecule, the results are not comparable. The existence of a DNA databank with 50,000 DNA profiles that can be compared to each other is the outcome of stabilization and standardization, which are the result of the efforts of many individuals, and the material and institutional resources of the FBI.

[5] Standardizing the protocols for producing DNA profiles involved many complex practical and conscious evaluations. When evaluations such as "good" or "bad" are made by the right person they become epistemic acts which can confer the status of "knowledge" on those objects deemed to be "good" (Derksen 2000).

The terms standardization and stabilization are often used interchangeably. Here I use the term stabilization to denote all the efforts that went into getting DNA profiles which were produced by many different people, in many different laboratories across the country, to come out in essentially the "same way."[6] This was necessary for DNA profiles done in one laboratory to be comparable to those done in another. Only when this type of consistency was achieved, could the CODIS database become a functioning database. Once this type of consistency exists, the process has become standardized. Here, I re-direct the reader's mind to Bowker and Star's definition of standards: 1) they are a set of agreed upon rules for the production of material or textual artifacts; 2) they span more than one community, and persist over time; 3) standards are used to make things work together over distance and different systems of measurement; 4) they are often enforced by legal bodies; 5) there is no guarantee that the best standard will win (1999, 13-14).

Standardization is one of the ways that knowledge gets "out of the lab" or out of its local context of production. Protocols become standardized when people in many different places are doing them in more or less the same way. The standardization of DNA profiling, as it occurred through the efforts of the FBI and TWGDAM, is perhaps a textbook example of classification achieved through distributed activity (Timmermans and Berg 1997). TWGDAM members met in Quantico four times a year, and during the early years, they took problems back to

[6] There is no agreed upon definition of stabilization and standardization. Martha Lampland (Personal communication, June 11, 2002) sees standardization as more of an "in-house" process, whereby disparate techniques, procedures, processes are assimilated to one another, made similar or compatible in some sense. She views stabilization as more external – the means whereby that which has been standardized is made more secure, fixed, less challengeable. We view the two terms in almost opposite fashion. I feel that stabilization is that process of working out the "kinks" in a procedure, as was done by the FBI and TWGDAM in all the validation studies of DNA profiling.

their own labs to work on. When they met every three months they would discuss and share their findings. Timmermans and Berg argue that "universality" has a certain tenuousness to it, and so they label it "local universality" (1997, 275). By applying this label, they want to emphasize the rather taken for granted idea (in the SSK literature) "that universality always rests on real-time work, and emerges from localized processes of negotiations and pre-existing institutional, infrastructural, and material relations" (275). In the case of DNA profiling, for many practitioners, the only pre-existing institutional and infrastructural relations they could draw upon was the existence of their own crime laboratories. In the beginning, except for two or three individuals, the community lacked any knowledge of the DNA molecule, or the techniques required to manipulate it. They came frequently to the well-equipped laboratories at the FSRTC in Quantico, and returned to physical spaces and organizations that were ill-equipped to produce DNA profiles. Bowker and Star (1999) note that the setting of standards may eventually involve legal validation or enforcement. This occurred with DNA profiling. The DNA Identification Act of 1994 required that laboratories that wanted grants to make their laboratories capable of producing DNA profiles, had to commit to following the TWGDAM protocols, sizing standards, and norms of interpretation. The DNA Identification Act of 1995 allocated millions of dollars a year for the upgrading of physical and intellectual resources – *provided that the laboratory conformed with the FBI standards for producing DNA profiles* (DNA Identification Act of 1994). In this sense, a set of professional or community standards became part of formal social structure through legislation.

The CODIS databank exists because a group of people were able to produce a protocol for making DNA profiles that is sufficiently robust that it can sustain the inevitable inter-technician variability in its use. This counts as the standardization of

the practice and production of DNA profiles. However, as Bowker and Star (1999) argue, that which appears as universal or standard is inevitably the outcome of "negotiations, organizational processes, and conflict" (44). CODIS is the distillation of all the individual efforts and group decisions made in the process of standardizing DNA profiling in the United States.

b) From Disparate Individuals to Competent Professionals: The Technical Working Group on DNA Methods

This group has been referred to frequently throughout this dissertation. They were brought together in 1988, invited to Quantico, Virginia, as guests of the FBI. The participants were mostly members of crime laboratories from across the United States and Canada, usually directors and their top technicians. Ian Evett, from the Home Office in the United Kingdom was also in attendance, as were other assorted academics such as Ray White, from the University of Utah, and George Sensabaugh, from the University of California, Berkeley. This group was brought together four times a year, at FBI expense, to work out the kinks and quirks of DNA testing. Initially, only two or three of the members knew anything about DNA. They all went through what constitutes a crash course in molecular biology, and eventually became proficient in performing the tests and interpreting DNA profiles.

They quickly divided into sub-committees to share the labor involved in establishing standards of quality assurance and technical proficiency. Because the FBI footed the bill for all expenses, small laboratories were able to participate on an equal basis with the larger metropolitan or state laboratories that could have funded their own programs.

What is remarkable is that in six short years, this group had educated themselves in the techniques of molecular biology, and attained enough group

cohesion, that their standards for quality assurance and proficiency were legislated as *the* standards to follow if local and state crime laboratories wished to procure Federal funding for their own DNA testing laboratories.[7] TWGDAM is a success story, and as detailed in Chapter Five, it was so successful that its organization gave rise to other FBI sponsored groups such as TWGBOMB and TWGDRUG. Eventually, in the late 1990s, the groups changed their named from "Technical" Working Group to "Scientific" Working Group, so TWGDAM is now properly referred to as SWGDAM.

A latent effect of the group cohesiveness found in TWGDAM was the professionalization of the crime laboratory community. In the late 1980s, the crime laboratory community was loosely organized. Through their meetings at Quantico, they became a very cohesive, tightly-knit community. Their professional status and credibility was so high by the mid-1990s, that they were the body named in the 1994 DNA Identification Grants Act as the body to control the future of DNA profiling, once the DNA Advisory Board had been disbanded. TWGDAM is an almost perfect example of Giddens' claim that the intended or unintended effect of social interaction, is social structure.

c) Law and Legislation as Formal Structures

The sociological status of law was of great interest to the founding fathers of sociology, and then it fell out of interest until the 1970s. Whether one takes the structural-functionalist view that laws reflect and reinforce a society's underlying core values, or the Marxist position that laws serve the interests of the most powerful sectors of society, written and unwritten laws are powerful enablers and constrainers of individual and institutional behavior. For Parsons, formal law was intrinsic to

[7] Laboratories also had to agree to follow the TWGDAM/FBI protocols for producing DNA profiles.

bringing order, or integration, to society. He said that "[t]he legal system ... broadly constitutes what is probably the single most important institutional key to understanding ... problems of societal integration (Parsons 1978, 52). Before Parsons, law, as rules pertaining to individual action, or statutes established by legitimate authorities, was a central concern to the founding "fathers" of sociology. For Marx, because law was part of the bourgeois state, it inevitably reflected the interests of that class, and therefore acted as a means of oppression of the lower classes. Additionally, Marx's very famous statements that the ruling ideas of a period are the ideas of the ruling class served to further emphasize that the "law" acted as a system of domination (Cain and Hunt 1979). In *The Division of Labour in Society,* Durkheim ([1893] 1964) argued that mechanically organized societies focus on retributive law, or punishment. On the other hand, organically organized societies focused on restitution, or restoring or repairing the damage done by those breaking the law. Durkheim also argued that crime served an integrative function for society – the breaking of laws and identification of "criminals" served to solidify the basic core values of a society. Among sociology's founders, Weber paid the most attention to the law, as it takes up most of the second volume of *Economy and Society* ([1922] 1978). Weber wrote about the theory, social role, and history of law in many different societies. Weber regarded law more positively than Marx, but also with the same dual edge: for Weber, law served an integrative function, but it was also the source of legal-rational domination in advanced capitalist societies. The most famous Weberian view of law is that of the legitimate use of force against members of a society. For Weber, "[a]n order will be called *law* if it is externally guaranteed by the probability that coercion (physical or psychological) to bring about conformity or avenge violation, will be applied by a *staff* of people holding themselves specially ready for

that purpose" (1954, 5). Donald Black defines law as "governmental social control" (1976, 2).

Including coercion in a definition of law sets "law" aside from norms or customs. However, organizations, institutions, even families, set up rules, norms and customs which are or are not complied with. Professions, such as medical doctors and lawyers, have earned or claimed the right to self-police and punish members for transgressions from the norms and customs of the profession (Abbott 1988). Moore (1973) calls these groups, including families, "semi-autonomous fields."

To put DNA profiling in the context of knowledge and order, DNA profiling is widely regarded by law enforcement practitioners as "the most significant forensic breakthrough of the century" (Hicks 1989; Sensabaugh 1999). Trust in the validity and reliability (or the solid epistemic status) of this technology underlies the formal legislation and the establishment of formal social structures designed to partially solve problems of order -- insofar as DNA profiling is a powerful tool utilized by law enforcement agencies, who have the legitimate right to the use of force to ensure order.

Some of the most important structures which depended on the stabilized status of DNA profiling, and concurrently reinforced its epistemic status were formal -- in Weber's terms they are rational-legal structures. Here, I refer to legislation, the DNA Identification Grants Act of 1994, which provided the material and legal grounds for the establishment of DNA laboratories across the country, and the establishment of a national DNA databank. However, before looking at the legislation, which provided the material resources for the creation of crime laboratories across the country, and gave legislated status to several other groups, I would like to examine the Combined DNA Indexing System, which is a social and material structure which

existed as a gleam in the eyes of the FBI from the moment they learned about the forensic potential of DNA.

d) The Combined DNA Indexing System

One of the most important formal, physical and social structures to be created during the history of DNA profiling in the United States is the Combined DNA Indexing System. CODIS is the *outcome* of successful standardization and stabilization of DNA profiling protocols and methods. It began in 1990 as a pilot project involving the FBI and 14 state and local laboratories. The DNA Identification Act of 1994 (Public Law 103 322) gave formal authority to the FBI to establish a national DNA databank for law enforcement purposes. The National DNA Indexing System (NDIS) became operational in 1998. CODIS is a three -tier system in which DNA profiles are stored as molecular weights (numbers) in local DNA databanks (LDIS). The local level is the "lowest" level of the CODIS system. The Local DNA Indexing System is where all DNA profiles originate, and are stored. LDIS operates at the level of the individual laboratory, whether this is for a city or a county. Local laboratories can share their profiles with the State DNA Indexing System (SDIS). This is the second tier of the CODIS system. It is up to SDIS members whether they wish to exchange their information with NDIS. The logic of the tier approach is that it allows local and state laboratories to operate in accordance with local legislation (Federal Bureau of Investigation 2000).

The FBI provides the material and intellectual resources necessary to operate the databanks and ensure comparability of entries between all levels. Is the existence of CODIS a story of FBI power and hegemony? Yes. Aside from the skills acquired by individuals and the group formation referred to in the previous section, the FBI provides, free of charge, all software for CODIS, along with installation, training and

support. Federal grant money for the creation and development of DNA laboratories is tied to following FBI protocols and quality assurance standards. As of the year 2000, CODIS was operating in more than 2000 laboratories across the country (Federal Bureau of Investigation 2000). As of June, 2001, 36 states, the US Army, and the FBI were participating in NDIS.[8] The Assistant U.S. Attorney speaks of the contribution of the FBI, specifically as it relates to CODIS:

> Dwight [Adams] is currently the Deputy Assistant Director for the FBI laboratory, and it's fair to say that ... if it were not for the Bureau, this DNA database or CODIS would not exist, that it was through their hard work and their research and their development of the software that created this database [and] that we're able to access this incredibly powerful technology (Asplen 2000, 1).

The National DNA Indexing System had 210,000 DNA profiles from 24 states, as well as those provided by the FBI. CODIS is comprised of two indexes: the "Forensic Index" contains DNA profiles taken from actual crime scenes. Matches made in these indexes can link crime scenes together, possibly identifying a serial criminal. Although the person's actual identity would not be known, the database would provide the evidence that the same individual had perpetrated several crimes. The "Offender Index" contains profiles from individuals convicted of sex offenses and other violent crimes. A match made between the Forensic Index and the Offender Index provides law enforcement personnel with the identity of the person they are looking for. Once the CODIS system has identified a match, individuals at the local level exchange the actual DNA profiles to make sure that the two profiles actually match each other.

