

# New Theory and Old Canards About Family Violence Research\*

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*In response to three alternative viewpoints to his Presidential Address, "Discipline and Deviance: Physical Punishment of Children and Violence and other Crime in Adulthood," Straus clarifies several key points regarding the theorized connection between physical punishment and the etiology of violence. He draws on his extensive prior research to justify his conclusion that criticisms of his work on family violence are unfounded.*

The commentaries on my article fall into two categories. Joan McCord's is mainly focused on an alternative theoretical model, and one which I think has promise. Those by Demie Kurz and Donileen Loseke are mainly "negative campaigning" against a straw man. They write about a person who studies violence without regard for gender, class, race, societal context, cultural meaning, or the unique characteristics of the family as compared to other social groups. Yet these are the very things that have been the hallmark of my research and on which I have made pioneer, and in some cases unique, contributions. Since they allege a failure to deal with these issues, part of my response must be to cite sections of my paper in this volume and many previous papers in which I deal with issues they allege I ignore.

## The Hidden Agenda

The allegation-by-allegation response to Kurz and Loseke which follows provides a basis for readers to judge for themselves whether these two critiques are "negative campaigning against a straw man." If I am correct, it raises the question of what led two accomplished scholars to misrepresent my research and to ignore the contrary evi-

dence in books and articles they themselves cite. I do not think the motive was a personal attack because I do not know either Kurz or Loseke. Rather, I suspect the distortions are a result of two elements: the first is what I will call "theoretical and methodological monism," and the second is writing in an advocacy mode when the situation calls for a scientific mode.

The methodological monism is manifest at many places and is succinctly expressed in Kurz's sweeping and unqualified assertion that "Straus' conclusions arise from conceptual, theoretical, and methodological flaws." The basis for this assertion cannot be the inadequacy of my data because, at several places in the paper, I emphasize the limitations of the data and say that their sole purpose is to suggest the need for more definitive research. In addition, despite those limitations, much of what I present has also been found by research using different conceptual, theoretical, and methodological approaches. For example, my discussion of McCord's data will show that, despite her explicit rejection of Cultural Spillover Theory and despite using a very different type of data, her findings on the criminogenic effects of physical punishment are parallel to those presented in my paper. Similarly, my as-

sertion that physical punishment is almost universal in American society is supported by many different studies using diverse methods. Sears, MacCoby, and Levin (1957), for example, used in-depth studies of toddlers and their mothers, as did Newson and Newson (1963). The concordance between these studies cannot result from using the Conflict Tactics Scales or a Conflict Theory Approach because, except for me, none used that approach. Given these facts, I conclude that by "flaws" Kurz means that I do not use the conceptual, theoretical, and methodological approaches that *she* favors, and, as I will suggest below, that I do not arrive at "politically correct" findings.

Both Kurz and Loseke imply that it is a failing on my part to not accept their conceptual and methodological monism. That is a charge to which I gladly plead guilty. In contrast to their implication that there is only one sound approach to sociological research, I believe that sociology needs both qualitative research, formal hypothetical deductive research and exploratory research, and phenomenological research as well as research focused on so-called objective events. The absence of any of these modes would handicap sociology and handicap efforts to understand such problems as racism, sexism, and vio-

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lence—social problems that both they and I are committed to ending.

Although, as will be shown below, I have done most of what Kurz and Loseke allege I ignore, there are, of course, issues and methods that I have not pursued. Unfortunately, the language used by Kurz and Loseke to point to these omissions conveys an almost totalitarian demand to follow one methodological, theoretical, substantive, and moral agenda. Despite the unfortunate tone in which it is presented, I agree that attention needs to be given to such things as the agency of individuals, to those who do not go on to commit violent acts, to how people interpret their victimization, to the motives of the parents, and to violent individuals who were not physically attacked as a child. Everything on such a list needs to be investigated, but not by every scholar. Science is a social institution, and the norms of that institution have evolved to provide a means of attending to the complexity of phenomena, if not by one scholar, then by others. That is the difference between science as an institution and the work of a single scientist. But Kurz and Loseke will not find such a division of labor satisfactory because, in my opinion, they believe they know the truth, and that everyone must conform to that (version of the) truth.

My second hypothesis concerning the distortions in Kurz and Loseke's critiques is that they occurred because the real focus of their attack is the findings on the high rate of assaults by wives that I and at least 20 other investigators report (Straus 1980, 1989; Straus, Gelles, and Steinmetz 1980). They and others see those findings as a threat to women and thus a threat that must be destroyed. Previous attempts to destroy these findings have taken a variety of forms, including picketing and shouting to prevent me from speaking. In the case of some colleagues, there have been attempts to block a promotion, a bomb threat, and pressure to suppress findings. I do not accuse Kurz and Loseke

of these activities. However, I believe that, intentionally or otherwise, their critiques are part of the long series of attempts to blame the messenger for the bad news about assaults by wives. Their attempt to discredit me as a scholar and to discredit the Conflict Tactics Scales (the instrument that has produced most of the findings on assaults by women) are part of what I believe is their real objective: discrediting the findings about assaults by wives.

I take solace in the thought that the lengths to which Kurz and Loseke have gone can be interpreted as a recognition of the power of the body of theory, methods, and findings that my colleagues and I have labored for more than twenty years to create. If our work had little or no impact, no one would bother to attack it, regardless of how erroneous they judged it to be. It is only work that has the potential to importantly affect science and social well being that is worth attacking. Such attacks have been the fate of some of the most important works in sociology in recent decades, including the classic study of educational desegregation by James Coleman and of race and class by William Julius Wilson. I do not presume to have made contributions equivalent to those of Coleman or Wilson, but the bitter criticism they had to endure helps to put the comments of Kurz and Loseke in perspective.

