

Differentiating Evidence from Advocacy in Evaluating Sweden's Spanking Ban: A Response to  
Joan Durrant's Critique of my Booklet "*Sweden's Smacking Ban: More Harm Than Good*"

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### **Summary**

Policy-makers need objective, unbiased evidence about how spanking bans have affected children's welfare. Toward that end, this paper replies to Dr. Durrant's criticisms of my booklet comparing our respective published evaluations of Sweden's 1979 spanking ban. Dr. Durrant's data sources indicate an increase in physical child abuse and of criminal assaults by juvenile delinquents since the spanking ban. Although these increases cannot be proven to have been caused by the spanking ban, the increases cannot be easily explained away, either. Dr. Durrant's claim of almost no Swedish child abuse fatalities is due to her unusually narrow definition. The spanking ban has nearly eliminated the kind of mild spanking that could stop escalations toward violence in disciplinary episodes, but perhaps as many Swedish children receive corporal punishment when a teenager or when a parent is "upset enough" as before the spanking ban. Although Dr. Durrant is more knowledgeable than I am about what she calls Swedish "support and care measures," her own statistics indicate that support and care measures include removal of children from their home in 46% of new cases in 1995, thankfully down from 60% in 1982. Other countries need unbiased, objective evaluations of spanking bans and the resulting balance between child protection and parent empowerment before discrediting generations of parental disciplinary practice.

### **Introduction**

"A rigorous evaluation of the effects of spanking bans in Sweden and other countries is sorely needed now that other countries are considering such bans. Published evaluations before 1999 included only 7 journal articles in English, leading to my call for "more timely and rigorous evaluations of similar social experiments in the future" (Larzelere & Johnson, 1999, p. 381). Durrant (1999a) also published an evaluation of the success of Sweden's ban in 1999. Given the importance of this issue, I want to briefly compare our respective conclusions and the evidence for them."

Thus I began a posting on the Child Maltreatment Research listserv in March 2000 in response to Joan Durrant's posting about her article. My posting was later updated and published by Families First as the booklet that Joan Durrant is now critiquing in her own booklet. The purpose of this paper is to respond to her critique, to help policy makers and others sort out the evidence and its conclusiveness. Before getting into the details, I want to acknowledge that Sweden has more relevant evidence available from their country than any other country that has banned spanking, at least to my knowledge. Even their data, however, are somewhat meager, mostly in Swedish, and are subject to various interpretations. By comparing our respective interpretations and critiques, policy makers can make a more informed decision about the possible effects of proposed spanking bans in their country.

### **Background**

Joan Durrant and I (in collaboration with Byron Johnson) both published evaluations of Sweden's 1979 spanking ban in 1999. Her article drew from her extended study in Sweden as part of her long-term interest in their spanking ban and its applicability to other countries, particularly her native Canada. My co-authored article reviewed all evaluations of Sweden's spanking ban or relevant evidence we could locate in the English language up to that time. We arrived at rather different conclusions. Durrant (1999a) concluded, "The Swedish ban has been highly successful in accomplishing its goals" (p. 435). Our concluding paragraph said, "No studies have demonstrated that the spanking ban has succeeded in reducing Swedish rates of child abuse" (p. 390), and the abstract ended with "The conclusion calls for more timely and rigorous evaluations of similar social experiments in the future." (p. 381).

In March 2000, Rolf Nilsson from Australia brought my article to the attention of the Child Maltreatment Research listserve, a group E-mail for professionals doing research on child maltreatment. He was not wholeheartedly supporting my article, but said that others might consider it and the possibility we had raised that "it might be hypothesized that the prohibition of all spanking eliminates a type of mild spanking that prevents further escalation of aggression within disciplinary incidents" (Larzelere & Johnson, 1999, p. 390). Joan Durrant responded with a posting summarizing her article and viewpoint. I replied with a posting that compared and contrasted our two articles on the following four main issues: (1) subsequent support for corporal punishment after their spanking ban, (2) subsequent physical child abuse, (3) subsequent assaults by minors against minors, and (4) subsequent supportiveness of social supports. Neither Dr. Durrant nor anyone else responded to my posting at that time.