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[8] Non-participating states include Alabama, Delaware, Hawaii, Idaho, Indiana, Iowa, Louisiana, Mississippi, Montana, Oklahoma, Rhode Island, South Dakota, Tennessee, Vermont, and West Virginia.

"Success" for CODIS is defined as an "Investigation Aided." This is a "hit", or a match between two DNA profiles that would not have occurred otherwise. As of the end of 1999, CODIS had produced over 600 hits which assisted in over 1,100 investigations (Federal Bureau of Investigation 2000). Demand for the services of CODIS have been enhanced by developments in the study of the human genome, resulting from the Human Genome Project, and by an awareness at the local, state and national levels of legislature that DNA technology is a powerful tool in the investigation of violent crimes.

In terms of formal structure, Weber's rational-legal structure, the 1994 DNA Identification Grants Act is at the top of the list. There were attempts at entering and passing legislation before the 1994 Act was passed, but they failed. The 1994 Act is important partly because it set aside money for the development of DNA testing laboratories across the country, and because it gave formal status to several groups.

CODIS is one of the social structures that evolved in the course of stabilizing, standardizing, and making DNA profiles part of the forensic toolbox. Its existence as a social structure was made possible through the agency (work) of many individuals in different institutional sites (different social worlds) in classifying VNTR alleles and standardizing the different protocols and validating aspects of the DNA profiling procedure. Bowker and Star would refer to CODIS as an information infrastructure (1999, 6). In this analysis, the successful classification of DNA profiles in CODIS is an outcome -- in a sense, it is a dependent variable. CODIS exists as a system of computer databases that links participating U.S. and Canadian databases relatively seamlessly. That it works successfully means that the labor involved in its creation has been rendered invisible.

Aside from the material and social relations embedded in it, but now invisible, CODIS can be viewed as a social structure because it brings different social groups into interaction, and requires their interaction for its success. For CODIS to operate properly, law enforcement agents must collect the evidence properly, a pristine chain of custody must be maintained, and technicians in laboratories across the country must follow the FBI's protocol for producing DNA profiles closely enough so that the profiles are comparable with those produced in other laboratories. These profiles must then be converted to molecular weights, and stored in computer databanks across the country. CODIS brings into relationship local, state and federal law enforcement authorities, as they all keep different databanks of DNA profiles. The system only works if these groups can successfully interchange information.

However, there were several problems of classification that had to be solved before CODIS could become a reality. Much of the DNA Wars (Chapter Four) were arguments over the allele frequency distributions that should be used to calculate random match probabilities. The arguments spanned the fields of population genetics, statistics, molecular biology, and biology. Whether distributions classified by race should be used in the calculations was the subject of heated controversy in 1992 and 1993. Earlier than that, in 1989 Dr. Eric Lander, acting as an expert witness in the *Castro* case, had brought to light problems with the actual production of DNA profiles, the measurement of fragment lengths and the standardization of measurement error (Derksen 2000). These were all problems of classification which involved standardization as a solution.

e) The 1994 DNA Identification Grants Act

The 1994 DNA Identification Grants Act (Public Law 103 322) is a social structure. It does what Bowker and Star claim is often necessary for standardization –

it provides a mechanism which legally enforces standards (1999, 13). Perhaps most importantly, the 1994 Act authorized the disbursement of powerful material resources -- money -- to be given to the FBI, and to state and local law enforcement laboratories to "enable them to develop and improve their ability to analyze DNA and to establish procedures to interface with the CODIS system." The money, $40,000,000, was to be disbursed over a period of five years, starting in 1996 (2).[9] The Act set the stage for the creation of a new social structure (which would also have material form) by authorizing funds to be released to the FBI for the formal creation of a national DNA databank which would be known as CODIS, but which had already begun to take form by 1994.

The 1994 Act also mandated the establishment of a DNA Advisory Board, headed by the Director of the FBI, and allocated funds to the FBI for administering the Board. The 1994 Act also helped to stabilize the FBI's standards. The legislation tied funding for state and local laboratories to adherence to TWGDAM's quality assurance standards and ASCLD-LAB accreditation. There was money available to any laboratory that wanted to develop their own DNA testing laboratory or participate in CODIS, *as long as the laboratory did their profiles in a manner consistent and comparable with those done by the FBI.* In this way, formal legislation acted to stabilize knowledge and practice by tying the necessary material resources to

[9] In 1995, an amendment to the 1994 Act was passed. Its main effect was to move up the funding for the FBI and local and State laboratories to improve their DNA profiling capabilities. In the original bill, the bulk of money was to be disbursed in 1999 and 2000. Members of the FBI and the American Association of Crime Laboratory Directors testified before Congress that setting up DNA testing programs and DNA databanking infrastructure had considerable start-up costs. They recommended that money be made available before 1999 for crime laboratories across the country to begin creating DNA testing laboratories and databanks. This would allow a "jump start" for local and State laboratories, and take the pressure of being the main public laboratory analyzing DNA profiles off the FBI laboratory sooner. The 1995 amendment made the major grants of $15,000,000 available in 1997, and $14,000,000 in 1998.

communities and forms of practice in addition to raising the standards of those groups to the status of legislation. According to the FBI, standards are "quality assurance measures that place specific requirements on the laboratory" (Federal Bureau of Investigation 2001). These Quality Assurance Standards were first published by TWGDAM in the *Crime Laboratory Digest* in 1989 and revised in 1991. In 1999 the FBI developed additional Standards for Forensic DNA Testing Labs (Federal Bureau of Investigation 1999a), and Standards for Convicted Offender Labs (Federal Bureau of Investigation 1999b).

Because DNA testing can be used to convict *and* exonerate, it is easy to view it as a win-win technology for everyone in society. However, throughout the development of DNA profiling, some people have raised concerns about the uses to which an individual's genetic information could be put (Rabinow 1992). In the mandate of NRC1, privacy issues were of particular importance (NRC 1992). In the United States, insurance companies could use genetic information linked to health issues to deny individuals coverage, due to genetic pre-dispositions to develop particular diseases.

Perhaps reflecting a concern that previously convicted persons not be "targeted" by law enforcement agencies, or that the DNA of convicted felons be put to eugenic-like uses, the DNA Identification Grants Improvement Act of 1995 amended the 1994 Act in a way that would avoid the potential scapegoating of specific types of individuals. The Amendment explicitly forbids the use of DNA profiles stored in DNA databanks to be used to "formulate statistical profiles for use in predicting criminal behavior" (2). It is not clear from the wording whether this applies to individuals or to the use of the databank as a whole. However, I believe the concern motivating the amendment was the protection of individual privacy, and to protect individuals who

had a DNA profile in the databank from being preemptively classified or being predicted as being at high risk for recidivism.

Although now, in 2002, most press focused on DNA profiling is positive, there are individuals concerned about privacy, and about the FBI holding so much genetic information. An on-line organization called Privacila.org is concerned about the FBI's CODIS database and movements in different states to widen the range of crimes for which one must submit a DNA sample. This organization is concerned with the revealing nature of DNA information.

> As with so many government databases, the original purposes of the CODIS database were entirely laudable. It was intended to collect only information about convicted sex offenders.
>
> Now that CODIS is established, however, the push to expand it has begun. In New York, Governor George Pataki wants to require anyone convicted of a misdemeanor to submit to DNA profiling. New York City's former police commissioner has said that anyone who is arrested should have to submit to the CODIS database.
>
> The temptation presented to bureaucrats and law enforcement by massive government databases of highly revealing DNA information are great. The law-abiding and innocent should resist the growth of databases like CODIS because they represent clear threats to the privacy, safety, and comfort of everyone (Privacilla.org 2000).

While it is true that everyday, more information can be gleaned from DNA, the DNA profiles, as stored in CODIS, are stored as molecular weights of the bands at each of the loci examined. The rest of the DNA is not coded or stored in the databank, although where possible, the FBI recommends keeping DNA samples indefinitely.

In 1988, there were no states that had the right to collect DNA samples from suspected or convicted offenders. As of 1998, all 50 states had passed legislation *requiring* the collection of blood samples from convicted sex offenders. All states also required that these samples be analyzed and stored in the *national* databank. Some

states have expanded their legislation to allow the collection of DNA from felons convicted of other violent crimes (Federal Bureau of Investigation 2000). In 1991 the FBI had issued guidelines for legislation which included suggestions for the scope of legislation in individual states, including the definition of violent crime; who should have access to DNA profiles; who they could be disclosed to; when, or if they should be expunged from databanks; and proposed penalties for unauthorized disclosure (Federal Bureau of Investigation 2000).

f) The DNA Advisory Board

The DNA Identification Act of 1994 mandated the establishment of a DNA Advisory Board, administered and headed by the Director of the FBI. The DNA Advisory Board was to "develop, revise and recommend standards for quality assurance" (Federal Bureau of Investigation 2000), and was to be terminated on March 9, 2000, unless the Director of the FBI extended the Boards' term (Eisenberg 1999, 4). The 1994 DNA Identification Act tied government funding for DNA laboratories to adherence to TWGDAM's standards. This is a clear example of the state using knowledge and material resources as power with which to control the actions of other institutional entities. Bowker and Star (1999) argue that standards are the outcome of negotiations, organizational processes and conflict (44), but it is clear that once knowledge reaches a certain point of stability, standardization can be enforced by government power.

The DNA Advisory Board was established in March of 1995. Former Nobel Prize winner Dr. Joshua Lederberg was appointed as the first chair (Eisenberg 2000). Members of the Board were appointed from recommendations made by the National Academy of Sciences and the National Association of Crime Laboratory Directors. The Act specified that the Board must include scientists from state, local and private

forensic laboratories, as well as molecular geneticists and population geneticists who were not associated with a forensic laboratory. Additionally, membership was to include a representative from the National Institute of Standards and Technology (NIST), the Chair of the Technical Working Group on DNA Methods (TWGDAM), and a judge. The head of the Board was the Director of the FBI. The initial Board consisted of sixteen members, including three non-voting members: an executive secretary, a quality control/assurance specialist and a medical/legal ethicist (Eisenberg 1999).

The Board was mandated by Congress to provide quality assurance guidelines, including standards for testing the proficiency of individual laboratories as well as the competence and skill of individual analysts. The Act directed the Advisory Board to adopt TWGDAM's quality assurance standards as interim standards (Eisenberg 1999). This is notable, given that in 1988, there were only two or three members of TWGDAM who knew *anything* about DNA profiling. The DNA Advisory Board produced two documents, "Standards for Forensic DNA Testing Labs" and "Standards for Convicted Offender Labs" (Federal Bureau of Investigation 1999a, 1999b).

In Accordance with the DNA Identification Act of 1994, the Director of the FBI specified that the Board was:

- To develop, and if appropriate, to periodically revise and recommend standards for quality assurance, including standards for testing the proficiency of forensic laboratories, and forensic analysts, in conducting analysis of DNA.
- To recommend standards, which specified criteria for quality assurance and proficiency tests, to be applied to the various types of DNA analysis used by forensic laboratories, including statistical and population genetics issues affecting the evaluation of the frequency or occurrence of DNA profiles calculated from pertinent population database(s).

- To recommend standards for acceptance of DNA profiles in the FBI's Combined DNA Index System (CODIS) which took account of relevant privacy, law enforcement and technical issues.
- To make recommendations for a system for grading proficiency testing performance to determine whether a laboratory is performing acceptably (Eisenberg 1999, 1-2).

A subcommittee to deal with population genetics issues was established and was comprised of Dr. Bruce Budowle (FBI), Dr. Chakraborty (University of Texas), Dr. Bernie Devlin (University of Pittsburgh), and Dr. Fred Bieber (Eisenberg 2000, 6). [10] Under the direction of Dr. Joshua Lederberg the Board strongly recommended the endorsement of the recommendations of the *second* NRC committee on DNA technology. It was felt that this document went a long way to resolving the population genetics and statistical issues involved in DNA typing, but that there were still a few questions. These few remaining questions were the rationale for the establishment of the subcommittee. Mostly the subcommittee focused on how DNA analysts should deal with mixtures of DNA (such as rape cases with multiple semen samples from the vaginal swabs, or mixed blood samples), database searches, source attribution and whether relatives of the accused needed to be accounted for in probability estimates. In most cases, the DNA Advisory Board recommended that decisions had to be made on a case by case basis (Eisenberg 2000, 6).