I will turn now to the specific points raised by each of the three commentaries.

### Response to Kurz

*"Strauss fails to provide a rationale for why corporal punishment should be privileged as one of the major factors contributing to adult use of physical force."*

This opening charge is contradicted by a preceding sentence and the concluding paragraph, where Kurz says "The causes and effects of physical punish-

ment of children are an important area of study." However, the bulk of her critique is couched in terms that reflect this allegation, so I will focus on that. It is a puzzling allegation because providing a rationale for the study of physical punishment is the sole purpose of my paper. Consequently, I infer that what Kurz means is that the theoretical argument and the empirical data suggesting the importance of physical punishment do not convince her. Of course, she is entitled to that opinion, and she might even be correct. However, I believe that attempts to advance a moral agenda by denigrating the efforts of those who have a slightly different agenda is a zero-sum approach that is likely to be self defeating.

*"Straus' model fails to locate the study of violent acts in their structural contexts, particularly in their context of gender, class, and race."*

*Gender.* The data sets used for ten of the empirical analyses permitted analysis by gender. (The other two, gathered by others, did not contain information on gender.) Of those with data on gender, four of the graphs in this paper permit an analysis by gender. For another three, analysis by gender was presented in the books or paper in which the research was originally reported. Thus, in seven out of ten empirical analyses, gender was explicitly analyzed. Moreover, the accusation that I omit gender is also contradicted by Kurz herself because at a later point she devotes considerable space to criticizing the analysis of gender that at this point I am criticized for omitting! At that point, rather than accusing me of ignoring gender, Kurz rejects my gender analysis because she believes these "conclusions arise from conceptual, theoretical, and methodological flaws." As noted earlier, I believe that the real problem is neither that I ignore gender nor that my theory and data are wrong but that some of my findings on gender and violence are not "politically correct."

*Class.* Although I present data on class only for Figures 9 and 11, such analyses were carried out on all ten of the data sets that contained the necessary information. Those data were not reported in this summary paper because I found parallel effects on middle and working class families and because of the need to keep the paper to reasonable length.

*Race.* I agree that race should have been in the model. However, the racial context of family violence, although not presented in this paper, has been the central issue of two previous papers (Cazenave and Straus 1979; Straus and Smith 1990). It has also figured in books such as *Behind Closed Doors* (Straus, Gelles, and Steinmetz 1980), *The Social Causes of Husband-Wife Violence* (Straus and Hotaling 1980), and *Intimate Violence* (Gelles and Straus 1988). The 1985 National Family Violence Survey went beyond merely replicating the 1975 survey. It included oversamples of African American and Hispanic families. Those data have been analyzed mainly by colleagues whose training and experience put them in a better position to understand them (Hampton and Gelles 1989). Also, race is an important variable in the parent-education experiment mentioned in the concluding section on Research Implications. Finally, even in articles where race has not been a central issue. I remind readers of its importance, as in the closing paragraph of my article on family violence in the forthcoming *Encyclopedia of Sociology*:

Child protective services, shelters for battered women, and treatment programs for wife-beaters are essential services. However, the root causes of child abuse and spouse abuse lie in the characteristics of the family and other social institutions. Consequently, social service for abused children, shelters, counseling, and prosecution are unlikely to have a major or lasting effect unless there are also changes in these institu-

tional characteristics. The changes must address the pervasive inequality between men and women within the family and in society at large; the high incidence of normatively legitimate violence, ranging from the almost universal practice of physical punishment to capital punishment, and bombing countries engaged in "state terrorism;" social norms toleration violence between spouses; and the stress and frustration experienced by millions living in poverty and under racial oppression.

Contrary to my alleged failure to "locate the study of violent acts in their structural context," that approach has been a principle focus of my work on family violence since the early 1970s. In 1972 I presented a theoretical paper on "Cultural Norms, Sexual Inequality, and Wife Beating" (Straus 1976). In 1973 I published a paper giving empirical data showing much higher assault rates in male-dominant families (Straus 1973). These papers were probably the first ever published by a sociologist on the gendered nature of assaults on marital partners. They were followed by similar analyses based on the 1975 National Family Violence Survey (Coleman and Straus 1986; Straus, Gelles, and Steinmetz 1980). Gender and class were part of my research well before the work on family violence, as demonstrated in papers on "family problem solving"—Kolb and Straus (1974), Straus and Straus (1968), and Straus (1971, 1988)—and they continue to be a central focus of my work (Straus 1989; Straus and Smith 1990).

*"Class and race are . . . key structural factors . . . [and] it is surprising to find that Straus assigns these factors such a minor position in his model. . . ."*

There have been a number of studies of class and race differences in physical punishment, but for some unexplained reason, instead of citing them, Kurz cites studies of extreme violence, especially homicide. Although homicide

and other extreme violence are not the subject of this paper, I have done research on that type of violence. I found the same large differences in homicide and severe assaults (Pass and Straus 1987; Straus 1977d; Straus, Gelles and Steinmetz 1980; Straus and Smith 1990; Wauchope and Straus 1990). Perhaps Kurz restricted citations to severe violence because the bulk of the research on physical punishment, including my own, finds little class or race difference (Erlanger 1974; Straus, Gelles and Steinmetz 1980; Stark and McEvoy 1970; Wauchope and Straus 1990). The general principle seems to be that the more severe the violence, the larger the class difference (Straus 1977d). Nevertheless, class is such a pervasive force that I will continue to examine it in every analysis, including analyses of physical punishment.

