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My Path to Sexual Science

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Learning to Tell a House from a Home:

One of the most significant gifts given to me by my parents was their acceptance of my personal choices. My mother, in particular, supported everyone's right to challenge the choices society imposed. She was an iconoclast who felt betrayed by a world that permitted so much injustice and unhappiness in our lives. My father was more of a pragmatist, and less confrontational, but he had high regard for intellectual questioning and the search for new knowledge. He was more willing than my mother to adapt to the world as it was, and then quietly and privately make what adjustments were needed. My mother was more like Don Quixote, compelled to battle, even though deep down she knew she could never really win. But, her efforts were not without value, for as George Bernard Shaw has noted, reasonable people conform, and so all progress depends on the unreasonable person.

The city I grew up in afforded me an ideal laboratory to develop my own ideas about sexuality. Scranton was an anthracite coal mining town in northeastern Pennsylvania. My father moved there from New York City in the early 1930s, shortly after my sister and I had started school. He was trying to rebuild the clothing factory he lost in the early years of the depression. Scranton, like most cities at that time, was in deep economic trouble and we lived close to poverty for a few years until finally my father's persistent struggles paid off and he was able to establish a new clothing factory.

At the same time, other entrepreneurial souls in Scranton were founding a different sort of business as their way of surviving the depression years. Houses of prostitution opened; scores of them, in the alleys behind twelve of the downtown streets. The impact on Scranton economically and culturally was dramatic. Prostitution brought in a large amount of wealth to many parts of the city and as a result the sex industry gained considerable political support and it thrived throughout the depression years. On a Saturday night the population of Scranton increased tremendously. Scores of cars came in from New York City and Philadelphia and many other smaller cities, loaded with men eager to "check out the Scranton girls".

Sexual services from black and mulatto prostitutes were less expensive than the same services from a white prostitute. The cultural script on race, class and gender for the 1930s and early 1940s was indelibly written in the social and economic structure of Scranton's houses of prostitution.

The houses brought money into the eager cash registers of the hotels, restaurants, gas stations, and bars. As a young boy, even I benefited economically from their existence. No, I did not become a child-pimp or a child-performer. My part-time role was more indirect than that. It was impossible to grow up in Scranton and not know where these brightly painted "pleasure houses" were located. I recall that when I was about 10 years old, cars would stop by me when I was walking with friends near the downtown area. The male passengers would lean out the car window and say: "Hey kid, where are the cat houses?" When I gave them directions, the men in the car would usually toss me a nickel or a dime. I was fascinated by the power of sex to so easily bring so many men, so far from their homes, into an economically depressed coal mining town. It reinforced the idea to me that there must be something very special about sexuality-- something mysterious, powerful, and extraordinarily rewarding.

When I was in high school I became a customer and at times I would stay in the parlor after having sex to talk and get to know the "working girls" better. Many of them were high school ages, about 16 to 18. They would make it clear in their conversations with each other and with me that they were not going to be like the friends or sisters they knew who worked for years in some factory sweat shop for low pay, with no future. They also did some things to protect themselves and their customers. They offered condoms to all who would use them and that started me on the road to safe sex early on. Also, they were regularly inspected for disease by medical people. Many hoped to earn good money, save it, open up a beauty shop or dress shop and eventually get married. I saw some of these same women years later, and a few had achieved that goal. But many of them found it wasn't such an easy path and left or headed downhill.

Bear in mind that a good deal of the condemnation of prostitution was not from people who supported equality in society for women. The rejection of sex without affection and particularly a negative view of female casual sexuality underlay much of the conservative criticism of prostitution. Some of today's feminist opponents of prostitution have similar views and would not accept prostitution even in a

fully gender equal society. Much of this anti-prostitution view is based more on a narrow view of what is acceptable sexuality, than on a striving for gender equality. In my mind, the harm in prostitution is not in the recreational approach to sexuality, but in the gender inequality and the illegality that still permeates that profession.

Mixing in The Religious Script:

Although my parents did not live strictly according to orthodox Jewish rules, they wanted me to be exposed to that way of thinking. Both my mother and father had parents who were very strict in their religious observances. In the early decades of this century, my father's mother, Rachel Goldworm Reiss, helped found a female rights movement within orthodoxy. She was an educated woman who in addition to speaking English and Yiddish could speak Hebrew, the language of the Torah. This ability was a rarity even among religiously educated Jewish men and it gave my grandmother more influence in her efforts to equalize women's rights in Judaism. She was a very determined woman, just like my mother was, but unlike my mother, she had the good fortune of having found a cause to which she could commit her energies.

Every day after public school, until I was Bar Mitzvahed, I was sent for two hours to attend an orthodox Jewish religious school. Every Saturday morning I would attend a four hour religious service. I wasn't too enthusiastic about this extra schooling but, like it or not, that religious training did have a profound impact on my views concerning sexuality. Over time the religious school education focused more and more on interpreting the meaning of the bible, which in Judaism is the Old Testament. We studied the Torah, the five books of Moses, line by line, word by word. We started with the assumption that the Torah was the word of God, but we were all obligated to examine it very carefully and argue the case for different interpretations of that holy script. I did not know it then, but I was learning a respect for logic and reasoning and careful examination of the deeper significance of human belief and behavior. That training was to serve me well in my professional work long after I gave up on Orthodox Judaism.

But along with the respect for logic and reasoning of my religious education came the value assumption that we ought to avoid sexuality outside of marriage. This was at the same time that I was giving directions to out-of-towners as to where the cat houses were located! The religious restrictions

included masturbation as well as petting and intercourse and so there was not much sexuality that was acceptable. This religious perspective inserted a third element in my thinking: I had a pleasure emphasis from my mother; a pragmatic emphasis from my father; and now, from my religion, an ethical emphasis that promoted guilt and shame. I was receiving, in spades, a typical conflicted American sexual socialization.

The freedom my parents gave me to work things out coupled with the restraints of my religious upbringing, encouraged me to search for a way to put these opposing pieces of my sexual puzzle into a meaningful guide for living. One of the first areas that I questioned was the double standard that afforded men more sexual rights than women. I reasoned that the double standard restraints on women's sexuality inevitably led to restraints on the range of partner choices that men had, and so self interest alone spoke against the double standard. My gender equality attitudes were supported by the fact that my mother was a very assertive woman who did not take a back seat to anyone. She also was a woman who let me know that she felt that part of the reason the world had shortchanged her was simply because she was a woman. Furthermore, my sister, Carol, who was just 16 months older than me, was honored as one of the two best students in her high school class. As I've mentioned, my paternal grandmother had been an active Jewish feminist and had fought in the early 1900s against special male privileges in orthodox religion. With all that in my background how could I accept any theory of inherent female inferiority and male dominance? I felt lucky to be able to endorse male equality!

It interested me that many of the men I knew were unaware of any inequity or hardship imposed by the double standard on women. The argument they often used was: "Women like it that way" or as Marilyn Quayle put it at the 1992 Republican Convention: "Most women do not want to be liberated from their essential natures as women." That kind of explanation did not cut much ice with me. The fact that some women accepted male dominance didn't make it right anymore than some slaves accepting slavery made that right. Furthermore, as I've said, I knew many women who, even in the 1940s, outright rejected such inequality and the reasoning underlying it.