Families First, a British organization, asked if they could reprint an updated version of my posting in their newsletter in 2001. I had initially been reluctant to consult to such an unknown group and requested that they send a recommendation from a British psychologist. The recommendation came from none other than the eminent psychologist, H. J. Eysenck, so I have consulted with this group from time to time since then. Later they wanted to make a booklet out of an updated version of my article in 2004, to which I also agreed. It was entitled, "Sweden's Smacking Ban: More Harm than Good." I agreed to have my work updated for their publications because (1) Dr. Durrant's assessment of the success of Sweden's spanking ban was being emphasized by spanking-ban advocates, and (2) decision-makers and the general public need to hear both sides of the evidence about Sweden. Joan Durrant then published her critique of that booklet in 2005, entitled, "Law Reform and Corporal Punishment in Sweden: Response to Robert Larzelere, The Christian Institute and Families First." (Families First collaborated with the Christian Institute to fund the booklet's publication.)

In her critique, Dr. Durrant summarizes what she considers to be my main conclusions and rebuts them, sometimes with new evidence. She strongly implies that my work is tainted by bias. This response will first address the four main issues, incorporating my actual conclusions and the new evidence presented by Dr. Durrant. It will then consider the charge of bias and make a final set of overall conclusions.

In part, our different perspectives can be explained by our distinct approaches to this topic. My impression is that the major scholarly/advocacy emphasis in her career is to understand Sweden's

experience in banning spanking and to advocate for such bans elsewhere. She is currently spending her seventh summer in Sweden to develop that expertise further. In contrast, my interest in the Swedish experience is secondary to my research on parental discipline in general (e.g., Larzelere, 2001; Larzelere & Kuhn, 2005). In the latter research I am well aware of the difficulty of sorting out the chicken-and-the-egg problem (i.e., causal direction between parents and children). That is, is parental spanking associated with child aggression because the aggression led to spankings or because the spankings increased the child's aggression? The spanking ban in Sweden provides one window into solving that problem, at least potentially. Advocates of spanking bans (e.g., Straus, 2001) predict that such bans will reduce violence in society. Supporters of strong parental discipline predict the opposite. Those differential predictions are what piqued my interest in the Swedish situation in the first place, leading to my review of all the relevant literature that Dr. Johnson and I could locate before 1999.

Now I will turn to the four central issues.

### **I. Public Attitudes Toward Spanking (called “Smacking” in Great Britain)**

Durrant: *“1. Larzelere’s claim: Because public attitudes shifted prior to the 1979 explicit ban on corporal punishment, the ban has not been effective” (p. 5).*

My actual conclusion: *“changes in attitudes toward physical punishment occurred before the 1979 legislation and have changed very little since then” (p. 7).*

I was responding to Durrant’s original claim that “the corporal punishment ban and ongoing public education campaigns appear to have been extremely effective in altering the social climate with regard to corporal punishment” (Durrant, 1999a, p. 444; Larzelere, 2004, p. 6), which she supported with a table showing that Swedish support for corporal punishment had declined from 35% in 1971 to 26% in 1981 to 11% in 1994. My sources indicated that the percentage was unchanged from 1978 though 1981 at 26%, after which that particular question was apparently never repeated in a national survey. Whether the 11% response to an entirely different question in 1994 is relevant depends upon one’s interpretation of another 22% who were “in principle against all forms of physical punishment, but can use such punishment if upset enough” (Larzelere, 2004, p. 6). Only 56% chose a third option against all forms of physical punishment in 1994. Dr. Durrant raised a valid point that a more comparable item in 1994 was double-barrelled, but that does not prove how much the 34% was inflated for those indicating partial support for mild/moderate physical punishment. Dr. Julian Roberts (2000) analysed these same data and concluded as I did that “The Swedish ban on corporal punishment did not affect public attitudes” (p. 1027).