*"[Straus] does not discuss the difficult methodological issues in defining abuse."*

The paper discusses these issues in relation to physical punishment but not abuse because the issue of this paper is physical punishment. However, as in the case of almost everything else that Kurz and Loseke allege I neglect, I have discussed both the methodological and the conceptual problems of defining abuse at length in many other publications (Gelles and Straus 1979; Straus 1979, 1989, 1990a,b; Straus, Gelles, and Steinmetz 1980).

*"Efforts by social scientists to predict adult functioning based on childhood experiences have been disappointing."*

This statement is given as a criticism of my assertion that "it is reasonable to assume that there may be lasting effects" and my call for research to determine if that assumption is correct. Kurz has a number of objections to that assertion and to research to test it.

The first criticism (which is also one made by Loseke) is based on an erroneous criterion for judging whether some

adverse circumstance affects adult behavior. The error can be illustrated in relation to the finding that two thirds of physically abused children do not subsequently abuse their own children (Kaufman and Zigler 1987). Kurz seems to align herself with those whose perception is limited to the "half empty" part of the glass and, therefore, believe this contradicts the Intergenerational Transmission Theory (e.g., Breines and Gordon 1983; Pagelow 1984). They ignore the "half full" part—that a third of parents who were themselves abused, abuse their own children. That rate is about 13 times greater than the child abuse rate of non-abused parents. It happens that research on smoking and lung cancer is very similar to that on having been abused and abusing. Two thirds of those who smoke a pack of cigarettes a day or more do not die of lung cancer or other smoking related disease. Yet we do not take that as invalidating the theory that smoking causes lung cancer. Closer to home, among the most totally male dominant couples in the 1975 National Family Violence Survey, 80 percent did *not* experience a husband-to-wife assault during the year of the survey. Does that invalidate the patriarch theory of wife beating? No, because the 20 percent rate of assaults on wives by those husbands is almost 10 times the husband-to-wife assault rate of egalitarian husbands (Straus, Gelles, and Steinmetz 1980:194).

How can we explain the failure of highly sophisticated and knowledgeable critics of the Intergenerational Transmission Theory to apply modes of data analysis with which each is familiar. As indicated earlier, I suggest it is because they were writing in an advocacy framework rather than a scientific framework. They were concerned that if anything except male dominance and other aspects of social inequality were to be recognized as causing wife beating, it would undermine efforts to create an egalitarian society and to allocate resources to protect victims of wife beat-

ing. I share those goals, but I think that attempts to achieve them by denying the influence of other factors undermines progress toward that goal because it forces us to work within an inadequate conceptual and theoretical framework (see Straus [1991] for a discussion of the dilemma posed when research findings seem to contradict the researcher's moral agenda).

A second criticism of the hypothesis that ordinary physical punishment has lasting negative effects is that "interactive effects are critical in human development." I could not agree more completely, and that is why I specified some of these contingencies in Box II in Figure 2, others in Figure 14, and still others in the text.

A third criticism of the hypothesis is that "Straus focuses only on acts of physical violence." As a criticism it is a *non-sequitur*, and as a description of my work it is false. It is a *non-sequitur* because the fact that children are injured by many other types of abuse and neglect does not negate the harm that may come from physical punishment. It is false because, as is the case with so many other allegations by Kurz, I have published on just these issues. For example, a 1983 paper does "investigate differences between 'typical punishment' and child abuse," and I have studied psychological as well as physical abuse (Straus 1974b, Straus, Gelles, and Steinmetz 1980; Straus and Sweet 1990; Vissing et al. 1991). As for sexual abuse of children, although it is something that I have not myself studied, I am proud to have founded a research program that does internationally esteemed research on this issue (Finkelhor 1979, 1986; Finkelhor, Williams, and Burns 1988; Finkelhor and Yllo 1985).

Another "criticism" that is both a *non-sequitur* and false is that I have not investigated the cause of child abuse. Even if this allegation were true, it does not follow that studies of the effects are invalid because the investigator has not

studied the causes. But, in fact, much more of my research has been on the causes of intra-family violence than on the effects. Most of my publications listed in the References section deal with causes. As for the causes of physical punishment (as compared to "abuse"), the entire left side of the theoretical model in Figure 2 is on that issue.

### **Alleged Deficiencies of the Conflict Tactics Scales (CTS)**

A substantial proportion of Kurz's critique is focused on the Conflict Tactics Scales or CTS (Straus 1979, 1989, 1990a). Kurz starts by asserting that the CTS measures only acts of violence without reference to the context in which the violence occurs. True enough. But that is not a limitation. It is up to each investigator to obtain data on those aspects of context which are relevant for her or his research. To regard this as a criticism has no more validity than to regard a Gini index of income inequality as invalid because it does not measure the social forces and the context which produce large disparities in income.

Just as there are many theories about what produces social inequality, and therefore a need for studies that obtain data to test each of those theories, there are many theories about what produces the high level of violence against children and spouses, and therefore a need for many studies to test those theories. Each such study must obtain information on the context and other variables which the investigator believes are important. Neither the CTS nor any other test or scale can include all the relevant aspects of context because, as understanding of family violence grows, so will the number of contextual and other variables. It would also make an impossibly long instrument. Ironically, even if we thought we knew all the aspects of context worth investigating, to include them in the wording of the CTS

items would preclude using the CTS to carry out those investigations.