The Board decided that compliance with its recommended standards would be enforced by requiring accreditation from the national Association of Crime Laboratory

[10] The composition of this Subcommittee is extremely interesting. For the most part, it is comprised of people who were violently opposed to the recommendations of the *first* NRC committee: Bruce Budowle had lobbied all throughout the DNA Wars for the re-instatement of the product rule, which NRC2 did. His only deviation was the article published with Eric Lander just before the O.J. Simpson trial where he said that he had no objections to the ceiling principle except that it was "overly conservative." Ranajit Chakraborty, the co-author of the rebuttal to the article by Lewontin and Hartl that began the DNA Wars. Bernie Devlin, a statistician who worked tirelessly with his wife, Kathleen Roeder and colleague Neil Risch to provide scientific evidence to substantiate the FBI's position and the validity of its databases. All these people were deeply involved in the DNA Wars, on the "pro" DNA side, but also on the "pro" FBI side.

Directors, Laboratory Division (ASCLD-LAB). This would insure the existence and operation of quality assurance and proficiency programs. Rather than setting a date by which laboratories should be accredited, the Board recommended that they seek accreditation with "all deliberate speed" (Eisenberg 1999, 2). Ironically, the FBI's own testing laboratory did not attain accreditation until quite late – June of 1999 (Adams 1999). On October 1, 1998, the FBI Director's "Quality Assurance Standards for Forensic DNA Testing Laboratories" was put into place and superseded the existing TWGDAM quality assurance guidelines. For laboratories across the country, the ramifications of this document were that to continue to receive funding under the DNA Identification Act of 1994, or to participate in the National DNA Index System, participating laboratories must follow the new standards, which required laboratory accreditation. However, since these new standards originated from the communal work of TWGDAM, the crime laboratory community did not experience a large change (Eisenberg 1999).

There was some concern among the community about the cost of the regular audits required by the DNA Advisory Board. An ASCLD-LAB audit or accreditation was an extremely expensive process, especially for large laboratories (Eisenberg 200, 9). At the end of 2000, the FBI was trying to write one document that all community members could use as a guideline for audits. Most labs were being asked to undergo audits every two years, and the consensus among the community was that "if a single document could be put together that could be utilized by as many of these groups doing audits and/or inspections, accreditations, that would go a long way to trying to simplify the process, [and] perhaps make the process much more cost-effective to laboratories" (Eisenberg 2000, 9).

Proficiency testing had been well addressed as a matter of concern in the first NRC report on DNA Technology. Under section 210303 of the DNA Identification Act of 1994, the DNA Advisory Board was charged with recommending quality assurance and proficiency standards for DNA testing laboratories. The DNA Identification Act required that The National Institutes of Justice investigate the possibility of independent blind proficiency testing. Blind proficiency testing is where the laboratory has no idea that the test it is performing is a part of a proficiency test, and not a real forensic sample. After considerable investigation, it was concluded that blind proficiency testing was simply not feasible, defining "feasible" as practical and possible in terms of costs and logistics. A blind proficiency test is very complicated to organize and conduct successfully. It requires the cooperation of the police, who must submit a sample to the lab as if it were real. All stages of the chain of custody must be maintained as if the sample were a real, valid forensic sample. For this to happen consistently and frequently places a considerable burden on police forces who are already understaffed and overworked. Logistically, so many parties have to be involved for blind proficiency testing to be carried out successfully that the concept has been abandoned. The National Institutes of Justice concluded that "by defining 'feasible' as possible and practicable in terms of costs and logistics ... a national blind proficiency testing program employing ... blind proficiency tests via law enforcement agencies, conduit laboratories and/or blind analyst models, ***is not feasible at this time***" (Eisenberg 1999, original emphasis).

The National Institutes of Justice included three recommendations in their report. First, that the new accreditation standards and the quality assurance guidelines advocated by the DNA Advisory Board be given some time to take effect. Second, that the DNA Advisory Board provide guidelines for more stringent external

case audits to be administered by ASCLD-LAB or another accreditation body as part of the accreditation process. Third, they acknowledged that while blind proficiency testing is possible, it is fraught with problems, including high costs, and that a blind proficiency testing program be deferred until it could be seen how the first two recommendations were taking effect (Eisenberg 1999, 3-4).

The DNA Advisory Board made several recommendations to the Director of the FBI. Proficiency tests were to be graded as satisfactory or unsatisfactory. A grade of satisfactory would be attained if there were no errors in the DNA typing data. The standards have built into them a requirement for upgrading the educational background and experience of forensic scientists. The guidelines specify a minimum level of education required to act as a Technical Manager or Leader. The Board strongly felt that individuals holding this position must have a minimum of a master's degree in biology, chemistry or a forensic science area, as well as a minimum of 12 credit hours in biochemistry, genetics or molecular biology or equivalent courses which would provide an understanding of population genetics and the foundations of DNA analysis. In addition, the Technical Leader must have a minimum of three years of experience working with forensic DNA (Eisenberg 1999). The Board recognized that there were many individuals acting as Technical Leaders who lacked the formal educational requirements they recommended, but who did have the three years of experience required. This issue was of great concern to the members of the crime laboratory community (Eisenberg 2000, 5). The American Association of Crime Laboratory Directors (ASCLD) and ASCLD-LAB established a credentials committee to review the credentials of such individuals and grant waivers for the educational requirements. This opportunity existed for two years from October 1, 1998, and the waiver is permanent and portable between laboratories.

The DNA Advisory Board was originally scheduled to be disbanded on March 9, 2000, but the Director of the FBI extended its tenure to December 31, 2000. It was recognized that the standards for forensic DNA laboratories and for laboratories dealing with convicted offender databasing may need to change after the DNA Advisory Board had been disbanded. In this case it was recommended that the Director of the FBI take his or her advice from the renamed TWGDAM: the *Scientific* Working Group on DNA Methods (SWGDAM). It was understood that this group would make recommendations of revisions to standards as necessary to the Director of the FBI after the disbanding of the DNA Advisory Board.

Within this recommendation is the distillation of all the hands-on work that TWGDAM members did in the late 1980s, as they learned to read and interpret DNA profiles, and as they simplified and standardized DNA profiling protocols. Ten years later, the informal social structures of interpretation developed between members of TWGDAM had been translated into formal, *legislated* standards of quality control and quality assurance. Over the decade between 1988 and 1998, TWGDAM members trained themselves, and attained such professional credibility that this community which previously had lacked any knowledge of DNA was put in the important position of advising and overseeing all aspects of its forensic application in the United States. This transformation of TWGDAM, from a group uninformed in the workings of DNA profiling, to a body overseeing its forensic application, is an example of the translation of individual agency and practice into social structure.

g) National Commission on the Future of DNA Evidence

The National Commission on the Future of DNA Evidence was commissioned by Attorney General Janet Reno, with the mandate of maximizing the value of DNA evidence in the criminal justice system (Travis 1999). DNA was seen to be central to

the criminal justice system because it covers the "entire waterfront in the investigative process, all the way from the collection of the evidence all the way into the courtroom and into judicial proceedings which occur even after the trial itself. That makes this an ideal test of the steps that we'll need to go through to bring the science out of the laboratory and to put it into the world where it will actually work."

The Commission had its first meeting on March 18, 1998, in the Great Hall at the Department of Justice, in Washington, D.C. The Commission was chaired by the Honorable Shirley S. Abrahamson, Chief Justice, Wisconsin State Supreme Court. The Executive Director of the Commission, Assistant U.S. Attorney Christopher H. Asplen, defined the five areas to be addressed by the Commission:

> (1) the use of DNA in post-conviction relief cases, (2) legal concerns including *Daubert* challenges and the scope of discovery in DNA cases, (3) criteria for training and technical assistance for criminal justice professionals involved in the identification, collection, and preservation of DNA evidence at the crime scene, (4) funding for essential laboratory capabilities in the face of emerging technologies, and (5) the impact of future technological developments on the use of DNA in the criminal justice system (National Commission on the Future of DNA Evidence 1998).

The Commission had a two year charter, and hoped to focus in-depth in each of these areas. The Commission held its first meeting on March 18, 1998, and its last "DNA Summit" July 27-28, 2000. Jeremy Travis, Director of the National Institute of Justice, recounted to the group that the Commission had its genesis in a phone call received from Janet Reno several years before the first meeting. Reno had read a newspaper article about an individual who had been incarcerated for eleven years, and who had been pardoned when DNA evidence, kept from his trial, had been analyzed and shown that he could not have been the person who committed the crime. Reno's question to Travis was 'how many cases like this are out there', and 'what can we learn from this'? (Travis 1998, 1). From this question an investigation of

28 cases was undertaken, and resulted in a book "Convicted by Juries, Exonerated by Science" (Connors et al 1996).

Travis and Commission members viewed the issues facing the Commission as being at the intersection of science and the law. A focus group including scientists, law and forensic practitioners was convened, and they asked "[h]ow do we think about the implications of this rapidly emerging area of scientific development for a very broad set of policy questions and area of practice?" (Travis 1998, 1). Travis felt that it would be in the best interests of the Institute of Justice, and the nation, to bring some "smart and wise" people together "on behalf of the nation and consider some of the very far-ranging implications of the advance... in the science of DNA" (Travis 1998, 2).

In addition to the five areas of focus outlined above, United States Attorney General Janet Reno had given the Commission several goals, which Jeremy Travis, Director of the National Institutes of Justice termed "stretch goals" because of the difficulty of attaining them within the time specified. The main objective was to interface with the scientific community and bring down the cost of DNA testing from several hundred dollars per test to tens of dollars per test. The second goal was to reduce the time involved in doing the test, reducing it from months to virtually immediate results. Reno's third goal was to make the test available right at crime scenes, rather than in DNA laboratories. In his words, the goal was to "think about the implications of the science for practice, and to also try to make the science more readily available to practitioners around the country" (Travis 1998, 3).

At the time of the formation of the Commission, the second National Research Council report had been out for two years. Ostensibly, arguments had been settled, and controversies had been closed. However, Justice Abrahamson's remarks about

the constitution of the Commission indicate that there were still factions within the social worlds which utilized DNA testing. She said that the "NIJ has attempted to gather at this table people from diverse backgrounds, people that may not necessarily agree with each other, people from professions who may have different points of view and perspectives and may not sometimes want to sit at the same table with each other. That's why we're a little separated, but we tried to put people who do agree next to each other, so we can proceed" (Abrahamson 1998, 2). The members were drawn from law enforcement, defense and prosecution lawyers, the National Academy, trial and appellate judges, victim advocates, laboratory personnel, ethicists and forensic scientists. Academic scientists were represented, including the chair of the second NRC committee, James Crow.

Over its two year tenure the Commission determined that law enforcement personnel at all levels, across the country, needed to be more informed about DNA technology, how to collect the evidence correctly and pass it along for testing. But more importantly, they decided that the new focus of DNA testing should and would be on post-conviction testing and exonerating the innocent (Leary 2000). Stories abound in the minutes of the Commission's meetings. One I remember particularly, about an individual the police were *sure* was guilty of rape, was recounted at the Commission's last meeting on July 28th, 2000 by Dwight Adams of the FBI:

> [There was a] case in Mansfield, Ohio where two victims of a rape identified in a lineup the individual pictured there [in a newspaper article held up by Adams] by the name of Earl Fuller. There was also other circumstantial evidence pointing to Earl Fuller as the rapist, but after DNA testing was performed in our laboratory on the evidence and his known blood sample, we showed that Earl Fuller could not have been responsible for those two rapes, although they were conducted or perpetrated by the same individual. The police thought that they or we had made a mistake, so they resent [sic] another known blood sample from Earl Fuller. We did it again. It showed the same result. We did it a third time. It still showed the same result. They believed us

> finally, and Earl Fuller was let out of jail. Ultimately the police identified the right individual, his blood sample was submitted and that individual was ultimately convicted (Adams 2000).

h) The Innocence Protection Act of 2001

In 2000, Congress passed the DNA Analysis Backlog Elimination Act and the Paul Coverdell Forensic Sciences Improvement Act, which together authorized an additional $908,000,000 over 6 years in DNA-related grants. The National Commission on the Future of DNA Evidence recommended to Attorney General Janet Reno that post-conviction DNA testing be permitted in the cases in which was deemed to be appropriate. In direct response, Congress passed the Innocence Protection Act of 2001 "to reduce the risk that innocent persons may be executed." The Act reflects a deep concern with the death penalty, and the travesty of justice that would occur if an innocent person were put to death.