Durrant: *“2. Larzelere’s claim: Physical punishment has not declined over time” (p. 5).*

My actual conclusion: *“This same Swedish survey found that the actual use of physical punishment had dropped very little” (p. 7)*

Her rebuttal compares 1950 statistics with statistics since 1980. She has a valid point that poor recollection might suppress memories of corporal punishment among the older generations. As she also points out, larger percentages of older people said that they could not remember (averages: 5.5% of 13-15-year-olds, 12.3% of 18-34-year-olds, 14.3% of 35-54-year-olds, and 18% of 55-74-year-olds). It would have been better to have the same question asked of each generation when they were the same age, but, as she says, “no truly longitudinal studies exist” (p. 16). My statistics corrected for the memory problem by reporting the percentage of *valid* answers in a given category (e.g., experienced physical punishment as a teenager), which one can approximate by dividing her correct statistics on p. 15 by the percentage of valid answers given by each age group. The biggest surprise to me was that 32% of 13-15-year olds born after the spanking ban reported experiencing physical punishment from their father and 31% from their mother, and that 17% had received such punishment from their father and 16% from their mother when a teenager (compared to a 16% average of valid answers in all older generations combined).<sup>1</sup> Dr. Durrant’s critique clarifies that the reported frequency in the vast majority of cases was only one or two occurrences. Other evidence cited by Dr. Durrant and the summary by Statistics Sweden (1996) indicates that the prevalence and especially the frequency of physical punishment dropped dramatically before the 1979 spanking ban and dropped somewhat thereafter, except for physical punishment when a teenager. The fact that frequency has been reduced more than prevalence is consistent with my speculation that their spanking ban might have primarily eliminated the kind of mild spanking that stops disciplinary escalations before parents get “upset enough” to punish physically out of angry frustration.

## II. Physical Child Abuse

Durrant: “3. Larzelere’s claim: *Physical assaults of children have increased*” (p. 5).

My actual conclusion: “*the best indicators of physical child abuse showed a 489% increase in physical child abuse cases classified as criminal assaults in Sweden from 1981-1994*” (p. 4) and “*This could reflect a change in reporting mechanisms, an actual increase, or other factors. Other countries need an unbiased, objective way of deciding among these alternative explanations before emulating Swedish policies*” (p. 8).

Dr. Durrant interprets this increase as due to changes in reporting, which is one possibility acknowledged by me, our mutual data source (Wittrock, 1995), and a recent investigation by Swedish psychiatrists (Lindell & Svedin, 2001). That possibility does not rule out the possibility that the increase may reflect an actual increase in physical child abuse cases, as acknowledged by Wittrock (1995), Lindell and Svedin (2001), and Dr. Johnson and me (1999).

Dr. Durrant raises the possibility that many cases being reported to police now are milder cases than would have been reported previously. I addressed that by noting that the percentage of aggravated assaults against all children increased 388% from 1984 to 1994, compared to a 277% increase in all reported cases. Aggravated assaults are punishable from 1 to 10 years according to Dr. Durrant, 1999a, p. 439. She says that it would be more appropriate to report the trend in percentage of cases that were aggravated assaults, which is therefore displayed in Figure 1. It shows the trend from 1984 though 1994 for all child victims (which Dr. Durrant uses) and for

children from 0 to 6 years of age, the majority of which were suspected cases of physical child abuse. The percentage of cases that involve aggravated assault are not decreasing overall, but, if anything, are increasing during the same 11 years when the physical child abuse rate was increasing 359% (after an initial 28% increase from 1981 to 1984). To explain this increase by a new wave of milder cases requires the percentage of aggravated assaults to decrease during this time period, which it did not. Dr. Durrant reports a new study in Swedish that concludes that the proportion of more severe cases decreased from 1990 to 1997 and that the percentage of aggravated assaults for all children reached a new low in 1996 (3%). I do not have access to these newer data and thus cannot evaluate that information at this time.