A Sex Educator in the Army

Another important influence on my thoughts and feelings regarding sexuality came from my hitch in the U.S. Army (1944-1946). I turned 18 in December 1943 and was drafted just weeks later. After army testing I was placed into a unit that was supposed to be sent to college for special training. Many of the men in this outfit already had started college when they were drafted and many came from upper middle class homes. Very few came from a coal mining, open prostitution town like Scranton. What most surprised me about these men was that, although they were about 18 to 22 years old, many of them were still virginal or had very limited sexual experiences. I had thought the world was like Scranton where very few boys passed age 18 without sexual experience--but I soon learned otherwise. The army changed its plans to send us to college for special training and instead sent most of us for basic training as part of a combat engineer battalion at Camp Chafee, Arkansas. The Army taught us how to build roads and bridges and then how to blow them up when we were done. Then they sent us overseas to try out our skills against the Nazis. We went to England for more training and then landed in Normandy.

Many of my army buddies saw that I was more comfortable with women than they were. I saw in these better educated men a chance to learn about things they knew more about, like good books, a wider range of music, how to play chess, and much more. As things worked out we made a fair exchange; they picked up from me ways of meeting and getting involved with women and I learned from them some of what they knew about literature, music, politics and more. I think we all benefited from the exchange. My army experience convinced me more than anything that biology is flexible and is not the major determinant of one's sexual attitudes and behaviors. These army friends were no different than my Scranton friends in their biological inheritance but they were a world apart in sexual attitudes and behavior.

Climbing up the Academic Tree

After the army I went to Syracuse University and in 1948 took a philosophy course that was particularly important in moving me in the my career direction. Our professor was critical of all premarital sex because he had witnessed how it could lead to pregnancy and disease. I rejected that global negative view because safe sex had worked very well for me in avoiding disease and pregnancy outcomes. I argued my ideas with him in class and in the written work I did for that course. I continued the discussion of this

issue in depth with the six college roommates with whom I shared the second floor of a home near campus. In order to try to convince my roommates about the worth of my ideas, I wrote a paper on choices among premarital sexual standards. This was my first written attempt at integrating the conflicting elements that existed in my own sexual socialization. In this paper, I presented the conventional view of marriage as the ideal place for a sexual relationship but I suggested that it was also acceptable to have sex under other conditions. I stressed the value of affectionate stable sexual relationships but I accepted recreational "body centered" sex as well. I felt that one could enjoy recreational sex, but it was best to make it a secondary and not a primary activity lest you miss out on the greater rewards of affectionate sexuality.

I rejected abstinence as *the* answer. I stressed the importance of the spirit and the intention in a relationship rather than following the strict letter of some demanding orthodoxy. I saw virginity as a mental state and not a physical state. Each sexual relationship actualized something very new in the world and so the fact of having had prior sexual relationships was not very important. In every new relationship, both partners were in a psychic sense virginal. I put forth these personal views equally for both men and women and rejected the double standard.

I was taking a personal position in accord with the standard I would later call "Permissiveness with Affection" and that I would herald as the wave of the future. The satisfaction I gained in writing this 1948 paper and arguing about it with my roommates encouraged me to plan to someday write a book on premarital sexual standards. Clearly, I was working out my own personal problems, but equally so, I saw that my problems reflected the cultural and social confusion regarding sexuality that has disturbed so many of our people for so long a time.

The Choice of a Career

After college I went to work for my father in his factory. But I was not happy doing that and after a year I decided to find a profession that would better fit my intellectual interests. But I wasn't sure what that might be. As my unhappiness at work increased I discussed my feelings with my old high school friend who was also one of my Syracuse roommates, Mort Friedman. I always liked to argue and debate controversial issues and one night after work Mort and I were having our usual debate about some social issue and Mort sighed, paused, and turned to me and said: "You know Ira, you really enjoy battling over

these controversial issues. Maybe you ought to be a college professor and get paid for doing that." This was a landmark experience. Mort's suggestion was so much in harmony with my gut feelings that I knew my life was about to take a dramatic turn.

I began to think intensely about going back to graduate school and getting a Ph.D. and becoming a college professor but I wasn't sure what field to choose. My first thought was philosophy for I loved the open questioning of that field but I still had my old feeling that philosophy left too much up in the air because it lacked an established tradition of empirical verification. I wanted a field that dealt with philosophical issues but that used scientific methods to help resolve differences. I also liked psychology, but I felt it was too morbid. Another college friend of mine helped clarify things for me. Ralph Forest had taken sociology and he told me that he believed sociology was just right for my type of interests and advised me to get my Ph.D. in that field. I still wasn't fully certain, but I made up my mind that I would start my graduate work in sociology but take courses in philosophy too and decide which one to major in during my first year in graduate school. My parents backed up my decision and I entered Penn State in the Fall of 1950 to work toward my Ph.D.

Academic Hurdles and the Race to the Finish Line:

The sociology department at Penn State was relatively new and had given its first Ph.D. just four years earlier, in 1946. The recipient was William J. Goode who years later was to make his mark in sociology and eventually become president of the American Sociological Association. My first sociology course decided my career choice for me. Luther Lee Bernard, a former president of the American Sociological Society, was my professor. He was 69 years old and this was to be the last course of his life but he exuded excitement about sociology and it was contagious. A few weeks into the semester I gave my first graduate report (on Egyptian Religion) to L. L. Bernard's class. I can still see L.L. staring at me with his blue eyes gleaming approval during my talk. My fellow grad students also responded with enthusiasm. Afterwards they followed me out of the class, asking me how I knew so much about Egyptian religion and how was I able to conceptualize it so clearly. I was thrilled. I had only read one book. I thought to myself: "Imagine what I could do if I really knew the area I was reporting on"!

This very positive reaction gave me the confidence that I could organize ideas into coherent patterns and make them interesting and understandable. I knew then that I could be a teacher and perhaps a writer and I could contribute something to sociology. Sociology was now my chosen major area. Philosophy would become my minor area for my Ph.D. The fire to learn was fully lit and I was eager to obtain my degree and start my career. I had found a pathway into the academy and I was anxious to fully explore what lay ahead.

By far the most influential professor for me at Penn State was a young, imaginative, free thinking sociologist named Edward J. Abramson. I took his course in social change and I liked the breadth of his qualitative view of sociology, the paradoxes he saw in society, and the personal respect he gave to even my sometimes far out ideas about society. As a newcomer to the field, I needed that sort of support, even though I covered that need up with my brash presentation of self.

Another very influential person in my first year was a fellow graduate student, William Bensch. He had just returned from a year's trip around the world and he was always willing to argue and discuss new ideas. We often disagreed but we also clearly showed the mutual respect that gave the lie to the ridicule we hurled at each others' views. Bensch, like Eddy, had the depth of cultural background and tolerance of difference to which I was attracted. I knew I had a lot to learn but I also was not about to give up my right to decide what I accepted. I always questioned things before I would accept them, even such widely accepted things as the poetic value of Shakespeare or the power of Wagnerian music. Finding acceptance for my feisty approach to learning enabled me to continue my cultural education.