Congress felt that in the past decade DNA testing had emerged as the most reliable forensic technique for identifying criminals when biological material was left at a crime scene. Additionally, "the scientific precision of the technique allows for the conclusive establishment of guilt or innocence in many cases." DNA testing was not widely available prior to 1994, and newer procedures have made it possible to obtain conclusive results with minute amounts of DNA. The Act notes that through the use of DNA testing, more than 80 people had been exonerated in post-conviction hearings, including ten individuals on death row, some who were within days of execution. The Act also allows DNA evidence to be used in a motion for a new trial long after the three year Federal limit and the two year State limit.

The Innocence Protect Act also supplies funding for post-conviction DNA testing in the Federal and State criminal justice systems. Because the defense bar is often under-funded, the Innocence Protection Act of 2001 has a clause ensuring

"competent legal services in capital cases," and it ensures the necessary funding to make that "competence" happen. In addition, the Act specifies that it is the responsibility of the Federal government to make sure that both sides in a legal case involving DNA evidence have the resources to utilize that evidence – that is, to pay for DNA testing. In the long run it was felt that this would enhance the "reliability and integrity" of the adversarial system. States accepting funding under the Innocence Protection Act must preserve DNA evidence indefinitely.

i) The Innocence Projects

One of the outcomes of the stabilization and widespread acceptance of DNA profiling technology is that the same lawyers who so wholeheartedly spearheaded efforts to de-rail the technology in its early years -- Barry Scheck and Peter Neufeld (defense counsel in the 1989 *Castro* case) are now utilizing the same technology to free those who have been wrongfully incarcerated. Scheck and Neufeld co-founded the DNA Task Force, under the auspices of the National Association of Criminal Defense Lawyers. In the early 1990's, these two lawyers quickly disseminated information to defense attorneys across the country which undermined the credibility of DNA profiling and provided under-funded defense attorneys with credible tools with which to attack the prosecution's powerful new form of evidence. After Eric Lander's involvement in the *Castro* case, the Task Force's first major "finds" were the reports written by Richard Lewontin and Daniel Hartl in the *Yee* case (provided by Scheck and Neufeld), which were faxed across the nation at lightning speed.

In 1992, Barry Scheck and Peter Neufeld co-founded the first Innocence Project, at the Benjamin N. Cardozo School of Law at Yeshiva University in New York City. Despite their robust opposition to the technology in the courtroom, Neufeld says that he and Scheck realized

> [A]s far back as 1989, 1990 – about the same time that the FBI opened up the first major DNA laboratory in the country to handle criminal cases – is that this is a much more robust technology than what they've been using for the last thirty years. We always had a feeling that things like eye witness identification are not terribly reliable. Wouldn't it be interesting to go back and look at some of those convictions with this more powerful technology and see whether or not they got the right man? So that's when it started, around 1990/1991, looking at these old cases (Kreisler 2001, 2).

The Cardozo Innocence Project[11] relies on the volunteer labor of law students and attorneys, who review thousands of cases from incarcerated people who claim they have been wrongfully convicted, usually of rape or murder. When appropriate, the Innocence Project arranges for DNA tests that might help to support their claim of innocence. As of April 2001, 87 convicts had been released from prison after DNA tests exonerated them. The Cardozo Innocence Project assisted in more than 45 of these cases (Chebium 2000, 2). One of the hurdles that Scheck, Neufeld and their volunteers faced was that many law enforcement agencies did not collect DNA evidence, or they did not know about the technology. In some cases the evidence was deliberately destroyed.

Professionally, Scheck and Neufeld have lofty goals: nothing less than the "complete overhaul over the criminal justice system with a new awareness of how to make it more reliable" (Neufeld, quoted in Chebium 2000, 3). Scheck and Neufeld were lobbyists for the Innocence Protection Act, which requires that the government pay for DNA tests which could prove an inmates' innocence (Chebium 2000, 2). The Cardozo Project carries an active caseload of 200 to 300 cases, with a backlog of 1000 cases (Kreisler 2001, 3). Since the formation of the Cardozo Innocence Project,

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[11] More information on the Cardozo Innocence Project's can be found at: http://www.cardozo.yu.edu/innocence_project/.

two dozen other law schools have set up Innocence Projects to handle the increasing caseload (Chebium 2000, 3).[12]

San Diego is the first county to provide free DNA testing for convicted offenders who claim they are innocent, and where DNA testing might exonerate them. The Innocence Projects have lofty goals, but they are limited in what they can do. Elisabeth Semel, who directs the American Bar Association's Death Penalty Representation Project, says that there are "wide and deep" problems in the criminal justice system, including differential treatment of minorities, and inadequate legal representation (Chebium 2000, 3-4). It is interesting that a technology which needed such massive efforts to establish the circumstances under which it could uniquely identify a human being, is now routinely used to exonerate the innocent – preventing some from ending up in prison, and providing the evidence for new trials to release others. This part of the DNA profiling story deserves its own history.

j) The FBI DNA Laboratory

The FBI Laboratory is a one of the largest forensic laboratories in the world, offering a full range of forensic services. The FBI Laboratory examines evidence from local, state and federal law enforcement agencies *free of charge,* and provides expert witnesses to appear in court. There are several sections of the FBI Laboratory which relate to DNA profiling. The *DNA Analysis Unit I* examines blood and DNA from all sources for all Federal agencies, United States attorneys, military tribunals, state, county and municipal law enforcement agencies in the United States and its Territories, and provides expert witnessing at the national and international level. This

[12] Other Innocence Projects include the Remington Center Innocence Project, affiliated with the University of Wisconsin, Madison Law School; the Innocence Project Northwest, affiliated with the Washington School of Law in Seattle, Washington (http://www.ipnw.org) and Northwestern University's Center on Wrongful Convictions.

unit examines crime scene evidence to determine if biological material is present, and if it is, the material is then subjected to DNA analysis. DNA profiles obtained through analysis in the DNA Analysis Unit I are stored in CODIS (Federal Bureau of Investigation 2000b). The *DNA Analysis Unit II* has three major responsibilities. It conducts research and does casework involving mitochondrial DNA,[13] missing persons, and the Federal Convicted Offender Program. There is a database consisting of the DNA of missing persons, called the National Missing Persons DNA Database. The database was proposed in 1996, and received funding from Congress in 1999. This program collects mitochondrial DNA from missing persons (if possible) and their mothers, brothers or sisters, and attempts to match the samples, aiding in the identification of deceased missing persons.

In May of 1998, two joggers found bodily remains on an old logging road in New Hampshire. Among the personal effects found near the body was a credit card from a 29 year-old-woman who had disappeared in July 1969. The medical examiner could not positively identify the woman. The woman's remains were sent to the FBI Laboratory's DNA Unit II for mitochondrial DNA analysis in October of 1999. The mitochondrial DNA sequence of the unidentified remains were compared to a mitochondrial DNA sequence from a blood sample from a maternal relative of the missing woman. These two DNA types matched. Further support that this woman was

[13] The nucleus of most cells in the body contain nuclear DNA, which is inherited from both the mother and the father. It is the DNA examined in the RFLP analysis discussed in this dissertation. Mitochondrial DNA is found outside the cell nucleus, in the cell's mitochondrion, and it is inherited only from the mother. Mitochondrial DNA analysis is used when the crime scene evidence consists of hair, bone, teeth, or degraded blood samples, which contain very little nuclear DNA. Mitochondrial DNA analysis is extremely sensitive, and can obtain results from old, degraded DNA samples. Also, because mitochondrial DNA is inherited only from the mother, it can be used in missing persons cases, to link mothers, brothers and sisters to missing persons. However, because all maternally related people carry the same mitochondrial DNA, it is not suitable for the purposes of unique identification, as is RFLP DNA analysis (Federal Bureau of Investigation 2000b).

who the DNA analysis pointed to was obtained when the sequences were searched in the mitochondrial population database, and no match was found. This was a "cold case" which probably could not have been conclusively solved without DNA analysis (Federal Bureau of Investigation 2000b).

The Federal Convicted Offender Program collects DNA samples from felons convicted of federal offenses and enters their profiles into CODIS. These profiles can be searched against any profiles in the three-tiered CODIS system. This program received funding in 2001. The third DNA related aspect of the FBI Laboratory is CODIS, the Combined DNA Indexing System, discussed in the next section (Federal Bureau of Investigation 2000b).

4) Summary

This chapter has presented some of the most important and salient social structures that were produced over the history of DNA profiling as it unfolded in the United States. The legislation, groups, organizations, protocols and informal agreements outlined this chapter are only partial listings of each of these types of structures that have formed in the country. Many more exist than I have detailed here. Some of these include the following:

- National Forensic DNA Review Group (Abrahamson 1998, 3)
- TWIGLET (Technical Working Group on Law Enforcement) (Abrahamson 1998, 3)
- TWIGI (Technical Working Group on Eye Witness Identification) (Abrahamson 1998, 3)
- DNA Legal Assistance Unit of the American Prosecutors Research Institute (Asplen 1998, 1)
- Forensic Science Review Board, State of New York (Barry Scheck, Commissioner). This is a regulatory body governing all crime laboratories in New York State. Scheck and Peter Neufeld helped to draft the legislation that established it (Asplen 1998, 5)

- Chicago Police Department, initiated a Cold Case Squad to investigate old homicides (Asplen 1998, 6).

Chapters Three to Six detailed the controversies, disagreements, research, processes of group formation, and consensus building that went into making DNA profiling a stable, valid and reliable method for establishing unique identity. These chapters highlighted some of the moments of contingency – times when decisions could have been made differently, resulting in a different current picture. However, the story as it unfolded seems, at the end, to be one of FBI power and hegemony. Almost before DNA profiling existed, the FBI had a clear vision of its power as a forensic tool. They had the institutional and material resources to bring the biggest names in the field to Quantico, to offer free travel, accommodation, meals, training, software, installation and support to anyone in the relevant community who would come and learn. A latent consequence of their actions was the professionalization of crime laboratory personnel. The group format of TWGDAM, of bringing together stakeholders from across the country, and working with them *in partnership* had never been tried before. Another latent consequence was the birth of many TWG's – TWGBOMB, TWGDRUG, and some of those listed above. The FBI learned that that their own resources were strengthened by bringing together professionals, and that forming solid professional groups who learned together aided in realizing their institutional goals. The latent consequences of the professionalization of the crime laboratory community deserve their own inquiry.

Formal social structures, such as legislation giving different bodies different rights and obligations, were created as a direct outcome of the development of DNA profiling. Social structures such as agreements among crime laboratory practitioners as to how to interpret autorads were the outcome of the process of negotiation which

stabilized this technology. All of these new social structures were the result of situated human labor. The labor is situated space where it took place at the behest of the FBI, in their institution, and with the material, institutional and intellectual resources of that organization at their disposal. Their labor was also situated within wider material, institutional and historical contexts. The outcome of the hard work was many forms of new social structures, which had the capacity, as social structures, to enable and constrain the behavior of people in new situations.

However, it is important to bear in mind in that while institutions come into being to further specific goals, over time, they have a tendency to drift away from their original goals (Barnes 1985, 12). Barnes makes us aware that institutions are not *necessarily* connected to the human ends which brought them into being. With a powerful technology like DNA profiling, which is the outcome of manipulating DNA, which is the "ultimate" ontological classifier – it is important to that society keep vigilant and ensure that the institutions brought into being around the need for a powerful forensic tool remain focused on that goal.

Chapter Eight

Conclusion

There is no question that since the discovery of DNA profiling in 1985 the technique has revolutionized law enforcement. Many members of the forensic community say that it is the greatest improvement in forensic science since the fingerprint, which came into vogue at the turn of the last century.[1] The now stabilized and standardized scientific knowledge embedded in a DNA profile has been put to use in the courtroom in two ways. First, when a match is declared, this procedure now provides extremely powerful evidence that the accused is the person who committed the crime, or at least left DNA at the crime scene. But this potent form of knowledge is also being used in exactly the opposite way -- to exonerate those who have been falsely imprisoned. With new procedures based on polymerase chain reaction (PCR) that can work with miniscule amounts of DNA -- even saliva from a 50 year old stamp – some people who have been proclaiming their innocence for years are having their DNA tested, and are being released from prison. San Diego County is currently the only county in the country which will pay for the DNA testing for anyone in prison who can make a good case that the testing would exonerate them.

