

BUCKING THE TIDE IN FAMILY VIOLENCE RESEARCH*¹

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BOTH FAMOUS AND INFAMOUS

The three best-known, but also bitterly criticized, aspects of my research on family violence are the National Family Violence Surveys, the research showing that legal and morally correct corporal punishment by parents has serious harmful side effects, and the development of the Conflict Tactics Scales (CTS) to measure family violence. These studies have been attacked from both the political right and left. The political right, for example, attacked my research as undermining the family because I found that husband dominant couples have the highest risk of violence, and that spanking children has serious harmful side effects.

Evaluations of the CTS illustrate both the esteemed and the disdained characteristics of my research. On the one hand, the CTS has been described as "...revolutionary because it allowed researchers to quantitatively study events that had often been ignored culturally and typically took place in private." Langhinrichsen-Rohling (2005). However, others describe it as invalid and producing misleading results because it uses the "phallicentric" (i.e., male) methodology of just counting the number of attacks. In this article I will trace out the origins of my approach to family violence research and address some of the criticisms, or at least identify papers in which I have done that.

My approach to family violence research is rooted in experiences and research decades before I began to study family violence, including childhood experiences. Although this article is organized chronologically, there are four underlying themes: First, is iconoclasm. I do not seek out idols to destroy, but if I come across them, I am usually tempted to try. Second, is the importance of making use of chance occurrences to select research topics. Third, is the conflict between ideology and evidence. Ideology can be an important motivating factor in science, but it can also blind researchers. Fourth, is the role of the personal history and the personality of the researcher in molding a scientific career.

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Colleagues who read a draft of this article say that it fails to reflect their view of me as a kind and helpful person, who is always available to students and colleagues. This is partly because, as proud as I am of that aspect of my career, my writing skills are not up to presenting it. The more important explanation is that most of the research issues I describe involve dissent from conventional wisdom, and some of the events involve conflicts with colleagues..

I will start with bits of personal history to illustrate that part of my approach to family violence reflects a life-long pattern of not being bound by conventions or fashionable beliefs, including scientifically fashionable beliefs. I do not object to conventions. In fact, conventions are necessary for a society to survive, and in some ways I am a very conventional person. I can't think of an instance where I set out to be unconventional for the sake of being unconventional. It is just that when I encounter something counter-intuitive or unconventional, it intrigues me. Sometimes I am drawn to studying counter-intuitive and unconventional phenomena like a moth to a light bulb. This has included research results that others might dismiss as anomalies.

HOW I AVOIDED BUCKING THE STATE DEPARTMENT

My father was the son of Russian Jewish immigrants. My mother was born in England, and met my father when she was visiting the US. I was born in 1926. My mother took me to England as a child to visit grandparents, and to France to see friends from her time in school there. I took many trips in the US with both my parents. I loved to travel and planned a career in the US Foreign Service as an economic affairs officer. To that end, my undergraduate degree at the University of Wisconsin was in International Relations. It combined economics and political science. That was interrupted by my being drafted in 1944. I was an assistant tank driver in Germany during the last two months of the war. I did not fit well in the Army and it made me realize that I would be a misfit in the State Department. When I returned to Wisconsin in 1946, I explored other careers. I took an introductory sociology course and found it interesting but lacking the hard evidence I liked in economics. In my senior year I was still undecided. By a lucky coincidence William H. Sewell, then a professor of rural sociology employed me to work on a study he was conducting. I became intrigued with that study and it changed my view of sociology. Sewell arranged to have me admitted to the graduate program in rural sociology.

BUCKING THE IDEALIZATION OF RURAL LIFE

Starting academic life with a mentor like William Sewell (who later became president of the American Sociological Association and president of the University of Wisconsin) was being born into an intellectual and academic privileged class. But for me there was also a

disadvantage. I grew up in New York City. I was very interested in rural life, but as an object of study rather than as something to be revered and preserved; just as today I am very interested in crime as an object of study, rather than as a lifestyle to be revered and preserved. My view of small town and rural life may have been excessively influenced by Sinclair Lewis's novel *Arrowsmith* (a staple in American literature courses at the time). It described the socially and intellectually stifling environment of small town America that Dr. Arrowsmith endured.

My first course as a graduate student was taken in the summer of 1948. It was a seminar on rural cultural regions. I wrote a paper on the modernization of Wisconsin dairy farms, using my wife's parents' farm as an example. I described in glowing terms how milking machines and other technology had freed dairy farmers from drudgery and virtual imprisonment; and how bread and *pasteurized* milk was delivered to the farm. I enjoyed the course and thought I had written a great paper, but it was a disaster. I received a grade of C. I had just graduated with honors only two months before. I was mortified. Bill Sewell, who had arranged my admission to the graduate program despite not having an undergraduate degree in sociology, must also have been at least embarrassed. He checked into it and found that the professor who taught the course believed that I "did not truly understand rural life." This was a way of saying that I was not an admirer of the kind of rural life he had experienced as a boy in the early nineteenth hundreds, and my paper had labeled it "drudgery."

Despite the rough start, I finished the MA in Rural Sociology in one calendar year. My thesis was a study of "Selected Factors in the Occupational Choice of Wisconsin High School Seniors." It challenged prevailing meritocratic assumptions about social mobility in the US. I found that students from high SES families chose higher status occupations, which is hardly surprising. The contribution of the study was in finding that rural students and low socioeconomic status students tended to choose lower status occupations even when they were in the highest IQ group. That was surprising at the time. The theoretical contribution was in showing that social factors (rural residence and family SES) affected this crucial life-choice as much as individual merit. The social causes of individual behavior have been a feature of my research ever since, as indicated by the title of the book I edited with Gerry Hotaling, *The Social Causes of Husband-Wife Violence* (1980b) and social causes is the main theme of *Behind Closed Doors: Violence in the American Family* (Straus, Gelles, & Steinmetz, 1980 (2006)).

SRI LANKA

During the 1948-49 academic year one of my teachers mentioned working with him on a research project in Venezuela. I was interested enough to also plan to become a specialist in

Latin American societies. However, in the spring of that year, a position to teach at the University of Ceylon came up. That intrigued me, perhaps in part because Ceylon (now Sri Lanka) was even more different from the US than Venezuela. I was lucky to be married to a woman who thought similarly, and I signed a contract to teach there for three years. In addition, we collaborated on research both during those three years (Straus & Straus, 1953) and later research in India (Straus & Straus, 1968).

The three years in Ceylon were tremendously important for my intellectual development. First, there was the influence of Bryce Ryan, the chair of our two (and later) three-person department. He was a gifted sociologist. His book on caste in Ceylon is universally considered a classic, and he was later elected President of the Southern Sociological Society. He was one of the most important of several mentors who shaped my career. Second, there was what I learned from the challenge of teaching sociology to students in Ceylon. The most widely used introductory sociology text books were largely books about American society, with unknown applicability to Ceylon. This was even more the case with textbooks on the family. The content was almost entirely about American families, and in those days, not even the diversity of American families. Designing these courses and finding appropriate readings helped me perceive the parochialism of much American sociology of the time. Third, even though I had only completed the MA degree the day before leaving for Ceylon, gathered the data for my PhD dissertation during those three years.