It is essential to study such things as the motivation of the actors, the extent of injury, and the sequence of events, but that information must be obtained separately from whether an assault occurred. If, for example, injury were part of the CTS, it would not be possible to show that assaults by men are more likely to produce an injury because only assaults that did produce injuries would be counted. If the CTS were designed so that the items reflect instances in which violence was used to maintain power and control, one could not test the hypothesis that male dominance leads to violence because male dominance would be a constant rather than a variable. Research using the CTS has included information on the type of issues that the CTS is criticized for omitting, such as injury, gender, race, class, power, and drinking. The fact that the CTS measures only acts of force is precisely the characteristic which enables these studies to provide empirical evidence that violence is used by men to maintain a dominant power position in the family.

*"The model makes society look like a system in which all parts have somewhat equal input into the creation of violent acts."*

This is another criticism in which Kurz contradicts herself. Early in the paper I argued that physical punishment is one of the most important factors and, early in her critique Kurz acknowledged my assertion of unequal input by disputing it. Moreover, Kurz's statement ignores the convention of listing the elements in a causal model without differential weights. That convention probably reflects the fact that at the time a causal model is formulated the weights are not known. When the needed research is done, these same conventions require presenting a "trimmed model" that omits non-significant paths and includes the estimated BETA or other

weights, as in the research of Baron and Straus on the social causes of rape (1987, 1989).

*"[T]here is no discussion of the power of the state to use violence or the state as a source of much of the legitimation of violence through war, the military, or the police."*

These and other aspects of the societal context are specified in Figure 2. They have also been the focus of previous empirical research (Baron and Straus 1987, 1989; Huggins and Straus 1980; Straus 1988). In fact, as I noted earlier, one of the hallmarks of my approach to the study of family violence (or any other aspect of the family) has been the assumption that such contextual variables are essential for understanding what occurs within the family.

*"The family is a different environment from the public world, with different traditions and prescriptions about the use of violence."*

This reads like a summary of what I argue in many publications (Gelles and Straus 1979; Straus 1977b; Straus and Hotaling 1980; Straus and Lincoln 1985). Kurz would probably counter argue that I did not say it in this paper. I did not because there is only so much that one can fit into the limited space of a journal article, and because it would be repeating what I said in many prior publications and which I may have assumed (rightly or wrongly) was already known by most readers.

*"[N]ot 'all' punished children . . . will grow up to [be violent]."*

This is one of many statements that makes me feel as though Kurz is writing about someone else's work. I have never suggested any such uniform effect. In fact, as noted earlier, it is critics of my findings on intergenerational transmission (e.g. Pagelow 1948) who argue that the theory is not supported because the data do not show such a one-to-one relationship between physical punishment or abuse and subsequent

violence. They cite my data showing that most abused parents do not themselves abuse as a basis for refuting the Intergenerational Transmission Theory.

*"Straus . . . lack[s] concern for who is committing acts of violence in adult life. . . ."*

Even within the severe space constraints of this paper, I show that the more physical punishment experienced by parents, the greater the probability of assaulting a spouse and strangers, and that this relationship applies to assaults by women as well as men. So, it is simply not true that I do not attend to who is the victim and who is the aggressor. What is true is that Kurz does not accept my findings on that issue. That is her privilege, but she should not cloak it as a lack of concern for gender on my part.

However, in reexamining the paper to respond to this point, I discovered that in one respect Figure 9 is misleading about gender. Figure 9 uses data that has been transformed to Z scores within gender. That makes the mean of both groups zero and therefore misleadingly suggests that women assault non-family members as much as men. In fact, the raw scores for the men are triple those for the women in this sample. Nevertheless, the similarity of slopes in Figure 9 indicates that physical punishment has almost as strong a relationship to non-family assaults for women as for men.

*"Straus' treatment of 'spouse abuse' has been heavily criticized by a number of social scientists who argue that women do not commit 'spouse abuse' the way men do."*

The five social scientists cited by Kurz are not the only ones to make this argument. Since first discovering the unanticipated high rate assault by wives, which I think was before critics wrote on this issue, I have consistently accompanied the findings with a discussion arguing that the causes and conse-

quences are very different (see, for example, Straus 1977c 1980; Straus, Gelles, and Steinmetz 1980). I argued, for example, that most violence by wives was probably in self-defense and that women were more vulnerable to physical, psychological, and economic injury. I did this despite the fact that the early studies did not have empirical data to really support these claims because it seemed so obvious to me.

The 1985 National Family Violence Survey was designed to provide the missing empirical evidence. In respect to injury, the findings were that assaults by men are seven times more likely than assaults by women to produce an injury that requires medical treatment (Stets, and Straus 1990). However, to my surprise and embarrassment, the results on who initiates violence showed that women strike the first blow as often as men (Stets and Straus 1990; Straus 1989). Rather than suppressing this fact (as urged by some of my colleagues and as was done by one of my colleagues) I published it. My belief is that it is important for the safety of women to acknowledge the fact of their own violence as one of many steps to end that violence and violence by husbands that it unwittingly helps to legitimize (Straus 1989). My view is well expressed by Barbara Hart in the preface to a book on lesbian battering:

"[Lesbian battering] is painful. It challenges our dream of a lesbian utopia. It contradicts our belief in the inherent nonviolence of women. And the disclosure of violence by lesbians . . . may enhance the arsenal of homophobes. . . . Yet, if we are to free ourselves, we must free our sisters" (Hart 1986:10).

"The validity of the data [on assaults by wives] is also in question because of differences in how men and women report information about their use of violence."