As noted, establishing unique identity with the use of DNA profiles can be used as evidence to support conviction or exoneration. The FBI reports that in 25% to 35% of the samples sent to their laboratory for analysis, the crime scene and suspect samples do not match. Defense lawyers Barry Scheck and Peter Neufeld were the first lawyers to extrapolate these numbers – if 25 to 35 percent of DNA samples sent

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[1] Simon Cole studies the history of the dermal fingerprint in the early 20th century. He shows how the professionalization of the community of fingerprint examiners was essential in establishing the credibility of the fingerprint as a unique identifier. Interestingly, he notes that it was never proven "scientifically" that fingerprints are unique to individuals (Cole 1998).

to the FBI laboratory were showing that the accused did *not* commit the crime, then they reasoned that 25 to 35 percent of convictions prior to DNA profiling might be false convictions. This was the genesis of the Innocence Project at New York's Benjamin N. Cardozo Law School. Scheck and Neufeld were also the first lawyers to use the knowledge both ways. When they acted as defense counsel in the 1989 *Castro* case in New York they were instrumental in showing the technical deficiencies of the procedure, and introduced the problem of DNA profiling to the social world of academia. They also started the DNA Taskforce, which helped often poorly funded defense attorneys across the country to have DNA profiling evidence declared inadmissible. And in contrast to all this activity, they also started the Innocence Project, which has spawned many others across the country. However, before the Innocence Projects could be effective, the controversies surrounding the technology had to reach some measure of closure.

1) The Complexities of Closure

Closure of the various controversies in the history of the stabilization and standardization of DNA profiling is complex, because it cannot be assigned to any one or group of people, one social world or any one time. The taken for granted explanation for closure is that the "science got better" over time, and thus the controversy naturally came to an end. To take this view would be to elide the tremendous amount of work it took to stabilize, standardize and disseminate DNA profiling across the United States. This work took place in many social worlds, and was done by many people, only a few of whom were scientists. Throughout the period of the controversy, once the protocols were simplified, the RFLP procedure itself did not change tremendously. However, the constellation of social relations surrounding the technology -- in many domains -- changed enormously.

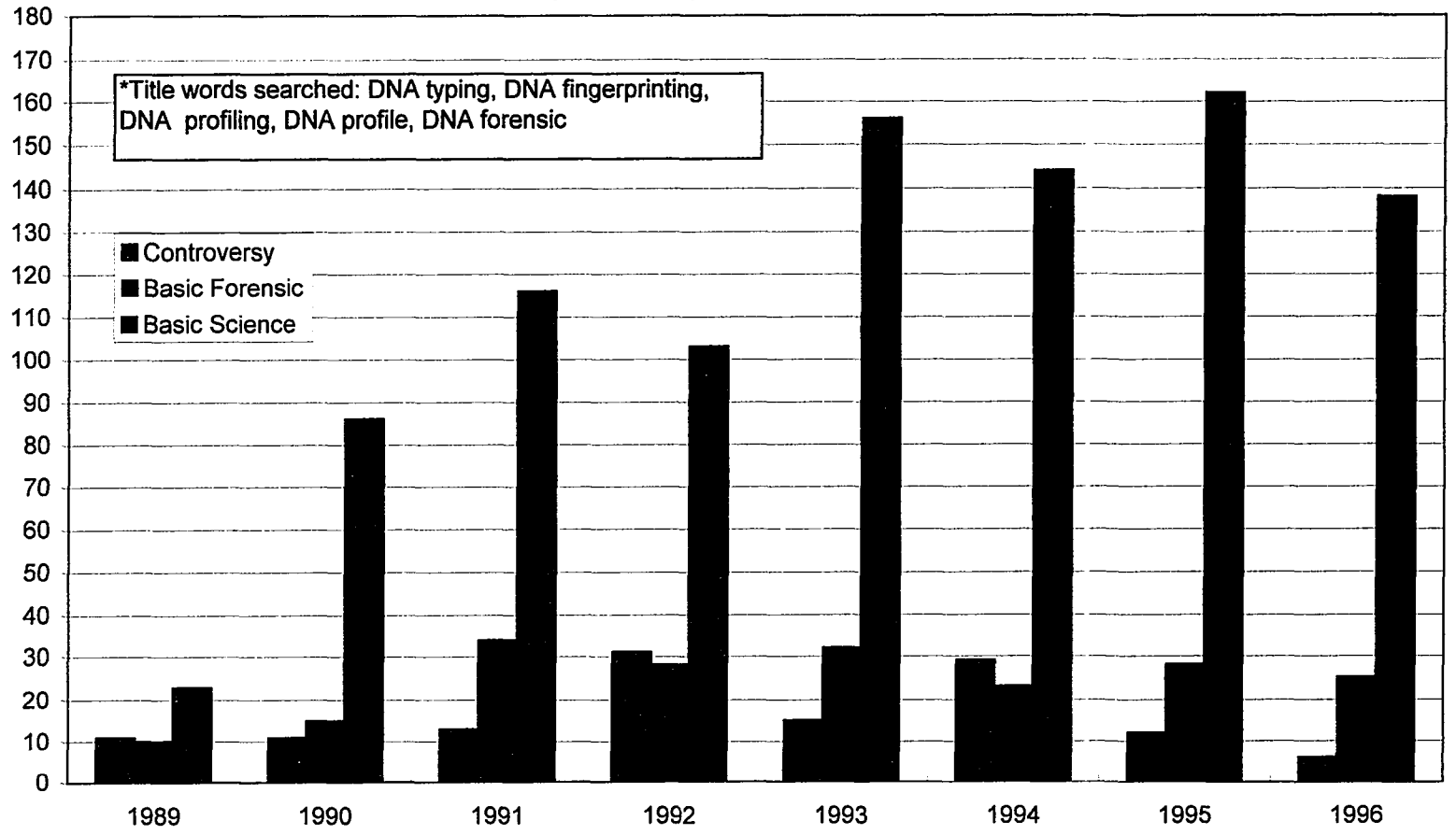
An interesting aspect of the development of DNA profiling is that the technology diffused very quickly from the forensic arena into the area of basic science. As shown in Table 3, in 1989 there were just over 20 articles published in basic science that referred to DNA profiling. In 1990, that number jumped to over 80, reached about 115 in 1991, and by 1993, there were 140 or more basic science articles published each year which referred to DNA profiling.

Different groups, in diverse social contexts, achieved closure of the many controversies analyzed in this study. Closure occurred at different times for different parts of the controversy, and in some cases, it appears just to have faded away. To some extent, it is possible to track and pinpoint the moments of closure. However, some aspects of the controversy, such as Lewontin's and Hartl's concerns over population substructuring, just seemed to fizzle out. Although Lewontin continued to protest even after the second NRC report was released, it seemed that he and his views had been marginalized, and there was no longer anyone listening.

a) Crises of Technique and Legitimation

The controversies can be classified broadly into those centering around technical issues, and those centering around legitimation. It is important to note that in every case, closure of controversies was brought about by communities of practice, not by lone scientists, or even groups of scientists (Lave and Wenger 1997). In this sense, we make a huge error if we assume that scientific controversies are always solved by scientists. Knowledge which interfaces with many sectors of society, and becomes so imbricated in the fabric of its social order, is created by many people other than scientists.

Table 2
Number of Articles Referring to DNA Typing*
Referenced by Current Contents, 1989-1996
Broken Down by Controversy, Basic Forensic and Basic Science



b) Technical Closure

Technical problems with DNA profiling arose early in its courtroom history, in the 1989 *Castro* case. Closure in this arena was brought about mostly by the efforts of the FBI and the crime laboratory community. They worked together to establish simplified protocols, and to train people who knew nothing about DNA to become competent in performing DNA profiling. The process of group formation had begun before the *Castro* case raised problems with the technical aspects of the procedure. The virtually unlimited resources of the FBI allowed TWGDAM to constitute themselves as a very strong community of practice. The formation of community is a necessary step before knowledge can be created and shared within a group (Lave and Wenger 1997).

As previously noted, the FBI was not an uninterested party in these proceedings. They had a deep interest in having crime laboratory professionals across the United States and Canada perform DNA profiling in essentially the same way, so that DNA profiles produced anywhere in North America would be comparable to each other. This would mean that the FBI could form a national databank of DNA profiles, which would aid in their major mandate of assisting in the solution of violent crimes. However, it was their success in constituting TWGDAM as a community of practice that allowed for the quick solution to many technical problems, the establishment of standards of interpretation, and protocols for quality assurance and proficiency.

It is one of the paradoxes of the history of DNA profiling in the United States that while the scientists were at war with each other, and during the time that the National Research Council convened two committees to "solve" the problems with DNA profiling, the FBI and TWGDAM – the community of practitioners – quietly

solved many technical and interpretational problems. By 1991 they had solved problems of interpretation, simplified protocols, and published quality assurance guidelines and proficiency guidelines. Their success in bringing about closure in this arena, despite the *lack* of closure in academia and the National Research Council, is evidenced by the fact that in 1994 -- the year that the second NRC committee began to meet -- the DNA Identification Grants Act was passed. This piece of legislation guaranteed money for any jurisdiction that wished to start up their own DNA profiling facility. However, access to federal money was made contingent on the laboratory adopting the FBI's protocols. The FBI and TWGDAM were also legislated as the bodies to oversee future developments in DNA profiling. These are all testaments to the position that the FBI and TWGDAM had garnered for themselves in the DNA profiling arena. The private companies had not gone out of business, but those that wished to deal with the FBI and larger state and local jurisdictions, also had to adopt the FBI's protocols for DNA profiling. By 1994 then, it seems that closure in the arena of the technical aspects of DNA profiling had been achieved. The formation of a community of practice, which became a professional community, was essential to this closure. Between 1988 and 1994, the members of TWGDAM forged trusting relationships, and members established credibility with other group members, thus allowing them to learn, share and transfer knowledge.

In academia, judging by publication activity, the DNA Wars reached their height between 1992 and 1994. A keyword search[2] of the Current Contents Database shows that there were about 30 articles published in 1992 and 1994 (see Table 3). The 1992 publications are mostly responses to the two December, 1991 *Science*

[2] Keywords searched were: DNA typing, DNA fingerprinting, DNA profiling, DNA profile, DNA forensic.

articles (Lewontin and Hartl 1991; Chakraborty and Kidd, 1991). The group of scientists who published the vitriolic prose between 1992 and 1994 were mostly those who had been called as expert witnesses in key court cases.

In the academic press, which includes publications by forensic scientists, the DNA Wars had virtually ended by 1995. There were 10 articles published that year, and half that number in 1996. This is interesting because the second NRC committee did not release its report until the spring of 1996. The release of the report did not generate much discussion, in either the academic or lay press. Instead of the front page coverage in *The New York Times* that the first report received, the second report was announced quietly on page 21 of the first section. By the time the second NRC committee released its report the DNA Wars were no longer raging in the scientific arena. Academics clearly did not see the National Research Council as the arbiter of the controversy, and the NRC report clearly did not bring closure to the controversy. This leaves open the question of the role that the two NRC committees played in the controversies surrounding the stabilization and standardization of DNA profiling.

c) Legitimation

The work of the two committees can best be seen as being about legitimation and credibility. After the debacle of the first NRC report, the Council had to do something to re-establish its credibility – in its own eyes, and probably those of the State. A highly contentious report that was widely cited as having no scientific basis was not good for the reputation, or the credibility of the National Research Council. There was a lot of bad press surrounding that report. The reputation of the NRC was not helped by the leakage of the contentious third chapter on population genetics long before publication. And then, on the eve of publication, a pre-press version of the

report fell into the hands of *The New York Times*' science writer Gina Kolata, who completely misrepresented its claims. Too much of the dirty backstage workings of the first committee had leaked out into the public eye. This alone might have been enough to call into question any knowledge claims made by the committee. However, the reports of fights among committee members, added to the violent reaction to the ceiling principle by the FBI and the academic community, meant that the National Research Council had to do *something* to re-establish its credibility. Its solution was to convene a second committee, stack it with quietly intelligent experts, and send it back to the drawing board with a very limited mandate: to solve the statistical problems surrounding the calculation of the random match probability.

The second report did just that. It is filled to the brim with complex mathematical formulas, and a correction factor for any possible population substructure, called theta. This second report provided a legitimate scientific foundation, which gave the community of practitioners a way to go on, at least as far as mathematical calculations were concerned. One ironic aspect is that the report reinstated the method of calculation that the FBI had been using in 1989, prior to the *Castro* case and the first NRC committee. A second ironic aspect is that by the time the report was released, the community of practitioners, and the community of academics had obviously been "going on" successfully for a number of years. However, had the National Research Council not convened a second committee, the negative fallout from the first committee might have hung like a pall over the Council and severely damaged its credibility.