BUCKING PSYCHOANALYTIC THEORY

My mentor Bill Sewell pointed out that there was no scientifically valid evidence for much of the then popular psychoanalytic developmental theory; for example, that early toilet training resulted in an "anal retentive personality." That fascinated me, perhaps because this was at a time (1948-49), when psychoanalytic theories were the rage among the general public, in the social sciences, and among pediatricians, social workers, and psychiatrists. I decided to do my PhD dissertation on the infant training aspects of psychoanalytical developmental theory. The results showed that in the cultural context of Ceylon, infants who were fed on a schedule; or who were toilet trained early, were *more* psychologically secure than the other children in the study (Straus, 1954, 1957a). In the discussion of those results I suggested that the explanation was the difference in social context between the rigidity of upper middle class Vienna in 1890's and the flexibility and unpredictability of life in Sinhalese villages where children may have lacked sufficient structure in their lives (Straus, 1966). I argued that infant care practices that denoted additional rigidity and constraint in Vienna, in the Sinhalese context, provided order and predictability to the child. That interpretation was disputed by the editor of a journal that

rejected one of the articles I submitted. I have had lots of articles rejected, so that is not the interesting point. The interesting point is that he was so committed to the fashionable psychoanalytic theory that he advised me to explore the implications of Freudian theories rather than testing them.

ADDITIONAL BUMPS ON THE RURAL ROUTE

I returned to the University of Wisconsin in 1952, finished my course work and PhD comprehensive examinations and, in 1954 took a job as Assistant Professor of Rural Sociology at Washington State University. In 1957 after three years at Washington State, I returned to Wisconsin as an Assistant Professor. At Washington State and at Wisconsin, much of my rural sociology research had a contrarian element. For example, in a study of factors that were related to success or failure of new farms in the Columbia Basin Irrigation project, I found that the more years of experience in farming prior to settling in the Columbia Basin, the greater the probability of failure. Farms where the wife participated in the farm work also had a higher failure rate (Straus, 1958, 1960). My colleagues treated these results as anomalies, but I was fascinated. I did qualitative interviews with some of the study participants and concluded that these results reflected the old "family farm" tradition and the trauma of farms lost to foreclosure during the depression. Unfortunately, the depression-taught lesson of "stay out of debt" was dysfunctional for success in the capital-intensive farming of the Columbia Basin. Without borrowing heavily, failure was likely.

Although my research on rural issues was starting to be recognized, my belief that rural life was something to be studied, not necessarily something to be preserved, was not popular with some of my senior colleagues. I also caused the department embarrassment by failing a graduate student on his dissertation defense. This was very hard for me to do because my style is positive and supportive of students; so much so that one colleague said he doubts there has ever been a student research idea that didn't arouse my enthusiasm. I was not sure if that was a compliment or a criticism, but either way it is true. In this case, the target of my vote was not the student. The purpose was to pressure the department to take action on something they knew about, but would not act on -- the inadequacy of the student's dissertation advisor. I could do this without denying the degree to the student because passing took only a majority vote. As I expected, the Dean of the Graduate School called me a few days later, and then in collaboration with the department chair, made informal arrangements to correct the problem. But this incident and my skepticism about rural life may partly explain why, year later I was told that I was unlikely to get tenure at Wisconsin. I was crushed. Perhaps due to the emotional upset, I had a minor car accident the day I got the bad news.

In the academic employment climate of 1959, being denied tenure was not necessarily a disaster. This was a period of academic growth. Within a couple of months I was interviewed for a position in the Child Development department at Cornell. I had never taught a course on the family, but I was considered for the job because of the papers on child development from my dissertation (Straus, 1954, 1957a) and the family aspects of my rural sociology research (Straus, 1958, 1960).

CORNELL, MINNESOTA, AND NEW HAMPSHIRE

Cornell

I had two great and influential years at Cornell. I can't remember any controversial issue, perhaps because I was too busy preparing new courses and learning enough about the sociology of the family to justify my being there. I was also busy learning everything I could from Alfred Baldwin, Urie Bronfenbrenner, Edward Devereux, and Harry Levin. They encouraged and helped me submit my first grant application to NIMH. That proposal reflected something else I inherited from Bill Sewell – the importance of instruments to measure social phenomena. Part of Sewell's early reputation resulted from his development of a socioeconomic status scale suitable for farm families. My research at Washington State University had included development of new measures for use in rural sociology (Straus, 1957b). The grant application was for a comprehensive review of methods of measuring characteristics of the family. It resulted in the book *Family Measurement Techniques*, which went through three editions (Straus, 1969b; Straus & Brown, 1978; Touliatos, Perlmutter, & Straus, 1990).

I think I fit the Cornell department because, unlike many sociologists, I am an admirer of psychology. I have always favored interdisciplinary research. Somewhat paradoxically, being an admirer of psychology and an advocate of interdisciplinary approaches probably helped me focus on what sociology can uniquely contribute. This was crystallized not long after I arrived at Cornell. At lunch with Urie Bronfenbrenner we were discussing how my research plans fit with the department's goals. He said, "If we wanted a psychologist, we could have hired one." In short, they wanted me to do research that a psychologist was not likely to do.

Perhaps the work that best exemplifies my contribution as a sociologist to understanding family violence is the introduction Gerry Hotaling and I wrote for the book *The Social Causes of Husband-Wife Violence* (Straus & Hotaling, 1980a). Some of the social causes it highlights are characteristics of the family that are socially desirable or necessary, but also increase the probability of violence. These include the fact that families typically consist of both men and women and two generations. The problem with this is that men and women and parents and

children differ in perspectives and interests. These differences result in the family being an institution with an inherently high level of conflict, which is part of the explanation of the high rate of violence in families. Moreover, there is both individual and societal reluctance to dissolve marriages, and parents can't simply leave children. These factors result in tolerating a certain level of violence by a partner, by children, and by parents. Other family characteristics which increase the probability of conflict and violence are ascribed rather than chosen roles, the presumed right of family members to influence each other's behavior, and family privacy which reduces the ability of others to intervene and help resolve conflicts.

Family Social Science Department At the University Of Minnesota

In 1961-62 I moved to the University of Minnesota because I was offered a full professorship (I was 35) and because of the opportunity to transform what was then the division of Home Management And Family Development in the College of Home Economics into a new interdisciplinary, research oriented department. At the time it was down to one professor who was near retirement. I took the job with the understanding that I would build an empirical science-oriented department. It started out well with my appointing a productive and creative researcher who was also an excellent teacher.

I wanted the research orientation to start with undergraduates and I developed a freshman level course on the family in which there were weekly lab projects (Straus, 1969a). At the start of the semester, the students in the class (and in some other classes) completed a questionnaire to provide the data for most of the labs. Each week the students read a journal article and then in the lab period did cross tabulations to see if their data replicated the original study. Most of the time it did, but when it did not, the discussion of why it did not was just as important a learning experience. For each lab project they wrote a two-page report covering the standard issues: objectives, sample, measures, results, and discussion.

I felt proud of the direction I was taking in developing the department and I believed I was on the track the university wanted. However, I found out that I had misperceived what was wanted when I tried to change the name of the name to department of Family Social Science. The Dean opposed it and said the emphasis on research (as compared to practice) was excessive. Despite her objection, I introduced a motion at a faculty meeting to change the name. It passed, but only by a narrow margin. I felt the prospects for the department were not good and I left the College of Home Economics to be full time in Sociology in the College Of Arts and Sciences. I was wrong about the future of the department I had started because, under leadership that combined a research orientation with a practice orientation, the Family Social Science department grew and eventually became nationally respected. In retrospect, I

also see that I had failed to do something essential when attempting to bring about change: involve the key stakeholders. I am a very socially concerned person, but I am not a social person. I love to discuss research or teaching, but in three years I had never invited the dean for dinner, never even had lunch with her, and only rarely spoke with her. This was not because of dislike or incompatibility. It was because informal interaction and personal ties are not high on my agenda. Yet, it does not take being a great sociologist to know that personal ties are crucial. Ignoring that was a mistake and it continues to be something that handicaps me.