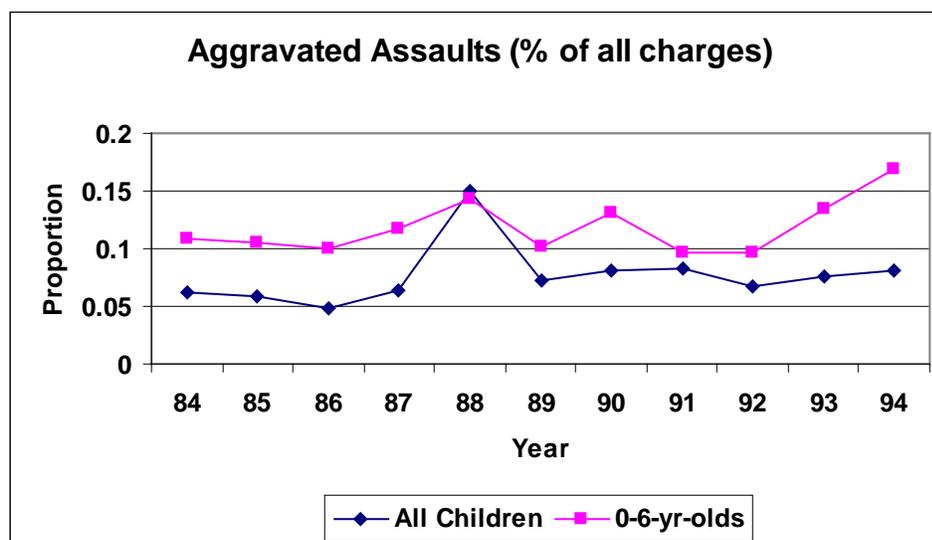


Figure 1. Aggravated assaults as a percentage of Swedish charges of criminal assaults against children, 1984-1994.

Durrant: “4. Larzelere’s claim: Swedish child abuse rates were higher than American rates following the 1979 explicit ban” (p. 6).

My actual conclusion: “Gelles and Edfelt (1986) . . . concluded that ‘Swedish parents report more pushing, grabbing or shoving than American parents and double the rate of beating children’” (p. 506 of Gelles & Edfeldt, p. 12 of Larzelere, 2004). I went on to explain why the fairest comparison is between their Swedish survey done in 1980 and an average of two American surveys done in 1975 and 1985, yielding an estimate that the Swedish rate of very severe violence (child abuse) was 49% higher than the average of the two American surveys.

One reason I used an average of the two American surveys was because Dr. Durrant had previously pointed out the different interviewing method and lower response rate in the 1975 American survey, differences which the 1985 American survey overcame. The importance of the Gelles-Edvelt study is that it compared nationally representative surveys of both countries, using the most widely used interview measure of physical child abuse. To the extent that Swedish interviewees believed the promised confidentiality, it avoided the problem of changes in what

gets reported as abuse to authorities. As I said in my booklet, I supposed that these surprisingly high rates of beating up a child (3.0% in Sweden vs. USA rates of 1.3% and 0.6% in 1975 and 1985, respectively) represented a temporary adjustment to the spanking ban, and that the prevalence of beating up children would subsequently decrease as Swedish parents adjusted to the spanking ban. That may be the case, but I have yet to see evidence of it. In any case, Dr. Durrant's (2005) comparison between American and Swedish rates of "all forms of violence" begs the question, because most or all of the difference occurs due to disciplinary spanking with and without an object (Durrant's Table 6).

Durrant: "5. Larzelere's claim: *The difference in child abuse mortality rates before and after the 1979 explicit ban was not statistically significant,*" whereas she claims that only "Nine children died this way in the 25 years between 1971 and 1996" (p. 6)

My actual conclusion: Dr. Durrant and I agreed that the Swedish mortality rate due to child abuse was commendably low both before and after the 1979 spanking ban. However, "*Durrant's claim of no child abuse fatalities since 1975 is contradicted by a Goteborg newspaper estimate of seven such fatalities annually in Sweden during the 1990s . . . [and] a recent UNICEF report*" (Larzelere, 2004, p. 8). *The UNICEF report concluded that "maltreatment deaths occurred at an annual rate of 0.5 or 0.6 children per 100,000 children under 15 from 1993-1997, compared to 0.4 to 0.9 for the United Kingdom, depending upon whether ambiguous cases were included or not"* (Larzelere, 2004, p. 8).