My first and most lasting love in Sociology was the development of theories that make sense out of some part of our social life. And so theory construction became my major area for my Ph.D. I was fascinated to find out how things worked in society and why things worked the way they did, especially in areas of controversy, like sexuality. I minored in Cultural Anthropology for my M.A. and minored in Philosophy of Ethics for my Ph.D. Both of these minors opened up windows into the universe of ideas that I eagerly wanted to explore. The years in graduate school were an intellectual feast for me and I gorged myself as much as I could. But I wanted to be a professor and be free of other people's rules and

regulations. I rushed through and I earned my M.A. and Ph.D. degrees and made up the undergraduate sociology courses, in just three years.

In the second of those three years I went to Columbia to take my Ph.D. course work and gain a different perspective. Once there I was very impressed by the theoretical abilities and interests of Robert K. Merton. He was only 41 then and four years later he was to become president of the American Sociological Association. Most importantly he was a very stimulating and exciting scholar. Unlike Eddy Abramson and Bill Bensch, Robert Merton was not that approachable and was far more systematic and scientific in his thinking. Nevertheless, his vast knowledge and ability to explain social events was exceptional. He influenced me to accept the worth of many of the ideas of the structural/functional approach and to put more emphasis on the scientific aspects of sociology.

In my third and final year I went back to Penn State to take my language and prelim exams and write my dissertation. What happened with my dissertation project was quite anxiety provoking and revealed a great deal to me about academic life. My dissertation was an ambitious project aimed at comparing sociological viewpoints concerning how to obtain the subjective thoughts and feelings of people in society. I compared the qualitative vs. quantitative methodological positions on this issue and developed my own perspective. This issue held great interest for me and was part of what was then a widespread debate about just how scientific sociology could be in studying people's subjective beliefs. This dissertation combined my interest in theoretical explanation with my interest in controversy.

A heavy dose of departmental politics interjected itself into my dissertation project. One of the faculty members on my committee, Walter Coutu, thought he had written the answer to my dissertation inquiry in his book (Coutu, 1949). I did mention his ideas in my first dissertation draft but I had not given them prominence, nor had I endorsed them. I strongly believed in my right to academic freedom but I was soon to find out how academic politics can compromise academic freedom, particularly if you are a graduate student.

My Ph.D. advisor, Arnold Green, was an associate professor. Walter Coutu, was a full professor, who would vote on Green when he came up for promotion. This worried Arnold Green and he told me that he hesitated to set up my final oral while my conflict with Coutu was unresolved. The heart of my

dissertation dealt with whether in the study of people's viewpoints you gave priority to the meaning and significance of what you are studying or whether you gave priority to the reliability of evidence supporting what you are studying. I felt that the significance of what was being studied about people's subjective viewpoints was of prime importance even if it could not easily be replicated. I did not disregard the value of reliability but I thought of it as secondary in importance. My position was in conflict with the views of Walter Coutu. I consulted with another committee member, Seth Russell, and he advised me to take a more middle of the road position in my final dissertation draft and afford significance and reliability a more equal share of importance. In the interests of finishing up in time to take my first job I modified the final draft of the dissertation.

But that was not enough to end the conflict. Coutu came down more on the side of reliability than I did and he thought his perspective resolved the controversy. Of course, I discussed his view in my final draft but did not describe it as having resolved the controversy. I had compromised as far as I was willing to go. But I had only a few weeks left in which to schedule my final oral before leaving for my first job, and Green was still unwilling to schedule it. I was getting desperate but then I thought of a way out of this dilemma.

Since Green respected rank I decided I needed to somehow position rank on my side. Seth Russell was the chair of the department and was therefore the highest ranking person on my committee. I could not talk directly to Russell about Green's hesitancy to set up my final oral without aggravating the interdepartmental conflict further and so I took another path. I called Russell and told him that Green and I had talked about having my oral on the 21st of August. I asked him, as chair of the department, for his approval to finalize this date. Russell thought my statement about talking with Green meant that Green had agreed to set up my oral. That was what I hoped my vagueness would accomplish. Russell endorsed the date for my final oral. It was true that I had talked about that date with Green. I simply neglected to add that Green had not agreed to schedule it then. Desperate times call for desperate measures.

After I spoke to Russell I proceeded to call Green and told him that Russell wanted my final oral to be scheduled on the 21st. I knew that Green respected rank and therefore he would not deny what he thought were the chair's wishes. Also, by Russell making the decision, Green knew he could not be

criticized by Coutu. Green agreed with setting up the oral. I then called Coutu and told him that Russell and Green had agreed to set up the oral on the 21st. He did not object. The rest of my committee all accepted the date. I had played the cards in the power game and won that opening round. But what would happen at the oral was still up for grabs.

My oral was scheduled at 3:30 on Friday the 21st of August. It was the last day possible for holding an oral before the Fall semester. I had to be finished this day if I was to get my degree before leaving for my first job. The oral began with the usual formalities. The difference of opinion with Coutu came out during the oral but in muted form. Coutu was not as critical as I had feared. Also, Green sought to shift the discussion to areas that would not arouse Coutu's disagreement. Russell tried to reduce the tension by suggesting minor changes that should be made in the dissertation. Russell had shown support for me throughout my stay at Penn State and he provided the needed leadership that afternoon to keep the oral on track towards a successful completion. During the oral the other four members of the committee expressed support for my dissertation. Then the discussion ran its course and Green asked me to leave the room so they could decide if I passed.

When I went out in the hall to wait the final decision of the committee I knew the start of my career was in jeopardy and I was very unsure of the outcome. The committee could have decided to ask me to radically re-write and thereby delay my degree a full year. I very much wanted to start my first job without that burden. Finally, Green called me back into the room and informed me that although I had to make a number of minor changes in the dissertation, I had passed. He did not express much in the way of congratulations but informed me in a matter of fact fashion. But, being passed was the only outcome that mattered to me. I don't know how Coutu voted but I do believe he voted to pass me because he came to the graduate student party that my friends had that night and personally congratulated me. So perhaps the problem on my dissertation was present more in Green's anxiety about promotion than with Coutu's disagreement with my dissertation. This experience was an early lesson in the place of power in the realm of ideas. There surely was and still is an adversarial dimension to academic freedom.

My Professional World: the Early Years:

A few weeks later, in September 1953, I started in my first job at Bowdoin College in Maine. Kinsey had received his bachelor's degree there thirty some years earlier. I learned that the College was thinking of giving Kinsey an honorary degree but they changed their mind for fear of criticism from Senator Joe McCarthy who was then spewing his bigotry even into the colleges and universities of our country.

The chair of my department at Bowdoin, Burton Taylor, asked me to teach a family course. I did not know it then but that course was to afford me my first legitimate platform for expressing my ideas about gender and sexuality. Having never taken a family course, I wrote away for all the texts in that area and proceeded to try to select one. I felt I knew something about sexuality--after all I was the unofficial sex educator of the 1273rd Combat Engineer Battalion and the author of a 1948 college position paper. I decided I would read the chapter on sexuality in each textbook and choose on that basis. What I found was shocking.