Class and Family In Three Societies Study

I left Cornell too soon for my colleagues there to see the impact they had on me. One of the most important was that I came to appreciate and admire their use of laboratory observation and experiments to test theories of child development. Not long after I left Cornell, I implemented this new found interest, combining it with my cross-national research interests, and with the program of research on family problem-solving that a group of us at the University Of Minnesota had developed (Aldous, Condon, Hill, Straus, & Tallman, 1971). I designed a study which permitted observing how family groups consisting of parents and one of their children dealt with a standardized problem, and what happened to family relationships when the task was manipulated to be an unsolvable problem. Did the frustration result in greater cooperation to solve the problem or greater conflict and disparagement? Did the frustration cement or alter the power hierarchy? Were there social class differences in these phenomena, and did they apply in Minneapolis, San Juan, and Bombay?

Some of my sociologist colleagues did not like the study. This reflected a common view among sociologists that laboratory analog experiments are "artificial" and lacked external validity. One reviewer for a leading sociological journal recommended against publication and said "Pseudo science – Ignores the cultural context" despite the fact that the study replicated the experiment in three societies precisely to examine the effect of the cultural context. Although several papers were published (Straus & Straus, 1968; Straus, 1967, 1968, 1970), I lost the battle to make laboratory experiments more widely used in sociology. Almost no sociologists use this method to study families. However, many psychologists are doing laboratory observational and experimental studies of families and also cross-national experiments. This is not because of the influence of my research, but because of the development of family psychology, to which they bring the experimental focus that characterizes much psychology.

BRINGING EMPIRICAL SCIENCE TO SOCIAL SCIENCE UNDERGRADS

When I moved from Home Economics to full-time in Sociology at the University Of Minnesota, the undergraduate course on the family in Sociology went from classes of 25 or 30 students when offered in the School Of Home Economics to 350 or more in sociology. I had to give up using the laboratory projects method of teaching. I preferred this method not only because it embodied an empirical evidence basis for teaching, but also because my favorite style of teaching is to be in the role of mentor and helper. The weekly lab periods allowed me to walk around the room and do just that. Although I could no longer use the lab projects method myself, I was eager to see others use it. I revised the projects and published them as *Family Analysis: Readings And Replication Of Selected Studies* (Straus, 1969a). In collaboration with Joel Nelson, I wrote a similar book for the introductory sociology course (Straus & Nelson, 1968). This book sold enough copies to finance an architect-designed house that I could not have afforded otherwise. But I never met anyone who used it until March 2008. Exactly 40 years after publication of *Sociological Analysis*, I gave a lecture at the University of Salford in Manchester, England. One of my hosts, was a recently retired professor who had used the book in his introductory sociology classes.

In 1968-69 I moved to the University of New Hampshire. This was my fifth move since the PhD. Why move again? The personal reasons were that I wanted do something different and a location which included ocean sailing and skiing. The professional reasons were that the position included a secretary and a research assistant, the challenge of helping to start a newly approved PhD program, and the opportunity to once again teach with weekly lab projects that enabled beginning students to "do" sociology, not just read about it and talk about it.

My love of teaching also found expression in founding with Richard Gelles the journal *Teaching Sociology*, and in research on "academic competence" in collaboration with Arnold Linsky. The findings from that research also bucked the tide. For example, this was a time of soaring enrollments in sociology and psychology, but our study of five thousand faculty in 16 universities and colleges found that sociology and psychology course received the lowest student evaluations in all 16 colleges. On the other hand, foreign language courses, which many students hated and were being eliminated as a degree requirement at some colleges, got high ratings. We suggested that the results reflected a discrepancy between student expectations at that point in history and what was actually taught. For sociology at that time many students wanted to learn how to be socially liberated and how to lead the revolution that would end the Viet Nam war, but what they got was Talcott Parsons "pattern variables." For psychology it was how to achieve inner peace, but what they got was rats running a maze. The high student evaluations of language classes were consistent with this explanation. Students in

a French or Spanish class needed to learn those languages, if only to meet university requirements, and they needed the instructors to accomplish that.

EXPULSION FROM SOCIOLOGY:

THE ORIGIN OF THE FAMILY RESEARCH LABORATORY

Sometimes the right thing happens for the wrong reason. That is certainly the case with the Family Research Laboratory (FRL). I often list myself as the founder of the FRL. However, the truth is that the FRL was founded in response to an effort to force me out of the sociology department at the University of New Hampshire. In that tumultuous period, I did have strong supporters such as Stuart Palmer and Arnold Linsky, but the majority of the department had a number of grievances. I recount these problems with some misgivings because the department has completely changed. Nevertheless, the grim events of 30 years ago provide an insight into some of the darker aspects of academic life.

One of the darker aspects occurred when the department sought to promote to full professor a popular teacher who taught third-rate philosophy under the guise of sociological theory and denigrated the idea that scientific methods could be used to study society. His dissertation was a study of an obscure sociologist with similar anti-science views. He had no publications whatsoever and I insisted that his dissertation be evaluated before we voted on his promotion. The evaluator reported that it did not even meet the standards for a MA thesis. Still he was promoted. That would never happen now in my department.

A conflict occurred over press coverage of research. My research on violence in the family was attracting national interest. I was on national TV and talk shows and the research was mentioned in many newspaper and magazine articles. Rather than seeing this as informing the public, some colleagues saw it as "publicity seeking." This was confounded with "political correctness" issues. Part of the objection grew out of the left-wing politics of the time and the Viet Nam war. One colleague accused me of diverting attention from the real social problems - racism, poverty, and the war, despite the fact that I was active in the anti-war movement. He also labeled the results which found more family-violence by blacks and by the poor as racist victim-blaming.

In the Viet Nam war protest era, being a recipient of a federal research grant was sometimes taken as a sign of complicity in the war. I had two grants and was thus, doubly complicit. There was an effort to require all grant applications to be reviewed and approved by the department before they could be submitted. Fortunately, it did not pass, which saved the embarrassment of the Dean having to overrule it.

Jealousy between faculty members can occur – at least that is my explanation for the reaction to the disproportionate number of the graduate students I attracted. The disproportion happened because I love being a mentor and get great satisfaction from helping students grow and accomplish things. In response to the uneven distribution, the department attempted to revise the Ph.D. program. After a year of effort, the revised program was submitted to the Dean of the Graduate School. The main change was a system of assigning graduate students to faculty so that if there were 20 graduate students and ten faculty members, rather than allowing students to choose their advisor, each faculty member would have two students assigned. I know that sounds unbelievable, which is exactly what the Dean said when he rejected the proposal.

Competition between related disciplines can also be a problem. My collaboration with psychologists and my respect for psychology was distrusted by some of my colleagues at the time. At an angry department meeting on an entirely unrelated issue, one colleague hurled what I presume he thought was the worst possible insult: "Why don't you go over to the psychology department where you belong?"

Sometimes moral absolutism blinds even the most thoughtful of academics. In collaboration with four colleagues, I was awarded an NIMH "training grant" which provided fellowships for five PhD students. The educational model was collaboration between graduate students and faculty. The grant provided funds for research to enable one member of the faculty each year to serve as the research director and, in collaboration with the students holding fellowships, to design a project and gather the data. The students were expected (but not required) to do their dissertation as part of the project they helped design and conduct. In the second year of the grant, one member of the grant faculty objected to this model. He said it was making students do the faculty's research and was exploiting students. The other faculty involved in the program pointed out that this was the model he had agreed to when the proposal was submitted, and that none of the students felt exploited. He was not convinced and took his complaint to the Dean. The Dean thought it was ridiculous. He continued to think it was exploitation and complained to NIMH. NIMH sent two distinguished researchers to investigate. They wrote an evaluation strongly praising the program. The complaining colleague never gave up. For the next nine years, until he retired, he would not even get in the same elevator with me.