My 1989 paper on assaults by wives, which is cited elsewhere in Kurz's cri-

tique, compared the responses of men and women. Early in the paper I showed that men under-report serious assaults. I therefore based the remainder of the paper on data obtained by interviewing women. This data as well as the data from male respondents shows that women assault their male partners at about the same rate as men assault their partners, and it is this data that shows that women initiate physical attacks about as often as men.

*"Straus does not take into account structural differences in power between husbands and wives."*

This is perhaps Kurz's most incredible assertion. The link between male dominance and violence is one of the most important contributions of my work since the early 1970s (Coleman and Straus 1986; Straus 1973; 1974a; 19776; Straus, Gelles, and Steinmetz 1980; Yllo and Straus 1990). From 1973 to 1977 the paper on "Cultural Norms, Sexual Inequality, and Wife Beating" was reprinted and distributed by many in the shelter movement. However, when my colleagues and I published data on assaults by women, it seems to have been abruptly "removed from the shelves." Kurz even cites one of these publications (Straus, Gelles, and Steinmetz 1980), but she seems to have removed from her mental shelves the chapter in which I show that the greatest violence against wives occurs in male dominant marriages.

*"Straus use[d] . . . large amounts of Federal money for research whose conclusions have vastly underestimated the harm done to women. . . ."*

The facts are exactly the opposite. Those Federal funds have financed the two National Family Violence Surveys. The rates of wife beating based on those two surveys were presented at a Congressional hearing in 1977 and also in hearings of the U.S. Civil Rights Commission (Straus 1978). The rates based on the 1985 national survey are in daily use throughout the United States. Al-

most every book, article, and pamphlet on violence between spouses cites them, either in the form of "more than 1.8 million women are severely assaulted by their husbands each year," or in the form of "a woman is beaten every 18 second." They have come to be regarded as so authoritative that they are frequently attributed to the FBI! Ironically, even those who denounce the National Family Violence Surveys cite these figures. These statistics have been extremely important as a basis of "claims making" in the struggle for allocation of resources (Aronson 1984; Best 1987; Gusfield 1989).

As for underestimating the harm done to women in respect to physical harm, the statistics on wife beating obtained by the Conflict Tactics Scales do the reverse. This is because, as the critics of the CTS are the first to note, the CTS itself measures only whether an assault has occurred. Only rarely do assaults result in an injury that is serious enough to require medical attention. The injury rate for assaulted women was 3 percent in the National Family Violence survey (Stets and Straus 1990; Straus 1989) and even lower (1.2 percent) in the only other study using a representative sample of women (Brush 1990). If the rate of wife beating were to be based on women who are injured enough to need to see a doctor, it would omit at least 97 percent of the estimated 1.8 million severely assaulted women based on the CTS. I have deliberately not urged the use of "injury adjusted" rates because I believe that the act of assault itself is the primary problem.

*"Others have argued that wife-battering is predictive of child abuse" (emphasis added)*

I have repeatedly made the same argument. In this paper it is item C.3 in Box I of Figure 2. Moreover, although Kurz cites Behind Closed Doors several times, she seems to have missed the fact that Chapter 5 of that book provides the most definitive evidence so far avail-

able on this point. The same data was used in an earlier paper in which assault by husbands was one of the items in an index of "risk factors" for child abuse (Straus 1979). Kurz also complains that I fail to take into account that women are usually the primary caretakers of children. Again she should read, not just cite, *Behind Closed Doors*. A similar incredulous section of Kurz's paper asserts that I do not take into account "structural conditions outside the individual learning model," such as lack of choice about child bearing due to lack of birth control and abortion. These and many other structural conditions are discussed at length in *Behind Closed Doors* (Straus, Gelles, and Steinmetz 1980:221-244).

### Response to Loseke

*"I question . . . [Straus'] sociological contributions and the political desirability of implementing [the paper's] call for social action, for these calls are based on an abstract, theoretical, and context-free model of social life."*

It is a pleasure to plead guilty to the charge that the paper is abstract and theoretical. That is what a theoretical work is supposed to be. As for "political desirability," I grant that a prohibition on spanking is inconsistent with the context of contemporary American society, but so were child labor laws at the turn of the century an affirmative action at mid-century. However, it is not out of context in several more enlightened societies. First Sweden and then other Scandinavian countries have made physical punishment by parents illegal. When Sweden adopted that legislation in 1976, it was the object of resentment and ridicule. In the course of just one decade, it has become widely supported.

In respect to the charge that my paper is context-free, Loseke and I must be defining context differently. As I see it, most of Boxes I and II of Figure 2 are specifications of context variables.

Moreover, I say explicitly that the variables in the diagram are "far from exhaustive" and "are intended only to illustrate some of the many factors. . . ." The purpose of such models is heuristic. So I welcome Loseke's additional context variables. If I have stimulated Loseke and others to expand the range of contextual variables, I will have achieved at least part of the purpose of this paper.

*"[M]ore sophisticated methodology [is needed]."*

I agree, and I inform readers of this at several points, starting in the second paragraph of the paper. Moreover, the empirical data, like the theoretical model is intended only to be illustrative. At several points, again beginning in the second paragraph of the paper, I alert readers to the limitations of the empirical findings. The limitations I point to are much more serious than some of the relatively minor details mentioned by Loseke. I concentrated on the more serious problems, and I chose a strategy of presenting a summary of many studies. Consequently, I did not have enough space to discuss such details as why I used raw scores for the  $X$  axis of some graphs,  $z$  scores in others, and quintiles in other; and why I plotted results separately for men and women (or boys and girls) in some graphs but not in others. As for including the  $N$ s for each category of the independent variable in only four of the graphs, that was an oversight. As a former journal editor, I can say with confidence that this is a frequent problem. Usually the omission is noticed by one of the referees and is corrected before publication. But presidential addresses, for better or worse, are not refereed. Moreover, in this case, the equivalent of a referee's comments were given to me when it was too late to correct the oversight.