Almost without exception every textbook in the family area condemned premarital sexual intercourse. Sex before marriage was painted as inevitably involving negative outcomes like pregnancy, disease, guilt, social condemnation, and the weakening of your future marriage. Your relationship might escape one or two of these; however, you could never escape all of them. The positive consequences of premarital sex like physical pleasure, psychological satisfaction, or preparation for marital sexuality were ignored or distorted. In general, pleasure, outside of marital sex, was conceptualized as evil and lustful. In the perspective of these textbooks, there was no affectionate basis for a premarital sexual relationship and selfishness and irresponsibility were the key motives.

Of course, some premarital sex fit the portrait being sketched in these textbooks but there was so much more that was left out or misrepresented. To accept what was said in these texts was equivalent to accepting as accurate a politician's description of his opponent. These textbook's treatment of sexuality were not scientific, nor were they even carefully reasoned. Data, such as from Kinsey, were either ignored or distorted. The textbooks were simply boldface traditional moral propaganda.

I was taken aback by the presentation of such dogma as social science. One of my major goals in my career was set by this experience. I vowed that I would present a more empirically based and logically reasoned view of sexuality in my family class and in my writings. Also, I would work hard to not allow my values to bias my presentation in my publications. I could not eliminate my values but I could work to keep them from blinding me as to what the world was like. Some of my early hesitancy to take a strong policy stance as part of my sociological analysis was tied to the destructive mix of value positions and sociology that I had found in those family textbooks. It took time for me to learn how to integrate my values and my policy suggestions into my work and still maintain the fairness and balance of the empirical work.

Just as I was writing my first two articles (Reiss, 1956, 1957) criticizing the family textbooks I had examined I met the woman who has been my life companion every since: Harriet Marilyn Eisman. Harriet has been my intellectual sparing partner as well as my editor and my emotional support since I met her in late January 1955, fell in love with her, and married her in early September 1955. She discussed all my writings with me and gave me much to think about. But it was only in my 1990 book that she allowed me to even list her name on the cover of the book. So as I explain my academic career, I want to here give major credit to the wonderful collaboration that Harriet and I have had on all my work since our marriage in 1955. She combined the finest traits of both my parents: the complete devotion of my mother, and the pragmatism of my father. Having Harriet in my life has made everything else in my life much more enjoyable.

Just weeks after we married I began a position at the College of William and Mary in Williamsburg, Virginia. I wrote the early drafts of my 1960 book on premarital sexual standards in the summers of 1956, 1957 and 1958. It was in that book, in the last chapter, that I predicted the sexual revolution that I believed was to fully appear in the late 1960s (Reiss, 1960a, pp.239-241). William Kephart at the University of Pennsylvania reviewed that book in sociology's top journal, The American Sociological Review in April of 1961. He commented on my prediction of college students leading a new sexual revolution by saying that in: "...the last chapter...Reiss apparently lost control of the typewriter." (Kephart,

1961, p.294). But my prediction was exactly the way our country went—so we best watch out for book reviewers who lose control of their typewriter.

I saw the major cultural trend among young people in our country to be a rejection of abstinence as “the one and only” sexual standard and as a questioning of the fairness of the double standard. The trend, as I saw it, was an endorsement of permissiveness with affection as an acceptable choice in place of these two ancient standards. In connection with writing about this new affection based standard I developed my ideas about how love develops in dyadic relationships and presented them in the form of my "wheel theory of love" which has over the years been one of my most popular articles (Reiss, 1960a, pp. 136-144, 1960b). My book also spoke about permissiveness without affection and spoke of some increases in that standard. But the dominant values in American society did not support sex without affection anywhere near as much as sex with affection. While it was true that I preferred the trends that I was reporting, I did analyze my prediction using the best data we had on cultural trends and worked hard to be as fair and unbiased as I could. The fact that permissiveness with affection did indeed become the most popular new sexual standard supported the fairness and accuracy of my empirical analysis and my theoretical interpretation of the changes in our society.

It was my questioning of the relative value of sex without affection that led me to correspond with Albert Ellis who was one of the few writers who supported a broader range of sexuality than I did. I wrote to Ellis about my reservations concerning the strong support for casual sex that he expressed in some of his writings. We corresponded and discovered that we were not quite as far apart as we thought and our correspondence and friendship built from that point forward. He is the person who first moved to found The Society for the Scientific Study of Sexuality. He convinced me to become a charter member of that organization when it began in 1957. Ellis is still a leader in confronting the dogmatism of the radical right. All of us who value choice in sexuality owe him a debt of gratitude for the many battles he has fought—and won.

The years at William and Mary rounded out my basic background in sociology and also in anthropology. There were only four of us in the department and so we each had to teach many different courses. We required a senior research paper for sociology majors. In 1958, with the encouragement of my

chair, Wayne Kernodle, I started a major study of premarital sexual standards. It was my way of testing the idea that many young people do indeed believe in newer sexual standards such as the one I called "Permissiveness with Affection." It was the increasing popularity of this new standard that I felt heralded a new sexual revolution in our country. Four of our senior majors chose to work with me on my project: Ron Dusek, Martha Fisher, Richard Shirey, and John Stephenson. John was the leader of the group and the one who would go on to get a Ph.D. in sociology, and become president of Berea College in Kentucky. All four students worked extremely long and hard during their entire senior year on this project. They had been caught up in the passion of this project and they were excited about the possible findings.

I discussed the development of my Guttman scales to measure premarital sexual permissiveness with this group of four students and informed them that their research would be its first testing ground. The 12 item scales measured premarital sexual permissiveness attitudes by asking about the acceptance of kissing, petting and coital behavior under conditions of various degrees of affection from "no affection" to "in love and engaged." This scale in both its original and in its shortened form has been used in scores of other research projects (Reiss, 1964a, 1967, 1998; Reiss & Miller, 1979; Schwartz & Reiss, 1995). But to test these scales and our hypotheses concerning causes of changes in the permissiveness levels they measure, we needed to get permission from local high schools and colleges to administer our questionnaire to their students. In Virginia, in 1958, this was not an easy thing to do. When my four senior students approached the principal of the white and the black high schools in Williamsburg they were turned down.

I knew that these principals feared public reaction but I also hoped that they would see the usefulness of knowing more about their students. I had to figure out how to change the priority of these two outcomes in their thinking. I went to see the white principal first and when he repeated his opposition to administering my questionnaire, I asked him how many of his high school girls had become pregnant last year. I then asked him whether he felt any responsibility to lessen the risk of pregnancy for these and for future girls. I also said to him: "Do you want to be seen as someone who chose to pass up the opportunity for increasing awareness of sexual standards that might help reduce unwanted pregnancy?" I stressed that this research was important to gaining knowledge concerning how to avoid outcomes like pregnancy. I also made it clear that I might well make a public issue of it if he continued to deny us permission. He

finally came around and said the 10th, 11th and 12th graders could participate but he would not let us give our questionnaire to the 9th graders.

The response from the black high school principal was more of an enigma. My chair, Wayne Kernodle, was friends with this man and he took me to his office and introduced me. The three of us sat down to talk and I explained the importance of this research for understanding our students better and gaining control over unwanted sexual outcomes like pregnancy. But instead of responding he kept changing the topic to the anti school-integration politics in Virginia. I kept trying to turn the conversation back to my study and he ignored my efforts. Finally, I realized that he wanted to see where I stood on the still hot issues of racial integration which the powerful Byrd political machine, represented by Governor Almond of Virginia, was resisting. I turned to him and shared a political joke I had heard. I asked him if he heard that the Byrd machine had inbred so much that it finally had produced an idiot--the Governor. He gave a loud laugh and said to me: "When do you want to give out the questionnaires?" The portrait of the politics of doing sex research in the 1950s was becoming clearer and clearer.