A continuing strain in academic life occurs because of the discrepancy between the ideal of equality between scholars and the reality of the academic class system. Universities differ tremendously in wealth and resources, which is part of the reason faculty at leading institutions,

on average, are much more productive. Within universities there are also often large class differences, such as between professors with endowed chairs and others. The precipitating event which led to establishing the FRL was an attempt to achieved greater equity in the resources available to department members.

My appointment included a secretary and a research assistant. They were part of the package the university offered to attract me from the University of Minnesota. Under a different department chairperson than the one who had negotiated this package, things changed. For each of two years, he instated to the Dean that it was intolerable for me to have a full time secretary while the entire department had to share one secretary, and for me to have the only research assistantship. When he became more insistent, the Dean decided to end the problem by suggesting that I organize a center or lab with a budget that would include those two salary lines. I was glad to have this protection. But the benefit was much greater. It enabled development of an interdisciplinary research group that for thirty years has made enormous contributions to understanding family violence.

Although the birth circumstances were unfortunate, once born, amazing things were accomplished. I am proud of what members of the FRL have accomplished, and proud of the theoretical and methodological contribution I made to those accomplishments. These are based on the principles that violence has multiple causes, that all forms of violence are interrelated, that development of new measures is a crucial element in scientific progress, and that large scale epidemiological surveys can be used to investigate issues previously thought to be limited to clinical population studies. By June 2008, members of the FRL had published 746 scientific papers, and 46 books. In addition to the topics of my research, members of the FRL conducted pioneer general population theory-testing studies of sexual abuse, marital rape, elder abuse, missing and abducted children, crimes against children, and internet victimization of children. In the early decades of research on family violence.

CORPORAL PUNISHMENT AND THE "DISCOVERY" OF VIOLENCE

Corporal Punishment Experienced By University Students

One of the things that attracted me from Minnesota to New Hampshire was the opportunity to resume the lab projects approach to teaching sociology to undergraduates I could do that at New Hampshire because at that time classes in sociology were limited to 35 students. A career changing event took place in my class the first year at New Hampshire .

It was 1968 and "family violence" had not yet been discovered by academics. No aspect of family violence was in the syllabus of my course on the family. I had been studying families for almost 20 years at that time, but I was so imbued with the idea of the family as the locus of

love and support that, despite a penchant for the counter-intuitive, I did not perceive that the family was also the locus of violence. I had actually seen it occur in one session of my laboratory analog study of family problem-solving under stress. Near the end of the fifth of the eight four minute "trials" when the problem was made unsolvable, the husband got angry with his wife, yelled "match the colors, you dummy," and kicked her. Instead of continuing with the other three trials, I thanked them profusely for their help, and said that that the experiment was completed. I felt terrible about what I had seen, but it did not occur to me that psychological and physical aggression might be frequent when families face a problem.

What I came to call "the paradox of family violence" – the idea that the family is both the most loving and the most violent of all civilian institutions -- did not occur to me until four years later. It happened as a result of a discussion in my undergraduate class on the family. A student described an incident when she was a senior in high school and had not come home from a date at the agreed time. In the ensuing argument, her mother slapped her face. I asked the class if that had happened to anyone else, and another student said it had. To look into this a bit more, I asked the class to take out a sheet of paper and answer two questions: whether their parents had ever spanked or hit them, and whether they had done this when they were seniors in high school. Ninety four percent of the parents had used corporal punishment (CP from here on), and 26% had done it when the student was a high school senior. I was amazed by the 26% because I thought CP was used only with toddlers. Because the 26% was based on nine students in a class of 35, I thought it was probably a small sample error.

The Discovery Of Family Violence And The Origins Of The Conflict Tactics Scales

Over the next few days, I could not get the 94% and 26% figures out of my head. I asked two colleagues to have their classes respond to the same two questions. The results were almost the same. The national concern with the violence of the times probably had something to do with my fixation on these percentages. Nineteen sixty eight was what, 40 years later, Newsweek called the "The Year That Changed Everything" (November 19, 2007, p. 42) because of the combination of the Vietnam War, assassinations, riots in 175 cities, and rising violent crime rates. It certainly changed me. I realized that the high percentage of students who were hit by their parents indicated that the violence in the streets and in Vietnam also existed in almost all American homes. I remembered the book *Patterns Of Child Rearing*, co-authored by my Cornell colleague Harry Levin (Sears, Maccoby, & Levin, 1957). There were only two children in their sample who were never spanked. Consequently they could not compare spanked and not spanked children. They had to compare low and high frequency spanked children. They found that the children in the high spanking group were more

aggressive and had a less well-developed conscience. It started me on the road to thinking of spanking children as the primordial violence, and that the effectiveness and side-effects of spanking urgently needed to be investigated.

I decided to develop a questionnaire to cover not only CP, but also sibling violence, and violence between parents during the year college students were high school seniors. This student-report instrument was later named Form A of the Conflict Tactics Scales (CTS). Because this early version of the CTS included questions on violence between the parents, it enabled me to systematically investigate not only CP, but also marital violence in a general population sample. I pursued these two lines of research simultaneously, but I will first describe the CP research and then take up the partner violence research.

SPANKING CHILDREN: THE VIRTUOUS VIOLENCE

Prevalence Of Spanking

Despite the fact that CP was what first drew me to studying family violence, the grant application for the 1975 National Family Violence Survey did not focus on CP. This was because I had already sensed that, although everyone was against wife-beating and physical abuse of children and wanted research that could help end those problems, few thought of spanking by parents as "violence" or as an important problem, and most child psychologists and pediatricians thought spanking was sometimes necessary. Nevertheless, because the key instrument of that survey was the CTS, the study provided the first nationally representative data on the prevalence and side effects of spanking. A substantial part of *Behind Closed Doors*, the book that reported those survey results (Straus, Gelles, & Steinmetz, 1980 (2006)), was on this legally and morally approved form of family violence. The survey confirmed that almost all American parents spanked toddlers, and that CP continued into the early teen years for over half of the children. Other analyses found that spanking had important harmful side effects. The 1975 study did not include infants, but the 1985 replication did and we found that a third of parents slap infants (Straus & Stewart, 1999) for persistent "misbehavior" such as repeatedly pushing food off a high-chair tray.

Beating-Up The Bearer Of The Bad News About The Side Effects Of Spanking

One of the guiding assumptions of the Family Research Laboratory was the idea that all aspects of violence are interrelated. This is well illustrated by finding that spanking is associated with an increased probability of violence against a marital or cohabiting partner later in life (Straus, Gelles, & Steinmetz, 1980 (2006), p. 109-113; Straus & Yodanis, 1996). I felt that these and other results on CP were so important that they needed to be made available in detail. This was done in *Beating the Devil out of Them: Corporal Punishment in American*

Families And Its Effects on Children (Straus, 2001, first edition 1994). This book, like most of my articles on CP, was met by skepticism and sometimes hostility.

General Public And African American Anger. The research showing that spanking is harmful, and my conclusion that it should never be used, was attacked from both left and right, but much more from the right. It aroused the ire of millions who believe that spanking is sometimes necessary and are worried about the well being of children who, without CP, would be "running wild." This was particularly strong among African Americans who believed that CP was necessary for parents to have control in the rough environment faced by so many African American parents. I was booed and shouted at for "trying to ram white middle class values down our throat" during a speech before a largely African American meeting of social workers.