2) Knowledge and Social Order

At the outset of this study, I defined two explicit tasks. The first was to provide a historical account informed by a science studies perspective, of how DNA

fingerprinting came to be a stable, standardized form of knowledge in the United States. The second task was to show how the activities of individuals and institutions involved in stabilizing knowledge about DNA fingerprinting brought about the creation of new social structures. There is also an implicit third task which has been hinted at throughout this dissertation. It is intimately, although not easily, connected to the second task. The implicit goal has been to articulate the relationship between knowledge and social order, and to show how solving problems of knowledge problems requires solving problems of social order – whether at the micro level of group interaction, or the macro level of federal legislation.

One of the tenets of the Sociology of Scientific Knowledge is that obtaining knowledge about the natural world requires knowledge of people. Shapin argues that the making of "thing-knowledge" involves the "ineradicable role of people-knowledge." However, the successful stabilization of "thing-knowledge" involves rendering the role of "people-knowledge" invisible (1994, xxvi). Epistemically stable knowledge, which is implicated in bringing order to our society, must appear to just "be," it cannot have a visible maker. Knowledge can be "discovered," but not "made." Knowledge in which the traces of maker are visible is seen to be "interested" knowledge, in the sense of having a vested, or hidden interest (Shapin 1994). This is knowledge which serves the purpose of the maker, or a special interest group, and is therefore not objective knowledge. Stable, successful, objective knowledge must have all traces of the labor which made it, and the subjective knowledge and social positions of the people who made it, erased, elided – *rendered invisible*. Then it is objective knowledge.

a) Task One: The History of DNA Profiling in the United States

DNA profiling followed an interesting, difficult path to stabilization in the United States. The major problem was with the correct method for calculating random match

probabilities – the probability that a given DNA profile could belong to a member of the pubic chosen at large. While initially, this seemed like a simple problem of multiplying VNTR allele frequencies across loci, it turned out to be a knotty problem which population geneticists and statisticians would not have had to deal with except for the needs of the justice system for a random match probability (Hartl 1997). It took two National Research Council committees on the subject to satisfy academicians that random match probabilities were being calculated "correctly." The first National Research Council Committee failed to come up with a procedure for calculating random match probabilities that satisfied the scientific and the forensic communities. The scientific community felt the recommended procedure, called the ceiling principle, was *ad hoc* and lacking in scientific foundations. The FBI felt that the ceiling principle provided random match probabilities that were too conservative, and was afraid that this would cripple its ability to use the new technology in the courtroom. The first official attempt at settling the controversy failed, and the National Academy of Science commissioned a second NRC committee to settle the statistical issues.

Beginning with the publication of the articles by Lewontin and Hartl (1991) and Chakraborty and Kidd (1991), the DNA Wars that erupted in the scientific community were characterized by heated and vitriolic prose, and partisan positions on the part of the scientists. An analysis of articles published on DNA profiling between the years 1988 and 1996 shows that the largest number of articles on the DNA Wars were published in 1992 and in 1994 (See Table 1). However, during this period, despite the wars raging in the scientific community, most courts continued to declare DNA evidence admissible. 1994 is also the year that Eric Lander and Bruce Budowle tried to "declare" closure to the controversies, by publishing their co-authored article in *Nature.* They did this on the eve of the OJ Simpson trial to bolster the credibility of the

technology. In the article, Budowle declared no opposition to the ceiling principle, other than that it was "overly conservative." In an interview three years later, he claimed that there was no scientific foundation for it whatsoever, and had nothing good to say about it (Budowle 1997). Eric Lander possibly had a very personal motivation for publishing the article, which was to enhance his standing in the eyes of the members of the National Academy of Science by reversing his position on DNA profiling (becoming a proponent instead of an opponent). In any event, this attempt at closure by fiat was noted, but interpreted cynically by most academics. It did not bring an end to the scientific controversies. As the *Nature* article by Lander and Budowle was published in 1994, the second NRC committee was formed to resolve remaining statistical problems, at the behest of the FBI. If no "war" or problems existed at that time, then why convene a second committee to resolve "outstanding statistical issues"?

Clearly, from someone's perspective, in 1994 the DNA wars were still raging. Richard Lewontin believes the second report was purchased and paid for by the FBI, and that it said what they wanted it to say (1997). This is a very strong claim, given the credibility of the National Academy of Science, the credibility of the National Research Council, and the perceived lengths to which the NAS and NRC go to vet committee members for conflicts of interest. I found no evidence among members of the second committee that they felt they were a "token" committee. They struggled deeply with issues of race, and with solving statistical problems which the disciplines of population genetics and statistics had not before confronted.

What I did find was that *despite* the DNA Wars, most courts accepted DNA evidence. *Despite* the DNA Wars, the 1994 DNA Identification Grants Improvement Act was passed, allowing the disbursement of start-up funds for DNA laboratories

across the country. Despite the unrest in the academic community, the 1994 Act legislated into existence a DNA Advisory Board, headed by the FBI, which tied funding for building DNA testing laboratories to following the proficiency and quality assurance guidelines established by the fledgling professional organization called TWGDAM, which was created, funded and sponsored by the FBI.

Despite the scientists' inability to settle their disputes, forensic DNA technology became increasingly entwined, formally and informally, in more and more facets of society. The Innocence Projects were started, and the technology was used to exonerate individuals who had been falsely imprisoned. The technology became part of the actions or the everyday repertoire of more and more individuals, institutions, lawyers, benchtop technicians, laboratory directors. All these individuals and institutions found a way to go on, despite the war going on in academia. They bootstrapped their way around the controversies in the scientific community, rather than waiting for them to be settled. The1994 DNA Act legislated DNA profiling into formal social structure – although technically, scientists had not yet reached agreement about how to go on, and NRC2 would not release its report for another two years. The 1994 Act gave DNA profiling *power*, as a form of knowledge, which was used to bring order to the crime laboratory community, and through them, to bring order in the courtroom. Clever lawyers used the reports submitted by expert witnesses in high profile cases in high ranking courts, such as the *Yee* appeal, in which the judge had ruled in favor of the FBI's protocols and against the criticisms of Lewontin, Hartl and others. The FBI continued to train forensic scientists in its Visiting Scientists program, the protocols for DNA testing were stabilized, and the procedure disseminated across crime laboratories in the United States and Canada. By 1998, all

50 states had passed legislation allowing or requiring the collection of DNA samples from convicted felons (see Appendix C).

None of this could have been anticipated in 1985, when Alec Jeffreys discovered the application of DNA technology to individual identity. As mentioned in Chapter One, the structural outcomes are the result of many individuals, often aided by powerful institutions, following their interests and goals. The fact that CODIS, a national DNA databank exists, is impressive. It required that all participants conduct their DNA profiling in essentially the same way. This feat of standardization, pulled off by TWGDAM and the FBI, is quite incredible.

There are two things that are important to bear in mind at this point. It would be a mistake to think that the controversy over DNA profiling was centered in the academic community. Its standardization and stabilization took place across many social worlds, and thus standardization was able to proceed, even though the scientists had not yet solved all the problems with which they were concerned. The ways in which DNA profiling became part of, and formed new social structures, follows quite closely the conditions that Bowker and Star (1999, 13-14) lay down for something to be considered a standard. Bowker and Star argue that the process of standardization is an on-going process, involving constant negotiation. Following their argument, a standard must be a set of agreed upon rules for the production of material or textual objects which span more than one community of practice (or locale) (Bowker and Star 1999).

DNA profiling fits these conditions. It is an interesting case of standardization across several domains – in particular, the 50 United States and Canada. Many of the groups of practice – the federal laboratories at the FBI, and TWGDAM, constituted of many crime laboratory practitioners from across the country and Canada, were able

to set and agree upon rules for the production of DNA profiles, to the extent that they were stable enough (similar enough) to be used form a national DNA databank (CODIS). Bowker and Star argue that standards are used to make things work together over distance and differences in measurement schemes. The success of CODIS at the national level, particularly with "cold hits", shows that DNA profiling was standardized to the extent that the 50 states and Canada could share DNA profiles, across distance, and over time.

Bowker and Star also argue that standards are enforced by legal bodies. They feel that without a means of legal enforcement, a proclamation of a standard will fail (1999). The Quality Assurance Standards produced by TWGDAM (TWGDAM 1989, 1991) were legislated in the 1994 DNA Identification Grants Act as *the* standards for crime laboratories to follow if they wanted federal money to set up their own DNA testing facilities. Despite the fact that the second NRC committee's report was not published until 1996, and in 1994 the scientists were still involved in the DNA Wars, many facets of DNA profiling were already standardized and stabilized and entrenched in new formal and informal forms of social structure.

b) Task Two: Agency and Structure, Knowledge and Order

I argued at the beginning of this study that the link between the micro level of individual action and the macro level of social structure could be better understood by utilizing concepts from science studies.[3] When scientific controversies are closed,

[3] Many of the concepts used in science studies came from sociology but were transformed by the discipline of science studies, and more specifically, by the work done by the Edinburgh school of the sociology of scientific knowledge. The way in which science studies have utilized these concepts differs from the way they are used in sociology. Most science studies investigations are historical, using contemporary historiographical orientations. These differ from more traditional histories of science in which the scientist was usually a lone "hero", and his (they were always men) success was inevitable (known as Whig history). The combination of sociological concepts with historical methods has resulted in a body of scholarship which has helped to break down a vision of science which has predominant since the Enlightenment.

and new knowledge is established, and a new social structure has come into being. The history of the stabilization and standardization of knowledge surrounding DNA profiling has served as a case study, through which I have demonstrated how individual action, exercised over time and within different institutional arenas, resulted in new knowledge and new social structures.

Through legislation, the new knowledge was incorporated into the formal social structures by which a society orders itself. This link between knowledge and social order is intricate, and this relationship has been an implicit theme, running throughout this analysis. To fit this history of DNA profiling into this model of the link between knowledge and order, we need to keep several things in mind. For my purposes, I follow the Edinburgh School of SSK in defining knowledge as accepted belief. Further, what we, in the twenty-first century define as scientists, are not the only people who "make" knowledge, and that academia is not the only domain in which knowledge is produced. On this view, knowledge can be made by many social groups in many social worlds. In this history, knowledge was made by the FBI, by TWGDAM, by the second NRC committee, and by scientists. Knowledge was made in many social locales -- the laboratory, in group interactions of TWGDAM, in the committee process of the NRC, in academic journals, and in the courtroom. The mistake in this analysis would be to concentrate on the closing of the controversial DNA Wars as the solution to the problem of knowledge vis a vis DNA profiling.

How the DNA Wars closed is not clear, but this is not unusual in scientific controversies. By 1996 publications relating to the controversy had fallen to almost nothing, and the second NRC committee released its report. While the first

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This is the view of science where all science is seen as progress, as good, and particularly, the view that the winner of scientific controversies wins because they have special access to the truth.

committee's report garnered attention on the front page of the *New York Times,* the second committee's released garnered a mere mention on page 21. However, this is not to say that the second NRC committee did not accomplish anything, it is to say that the rest of the relevant communities did not wait for this body to settle the problems with which it was charged, and publish its report (almost two years late). By the time the second NRC committee had published its report, knowledge of varying forms was, as Bowker and Star say, imbricated in the social structure. However, NRC2 laid down, in meticulous detail, the "rules" for calculating random match probabilities. It provided a solid mathematical foundation for its recommendations, and affirmed that the FBI's "way" of doing DNA profiles and calculating random match profiles was, for all intents and purposes, correct. They FBI returned to calculating probabilities in the way that they had developed in the late 1980s, with one slight change. *Theta,* a correction factor for the possibility of the existence of sub-populations, was added to the multiplication rule. The second NRC committee laid down the rules for going on. It is possible, indeed it seems quite probable, that the second NRC committee was quietly engineered to be an unproblematic committee which would provide an outcome that would be perceived by scientists and judges as a scientific consensus. However, if one peruses the second report, it is filled to the brim with mathematical formulae. The second report appears to provide the epistemic foundation required to count as "scientific grounding" for the calculation of random match probabilities.