Beckett (2005) recently exposed the widely quoted "Swedish myth," that "banning corporal punishment will bring about a radical reduction in child maltreatment deaths," a myth he traced to Durrant's (1999a) article. Although she correctly stated that the annual Swedish child abuse fatality rate was as high as 0.9 per 100,000, which is consistent with the estimate by UNICEF in 2003 (0.6 per 100,000, the 7<sup>th</sup> lowest among 27 wealthy countries), Beckett showed that Durrant adopted Somander and Rammer's (1991) narrow definition of "fatal child abuse" as unintentional child death solely from being disciplined by a parent. Her definition of fatal child abuse specifically excluded child deaths due to maternal postnatal depression, homicide/suicide events, child neglect, and neonaticide (Durrant, 1999a, p. 441). According to Somander and Rammer, whose definition Durrant relied upon, it also excluded child deaths due to (1) poverty, (2) marital conflicts, (3) alcohol abuse, (4) sparing the child the kind of life led by the perpetrator, and (5) giving no reason for killing the child. This unusually narrow definition of "fatal child abuse" is rarely acknowledged when statements are made such as the following statements by Durrant (2005): except for 5 children between 1971 and 1975 and four others between 1990 and 1996, "*In all the remaining years, the annual incidence of child abuse mortality was zero. . . . It has never exceeded one death in any year since 1971*" (p. 21, italics hers). As Beckett (2005) pointed out, such a narrow definition might be appropriate in an analysis to compare rates of fatal child abuse rates before and after the 1979 spanking ban in Sweden, but it is totally misleading when comparing rates between countries, because no one else uses such a narrow definition.

### **III. Violence by Young Perpetrators**

Durrant: “6. Larzelere’s claim. The 1979 corporal punishment ban has caused youth violence to increase” (p. 6).

My actual conclusion: “those raised after the ban on smacking are increasingly likely to be perpetrators of assaults as they grow up” (p. 9).

I endorse Dr. Durrant’s excellent hypothesis that if the spanking ban reduced violence in that generation, it should be reflected in long-term changes in the distribution of perpetrators of criminal assaults by age. Her hypothesis corrects for societal changes such as in the severity of assaults reflected in criminal records. Table 1 repeats the relevant data.

Table 1

*Frequency of Criminal Assaults Against 7-14-year-old Children in Swedish Criminal Records (Wittrock, 1995)*

Age of suspect	# of 1984 Suspects (Birth Year)	# of 1994 Suspects (Birth Year)	% Increase
Under 15	116 (1970+)	718 (1980+)	519%
15-19	107 (1965-69)	354 (1975-79)	231%
20-24	12 (1960-64)	28 (1970-74)	133%
25-29	19 (1955-59)	29 (1965-69)	53%
30-39	68 (1945-54)	151 (1955-64)	122%
40-49	47 (1935-44)	116 (1945-54)	147%
50+	24 (< 1935)	57 (< 1945)	128%

Dr. Durrant makes the case that the dramatically larger increase in those raised after the 1979 spanking ban is due to police enforcement of anti-bullying measures in schools. That might be a possibility, but it would behoove other countries to obtain corroborating evidence of this before emulating Sweden’s example. At least two studies in Sweden were initiated in the 1990s because of societal concerns about increasing youth violence. One rationale for one study was that “There is also much evidence that our [Swedish] society has a growing propensity for violence” (Statistics Sweden, 1996, p. 5). Another study’s rationale began, “Since the mid-1980s, the Swedish public has been increasingly concerned about juvenile violence” including “football hooliganism, excessive celebrations on Midsummer eve, acts of violence with racist and xenophobic motives, squatters’ actions, street fights between politically opposed groups, violence at school, and recurrent juvenile tumults at the end of summer vacations, between gangs of Swedish or immigrant background, and skinheads and groups of young female ‘kickers’ . . . In light of this, it seems difficult to deny that the Swedish society in recent years has been hit by a wave of juvenile violence” (von Hofer, 1995, p. 1). Juvenile violence clearly appeared to be increasing during the 1990s according to Swedish social scientists, warranting studies to understand it. After looking at various trends, von Hofer said that it was inconclusive whether recent trends in juvenile violence (i.e., 16-20-year-olds) were actually increasing or were merely