On the political right was opposition from fundamentalist churches and organizations such as the Family Research Institute, a Protestant fundamentalist organization which has preserving the "right to spank" as an important objective. In 2007 a group of pediatrician who believe that CP is necessary established a new organization, the American College of Pediatricians, to issue pro-spanking press releases that the public would think were from the American Academy of Pediatrics.

From the left, my focus on CP as a major form of family violence was seen as a distraction from the "real" family violence – wife-beating. Breines and Gordon, (1983, p 504-5), for example, derided my "general abhorrence of violence" and found it "unfortunate" that I identified physical punishment as violence. Feminist ire was also aroused by the results showing that the more CP a person experienced as a child, the higher the probability that person would physically attack a partner later in life. Moreover, this finding applied to violence perpetration by women as well as men (Douglas & Straus, 2006; Straus & Yodanis, 1996). These results were irksome and not believable because they contradict two pillars of the feminist approach: the idea that partner violence was committed almost exclusively by men and that it was done to keep women socially subordinate to men.

Academic Opposition

Although feminists and Protestant fundamentalists were the most vocal critics of the CP research, the most damaging effect was the opposition of academics. They were the most damaging because academics control research funding and publication. In 40 years of almost continuous grant support from NIH and NSF, no research proposal on CP was ever funded. All my research on CP (over 30 papers, a book and another in process) was funded by what can be called the Robin Hood funding mechanism -- taking from well endowed projects to feed the poor starving research on CP. On the publication side, my papers on CP have been scrutinized

more rigorously and more extensively than other papers. For example, is the paper which presented the results of a longitudinal study which found that CP boomerangs and is associated with an *increase* in antisocial behavior two years later. I was asked to provide the computer output of the statistical analyses and the raw data file. That is a legitimate request, but it is the only instance of such a request in more than 200 published papers. Another paper was sent to five reviewers instead of the usual three.

The title of a review in *Contemporary Psychology* conveys the predominant reaction of my fellow social scientists. It is *Beating The Devil Out Of The Reader* (Holloway, 1996). I believe the reviewer was so incensed at the conclusion that children should never, ever be spanked, that she did not read beyond the opening chapters (Straus, 2001, pp v – ix). One indication of not having read the book is her claim that the book did not address a favorite theory of academic defenders of CP: that, if spanking is done by loving parents, it is harmless. But there is an entire chapter reporting an empirical study in which parental love and support is the moderator variable.

Ignoring the Evidence

By far the most damaging reaction to the CP research was no-reaction, by which I mean they simply ignored the evidence. I will mention a few of the many examples.

National Academy Of Sciences Panel. I was a member of a panel on child abuse. I presented the panel with several studies showing a clear link between using spanking and an increased probability of physical abuse (Straus & Yodanis, 2001). Two studies, for example, found that two thirds of cases of physical abuse known to child protective services began as CP, but escalated out of control. The committee voted not to include a recommendation to end CP as a means of preventing physical abuse, even though the evidence was stronger than the evidence behind almost all the prevention steps they did recommend. The only thing I could do was write a dissenting opinion (National Academy of Sciences, 1993).

Special Issues Of Two Leading Journals. In 1999, *Child Abuse and Neglect* published special topic issues on prevention of child abuse, and in 2006 the *Journal of Interpersonal Violence* did the same. Neither had a single word on CP. I wrote a commentary on the first of these special issues to summarize the evidence on the link between CP and physical abuse and recommend ending CP as a major strategy to prevent physical abuse (2000). The omission seven years later from *Journal Of Interpersonal Violence* is especially telling because by that time the evidence was even stronger and had been summarized in a widely read meta analysis of 88 studies. Ninety-three percent of those studies found harmful

side-effects of CP (Gershoff, 2002). This must be a record for consistency between studies of child development.

Child Development Textbooks Ignore CP. In the late 1980s I happened to be looking at a child development textbook and was amazed to find not a word about parents spanking their children. I was amazed because over 90% of American parents spank toddlers, at least occasionally; and because this has been documented by research since the 1930's (Anderson & Anderson, 1932; Sears, Maccoby, & Levin, 1957; Straus, Gelles, & Steinmetz, 1980 (2006)). This led me to examine ten other textbooks. I found two thirds had nothing at all on CP, and those that did allocated an average of only half a page to the topic. None recommended no-spanking. A few years later I checked on books published in the 1990's and then in books published in 2000 to 2006. There has been essentially no change.

Twenty Studies. In 2003, when an article "Twenty Studies That Shook Up Child Development" (Dixon, 2003) was published, I thought a breakthrough had occurred. I rushed down the list and was overjoyed to see that my longitudinal study listed. That study was unique at the time because it was longitudinal and could therefore investigate whether, as almost everyone believes, CP when judiciously administered, has the long term benefit of increasing the probability of good behavior. Contrary to the conventional wisdom, I found that CP is associated with a subsequent increase rather than decrease in antisocial behavior. The joy in seeing my article listed was short lived. When I read the article, I discovered that "shook up" meant aroused controversy and skepticism, not influenced the discipline. Nevertheless, problems associated with spanking have started to appear in sociology of the family and juvenile delinquency textbooks. I hope this means that child development textbooks of the 2010 decade will also cover it more adequately.

LET IT ALL HANG OUT

As mentioned previously, the early student-report version of the CTS was not restricted to corporal punishment. It also provided data on violence by the students against their parents, violence between siblings, and violence between their parents. One of the first uses of the data on violence between the parents was to test the then fashionable "let it all hang out" approach to preventing and treating marital relationship problems. During this period (late 1960's-1970's) a pop psychology developed which, among other things, reflected elements of Freudian psychology and the counter-culture of the times. It emphasized "authenticity" and "honesty" in human relationships. But many of the behaviors advocated under these great sounding terms were acts of psychological aggression. Couples were advised to be "honest" in their relationships by not covering up their anger. Yelling, screaming, and "calling a spade a spade"

were deemed more "honest" and to have a cathartic effect by "releasing" pent up anger. Couples were also advised to "work off" their aggression by such things as beating a mattress with a tennis racket, or engage in mock fights with Styrofoam bats. I was fascinated and repelled by this approach to relationships, and to "catharsis" as a means of dealing with aggression. They seemed to me like routes to human relationship disasters and family violence rather than preventive steps. Does telling a partner that he or she is mean or a lousy lover clear the air or does it make it harder to resolve those problems? Does pounding a bed with a tennis racquet when you are angry have a cathartic effect and reduce the probability of pounding a spouse, or the opposite?

The Conflict Tactics Scales permitted an empirical study of the catharsis approach because the Verbal Aggression scale measured giving vent to one's anger. It could be used to test whether being "honest" with a partner was associated with less physical aggression (the theory of the time) or associated with more physical aggression. The results were reported in my National Council On Family Relations presidential address "Leveling, Civility, And Violence In The Family" (Straus, 1974b). As I expected, the results showed that more psychological aggression against a partner, the more physical violence against a partner. I concluded the article by arguing that, except among a subpopulation of severely inhibited persons, the problem was the opposite of being too buttoned up. I argued that couple therapy should instead focus on modulating anger, much as today's anger management programs do that to prevent and treat partner violence (Stith, Rosen, & McCollum, 2003). However, because this was a cross-sectional study the results were not clear evidence against the catharsis theory, even though they were consistent with excellent experimental evidence showing that catharsis in the sense of expressing anger, increases rather than decrease subsequent aggression (Hokanson, 1970). But a few years later, a prospective study (Murphy & O'Leary, 1989) showed that psychological aggression at Time 1 was associated with an increased probability of subsequent physical violence.