*"[V]iolence spills from 'legitimate' to 'illegitimate'. . . . But sociologists know [this]."*

I wish this were a valid criticism of Cultural Spillover Theory, but I fear it is not. To start with, I do not know the basis of Loseke's assertions that "sociologists know this." Even as eminent and humane a scholar as Robert Coles defended physical punishment just this year. But one does not need to go that far afield to find scholars who doubt Cultural Spillover Theory. Readers of these critiques will have noted that both Demie Kurz and Joan McCord are among the doubters. Moreover, I myself do not claim to "know this." Both the present paper and the book testing the spillover of legitimate violence to rape (Baron and Straus 1989) present "Cultural Spillover" as a theory to be investigated, and both stress the ambiguity and limitations of the findings. Even the evidence in the award winning book by Archer and Gartner (1984) is far from definitive.

*"[T]he 'cycle of violence' component of this theory . . . remains a 'folk theory'."*

This statement ill behooves someone who only a few paragraphs earlier objects to scientists telling others what is correct. It displays a disrespect not only of the importance of attending to "the world of lived realities," but also of the scientific evidence. A review of 57 studies of the antecedents of husband-to-wife violence found that of the 97 "risk markers," violence in the family of origin was the single most consistent antecedent (Hotelling and Sugarman 1986). Finally, just a paragraph later, Loseke seems to contradict her disparagement of the cycle of violence as a folk theory when she labels the idea of spillover from legitimate physical punishment to criminal violence as "an abstracted, theoretical, and scientifically created model."

*"The world of lived realities is not so neat, tidy, scientific."*

I agree that it is important to uncover the situated meanings and contingencies that characterize the occurrence and

the consequences of physical punishment. In respect to contingencies, the paper gives examples of some of the many that need to be taken into account. In respect to situated meaning, it is unfortunate that Loseke thinks that research on physical punishment is not scientifically or politically worthy because, if that were not the case, her skills in this mode of research could make important contributions to our understanding of physical punishment.

*"[Straus] asks readers to suspend the folk methods for evaluating the meanings of violence . . . to judge all violence as morally equivalent."*

As with so many of the allegations in Loseke and Kurz's critiques, this must refer to someone else. Just because this paper is in the abstract theory tradition does not mean that I regard that as the only route to understanding society. Only few months ago a referee of another paper insisted that I had to remove a discussion of folk theories from that paper as a condition of publication.

I do plead guilty to "condemning all violence" even though this is contrary to folk beliefs. However, the idea that I regard all violence as morally equivalent is so absurd as to be hardly worth denying. I need only point to my repeated assertions concerning the lack of equivalence of wife-to-husband violence as compared to husband-to-wife violence, despite their near equal frequency of occurrence. I have repeatedly asserted that assaults by husbands are the first priority for intervention (Straus 1977c, 1980, 1989; Straus, Gelles, and Steinmetz 1980; Straus and Smith 1990).

*"My perspective as an interactionist, feminist, and interpretative sociologist leads me to remain unconvinced that Straus' text represents the world of real people in real time. . . . Yet Straus calls for practical action when he asks readers to add 'physical punishment' to the list of social problems to be eliminated."*

It is, of course, Loseke's right (and even obligation) to express her doubts. But she errs in implying that by voicing these opinions, they are proven. Similarly, although the theoretical and methodological perspectives that she espouses are much needed, implying that they are the only valid approaches, and citing Schutz (1962) does not prove that all other methods and theories are invalid.

As for "Straus calls for practical action," I do not remember such a call in this particular paper, but I am proud that it is in other papers and in many speeches. I have never thought of myself as a "value neutral" scientist (Straus 1991). Is Loseke taking up a call for "value free" sociology? Obviously not; rather she is again expressing her opinion that ending physical punishment by parents is not high in her priorities. But it is high in my priorities and, ironically, one of the reasons is the same as her reason for the opposite view: both of us are deeply committed to ending wife beating. The difference is that I believe that part of the genesis of wife beating occurs when the experience of physical punishment teaches children that it is morally correct to hit if a family member persists in doing something you define as wrong, fails to do something you want them to do, or persists in annoying you. These are typical justifications in the accounts of both parents and men who batter.

*"It is clear that individual behavior is the issue, and this is what [Straus believes] must be changed. . . . [S]ocial and cultural determinants of crime and violence . . . are in the model, but . . . they are not in the center."*

This allegation is falsified by many things in the paper, including the section on Cultural Spillover Theory where I identify it as a "Macro-Sociological Theory" and note that "crime is not just a reflection of individual deviance." It is further falsified by the 1980 book on *The Social Causes of Husband-Wife*

Violence and numerous other theoretical and empirical studies over the last 20 years in which social and cultural determinants have been a central issue, including such articles as "Cultural and Social Organizational Influence On Violence Between Family Members" (Straus 1974a), "Sexual Inequality, Cultural Norms, and Wife Beating" (Straus 1976), "Some Social Structural Determinants of Attitudes And Behavior: The Case of Family Violence" (Dibble and Straus 1980), "Patriarchy and Violence Against Wives: The Impact of Structural and Normative Factors" (Yllo and Straus 1990), "Marital Power, Conflict, and Violence (Coleman and Straus 1986), "Four Theories of Rape: A Macrosociological Analysis" (Baron and Straus 1987, 1989).