being enforced more often. His final sentence: “No doubt, there is reason to look deeper into the matter.”

One way to corroborate the effect of the anti-bullying campaign on criminal statistics would be to identify the year the campaign was initiated, which should correspond with a sharp increase in criminal assault statistics, hopefully followed by a reduction thereafter. In contrast, the trend in juvenile criminal assaults shows a smooth accelerating increase in assault by minors against minors through 1994.

#### **IV. Safey Networks of Support for Child-Rearing in Sweden**

Durrant: “7. Larzelere’s claim: 46% of children in the child welfare system have been removed from their homes” (p. 6).

My actual conclusion: “*what Durrant calls ‘support and care measures’ consist of removal from the home for 46% of new children in the system, down from 60% being removed from the home when the programme started in 1982*” (p. 11).

I calculated these figures from Durrant’s (1999b) Table 1, which lists the number of new cases in two programs in 1982 and in 1995. Of 7,159 new cases in the voluntary program in 1995, her table lists 2,653 in “out-of-home care,” whereas all 1,202 new cases in the compulsory program were in either “short-term care” or “long-term care,” both of which are out-of-home care according to my sources. Summing those numbers yields my 46%.

As indicated previously, I do not have access to Durrant’s source data on this issue, and it is difficult to reconcile Swedish critics’ views with Durrant’s portrayal. If juvenile delinquents are counted in the Swedish social welfare system, that could indeed account for some of the 46% of new cases in out-of-home care. Child protective service workers have a difficult task everywhere in protecting children without unjustified family intrusions. It would be helpful to policymakers to have an unbiased, objective evaluation of the balance achieved between child protection vs. unnecessary family intrusion in Sweden and in other jurisdictions.

Durrant: “8. Larzelere’s claim: *Children are much more likely to be removed from homes in Sweden than in other European countries*” (p. 6).

My actual conclusion: “*Two [Swedish] sources have claimed that children are much more likely to be removed from their homes in Sweden than in other European countries, although I cannot vouch for their statistics*” (pp. 11-12).

It still seems to me that Dr. Durrant corroborates the number of children in out-of-home care in Sweden cited by these two Swedish critics of their social welfare system, if one understands their numbers to be the total number in out-of-home care in a given year, not just the number newly placed in out-of-home care that year. Ivarsson’s (1983) 22,000 is less than Durrant’s 22,807 for 1982, and Westerberg’s (1999) 15,000 is less than Durrant’s 16,997 for 1995 (Durrant, 1999b, p. 70). I still cannot vouch for the critics’ comparative numbers in neighboring European countries

or whether the critics are correct that children are much more likely to be removed from Swedish homes than from British or German homes. As noted previously, new cases in compulsory out-of-home care increased 7% from 1982 to 1995 (Durrant, 1999b, p. 70).

Durrant: “9. Larzelere’s claim: *Before other countries follow Sweden’s example of a smacking ban, they should consider Sweden’s increase in child abuse and criminal assaults*” (p. 7).

My actual conclusion: “*Before other countries follow Sweden’s example of a smacking ban, they need to explain Sweden’s subsequent increase in child abuse and criminal assaults*” (pp. 14-15).