THE GENDER SYMMETRY CONTROVERSY

As was the case with the research on corporal punishment, the research on partner violence (PV from here on) was attacked by both the left and right. My first papers on PV focused on demonstrating the high prevalence of PV and on the relation of male-dominance to violence against women (Straus, 1973, 1974a, 1976, 1977, 1978). They were cited approvingly by almost everyone, especially feminists, and I was on many radio and TV shows.

Right Wing Objections. The situation changed when *Parade*, a national Sunday newspaper supplement, published an article on wife-beating in which I was quoted as saying

that an essential step in ending wife beating was to get rid of the idea of the husband as the head of the household. Previously, I had been quoted many times as saying that a crucial step is *equality* between husbands and wives. Calling for equality aroused no controversy because Americans, by definition of the Declaration of Independence, are committed to equality. However, when it came to specifics, it took a long time before most Americans extended voting rights to women and accepted African Americans as equal. It was not until the 1967 that the supreme court ruled that laws prohibiting interracial marriage are unconstitutional. Similarly, when "equality" in the family is translated into the specific that the husband is no longer the head of the household, the lines were drawn. Letters to the editor, to me, to the University President, and to NIMH, labeled me as anti-family. Some demanded the university and NIMH close down my research because it was undermining the family. I received two bibles with marked passages.

Left Wing Objections. Stronger criticisms from the left greeted the papers and books reporting approximately equal rates of physical attacks on partners by men and women, including *Behind Closed Doors: Violence in the American Family* (Straus, Gelles, & Steinmetz, 1980 (2006)). The second National Family Violence Survey in 1985 (Gelles & Straus, 1988; Straus & Gelles, 1986) also found approximately equal perpetration rates by men and women. By the late 1980's the two national surveys and over 20 studies by others removed all vestiges of uncertainty for me about gender symmetry in perpetration of PV. I became convinced that, in contrast to male predominance in almost all other forms of violence, when it comes to partner violence, women physically assault male partners at about the same rate, and with about the same intensity as men assault female partners. Although women are injured more often, about a third of the injuries are sustained by men, including a third of deaths inflicted by a partner males (Rennison, 2000; Straus, 2005). This led to a series of methodological and theoretical articles to explain gender symmetry in PV and the implications of gender symmetry for prevention and treatment of PV (Straus, 1991b, 1993, 1995, 1999, 2005, 2006; Straus & Gelles, 1990; Straus, Kaufman Kantor, & Moore, 1997; Straus & Smith, 1990). These publications escalated the already harsh criticisms of me and my colleagues who represented what is sometimes called the "family violence" approach to understanding PV, as compared to the feminist approach. A core proposition of the family violence approach is that multiple causes are required to explain violence between partners, only one of which is male dominance in families and society; whereas a core proposition of the feminist approach is that patriarchy is the overwhelming cause of PV (Dobash & Dobash, 1979; Yllo & Bograd, 1988).

Expulsion As A Feminist. In 1976, I would have instantly been named as a key feminist research on family violence. But as a result of continuing to publish papers presenting evidence on gender symmetry in PV and, even worse, insisting that ending violence against women had to include steps to prevent and treat violence *by* women, I was, in effect, excommunicated as a feminist. This hurt because, then as now, I consider myself a feminist in the scholarly sense (see for example, (Straus, 1976)) even though not in the political activist sense. The excommunication was enforced in many ways, such as telling two graduate students they would never get a job if they did their dissertations with me, and blocking my nomination for any position in the American Sociological Association.

Despite trying to prevent my running for president of the Society For The Study of Social Problems, I was elected president in 1989. However, at the presidential address, the first rows of seats were occupied by critics. When I started to talk, they stood up and walked out. Moreover, when the presidential address paper was in press (Straus, 1991a), I found out from a friend that three critiques had been invited. My presidential address may be the only instance in the 55 years of publishing presidential addresses that there have been critiques of a president's address. I did not object to that. What I found shameful is that it was hidden from me, and even more shameful that my request to see the critiques and write a rejoinder was refused. Only after I said that I would ask the Board of Directors to require the editor to publish a rejoinder did that come about. My Social Problems society presidential address was on corporal punishment of children, not on PV, but it was used to critique my research on PV. Correctly or incorrectly, I inferred that the main motivation was to discredit me as a means of discrediting the politically intolerable research evidence.

An extreme example of attempting to discredit the evidence by discrediting me occurred when the head of the Canadian Commission on Violence Against Women said at a public hearing that you can't believe anything Straus says because he beats his wife and sexually exploits students. I complained to the Minister of Women's Affairs and the commission chair wrote a semi-retraction. This was not an isolated instance. During these 20 years of being maligned, I kept reminding myself that it is in the nature of social movement to go to excess in order to achieve even a small movement in the desired direction. Because I shared the goal of gender-equality, my response was to keep focusing on the empirical studies and their implications.

(Insert Figure 1 about here)

Nevertheless, these attacks hurt because, as I said, I consider myself a feminist and a commitment to equality in all aspects of society, including between men and women, has been

a part of my social heritage, beliefs, and behavior all my life. Because my critics find that hard to believe, I will mention some relevant publications and a personal incident. At the personal level, a small example occurred when I arrived in Ceylon for my first full time teaching position in 1949. A reporter came out to the boat before it docked to interview me. The front page story the next day said "Local husbands please note" (Ceylon Daily News 19 November 1948). The reporter had found me in the laundry room ironing my wife's dress. Many men in that era would have been embarrassed. I felt proud to be setting that example.

At the professional level, I served on the American Sociological Association Committee on the Status of Women (1973-75), and chaired the committee on non-sexist terminology (1974-75). I was nominated for these positions because I had published feminist influenced studies on marital power relationships and the subordinate role of women and girls in family relationships (Straus & Straus, 1968; Straus, 1967, 1971). Moreover, the very first study using the early student-report version of the CTS found that male-dominance was related to PV (Straus, 1973), and that is what I emphasized in talking about the study with colleagues and to the press.

That 1973 study, like studies by others since then (reviewed in (Straus, 2007b)), also found that female as well as male dominance was a risk factor for PV. This did not seem surprising to me because a key assumption of conflict theory (the theoretical basis of the Conflict Tactics Scales) is that inequality leads to violence (Coser, 1956). Thus, inequality in the form of female dominance should also lead to violence. Moreover, finding female dominance was associated with PV did not contradict my belief that ending male dominance was the key to ending PV because I assumed that male-dominance was the prevailing social arrangement almost everywhere. Consequently, my early family violence research was swimming with the feminist tide, even though from the beginning I also emphasized that PV has many causes, not just male-dominance. But the emergence of the battered women's movement made it a tidal wave that that overwhelmed consideration of any cause except "patriarchy" and drowned any evidence of mutual violence (Straus, 1992). In 2006 I finally decided to document the processes used to hide, deny, or distort the evidence on gender symmetry (Straus, 2007c), and to explain the reasons for this massive scientific cover-up, sometimes bordering on fraud (Straus, 2007e).

THE CONFLICT OVER THE CONFLICT TACTICS SCALES

The Conflict Tactics Scales (CTS) were developed in 1969-70, just before the emergence of the battered women's movement. The theoretical bases were sociological conflict theory (Coser, 1967) and family systems theory (Haley, 1976). The former guided me to think of conflict as an inherent and necessary part of human relationships and the latter guided

me to think of violence as a social interaction that took place in the context of an interrelated system of relationships. Therefore, to understand PV, it was necessary to know what both partners were doing in relation to each other, in relation to the children, and what the children were doing in relation to each other and to their parents. Consequently, Form A of the CTS repeated each of the questions for each of these family relationships.