We gave out our questionnaires to the two Williamsburg high schools and also to two colleges (William and Mary and Hampton Institute). The four students on the project worked with our primitive McBee card system. It was a pre-computer method of analysis used to test whether our premarital questions produced a usable Guttman type scale. The answer came one night in the Spring of 1959. I remember that night very well because we lived right across the street from the campus and there was a "panty raid" that night. Harriet and I were observing the event with interest from our window. The phone rang and I thought it must be about the panty raid on campus. But it was John Stephenson and with great emotion he said to me: "They work! They work! The scales work! They meet all Guttman scale criteria, they are valid!"

The students and I celebrated that event with much gusto. It was like the discovery of some new chemical element, for there was no existing scientific scale to measure premarital sexual permissiveness. This was an important step towards systematizing the analysis of premarital sexuality and getting away from the very unscientific presentations that abounded in the textbooks and in the media. These scales have since come to be very widely used. I have since published a 4 item version that can be used instead

of the two 12 item male and female scales (Reiss, 1998). This new very short version has been tested in the U.S. and Sweden and found to meet Guttman scale requirements (Schwartz & Reiss, 1995). The four item scale is more convenient and one could even use just a single item from that scale if questionnaire space was at a premium (Question 3).

In 1959 I left William and Mary and went to Bard College. I applied for a National Institutes of Mental Health (NIMH) government grant to support analysis of my research data. In 1960 I received approval of the first of three NIMH grants that for four years were to support my analyses of these data and my gathering of a new nationally representative sample of adults to further test the scales and the sociological predictors of scale values. When I was awarded the grant, NIMH told me to change the title of my grant so that it did not contain the word sexual, or else, the grant might not get through all the official government checks. Even in the 1990s grants researching sexuality have run high risks. It is sad to see how powerful the uninformed and narrow minded still are in our political system.

My new research grants tested out many of the ideas in my 1960 book. That book sought to explain sexual standards in America and predicted dramatic sexual changes by the end of the 1960s (Reiss, 1960, pp. 239-241). The notion that someone could fairly analyze our society and accurately predict such changes was not widespread at that time. I hoped that my research grants would yield knowledge that would dissuade many of such pessimistic beliefs. A job offer from the University of Iowa followed very soon after the book appeared and I decided that it was time to leave the liberal arts college world and explore the Big Ten Universities. I've always valued my three liberal arts college positions--particularly William and Mary. But I thought it was time to move on and explore an environment that would provide me with better research support for analyzing the data and other work connected to my NIMH grants.

The University of Iowa afforded me good technical support in the way of staff and equipment. The first day I was on campus I came in contact with Don McTavish, a Ph.D. student in sociology. He was quite knowledgeable about computers and I hired him that same day. He was very helpful in my analysis of data during the next three years that he stayed with me. He put our data on IBM cards that could be used by the mainframes then in existence. In 1962 I applied for and received a supplement to my NIMH grant that allowed me to try out my scales on a nationally representative sample of the country. The

National Opinion Research Center (NORC) at the University of Chicago was doing “amalgam” surveys where you could buy a segment of time and have your questions asked of a nationally representative sample. My questions were in the NORC June 1963 national sample. It was the first scale analysis of sexual attitudes done on a representative national sample and today it affords us a benchmark measure of the premarital sexual attitudes in this country just shortly before the sexual revolution exploded forth in the late 1960s. These data are now archived at the Kinsey Institute at Indiana University and available for use by other researchers. I have recently used these benchmark 1963 data and analyzed trends from other national surveys up to 1998 (Reiss, 2001).

In 1967, my book analyzing the national sample and the regional school samples was published. It received a very favorable reception and the "autonomy theory" I put forth to explain change in sexual permissiveness has inspired scores of research in the years since (Reiss, 1967; Reiss & Miller, 1979; Schwartz & Reiss, 1995; Hopkins, 2000). As noted, I used a national sample as well as student samples to test my Guttman scales of premarital sexual permissiveness and to analyze a number of explanatory variables derived from my 1960 book and elsewhere. The “autonomy theory” that I developed explained changes in premarital sexual permissiveness as due to increased freedom for youth from adult institutions and increased acceptance of sexuality in the culture at large. This autonomy theory was a broad theoretical integration of the seven theoretical propositions that I developed in this book to explain more specifically the social forces that alter premarital sexual permissiveness levels (Reiss, 1967, Chapter 10). The events that happened in America in the decade after the publication of this book fit very well with the predictions that the autonomy theory made about causes of changes in sexual customs in our country. For example, the theory predicted that the group whose autonomy (ability to run their own life) increased the most would be the group whose premarital acceptance of sexuality would increase the most. Of the four race/sex groups, white females were showing the greatest increase in autonomy and so I predicted their increase in acceptance of sexuality would be the greatest. That was precisely what happened in the decade from the late 1960s to the late 1970s.

The Move to Minnesota:

I was at a crossroads in 1967--what to do next? My two books had attracted a great deal of favorable attention and job offers and other opportunities were coming my way. I chose to write a sociology of the family textbook as my next project. Ever since I saw those family textbooks back in 1953 when I first taught a course on the family, I had wanted to present a sociological analysis of the family in what I believed was a more reasonable and accurate fashion. I had been teaching family courses regularly since I received my degree. In 1965 I published an article arguing that nurturance of the newborn was the only family function that was present in every society (Reiss, 1965a). I tied that conception into a deeper understanding of the place of the family in society and I wanted to express my thinking on this in a textbook. I also desired very much to write a text that integrated human sexuality into the understanding of the family more than current family textbooks did.

This family textbook eventually went into four editions (Reiss, 1971, 1976, 1980; Reiss & Lee, 1988). That textbook writing took up a good deal of my time and energy especially during the 1970s. I sometimes wonder just how different things might have been had I focused more on my work directly in the sexuality field and not ventured so deeply into the sociology of the family area. But, I did use my sexuality research and theory perspective in my family textbook. Also, during the 1970s I surely was not spending all my time on the textbook. I consistently moved back to sexuality research and theory projects because that was still clearly my number one substantive interest (Reiss, Banwart, & Foreman, 1975; Reiss & Miller, 1979; Reiss, Walsh, Zey-Ferrell, Tolone, & Pocs, 1980a; Reiss, Anderson, & Sponaule, 1980b).

In January 1969, as I was working on the first draft of my family textbook, the University of Minnesota offered me a position in the department of sociology as the Director of the Family Study Center. Reuben Hill wanted to step down from that position in the Family Study Center which he had helped found. Interestingly, he selected me despite the fact that he knew I had long been critical of some of his views about sexuality (Reiss, 1957). I accepted the offer and joined what was probably then the strongest program in family sociology in the country. Oddly enough, Reuben Hill, the conservative Mormon and Ira Reiss, the liberal Jew, got along very well with each other. We both accepted each other's diverse viewpoints and our strong sense of loyalty to good research and theory helped bind us together during the years we were

colleagues at Minnesota. Reuben was the most helpful colleague I had during those years. He and his wife Marion became our friends. Harriet and I were both very grieved by his death in 1985 and by Marion's passing in 2001.