Steven Shapin argues that knowledge and order are mutually constitutive (Shapin 1994; Shapin and Schaffer 1985). The Edinburgh school of the sociology of scientific knowledge – of which Shapin is a founding member -- makes the very strong claim that social order is constituted by cognitive order, and vice versa. They

argue that how we maintain ourselves in communities is intimately connected to what we hold to be true about the world. One of the contributions of this study is to explicate the way in which social order and cognitive order (knowledge) are linked. This study has shown that problems of knowledge and problems of order are solved together, but the intervening mechanism by which that solution occurs is the formation of social structure.

The claim that social order and cognitive order are intimately linked is a powerful one, and despite its foundations in Durkheimian sociology (Durkheim [1912] 1965) it is unfamiliar to most sociologists.[4] Steven Shapin's answer to the problem of order – why society is not a war of all against all? -- is that

> the fabric of our social relations is made of knowledge -- not just the knowledge of other people, but also knowledge of what the world is like – and, similarly ... our knowledge of what the world is like draws on knowledge about other people – what they are like as sources of testimony, whether and in what circumstances they may be trusted (Shapin 1994, xxv-xxvi).

The credibility of persons and things is at the heart of both stable knowledge and social order. In this history of DNA profiling as it unfolded in the United States, the role of knowledge in bringing about social order is more explicit than it might be in other situations. The criminal justice system and the judicial system are the social structures we turn to when the order of our lives is disrupted – by a robbery, a rape, a murder – or a crime against person or property. In this case the interface between the legal system and science provided a unique opportunity to see the extent to which knowledge and social order are linked in the twentieth century. Shapin and Schaffer

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[4] In the last chapter of Durkheim's last major work, the *Elementary Forms of the Religious Life,* we can see Durkheim struggling with a nascent understanding of the social foundations of knowledge. Durkheim, being a child of his time, struggles within the constraints of the empiricism he worked so hard to validate. His analysis in final chapter of the *Elementary Forms* lays the groundwork for a social basis of knowledge – he just cannot quite make the leap to including scientific knowledge in this framework, but if you read between the lines, the idea is there (Durkheim [1912] 1965).

claim that that to know anything – to "have" knowledge – requires politics. To call something "true" requires that rules of association between people have been laid down, and that they have agreed to follow certain conventions in their dealings with each other (1985, 342).

Shapin's claims are made about the 17th century, where knowledge makers had not yet differentiated into fields or disciplines. Rather, they were known as "natural philosophers." In the 20th century, there are fields and disciplines, each with a specific "jurisdiction" (Abbott 1988) over who has the right to make truth claims about particular bits of the natural world. Professional groups have earned the right to self-police. As we saw in the DNA Wars, intense controversy can arise if even very smart people from the wrong discipline try to make knowledge production rules for another discipline. Following Shapin (and Durkheim), any group that produces knowledge must have in place either explicit or implicit rules on how to make knowledge. The group may have to establish their own conventions about knowledge production, what kinds of things or events can be questioned, what can normally be expected to happen, what is unexpected, and perhaps the most difficult to see -- what kinds of knowledge and understandings are taken for granted. A group must agree on events which count as anomalies, and what kinds of occurrences constitute evidence and proof (Shapin and Schaffer 1985, 225). These are social because they are established through interaction: agreements become conventions. They are also integral to the production of knowledge.

To bring these claims into the 20th century, and into the context of agency and the creation of new informal and formal social structure -- or social order -- I will draw some examples from the stabilization of DNA profiling. When the members of TWGDAM were meeting for the first year or so, they established a "politics" that

worked for the group. Shapin says that "rules and conventions must be laid down between the people attempting to make the knowledge." This group worked out rules and conventions as they went along. Events such as "The Good, the Bad and the Ugly," where they argued and hashed out "correct" interpretations for difficult autorads were exactly the group processes that allowed them to stabilize what I call an informal structure of interpretation. An example where a group failed to successfully work out rules and conventions that would increase the probability of producing knowledge is the first NRC committee. During the tenure of the committee, two strong personalities, Eric Lander and Thomas Caskey, were at loggerheads over a practical solution to the problem of how to calculate random match probabilities. Amidst accusations of conflict of interest, Caskey resigned at the last meeting on December 21st, 1991, and the rest of the Committee reluctantly agreed to propose Lander's ceiling principle as a conservative solution to the problem of calculating random match probabilities. However, this solution failed to please the FBI, as they felt it was *too* conservative. They asked for another committee, and they got it. The solution provided by NRC1 also met with great hue and cry from the academic population genetics and statistics communities, as they could find no "scientific grounding" for the principle.

I argue that at least part of the failure of the first NRC committee to produce a working solution for calculating random match probabilities is that they were unable to establish a working order as a group. There were strong cleavages within the group, strong likes and dislikes at the personal level, and an utter failure to find a way of "going on" peacefully. They failed to establish conventions for knowledge production as a group of experts focused on one particular problem, and the conventions of knowledge production and good behaviour that they brought with them from their

home disciplines seem to have been consistently checked at the door of the meeting room.

In contrast, after the debacle of NRC1, the second NRC committee was hand picked, not only for people with high degrees of credibility in their fields (which NRC1 also had), but also for the temperaments of the individual members. A chair with a strong personality, impeccable academic credentials, and plenty of time to work on the project was a key goal (Fischer 1997). Study director Eric Fischer said that he was determined that the kinds of in-fighting and fractures which characterized NRC1 would *not* happen on his committee. Fischer and his colleagues succeeded in establishing a committee which could work together.

Referring back to the 17th century, Shapin and Schaffer argue that:

> knowledge-production depends not just on the abstract exchange of paper and ideas but on the practical social regulation of men and machines. The establishment of a set of accepted matters of fact ... required the establishment and definition of a community of experimenters who worked with shared social conventions: that is to say, the effective solution to the problem of knowledge was predicated upon a solution to the problem of social order (Shapin and Schaffer, 1985, 281-2).

The second NRC committee was hand-picked to increase the probability of social regulation, and thus set the stage for successful knowledge production. The second NRC committee was successful in establishing a community which worked together peacefully. The population geneticists and the statisticians and the members from other disciplines were able to define a set of shared social conventions – procedures for calculating random match probabilities under a variety of circumstances. The group was successful at establishing what could be taken for granted, what needed to be questioned, what the "normal" state of affairs was, and what constituted an anomaly. More importantly, they were able to lay down clear rules

for "going on." They gave the crime laboratory community, academia, and other stakeholders "scientifically grounded" procedures for calculating random match probabilities.

One of the reasons that NRC2's procedures were seen as being scientifically grounded is that they were successful at creating order and establishing conventions of knowledge production. The first NRC committee could not transcend the cleavages between its very strong personalities, who were on opposite sides of the fence. The first NRC committee failed in part because it could not keep its dirty laundry -- its backstage workings -- out of the public eye (Hilgartner 2000). Their quarrels and in-fighting spilled outside the Academy and into the light of day, partly through leakage of contentious chapters. They failed to work together peaceably, and failed to produce a credible product.

The second NRC committee was extremely successful at keeping their backstage work hidden, which made the report they produced more credible (Hilgartner 2000). They laid out in meticulous detail the kinds of things that could be taken for granted – for example, they established that random match probabilities calculated with the "wrong" database did not result in probabilities that differed substantially from those calculated with the "correct" database. So, amongst themselves, they decided that race was not an issue, that what "normally could be expected to happen" is that all random match probabilities are small, no matter what database was used to calculate them. They laid out the mathematical equations and foundations that would allow outsiders to know how to go on in the right way.

By 1997, legislation had been passed in all 50 states regarding the collection of DNA from convicted felons. In 1994, the DNA Identification Grants Act was passed at the Federal level which allocated money for the development of DNA testing

laboratories, the establishment of a DNA Advisory Board, tied funding to the following of TWGDAM's Quality Assurance Standards, and set the Director of the FBI in charge of it all. The 1994 DNA Identification Grants Act helped to further stabilize knowledge about DNA profiling by providing the material means to develop more DNA testing labs. It also helped to bring order to society, by legitimating a powerful form of knowledge. DNA profiling is a powerful new tool for the criminal justice system, different from any other forensic advance made.

In closing, we come back to the relationship between law and science. The ability to distinguish between individuals, on the basis of ancient blood stains, old saliva, or the root of a single hair, has given the justice system a sword with two blades. It can act as evidence for conviction, and it is an equally powerful tool that can provide evidence for exoneration. For many generations, people have fought for those that they believe have been falsely imprisoned. The only tools they could use to wage the battle were to find witnesses, or to hope for a confession by the real perpetrator of the crime. Peter Neufeld and Barry Scheck, the first lawyers to challenge the validity and reliability of DNA evidence, were also the first lawyers to establish a project to use it to free the innocent. In their Innocence Project, two out of every three cases investigated are found to have been wrongfully imprisoned. For Scheck and Neufeld, the relationship between a stabilized form of knowledge, and social order is freedom:

> Now the fabric of false guilt is laid bare, and the same vivid threads bind a wealthy Oklahoma businessman and a Maryland fisherman: Sometimes eyewitnesses make mistakes. Snitches tell lies. Confessions are coerced or fabricated. Racism trumps the truth. Lab tests are rigged. Defense lawyers sleep. Prosecutors lie. DNA testing is to justice what the telescope is for the optical glass, but a way to see things as they really are. It is a revelation machine (Scheck, Neufeld and Dwyer 2000, xv).

As the twenty-first century opens DNA profiling seems to be a "revelation machine." For many, its power lies in its scientific validity – it is now accepted that DNA profiling, when correctly done, can establish unique identity. That we now see it as scientifically valid lies partly in the successful erasure of the labor of the thousands of individuals who pursued mundane streams of activity over an almost 15 year period.

In *About Science,* Barry Barnes compares the institutional aspects of the army and science. He says that at no time has science turned back "on the hand that feeds it" in the way that many armies have. However, he advises caution in dealing with science and its products, *because* science is a product of social action:

> [I]t is important to remember that science is an institution[.] It is an institution the enormous potency of which has so far mainly been directed at what people consider to be worthwhile ends. But there is no necessary connection of the institution and these ends. We ourselves have to take care that the connection between science and human ends is as we would have it to be. And however long the connection remains as we would have it, we must nonetheless never allow ourselves to forget its contingency and its insecurity (Barnes 1985, 13).

Much has changed since Barnes wrote these words in 1985, but not his message. DNA profiling has been used to serve society's need for protection from violent crime, and to exonerate those who have been falsely accused of crimes. It is easy to agree that these are ends which most members of society would think are good and worthwhile. But surveillance technologies like DNA databanking, generally start with a small number of people that everyone can agree should be watched, and the circle of those deemed worthy of surveillance widens over time. Some states have now made it legal to store DNA profiles from people who have been charged, but not convicted, of a violent crime. This indicates a widening of the circle of individuals on whom the state has highly detailed, genetic information. We live in an age where

discoveries in science in general, and molecular biology in particular, are far outpacing our moral capacity as a society to deal with. There has been talk that human beings are being cloned, despite widespread agreement among scientists that they would not pursue that particular path of knowledge. Although Habermas 1968 incorrectly accepted logical positivism as an accurate picture of science, he was correct in saying that scientific knowledge can tell us what "is," but not what "ought" to be.

Here I have not tried to prove that science is a social activity – that has been ably done by others. Instead, I have tried to show how the creation, stabilization and standardization of knowledge *necessarily* results in the creation of social structure. Knowledge about DNA profiling was not just "discovered" and it did not just "appear." It was not the product of a lone heroic scientist at work in a laboratory. Stable knowledge results from the labor of many humans, interacting with nature, who find the limits to which nature can be bent to serve human interests and goals. When those limits are found, that knowledge becomes part of the fabric of our social order -- we integrate it into the way in which we live together. By looking at the production of knowledge over a long enough stretch of time, we can see how it may have its genesis in a small laboratory, but in the end, it becomes part of the fabric of our lives.

This story could have turned out differently. In the United States, small local areas, cities, counties or states have jurisdiction over different aspects of the criminal justice system. Had the FBI not invested so much money and time into creating a community of professionals, each little region would have had to develop their own DNA profiling procedures. If this were the case, it is likely that the private sector would have stepped in to fill the gap, as small counties would not have had the resources to do the expensive validation studies required by the courts. These types of studies

were required to prove to the courts that the form of DNA profiling that they were proposing to use had been generally accepted in the scientific community. It is possible that the larger counties, like Miami's Dade County, would have developed their own DNA profiling techniques. Dade County was the only county in the United States that had a molecular biologist on staff shortly after the discovery of DNA profiling, and was moving in the direction of setting up its own DNA profiling laboratory when the invitation from the FBI to join TWGDAM came in.