Although the CTS was not designed on the basis of feminist theory, from the very beginning I expected it to provide the data on family violence needed to test and validate feminist theory. Results with the CTS have consistently shown that male-dominant households had the highest rate of PV (Coleman & Straus, 1986; Straus, 1994; Straus, Gelles, & Steinmetz, 1980 (2006)). This research was praised by battered women service providers and academic feminists because it focused on female victims and patriarchy as a major cause. That reversed when I finally recognized the importance of the many studies showing that the same percent of women as men physically assaulted their partners. By 1986 there were at least 23 such studies, including two with nationally representative samples. These results were regarded as outrageous and unbelievable by those who had struggled to aid battered women and by their academic allies. The effort to refute this evidence took a number of forms, including accusations of misogyny, criticism of the research design, and invariably, by claiming that the CTS provided misleading or invalid evidence.

The CTS, like any instrument, has limitations. A major, but rarely mentioned, limitation of the first version (corrected in the revised CTS) is that it did not measure sexual violence. However, there are numerous invalid criticism (Straus, 1990, 2007a). I will comment on the criticism which is given in almost every article that uses CTS data. It is that "just counting blows" is misleading because it does not take into account the different effect that these blows have on women and does not take into account the context leading to PV by women. I certainly agree that data on context, motives, and consequences such as injury are essential to understand PV. I have measured such variables in every study, without a single exception. Prevalence rates are important, but the most important aspect of my research has always been testing theories about why PV takes place and testing theories about the consequences. However, to do that, context, causes, and consequences must be measured *separately*.

The necessity of an instrument which measures what the supposedly makes the CTS invalid (measuring only the number and severity of assaults) can be seen more clearly by an analogy with an instrument that is not as emotionally and publically charged, such as a spelling test. Is it a crippling limitation of a spelling test that it "only counts" the number words a child can spell? No, it is an advantage, or more correctly, it is essential that a child's score does *not*

also include the context that might lead the children to spell well or badly. It is also essential the test score does *not* indicate whether the child's school work or psychological adjustment has been harmed by bad spelling. Those are crucial questions, but to answer them it is essential to have separate measures of the context and the consequences variables. Unless they are separate, it is not possible to compute a cross-tab or a correlation to find out if, and how strongly, a possible cause, context, or consequence variable is related to bad spelling. Another reason measure of partner violence should not depend on whether there was an injury is that, if injury is a criterion, it would result in a vast underestimate of the extent of partner violence because at over 90% of assaults on a partner do not result in physical injury.

I have responded to these and other erroneous bases for claiming that the CTS gives invalid and misleading results (Straus, 1990; Straus, Hamby, Boney-McCoy, & Sugarman, 1996). These attacks on the messenger bearing the bad news are understandable, even admirable, for service providers who want to refute anything that might hinder helping their clients, but it is inexcusable for social scientists to continue to make these and other erroneous claims. The reasons for the cover-up effort are described elsewhere (Straus, 2007e), as are some of the methods used to hide or deny the evidence (Straus, 2007c).

TRENDS IN FAMILY VIOLENCE

In the 1970's, cases of child abuse had increased by about 10% per year and hundreds of shelters for battered women were opened. There was virtually complete consensus that the United States was experiencing an epidemic of child abuse, and of wife-beating. This did not seem right to me because of the tremendous growth of "protective factors" such as the increasing educational level of the population, increasing age at marriage and age at birth of the first child, fewer children per couple, growing availability and use of family therapy, decreasing use of CP, national programs to increase awareness of child abuse, and the nationwide establishment of child protective services which presumably provides assistance that will lower the probability of subsequent abuse. These same changes, are also protective factors for wife-beating, to which must be added increased gender equality and the efforts to end domestic violence by the women's movement. In 1979, I presented a paper at a symposium celebrating the first Year Of The Child (Straus, 1979) which listed these and other protective factors and also risk factors. I concluded that increases in protective factors probably outweighed increases in risk factors and that the net effect should be a *decrease* in both child abuse and spouse abuse.

In 1985, the second National Family Violence Survey allowed Richard Gelles and me to test that theory. We found that child physical abuse had decreased by 47%, and wife-beating

had decreased by 27% (Straus & Gelles, 1986). These results were greeted with doubt by child protective workers and hostility by feminists. The doubt was because it contradicted their daily experience of more and more cases. The hostility occurred because we found a large decrease in male violence toward female partners, but no decrease for PV by women, and because we suggested this might be the result of the domestic violence campaign ignoring female perpetration. The *Christian Science Monitor* interviewed a leading feminist criminologist (Richard Berk) (18 November 1985, pp. 3-4). He said "Given all we know about the pattern of crime statistics, a 47% drop is so unprecedented as to be unbelievable. Never before has there been a drop of that magnitude, that rapidly." On the contrary, other crime rates did change that much and that fast. The homicide rate increased by over 100% between 1963 and 1973. Then from 1980 to 1984 homicide dropped at a faster annual rate than our studies found for male PV. I believe this is another example of an ideological or theoretical commitment blinding social scientists to the evidence. Since then, a national survey by Kaufman Kantor using the same questions in 1992 found a continuation of the decrease in assaults by men, and again no decrease for women (Straus, ; Straus, Kaufman Kantor, & Moore, 1997). Most recently, the research of Finkelhor and Jones has found sustained decreases in rates of child physical abuse (Finkelhor, 2008; Jones & Finkelhor, 2003).

TWO OTHER STRUGGLES AGAINST THE TIDE

I have done lots more that is out of the ordinary, such as assisting a physicist colleague in a study of cosmic rays that required setting up experiments at 20,000 feet in the Himalayas. I will describe just two recent examples to indicate that, at age 81, I am still swimming against the tide.

A New Approach To Research On Discipline

In 2005, I decided to broaden my research from corporal punishment to a more comprehensive examination of discipline. As always, a first step was to develop a multidimensional instrument to measure discipline - the *Dimensions Of Discipline Inventory* (Straus & Fauchier, 2007). Measures should be based on a clear definition, but I discovered that, although every child development text book devoted space to "discipline," few defined it. Even comprehensive and presumably authoritative works such as the *Encyclopedia of Applied Developmental Science* (Fisher & Learner, 2005) failed to define discipline. The most dramatic example is the 2,640 page *Handbook of Parenting* (Bornstein, 2002). The term discipline is used hundreds of times by the authors of different chapters, but none defined it. I then did a content analysis of ten child development textbooks published between 2000 and 2006 and found that only three defined discipline.

Even more interesting than the lack of definitions was the nature of the three definitions. Discipline was defined as anything parents do to bring up a well-behaved child, including providing love and support and exemplifying good behavior. This is so all-encompassing that it is useless. The uselessness of such a definition may be why the authors of seven out of ten child development textbooks declined to define discipline. At the other extreme, for many parents "discipline" means punishment, or often spanking. That is worse than useless because it interferes with perceiving that most of what parents do to correct misbehavior is not punitive; for example, telling the child No, explaining what is wrong, or diverting the child to another activity. I toyed with writing an article critiquing the concept of discipline and arguing that it should be abandoned. But that is unrealistic because, as presently defined, it is such a widely used (even though scientifically useless) concept. My solution was to add a qualifier and refer to what the Dimensions Of Discipline Inventory measures as "corrective discipline."

International Dating Violence Study And International Parenting Study

The international interests that led to my research in Ceylon in 1949, to the comparative study of families in Minneapolis, San Juan, Bombay study in 1964, and to visiting professorships in England in 1975 and Belgium in 2003, are still stirring within me. They led to two multi-nation studies.