At the 1971 National Council on Family Relations meeting, Reuben Hill and I together with Ivan Nye and Wesley Burr organized a major project to present all the existing theories in the family field in a formal, scientific fashion in a two volume work. The book was published in 1979 after eight years of effort to get some of the best people in the family field to write up their formal theories on various substantive areas of the family. (Burr, Hill, Nye, & Reiss, 1979). Together with Brent Miller I wrote a chapter for this book analyzing the recent testing and development of my 1967 Autonomy Theory (Reiss & Miller, 1979). In the 1970s I was more zeroed into the formalization of theory as the key way to develop our theoretical understanding of the family. Today I would broaden that approach to include more qualitative data and more policy implications.

The role of Director of the Family Study Center at the University of Minnesota stressed administration work and the obtaining of research grants and Fellowship Grants for graduate students. I have never liked administering other people's problems. I stayed as director for five years but halfway through that term I became eager to get out of that role. I wanted to be free to focus more fully upon my own research and theory work.

I was once again searching for a new challenge. I had a sabbatical in 1975-76. In order to gain a broader view of how gender and sexuality are tied into different societies I spent that year teaching and doing research at Uppsala University in Sweden. By being a foreigner for that year I learned how important it is to understand the cultural and social context of sexuality and gender. It was obvious that the Swedes had a deeper acceptance than we do of premarital intercourse as a legitimate option for young people. The public display of condom advertisements on billboards and their availability in street machines was one indicator of this. The lower disease and pregnancy rates in Sweden made me aware that this greater acceptance and better preparation for sexuality was indeed working very well. I have found my experience in Sweden very helpful in developing my cross cultural perspective on sexuality, gender, and the family (Reiss, 1980).

Part of what I learned about Sweden concerned extramarital sex. Sweden seemed to have high rates of premarital sex but moderate rates of extramarital sex. I decided to examine extramarital sex attitudes in America and search out their best predictors. Together with one of my doctorate students, G.C. Sponaugle, and my colleague Ron Anderson, I undertook to analyze U.S. national data and develop a theory explaining attitudes towards extramarital sexual permissiveness. We used four different years of the General Social Surveys data on extramarital sex. That sample was gathered annually by NORC and was representative of the nation. They used only one rather general question to measure extramarital attitudes. But this was the best data available for a national sample. We carefully analyzed some hypotheses about possible predictors of changes in extramarital sexual attitudes. It turned out that there were three major factors influencing extramarital attitudes. I would sum them up as: (1) how happy one's current marriage was, (2) how highly one valued sexuality, and (3) how intellectually flexible one's attitudes were (Reiss, Anderson, & Sponaugle, 1980b). The findings contradicted the popular, simplistic view that the first factor, unhappiness of marriage, was the key answer to why extramarital sex was accepted. It was not even the strongest predictor. Several replicative studies of our work followed (Glass & Wright, 1992; Sponaugle, 1993; Reiss, 1998). I had thought about doing a book on extramarital sexuality but I felt I had done enough in this major project and I resumed my search for another area to explore.

Coming Full Circle

By the 1980s trends toward the increased acceptance of sexuality stalled and sex negative resistance increasingly surfaced. I felt the need to speak out more in my publications about the directions that we as a society should be taking and explaining why the current reactionary movement would not resolve our sexual problems. I believed that if I was aware of my own values and made a conscious effort to avoid bias, I could make my value assumptions explicit and still be fair and balanced in analyzing the sexual situation in our country and in proposing solutions. The increased resistance to sexual changes had made me more conscious of the inevitable mix of science and values when we study human behavior of any kind. I also saw more clearly the need for becoming part of the policy scene by going beyond describing and explaining. I increasingly felt that part of my role as sexual scientists should involve prescribing ways of better containing our many sexual problems.

By 1980 I also decided that I wanted to reach an audience broader than just sociologists. One project that had interested me even back in graduate school was to examine the writings and data on sexuality cross culturally with the aim of arriving at a sociological explanation that would be useful in understanding sexuality in any society. This sort of cross cultural explanation I thought would be useful to any social scientist and also to clinicians and social workers who dealt with problem areas of sexuality. I had always had a strong cross cultural interest and had taught Anthropology for four years at William and Mary. My goal in this new book project was to find those parts of human societies that are most strongly linked to our sexual customs and to build an explanation of how and why the specifics of those linkages differed in various societies around the world. This cross cultural project was an immense undertaking. It was to occupy me for almost the next five years.

I read all the anthropological literature I could find on sexuality in various societies. In addition, I used the Standard Cross Cultural Sample (SCCS) of 186 non-industrial societies to check out a number of my ideas (Murdock, & White 1969). I arranged to get permission to add to the SCCS several new sexuality codes that other researchers working with this sample had developed. My project was very broad and there were times I wondered if I would ever be able to finish this work. But eventually I developed my "PIK" theory which stated that the crucial linkages of sexuality in any society were in the areas of Power, Ideology and Kinship (PIK). I tried to explain why sexuality was always linked to PIK and how and why the specific nature of this triple linkage differed in various societies (Reiss, 1986, Chapter 8). I was pleased with the end result because I believe I had presented a broad macro sociological perspective explaining similarities and differences in the socio/cultural linkages of sexuality in human societies. Up until this time sociologists had not put forth such a cross cultural theoretical explanation of sexuality.

The comparative approach I used was contrary to the fads and fashions, that is, the politically correct view of the day. The popular view then and still now is that each society is unique and thus we cannot really compare different societies in any meaningful way. I accepted the distinctiveness of each society but I rejected the view that comparisons were not possible. We do compare when we examine people. People are certainly different from each other but surely we can compare them and still accept that they differ in some respects. Science is not possible if such comparisons cannot be made. When the book

appeared in 1986 it met with surprisingly strong support from some quarters. For example, Ron Moglia at the graduate program in human sexuality in New York University and others wrote that they were using this book in their cross cultural sexuality courses. But my ideas did not reach as far as I wanted into the broader professional audience. Nevertheless, I have noticed in recent years that this book is being cited by other writers who share my desire to understand the similarities as well as the differences in sexuality around the world (Suggs & Miracle, 1993; Deven & Meridith, 1997). Perhaps the politically correct academic stranglehold on cross cultural comparisons is beginning to weaken. Science thrives best in an open atmosphere rather than in one where certain types of projects are blocked by prejudgments of their worth (Reiss, 1999).

I had taken a significant step in this book away from writing only for sociologists. The change was refreshing to me but it increased my appetite for reaching an even broader segment of the professional world and also the educated public. I once more anguished over how to do this and what sexuality topic to explore. As I mentioned above, there was a radical right sexual element growing in our society and that more than anything else motivated me to become more prescriptive in my new project. The reactionaries pushed for returning to an “abstinence only” solution for our sexual problems such as HIV/AIDS. I knew that approach was doomed to failure but I also knew that the American public was very poorly informed and still harbored the Victorian virus that could be activated to support a return to older narrow sexual standards. I wanted to do what I could to show the public that the resolution to our sexual crises was not to be found in the past but in a changed future (Reiss & Leik, 1989).