*"Massive education programs . . . [aimed at] . . . child abuse . . . have not yielded desired . . . changes in and of themselves."*

Of course education programs "in and of themselves" do not end child abuse. As Loseke would be the first to point out, almost all social behavior is contingent on "context" and is the result of multiple influences. The multiple influences affecting family violence are discussed in my paper on "Societal Change and Change in Family Violence From 1975 to 1985 as Revealed by Two National Surveys" (Straus and Gelles 1987) where I suggest that along with structural changes—such as a reduction in unemployment, a higher age at marriage, more women in the paid labor force, and increased availability of divorce—"massive education programs" were one of the reasons for the decrease in child abuse from 1975 to 1985.

*"What would a non-corporal punishment be?"*

Loseke seems to jump to the conclusion that because I am opposed to corporal punishment, I favor "non-corporal punishment." On the contrary, my purpose in setting the theme for the 1990 an-

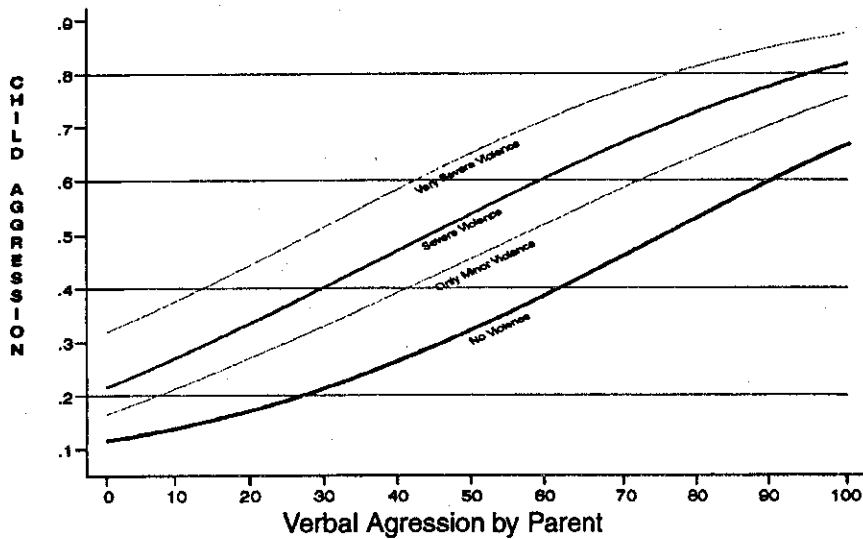


Figure 1 • Child Aggression by Parental Use of Verbal and Physical Punishment

nual meeting of the Society For the Study of Social Problems as "Coercion and Punishment: Solution Or Cause of Social Problems?" was to question all types of punishment. As I stated:

I am inclined to the view that punishment, even in the service of the most worthy of causes, tends to poison the cultural ambiance. . . . The answers to [the question posed by the theme title] are likely to be full of contradiction and irony. For example, wife-beaters can be jailed, but the economic costs to the family and society may be tremendous, [but] is compulsory "help" for wife-beaters and compulsory treatment for the mentally ill non punitive? (Straus 1990c).

In respect to physical punishment of children, I do not think that the alternative is another type of punishment. Indeed, my own research suggests that if parents switch to non-physical punishment such as verbal attacks, children would be worse off (Vissing et al. 1991). The same applies to actions of the state and that is why I favor the approach of the Swedish law that prohibits any physical punishment by parents but does not provide for punishing parents who violate the law.

"Straus asks readers to condemn only individual violence."

Loseke appears to have overlooked Figure 2. Although that figure is presented as only illustrative, it nonetheless identifies a number of aspects of societal violence—including capital punishment, vigilante justice, sports violence, media violence, state law authorizing teachers to hit children, and war. In addition to building societal violence into the theoretical model, it has also been an integral part of my empirical research. For example, my macro-level research on rape examines the effect on rape of state-to-state differences in societal violence by means of a "Legitimate Violence Index" (Baron and Straus 1989: Chapter 7).

"In summary, implementing the social policy agenda advanced by Straus' text would be . . . a waste of social resources at best; at worst, such policies would justify more control over disadvantaged groups and divert our attention from social practices promoting discipline and punishment in their many forms."

Although I would like to see a no-spanking policy, a call for such a policy is not in this paper. It is omitted because the purpose of my paper is to stimulate re-

search that might ultimately justify a policy of no-spanking in the United States as in Scandinavia. Loseke must have access to information that the rest of us do not to be so certain that the hypothesized adverse effects of physical punishment are not present or not great enough to be a concern and not even worth investigating.

"[Straus uses] a seemingly scientific justification for replacing control of the body with the 'innumerable mechanisms of discipline' (Foucault, 1979:303) that control the soul."

I would gladly plead guilty to this allegation if it were rephrased as "a theoretical argument for research on non-punitive methods to recreate humanity with a gentle soul." A gentle soul is a product of society as much as a hostile and aggressive soul (Montague 1978; Straus 1977a). The creation of a humane society has to avoid the *Lord of Flies* (Golding 1962) as well as 1984 (Orwell 1949).

### Response to McCord

It is a pleasure to turn to McCord's thoughtful paper. I agree with her fundamental assertion that "aggression can be triggered by non-physical punishments and neglect." In fact, as shown in Figure 1, my own research found that the effect of non-physical punishment is greater than the effect of physical punishment (Vissing et al. 1991). However, that does not mean that I agree with everything McCord proposes.