I agree with Dr. Durrant that there is no conclusive proof that Sweden’s spanking ban is the sole *cause* of the subsequent increases in criminal statistics on physical child abuse and criminal assaults by minors against minors. Changes in what gets reported to the police might possibly account for all of the increase, even though they increased more than 5-fold or 6-fold from 1981 to 1994. If I were a policy-maker in another country, I would want to be sure that such artifacts accounted for these changes, and therefore that they do not represent real increases in these problems. At the very least, there is no evidence that violence has decreased in Sweden as a result of its spanking ban in 1979, either in the form of physical child abuse or in assaults by juvenile delinquents. Other countries need an objective, unbiased evaluation of spanking bans to ensure that their unintended detrimental effects do not outweigh their beneficial effects.

Dr. Durrant has a tendency to imply that, once she has discovered one possible alternative explanation for the 5-fold increase in child abuse or teenage violence, that this proves that neither type of violence was actually increasing during this time period. An alternative explanation remains just that – an alternative explanation. Evidence for the alternative must be compared with evidence for the original possibility, in this case that these increases represent genuine increases in physical child abuse and teenage violence, at least in part. In any case, it is an even bigger stretch to suppose that actual violence was *decreasing* while these crime statistics were increasing so dramatically.

Second, on some issues she now claims that the desirable effects of Sweden’s policy changes came primarily *before* the spanking ban. If so, then other countries should be adopting earlier aspects of Swedish policies. The pendulum of childrearing advice has made a huge swing in the Western world during the past century, from a very authoritarian viewpoint toward a permissive perspective, which was already prevalent in Sweden at the time of their spanking ban. It may well be that a combination of love and limit-setting provides an optimal alternative to both the authoritarian and permissive extremes. Dr. Diana Baumrind’s (1973) authoritative parenting, which combines nurturance, good communication, and firm control, has consistently been associated with optimal child outcomes. Firm control was enforced at least occasionally with spanking in all Baumrind’s original authoritative families. Research is needed to determine what kinds of disciplinary tactics are necessary to achieve authoritative parenting, especially with temperamentally difficult children.

The 1996 survey by Statistics Sweden also found that 83% of Swedish adults under 35 said that “child rearing is difficult,” and only 10% said it was easy. Consistent with this, American

psychologists noted that Norwegian “parents seem often to be immobilized by unreasonable requests made by the child” (Patterson & Fisher, 2002, p. 74). I don’t know whether other cultures find parenting as difficult as do parents after their disciplinary options are restricted by spanking bans, but this might be a problem exacerbated by such restrictive policies.

### **Charges of Bias**

There is plenty of bias to go around on this topic, and Dr. Durrant implies that I am full of it. This is a common human problem, as verified by psychological research on the confirmation bias, the human tendency to remember evidence that confirms our preconceived ideas and to ignore contradictory evidence (Myers, 2004, p. 388; Wason & Johnson-Laird, 1972). The religious tradition I belong to confirms it as well. Jesus updated Jeremiah’s observation of the deceitfulness of the human heart by saying that I am much better at detecting the smallest speck of bias in Dr. Durrant’s eye than I am at noticing the huge log of bias in my own eye (Matt. 7: 3-5, paraphrased). Of course, the principle applies in both directions. The scientific method emphasizes methods to minimize human bias, and I have thus tried to use those methods as rigorously as possible in my work on parental discipline.

The implication that I have had a religiously based preconception that I intend to prove regardless of the evidence is false.<sup>2</sup> My discussion of some relevant scriptural passages were quoted from a response I was asked to write to an article by Oosterhuis in *The Journal of Psychology and Theology*. Her article quoted 8 biblical passages and cited 7 references to psychological research. My commentary quoted 2 biblical passages and cited 26 references to psychological research. I preceded the section that Dr. Durrant quoted from by saying, “I agree with Oosterhuis that many parents use corporal punishment inappropriately and that this tends to be true of conservative Christian parents” (p. 145). The passage itself was my amateurish attempt to suggest a biblical argument against over-use of spanking in case religious clients were ever inclined to defend inappropriate spanking biblically. Toward that end, my main conclusions were that the original Hebrew word for *rod* in Proverbs might have meant a flimsy instrument incapable of physical harm, that biblical discipline was always motivated by love, and that any disciplinary punishment endorsed in Proverbs should be phased out in favor of verbal correction as children grew up.