Projects that have a broad societal applicability have always been most interesting to me. I see as my strength the ability to interpret and explain very diverse phenomena. But I had not applied that ability to finding solutions for our society’s sexual problems quite as directly as I was to do now. I chose the four American sexual problem areas that begged for problem resolutions: HIV/AIDS, rape, teenage pregnancy and child sexual abuse. I explored what in our society had produced our high rate of these problems and what changes in society would best bring these four unwanted problems under better control.

As my analyses of these four problem areas proceeded and I examined the major studies in these areas, I found myself confirming that the traditional sexual ethic of our society had seriously exacerbated,

rather than helped, in all these problems. It was also increasingly clear that a new pluralistic sexual philosophy was evolving that would help us in managing these sexual problems. That new philosophy was what I called HER sexual pluralism. This sexual ethic asserted that if a sexual relationship was Honest, Equal and Responsible (HER), then it was morally acceptable whether it was between two men, two women, a man and a women, two sixteen year olds or whatever. Also, the amount of affection was not the number one value. The honesty, equality and responsibility in a relationship were the key moral criteria of sexual choices in that relationship. Love by itself was not the answer to a moral choice. Surely there are many love relationships with severe gaps in HER and non-love relationships with greater HER. This change toward an HER sexual standard in America in the late 1980s was as significant to me as the change to permissiveness with affection was in the late 1950s. It heralded a new way of conceptualizing sexuality by the American public. This was, I believe, the final phase of the sexual revolution that had begun a generation earlier and it afforded the best guide to containing our major sexual problems.

Writing this more prescriptive book afforded me such an exhilarating feeling. I was finally, openly involving much more of myself in my professional work. Now, I was speaking as a whole person and not just from a “pure” scientific position. Now my work was more of a human endeavor and less of an antiseptic science project. Now I could develop a more humane model of science. It was about time!

But I want to be clear about this new sexual standard. HER sexual pluralism does not say anything goes. It demands an ethical approach that shows concern for one's partner. That concern for partners is the minimal price of admission to the arena of ethical sexual choices. What choices one makes within an HER negotiation will vary by where one is in their life and what other values one holds. This minimalist HER philosophy would encourage people to take responsibility for working out such ethical choices and to be open to changing their specific preferences when their life situations and their values change. But not all choices are acceptable. HER pluralism did not require a commitment to deep affection but it did require a concern for treating the other person in an honest, equal and responsible fashion. Such a relationship would have to avoid the use of force and exploitation. HER sexual pluralism surely qualifies as a revolutionary perspective compared to our Victorian and Puritan ethics. It is also a significant change from the much more narrow permissiveness with affection standard. Finally, I should also stress that I was

not inventing this HER sexual ethic. It was very much the ethic I had encountered in Sweden and increasingly found evidence for in America.

In this 1990 book I was directly confronting the absolutism of those traditionalists who said we should reject teenage sex, reject homosexuality, and reject sex outside of marriage. I examined the evidence of trends all over the Western world towards HER sexual pluralism and argued that we are now in the midst of a new Western sexual revolution. Today's revolution concerns sexual attitudes and not so much sexual behavior. It is a more silent revolution. And as such it is only partially recognized but it is rapidly spreading in the Western world and we will increasingly become aware of it. This change will complete the sexual revolution that began in the late 1960s.

I made a number of predictions in this 1990 book that I thought would occur before the end of the 1990s. In my 1997 revision of that book I checked them out. I had predicted lower teenage pregnancy rates, increased condom use, decreased HIV/AIDS rates, decreased rape rates, and greater tolerance for gays and lesbians (Reiss, 1990, pp. 234-236). By 1997 almost all these changes were clearly happening (Reiss, 1997, pp. 13-14). I also had predicted that by the end of this decade our country will have explicitly endorsed the HER sexual pluralism ethic. The evidence from national surveys indicated that I was right on the money (Reiss, 1997, Chapter 10). More recently, the American public's reaction to President Clinton's affair with Monica Lewinsky perhaps most clearly indicates that we as a nation have moved toward a more pluralistic approach to sexuality. Two thirds of the public consistently said that no matter what they thought of Clinton's affair, they felt it was not the business of the public and he should remain in office. Few of us would have predicted such a supportive view for the privacy of sexual behavior that most Americans personally reject.

My 1990 book exemplified a much more direct legitimate role for sociological work in coping with social problems. My approach today surely does not deny the value of less prescriptive science and mainstream research work. Rather, I am validating the need to add a prescriptive dimension to our work and to develop a portrait of what we "could" be like, rather than focusing only on what we are like. This stance also incorporates elements of my views as a graduate student emphasizing the importance of what is significant, and lowering the priority of focusing so heavily on what is precisely measurable. It is a

reemphasis of my admiration for the work of people like Gunnar Myrdal (1944) who long ago was bold enough to suggest how we might resolve our "American Dilemma" about race and equality. I sought to reclaim many of the powers that social scientists abdicated to the politicians. I am convinced that we in science need to work together with those in power to develop a common vision of how to change society, and not give the "vision thing" completely over to the politicians. I fully believe that we can, with conscious effort do this and still maintain fairness and balance in our scientific work.

Quo Vadis?

Surely sociology has played a major role in my personal life. It has helped me in my own ethical decisions. But I want to be very clear that I still firmly believe that our personal moral views should not be allowed to overwhelm our scientific integrity. I do not want to reproduce the biased stances taken by the 1953 family textbooks. The rigor, logic, empiricism, and openness to new ideas, of science must be preserved and given top priority. But instead of seeking the impossible goal of being value free, we need to work harder to be value aware and value fair (Reiss, 1993, 1997b, 1999). In that way we become more likely to be aware of how our values can bias our work and thus more likely to consciously give priority to avoiding that pitfall. Instead of allowing our values and assumptions to unknowingly creep into our work, or pretending that values have no impact on our work, we need to stress value awareness and value fairness so as to expose our values while working to prevent them from overwhelming our science.

I now feel that I have reached a much broader audience; not a mass audience, but a broad audience of educated Americans. Groups like Planned Parenthood responded enthusiastically to my 1990 book. I wrote the book without jargon and with no tables or graphs. I worked for 18 months with my nephew, Spence Porter, a New York playwright, who helped to teach me how to add dramatic quality, use anecdotes, and make my ideas relevant to current events. My wife, Harriet, increased my awareness of the reader's needs and gave me deeper insights into a woman's perspective on gender equality issues. By the end I had improved my ability to write in clear, plain, and interesting English, and I hope I have retained some of that in this autobiography.

Up until the mid 1980s I believed that sexuality research and theory would do best by remaining as a sub-field in the established disciplines of sociology, psychology, anthropology, biology and such. But

as I began to write for a more multidisciplinary audience, I changed my thinking. I realized that sexuality had been held back by being a sub-field and often a low ranked one, in other disciplines. I then began to argue for establishing a separate discipline of sexual science in order to advance the credibility and support for our work. Finally, in 1998 I persuaded both the Society for the Scientific Study of Sexuality (SSSS) and the American Association of Sex Educators, Counselors and Therapists (AASECT) to form a joint taskforce to work to encourage the development of a PhD in sexual science at one of our major universities. Since I retired from the University of Minnesota in 1996, I had time to chair this taskforce. I wrote up a general outline of a Ph.D. program in sexual science that spelled out a multi-discipline and multi-university format for this new sexual science field (Reiss, 1999). In May of 1999 our taskforce met at the Kinsey Institute at Indiana University and were successful in persuading the Kinsey Institute to adopt this perspective and work towards the establishment of a PhD program in sexual science at Indiana University.