I think that McCord errs in suggesting that because her findings (and mine also) show that non-physical punishments and neglect are risk factors for violence and other crime, this "raise[s] doubts that acceptance of norms of violence account for transmission of violence." What is wrong with this type of reasoning might be more easily perceived if we consider the abundant evidence that failing to cuddle, play with, talk to, and read to infants and toddlers

is associated with underdevelopment of basic cognitive skills. However, malnutrition and several other factors adversely affect cognitive development, some to a much greater extent than lack of interaction with the child. But no one would argue that because these other things can do even more damage, we do not need to be concerned about the level of parent-child interaction. It is similarly inappropriate to argue that because verbal assaults and neglect can be even more criminogenic than physical punishment, we do not need to worry about physical punishment.

Cultural Spillover Theory was not intended to explain anything except the irony that use of violence to maintain social conformity tends to increase the rate of social deviance. The fact that Cultural Spillover Theory does not explain the effects of non-physical punishments or neglect does not invalidate the theory. It simply means that other processes can also engender aggressive and antisocial behavior. The etiology of violence (and many other types of behavior) is characterized by "equifinality," that is crime can arise out of any one of several antecedents or combinations of antecedents. Since I have never believed in single causal factor theories, I do not find that troublesome, and I welcome McCord's effort to develop "Construct Theory" as a more comprehensive mode of understanding some of the many antecedents of crime. I include physical punishment among those antecedents, and McCord's closing paragraph suggests that she holds a similar view. If that is the case, it is counter productive to pose Construct Theory as "an alternative theory." Moreover, my reading of the data that she presents indicates that her own findings are consistent with Cultural Spillover Theory.

McCord's interprets her Table 1, for example, as "evidence . . . [that] gives another picture." But plotting that data (Figure 2) reveals a pattern that strongly supports Cultural Spillover Theory. It

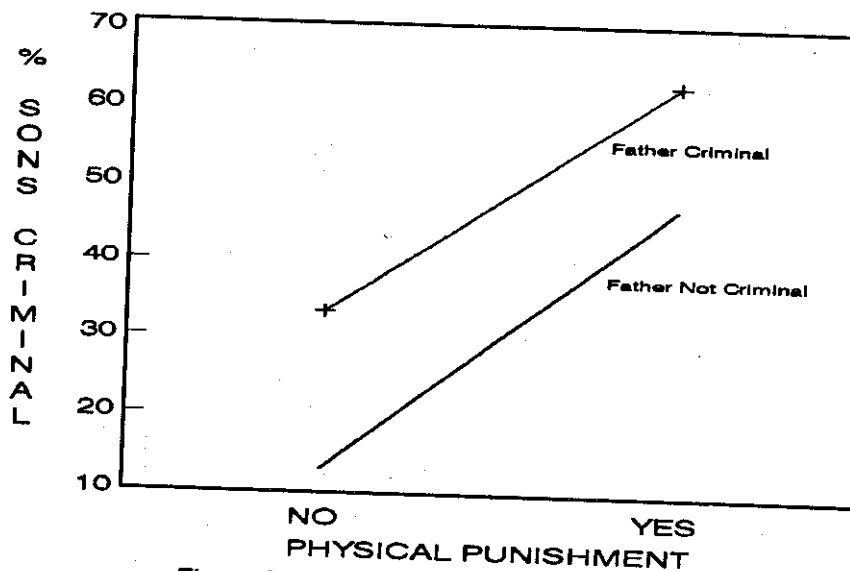


Figure 2 • Criminality of Sons by Father's Use of Physical Punishment (McCord this volume)

shows that physical punishment is strongly associated with an increased probability of the child engaging in criminal behavior among the sons of both criminal and non-criminal fathers.

As for McCord's Table 2, it is not adequate to answer the question she raises because two of the cells of the implicit 2 by 2 cross tab are combined. However, the data that is presented is consistent with the specification of parental "support" in Figure 14 of my paper: McCord's table 2 shows that the presence of parental affection mitigates the effect of physical punishment, and the absence of parental affection amplifies the criminogenic effects of physical punishment. But even with parental affection present, sons of punitive parents have a higher crime rate than other sons.

The re-analysis of Widom's data in McCord's Tables 3 and 4 is intended to "differentiate effects of neglect from effects of violence." This data addresses the issue of my paper only indirectly because it refers to physical "abuse" not socially legitimate physical punishment. Nevertheless, all four comparisons of physically abused children with controls show a more severe criminal record for the physically abused than

the control cases. The neglect cases also tended to have a worse criminal record than their controls, and McCord interprets this evidence against Cultural Spillover Theory. However, that would only be true if Cultural Spillover Theory is a "middle range" theory, and middle range theories do not claim to be complete explanations of a phenomenon. Reference Group Theory (Merton and Kitt 1950), for example, does not claim to be the only explanation of the many aspects of attitudes and behavior that it helps us understand.

It may be that sociology is past the point where Cultural Spillover Theory, Reference Group Theory, and other middle range theories are helpful. I infer that McCord takes this position, and consistent with this view, she offers "Construct Theory" as a more comprehensive alternative. I support her efforts to develop such a theory, including making it more comprehensive by explicitly including the effects pointed to by Cultural Spillover Theory.

I can think of no more fitting close than to re-assert my belief that, as McCord states in her closing sentence, "If the substitute for physical punishment were to be non-physical punishments, the consequences could be no less under-

mining of compassion and social interest."

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