International Dating Violence Study. In 2001 I began organizing an international collaborative effort to study violence against dating partners by university students. The target was collaborators in 30 nations. The final group consisted of a consortium of researchers at 68 universities in 32 nations. In 2002, when there were consortium members in about ten of the proposed target of 30 nations, I submitted a proposal to fund the study. The NIMH review panel turned it down. One of the objections was that the sample violated every rule for good sampling. The proposal purported to compare nations, but it studied only students. Moreover, it did not study a representative sample of students, only students who were in classes where the consortium member could administer the questionnaire, and typically at only one non-randomly selected university out of the many in a nation. I proceeded with the study anyway because, even though the students in the study are not representative of their nation, I believed that their behavior and beliefs reflect the influence of the national context in which they live and would therefore show meaningful differences between nations. The data gathering was finished by 2006. There is now ample evidence that the gamble paid off. The validity of the data is shown by the correlation between seven concepts as measured by the International Dating Violence Study and as measured by studies using representative samples. The correlations that range from .43 to a high of -.69 (Straus, 2007d). The -.69 correlation is between scores on a concept

that is very important for understanding PV -- male dominance in the relationship. This correlation shows that the more male dominance reported by the students in this study, the lower the score on the female Gender Empowerment Measure published by the United Nations Development Program (2005).

International Parenting Study. The evidence on the validity the International Dating Violence Study, and the 24 papers so far published, encouraged Angele Fauchier and me to develop a study using the same type of sample, but focused on parent-child relationships – the International Parenting Study. The *Dimensions Of Discipline Inventory* is one of the main instruments in the questionnaire. The target is to include 40 nations, and as of this writing researchers in 20 nations are on-board.

Intimate Terrorism

The most recent example of bucking the tide is a paper challenging what may be the most widely praised conceptualization of PV in the past decade – the concept of "Intimate Terrorism" (Johnson, 2006). One reason for the popularity of this concept is that it is part of a typology of PV that recognizes the inherently dyadic nature of PV. Intimate Terrorism may also have gained popularity because it is described as “perpetrated almost entirely by men” (Johnson, 2006). Thus, it provides a way to keep the blame for PV focused on male perpetration, while at the same time recognizing the dyadic nature of PV. However, empirical research shows that, except among populations already consisting of high violence male offenders, such as those arrested for PV or the partners of women in shelters for battered women, the percent of male and female "Intimate Terrorists" is about the same (Straus & Gozjoko, 2007).

BUCKING THE TIDE DOES NOT MEAN WORKING ALONE

Bucking the tide, is not the same as ignoring or rejecting the ideas and help from others. In almost sixty years of teaching and research, the number of people I am indebted to is staggering and wonderful. A few have already been mentioned. I would not be feasible to describe each of their contributions. So I will use categories rather than individuals, except to mention some examples. I hope the others will excuse me.

First there is the support staff – secretaries, business managers, statistical aids. The work of Sigi Fizz as my secretary (now more appropriately called Administrative Assistant) for 25 years, and Doreen Cole for the last 15 year are a large part of the explanation for my productivity. They have done everything well and developed more efficient approaches to many tasks, and they have rescued me from a million mistakes.

I am tremendously indebted to my graduate students and research assistants, many of whom were also co-authors, sometimes first author. Others have instigated crucial turning points. For example, it was Richard Gelles, who proposed the research I am best known for -- the National Family Violence Surveys (Straus & Gelles, 1990; Straus, Gelles, & Steinmetz, 1980 (2006)). Kirsti Yllo's dissertation on state-to-state differences in the status of women (Yllo, 1980, 1983) led me to establish the State and Regional Data Archive and ten years of research using the 50 states as the cases (Baron & Straus, 1989; Linsky, Bachman, & Straus, 1995). Ignacio Luis Ramirez's dissertation on violence between dating partners by Mexican American and Non-Mexican university students in Texas (Ramirez, 2001) started me on the International Dating Violence Study (Straus, 2007b).

Finally, there are the faculty and post-doctoral fellow colleagues. Their imprint on my development as a researcher and teacher is so large and so crucial that I can honestly say I would not be writing this today were it not for them. The fact that it is not feasible to acknowledge them individually can be grasped from the fact that there have been more than 60 post-doctoral fellows in the past 30 years. But I could not fail to mention my closest colleague, David Finkelhor. I started benefiting from his creative and incisive thinking during the course of his dissertation research, and for more than 25 years he has been a treasured colleague and mentor.

WHAT STEERED THE BOAT AND KEPT IT AFLOAT

It is true, but only part of the truth, that three principles guided my career. First, is a commitment to the principle that scientists have an obligation to both question and report what they find, no matter what the prevailing scientific theory or public opinion, and no matter what their personal opinion. Second, is a fascination with the unexpected, counter-intuitive, or controversial. Third is adherence to the principle that I think was stated by Alexander Fleming when asked about his discovery of penicillin: if you stumble on to something interesting, drop everything else and study it.

Those three principles, however, are not the whole explanation of what led me to so often swim against the tide. It is likely that there is also a personality element that first manifested itself when I was a toddler. I was, to understate things, a "difficult child." I was "willful" even when there was no obvious reason. For example, my mother wanted me to learn French (she had spent years in a convent school in France) and spoke to me in French. However she told me that when I was four years old, I refused to say another word in French and became angry at her if she did. More serious was that I was a disobedient and defiant child, both at home and in school. I was seen by counselors and psychologists for what would

probably now be called "oppositional defiant" behavior. The public school I attended "advised" my parents to send me to a private school, and mentioned a military school. Fortunately, they sent me to the Hessian Hills School, a bastion of "progressive education." There was no fixed curriculum to rebel against. In each class the teacher asked what I wanted to find out, and then helped me do it. Nonetheless, I got into trouble.

On the other hand, even as a child, my rebelliousness, had positive aspects. In 1938 when I was 12, even though forbidden to do so, I bicycled the 50 miles from my grandparent's house in London to their house in Ovingdean on the English Channel. In 1940, at age 14, to the despair of my parents, I set out for a summer of biking. I took the overnight boat that used to run from New York to Boston. Then I biked up the coast of Massachusetts, New Hampshire, and Maine to the Maritime Provinces, and then to the tip of the Gaspé in Quebec. Then I biked the St Lawrence Valley to Montreal. I ended the trip by hitching a ride on a barge from Lake Champlain Vermont to New York, rounding Manhattan Island and the Statue of Liberty in grand style (for a barge ride) to the Barge Canal Terminal in Brooklyn, which was not far from home in Jamaica, New York. Why did my parents let me do this? They had almost no choice. They did refuse to let me go. I said I was going to do it anyhow. I had saved money from a newspaper route to cover part of the trip. I assumed that when my money ran out, they would send more. They did.

It is hard to know what prevented my oppositional-defiant behavior from developing into full blown delinquency and antisocial behavior. But I think a crucial part of it was the devotion, love, and persistence of my parents. They struggled continually to correct my misbehavior, and, despite extreme provocation, never used harsh punitive methods. I cannot remember a single instance of being spanked, hit, or demeaned. As I mentioned, rather than follow the recommendation to send me to a military school, they chose a school which provided almost nothing for me to rebel against (but I did). I was there for what would now be called middle school. I finally straightened out, at least enough to go to Jamaica High School as a sophomore. My mother must have had one the rare opportunities to feel good about her efforts, and about me, when I became interested in French, and became president of the French club. It was then that she told me about my refusal to speak French as a toddler. Another part is the constant example of moral, socially committed, and responsible behavior they provided. To take one small indicator, in the 1930's my father brought home a picture of an elderly African American couple looking up reverently at a picture of Abraham Lincoln. That picture was in my house all during my childhood. It is in my house to this day.

In 1992 I married Dorothy Frost Dunn. After we were married I discovered that she had never spanked her children. No wonder we were attracted to each other. I had found a soul-mate who has helped me keep my boat afloat and on the best course I have ever sailed.

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