The first major step in this direction was taken by the Kinsey Institute when they succeeded in obtaining government and private grants that allowed them to bring in top sexual scientists from around the country to conduct its first summer institute in the summer of 2001. This is just the first step toward a separate PhD in Sexual Science but it is a most important one. This planned PhD program would be the first at a major American University and it will do much to establish the legitimacy and credibility of sexual science (Reiss, 1999).

I am an optimist and I strongly believe that the field of sexual science will prosper in this new century. We can make the 21st century a time during which we as a people will increasingly learn how to reduce our sexual problems while at the same time learning how to achieve more of the rewards of sexuality. I believe that we are on the threshold of major advances in society, in sexual science, and in our personal lives. We can all play a role by supporting these important trends.

References

Burr, Wesley, Reuben Hill, Ivan Nye and Ira L. Reiss (Eds.), Contemporary Theories about the Family, Volumes One & Two. New York: The Free Press, 1979.

Coutu, Walter, Emergent Human Nature, New York: Alfred A. Knopf, 1949.

Deven, Fred, and Philip Meredith, "The Relevance of a Macrosociological Perspective on Sexuality for an Understanding of the Risks of HIV Infection," pp. 142-158 in: Luc Van Campenhoudt, Mitchell Cohen, Gustavo Guizzardi, and Dominique Hausser (Eds.), Sexual Interactions and HIV Risk: New Conceptual Perspectives in European Research. London: Taylor and Francis, 1997.

Glass, Shirley P. and Thomas L. Wright, "Justifications for Extramarital Relationships: The Association Between Attitudes, Behaviors and Gender", Journal of Sex Research, (August, 1992), 29, pp. 361-387.

Hopkins, Kenneth Wu, "Testing Reiss's Autonomy Theory on Changes in Non-Marital Coital Attitudes and Behaviors of U.S. Teenagers: 1960-1990", Scandinavian Journal of Sexology, (December 2000) 3, pp.113-125.

Kephart, William M., Book review of: Premarital Sexual Standards in America, in: American Sociological Review, (April, 1961), 26, pp. 294-5.

Murdock, George P., & Douglas R. White, "Standard Cross Cultural Sample," Ethnology, Vol.8 (October, 1969), pp. 329-369.

Myrdal, Gunnar, An American Dilemma. New York: Harper & Brothers, 1944.

Reiss, Ira L., "Sexual Attitudes and Behavior," in Neil J. Smelser and Paul B. Baltes (Editors in Chief) International Encyclopedia of the Social and Behavioral Sciences, Oxford, England: Elsevier Science Limited, 2001.

Reiss, Ira L., "Evaluating Sexual Science: Problems and Prospects," Annual Review of Sex Research, , 1999, 10, pp. 236-271.

Reiss, Ira L., "Reiss Male and Female Premarital Sexual Permissiveness Scales," pp.496-498, and "Reiss Extramarital Sexual Permissiveness Scales," pp. 226-228 in: Clive M. Davis, William L. Yarber,

Robert Bauserman, George Schreer, and Sandra L. Davis (Eds.) Handbook of Sexuality-Related Measures. Second Edition, Thousand Oaks, CA: Sage Publications, 1998.

Reiss, Ira L., Solving America's Sexual Crises. Amherst, NY: Prometheus Books, 1997a.

Reiss, Ira L., "An Introduction to the Many Meanings of Sexological Knowledge," pp. 21-30, in Robert T. Francoeur (Ed.) International Encyclopedia of Sexuality, Volume 1, New York: Continuum Publishing Group, 1997b.

Reiss, Ira L., "The Future of Sex Research and the Meaning of Science," Journal of Sex Research, (February 1993), 30, pp. 3-11.

Reiss, Ira L., An End To Shame: Shaping Our Next Sexual Revolution. Amherst, NY: Prometheus Books, 1990.

Reiss, Ira L., Journey Into Sexuality: An Exploratory Voyage. New York: Prentice-Hall Inc., 1986.

Reiss, Ira L., "Sexual Customs and Gender Roles in Sweden and America: An Analysis and Interpretation," pp. 191-220 in: Helena Lopata (ed.) Research on the Interweave of Social Roles: Women and Men. JAI Press, 1980.

Reiss, Ira L., Family Systems In America. Holt, Rinehart and Winston, Inc. 1971. (Second Edition 1976; Third Edition 1980; Fourth Edition 1988. The fourth edition was coauthored with Gary R. Lee.)

Reiss, Ira L., The Social Context of Premarital Sexual Permissiveness. Holt, Rinehart and Winston, 1967.

Reiss, Ira L., "The Universality of the Family: A Conceptual Analysis," Journal of Marriage and the Family, (November, 1965a), 27, pp. 443-453.

Reiss, Ira L., "The Scaling of Premarital Sexual Permissiveness," Journal of Marriage and the Family, (May, 1964a), 26, pp. 188-198.

Reiss, Ira L., Premarital Sexual Standards in America. The Free Press, 1960a.

Reiss, Ira L., "Toward a Sociology of the Heterosexual Love Relationship," Marriage and Family Living, (May, 1960b), 22, pp. 139-145.

Reiss, Ira L., "The Treatment of Pre-Marital Coitus in 'Marriage and the Family' Texts," Social Problems, (April, 1957), 4, pp. 334-338.

Reiss, Ira L., "The Double Standard in Premarital Sexual Intercourse: A Neglected Concept," Social Forces, (March, 1956), 34, pp. 224-230.

Reiss, Ira L., and Robert K. Leik, "Evaluating Strategies to Avoid AIDS: Number of Partners Vs. Use of Condoms," Journal of Sex Research, (November 1989), 26, pp.411-433.

Reiss, Ira L., Ron Anderson and G.C. Sponaugle "A Multivariate Model of the Determinants of Extramarital Sexual Permissiveness," Journal of Marriage and the Family, (May, 1980), 42, pp. 395-411.

Reiss, Ira L., and Brent C. Miller, "Heterosexual Permissiveness: A Theoretical Analysis," pp. 57-100, in: Wesley Burr, Reuben Hill, Ivan Nye and Ira L. Reiss (eds.), Contemporary Theories about the Family, Volume One. The Free Press, 1979.

Reiss, Ira L. Albert Banwart, & Harry Foreman, "Premarital Contraceptive Usage: A Study and Some Theoretical Explanations," Journal of Marriage and the Family, (August, 1975), 37, pp. 619-630.

Suggs, David .N., and Andrew W. Miracle, (Eds.), Culture and Human Sexuality: A Reader. Pacific Grove, CA: Brooks/Cole Publishing Company, 1993.

Schwartz, Israel and Ira L. Reiss, "The Scaling of Premarital Sexual Permissiveness Revisited: Test Results of Reiss's New Short Form version," Journal of Sex and Marital Therapy, (Summer, 1995), 21, pp.76-86.