My willingness to follow the data wherever it leads is illustrated by the first sentence in the concluding Discussion section of my first publication on spanking: “Most of the results of this study support the view that moderate physical punishment provides a training ground for violence, a training ground that differs from child abuse only by degree” (Larzelere, 1986, p. 33). Later on, the major purpose for my most important original research was to discover what we should tell parents of preschoolers to use instead of spanking when we tell them not to spank. I was surprised to discover that the combination of punishment and reasoning was the most effective disciplinary response to 2- and 3-year-olds’ most common misbehaviors and that nonphysical and physical punishment worked equally well in that combination (Larzelere, Schneider, Larson, & Pike, 1996).

I do have a bias, however, one possibly rooted in my childhood. I grew up in a working class family, who only consulted with one psychologist, to give an IQ test to my Down Syndrome

sister. Consistent with that era, he concluded that a child with such a low IQ could never learn to read and write and thus should not be taught such skills in school. The school never reconsidered until my mother taught her to read and to write. I was unaware of my cousin's frontal lobotomy and my favorite uncle's shock therapy until adulthood, but these early experiences may have given me a lifelong skepticism about premature conclusions confidently proclaimed in the name of psychology. When premature conclusions turn out to be wrong, they affect many ordinary people, who often do not have the ability to overcome the bad advice like my mother did. Therefore I have a strong bias that psychological conclusions should be as well-supported as possible by scientific evidence before we impose those conclusions on other people. The best scientific evidence is especially crucial before we start telling other ethnic groups, other religious groups, and other countries that they should never use even the mildest form of spanking – that we Western experts know better than many previous generations of their parents.

### **Conclusion**

Policy makers must do their best to distinguish objective evidence from advocacy-based distortions of the evidence – on this topic as on many others. I have therefore had the main data source translated into English (Wittrock, 1995) and made available on a colleague's website at <http://people.biola.edu/faculty/paulp/>. Interested persons can examine the data on criminal assaults against children in Sweden from 1984-1994 for themselves and compare our respective interpretations of those data. My other evidence is in English, including three recent articles that corroborate my critique of Durrant's evaluation. Roberts (2000) showed that Swedish attitudes about parental spanking changed before their 1979 spanking ban, not afterwards. Beckett (2005) exposed the Swedish myth that the spanking ban virtually eliminated child abuse fatalities. Two Swedish psychiatrists concluded that “the number of physically abused children reported to the police in Sweden has increased [between 1986 and 1996]” (Lindell & Svedin, 2001, p. 150).

My previous conclusions therefore remain appropriate. Although I do not question the good intentions of Dr. Durrant, “there is no objective evidence that the overall situation has improved for children in countries that have adopted smacking bans” (p. 15). Second, “the most appropriate conclusion seems to be that *how* parents use any disciplinary tactic (including smacking) is more important for its effect on children than *whether* they use it or not” (p. 15). Finally, “Policy makers need more clear-cut, unbiased evidence that smacking is invariably detrimental before they impose the view of anti-smacking advocates in the face of generations of disciplinary practices by parents in most cultures” (p. 16).

### **Endnotes**

<sup>1</sup>e.g.,  $(100-68-18)/(100-18) = .17$  for fathers of 55-74-year-olds, using 68% from Durrant's Table 4 and 18% missing answers above for this age group.

<sup>2</sup>Part of Dr. Durrant's ad hominem attack concerns my previous affiliation with Biola University. The name, Bible Institute of Los Angeles has not applied since the 1950s, when it moved outside Los Angeles and had expanded to a religious liberal arts college. The Ph.D. program I taught in was founded in Rosemead, California, and combined with Biola College in the 1970s to make it Biola University. Its doctoral programs in clinical psychology have been accredited since 1980 by the American Psychological Association. Thus Biola University has grown from a religious training college to a small comprehensive university, a typical developmental pattern among private American colleges historically.

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