This case study has shown, in the domain of science and technology, how agency can result in the formation of social structure, when agency is followed over time. It has demonstrated that scientific knowledge does not reside solely in academia, but rather when new knowledge is being created, the divisions between social worlds such as the criminal justice system and academia become blurred and indistinct – such as when scientists who appeared as expert witnesses in court cases used their expert witness reports as the basis of peer reviewed publications. This study has also shown how the this new form of knowledge has become integrated into our social order, through the many ways, and the many domains, in which it has been deployed, stabilized, and standardized.

Appendix A
Glossary of Acronyms and Terms

Acronym/Term	Description
Autorad	Short term for autoradiogram or autoradiograph (they are used interchangeably. An autorad is a piece of photographic film to which bits of DNA have been pressed against for a period of time (sometimes as long as a week). The bits of DNA are treated with a radioactive "probe" or "marker" that make them expose the photographic film. In this way, they DNA leaves a photographic impression on the film.
Bandshifting	When a DNA profile is made, denatured (separate strands) DNA is inserted in agarose gels, and a difference in electrical potential is applied to the gel. There are always several bands on the gel: a sizing band, victim DNA, suspect(s) DNA, and known DNA. Bandshifting occurs when one lane on the gel runs faster or slower than the other lanes. This means that bands that should be in the same position in all lanes, appear either above or below where they should be.
Binning of VNTR alleles	VNTR allele lengths cannot be measured exactly, because current agarose gel and electrophoretic technology lack the ability to distinguish between alleles which differ only slightly in length. Therefore, VNTR allele lengths are placed into "bins," which are groups of lengths, rather than being recorded as discrete lengths. The process is similar to the way age can be coded into 5 year age groups rather than exact years, months and days since birth. The difference is that with age, we can usually establish the exact age if we want to.
CODIS	The FBI's Combined DNA Indexing System. It exists at three levels: LDIS (Local); SDIS (State); and NDIS (National). When a DNA profile is shared at the National level, it is an entry in CODIS. CODIS stores the DNA profiles of felons convicted of a federal charge, and of unknown DNA from missing persons cases, murder cases, and other violent crimes where DNA is left behind and the perpetrator is unknown.
DNA	Deoxyribonucleic acid – the molecule in the shape of a double helix that is believed to hold within in the "commands" for the phenotypical existence of an individual.
DNA Advisory Board	A Board brought into being by the 1994 DNA Identification Grants Act. The Board was headed by the Director of the FBI, and its job was to oversee proficiency and quality assurance of DNA testing in the United States.
FBI	Federal Bureau of Investigation
FSRTC	Forensic Science Research and Training Center, FBI Institution, at Quantico, Virginia.
Population substructure	Population substructure means that the population is not homogeneous, but is composed of separate groups which mate with each other. In other words, the groups do not mate at random with any member from any group. The problem is that the allele frequencies of the genes could be different in

	these sub-groups than in the large population. The problem is not unlike the analysis of variance test in statistics, where by comparing variances of the groups, the researcher attempts to decide if there are separate groups in the distribution or if the observed variation is due simply to random sampling fluctuations.
PCR – Polymerase Chain Reaction	This is a procedure which amplifies very tiny amounts of DNA, thus providing enough DNA for DNA profiling. The procedure was developed at Cetus Corporation in 1986, and Kary Mullis won a Nobel prize for its discovery. See Jordan and Lynch (1992; 1998) for a full explication of its stabilization and entrenchment as one of the new workhorses of molecular biology.
Restriction Enzyme	An enzyme that seeks out a particular sequence of DNA and "slices" the DNA at that point. Different restriction enzymes are used at different sites along the DNA molecule.
RCMP	Royal Canadian Mounted Police. The RCMP are a federally funded police force. They provide police services in small towns that cannot afford to train and maintain their own police forces, and in areas of provinces that fall outside major metropolitan areas. They work cooperatively with city police forces, as city police forces have limited forensic capabilities. All DNA testing in Canada is sent to a local RCMP laboratory.
RFLP	Restriction Fragment Length Polymorphism is a technique for DNA profiling that uses sections along the DNA molecule known as VNTRs (variable number of tandem repeats). These VNTRs vary in length from individual to individual (thus they are "polymorphic"). "Restriction fragment length" refers to the length of the fragment that is "cut" from the DNA molecule with restriction enzymes. These fragment lengths are processed and compared.
Southern Blotting	A procedure by which radioactive probes are applied to DNA which has been through electrophoresis. The probes are specific to a particular sequence of DNA, they "seek it out" and bind to it. In Southern Blotting, the gel from the electrophoresis process is pressed up against a very thin nylon membrane. This membrane is then pressed against an X-ray film, where the radioactivity leaves a "picture" of the bands on the X-ray film.
TWGDAM	The *Technical Working Group on DNA Methods*, funded by the FBI, based in Quantico, Virginia. TWGDAM was formed in 1988. Its members were drawn from the crime laboratory community of the United States and Canada
VNTR	Variable Number of Tandem Repeats – sequences of DNA that do not code for any proteins. These sequences recur at varying intervals along the DNA molecule. Each individual has a different number of repeats of the same sequence.

Appendix B

Timeline of Events in the History of DNA Profiling in the United States, 1980 – 2002

Date	Social World	Event
1980	Academia	Discovery of hypervariable regions of the DNA molecule
1983		
1983	FBI	Bruce Budowle hired by the FBI's Forensic Science Research and Training Center
1985		
1985	Academia	Alec Jeffreys discovers DNA "fingerprinting," Leicester University, UK
1985	FBI	Research begins into possible forensic applications of DNA technology
1985	FBI	Bruce Budowle goes to Cancer Lab of Dr. Raymond White at Howard Hughes Medical Institute at University of Utah. Dr. White developed one of the most commonly used and robust markers used in DNA profiling, known as D2S44 (or to White as YNH24). (He sold the rights to Promega.)
1986		
1986	FBI	Members of FBI Forensic Science Research and Training Center (FSRTC) [illegible] especially Bruce Budowle, travel to Lifecodes, Cellmark Diagnostics, and to the United Kingdom
1987		
1987	FBI	FSRTC creates a team dedicated to researching DNA profiling
1987	[illegible]	[illegible]
1987	[illegible]	First case in which DNA evidence [illegible]
1987	[illegible]	Pennsylvania says Vernon's case stands as the first [illegible]
1988		
1988	[illegible]	[illegible]
1988, May 31 – June 2	FBI	FBI hosts training seminar called "DNA Technology in Forensic Science"
1988	FBI	Holds symposia/training sessions
1988	FBI	Begins to train its own casework personnel in Washington, D.C. in DNA analysis methods
1988, July	FBI	FBI hosts its first Visiting Scientists program, a four-month program where crime laboratory personnel from across the U.S. and Canada came and learned how to do DNA profiling according to FBI protocols
1988, October	FBI	FBI case work laboratory in Washington, D.C. opens
1988, November	FBI	Forms Technical Working Group in DNA Methods (TWGDAM)

1988: December	FBI	Laboratory in Washington, D.C. begins casework analysis.
1989		
1989: January	FBI	Hosts first Technical Training Course for 40 crime laboratory members.
1989	Government	Subcommittee on the Constitution of the Committee on the Judiciary United States Senate on Genetic Testing as a Means of Criminal Investigation, March 15, 1989,
1989: March	FBI	Sixteen laboratories are on the waiting list for the 4 month Visiting Scientists Training Program.
1989: April	FBI	First case using DNA evidence from the FBI Laboratory brought to trial in Hawaii. Dr. Dwight Adams was the expert witness from the FBI.
1989: April - July	FBI Publication	TWGDAM publishes its first set of "Quality Assurance Guidelines" in the April-July issue of *Crime Laboratory Digest*.
1989	Academia	Eric Lander publishes first article critical of DNA profiling in *Nature* titled "DNA Fingerprinting on Trial"
1989: June	FBI	Hosts second Technical Training Course for crime laboratory personnel to transfer DNA protocols.
Late 1989	National Academy of Science (quasi-independent, multi-disciplinary)	National Academy of Science commissions a report from the National Research Council titled "DNA in Forensic Science."

1990		
1990, January	FBI, National Institutes of Justice, Nationals Institutes of Health, National Center for Human Genome Research, Alfred Sloan Foundaton, State Justice Institute	These bodies provide the funding needed for the National Academy of Sciences to proceed with a report on DNA profiling.
1990	FBI	Pilot Project of Combined DNA Indexing System (CODIS) database involves FBI and 14 state and local laboratories.
1990	State Pulls in Academia as expert advisors	Office of Technology Assessment commissions a report on DNA Profiling, Advisory Panel is chaired by Thomas Caskey, Baylor College; Eric Lander also a member.
[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]
1990, December	FBI	By this time, the FBI had testified in about 60 admissibility hearings as to the validity of DNA evidence.
1991		
1991, January 3	Government	Representative Frank Horton (R-NY) introduced bill H.R. 339 titled the "DNA Proficiency Testing Act of 1991." This act proposed to set aside $5 million for the purchase of DNA related equipment in State and local crime laboratories, *providing that* the lab requesting the funds would perform DNA analyses which would meet or exceed the Quality Assurance standards set by TWGDAM in 1989.
[illegible]	[illegible]	[illegible]

1991	Academia / Academic Journals	Lewontin and Hartl prepare, submit and have accepted in the journal *Science* an article critical of DNA profiling, based on their *Yee* report.
1991	Academia / Conference	At the Meetings of the International Congress of Human Genetics, Dr. Kenneth Kidd approached a senior editor of the journal *Science* to complain about Lewontin/Hartl article, others, including Thomas Caskey approach Editor-In-Chief Daniel Koshland independently.
1991, December 21	Academic Publication	Articles by Lewontin and Hartl, and rebuttal by Chakraborty and Kidd published in *Science*. **Officially sets off the DNA Wars.**
1991, December 21	National Research Council	Thomas Caskey forced off the NRC committee at its last meeting, supposedly because of a conflict of interests (he had substantial financial interests in a large DNA fingerprinting company and his laboratory had a very large grant from the National Institutes of Justice). He was the main opponent to the "ceiling principle", and when he left, the committee agreed to go forward with the recommendation that random match probabilities be calculated according to the ceiling principle.
1991	FBI	FBI using only Caucasian and Black as reference populations for calculation of random match probabilities.
1991	FBI	FBI issues legislative guidelines for the collection of blood samples from convicted offenders and the storage and analysis of these samples in State DNA databases.

1992		
1992, February 28	**Academic Publication**	**First round of letters published in *Science* in response to Lewontin and Hartl, Chakraborty and Kidd's articles in December 1991 issue of *Science*.**
[redacted]		
1992, April 14	Popular Press	Gina Kolata, science writer for the *New York Times*, publishes an article *before* the NRC report has been printed, saying that it does not support the use of DNA profiling in the courtroom.
1992, April 16	National Research Council	NRC's report "DNA in Forensic Science" rushed to print because of Gina Kolata's article.
[redacted]		
1994		
1994	Government	DNA Identification Grants Act of 1994 passed into law as Public Law 103 322.
1994	Government	1994 DNA Identification Act (Public Law 103 322) formalized the FBI's authority to establish a national DNA index for law enforcement purposes.
1994, October	Academic Publication	On the eve of the O.J. Simpson trial, arch rivals in the DNA Wars, Eric Lander and Bruce Budowle publish an article together in the journal *Nature* that the DNA Wars were over.
1995		
1995, March	Government Advisory Body	DNA Advisory Board Established.
1998		
1998, October	DNA Advisory Board	The FBI Director's "Quality Assurance Standards for Forensic DNA Testing Laboratories" was put into place, superseding the existing TWGDAM quality assurance guidelines.
1998, October	FBI	National DNA Indexing System (NDIS) became operational. NDIS is the highest level in the CODIS system – below it are the local and state levels.
1998	Government	All 50 states have passed legislation authorizing the collection of biological samples from convicted offenders.
2000		
2000	Government	Congress passed the DNA Analysis Backlog Elimination Act
2000	Government	Congress Passed the Paul Coverdell Forensic Sciences Improvement Act
2001		
2001	Government	Innocence Protection Act of 2001 passed

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