



# THE ADVISOR

AMERICAN PROFESSIONAL SOCIETY ON THE ABUSE OF CHILDREN

## NEWS

**APSAC FILES SECOND AMICUS BRIEF BEFORE U.S. SUPREME COURT; PLANS FOR APSAC'S FIRST NATIONAL COLLOQUIUM UNDERWAY; NOMINATIONS SOUGHT FOR BOARD ELECTIONS**

—by Theresa Reid

### New Amicus Brief

APSAC has filed its second amicus brief before the U.S. Supreme Court. The case is *Montana v. Imlay*. At issue is whether completion of an offender treatment program can be required as a condition of probation.

Donald Imlay was found guilty in Montana of molesting a little girl. He was given probation on the condition that he complete a sex offender treatment program. Like most such programs, the one to which Imlay was sentenced required that he overcome denial about his offense. Having maintained his innocence throughout his trial, Imlay continued denying the offense during treatment. The therapist told the court that Imlay's persistent denial made him unsuitable for outpatient treatment, and recommended that Imlay receive inpatient treatment. But the only inpatient sex offender treatment facility in Montana is in the state prison. When Imlay appealed, the Montana Supreme Court found that mandated offender treatment which requires the offender to overcome denial violates the probationer's Fifth Amendment rights against self-incrimination.

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## RESEARCH AND PRACTICE

### A PRACTITIONER'S GUIDE TO INTERPRETING RESEARCH RESULTS

—by Kathleen Kendall-Tackett, Linda Meyer Williams, and Paul Stern

*APSAC encourages research and discussion of research findings in the area of child abuse and neglect. We understand that sometimes research is difficult to understand, and in fact can be downright incomprehensible. As a service to our members, The Advisor thought it might be helpful to share the following exchange of letters between prosecutor Paul Stern and researchers Linda Meyer Williams and Kathy Kendall-Tackett.*

Dear Linda and Kathy,

Help! I am a trial lawyer, and I am the first to admit that we talk funny, with all those Latin phrases (for years I thought *Lis Pendens* was a woman in my first-year law class). But at least there are law dictionaries out there. I read a lot of research articles, and you folks have a language all your own, and I can't find a research guide anywhere.

What the heck is a "cohort"? Why is *p* always less than something, never more than, and what kind of expression is that, anyway? I prosecute deviants, but after seven years at it, the only thing I know for sure is that there is no such thing as a standard deviation—yet you guys talk about them all the time. If a deviation was standard, it wouldn't be a deviation, but a societal norm. But norms are something else again.

Please tell me what this stuff means. De-mystify your vocabulary. And how in the world am I supposed to figure out if what I am reading is good research or bad research? Can I really rely upon a study that draws wide-ranging conclusions based on a sample (I know, an "n") of 100 people?

Is a study which is of 500 subjects ten times better than one of 50? How do I tell if this is really new information — information I should ask a judge, a jury, and an expert witness to rely upon? Help.

48% sincerely, 38% cordially, and 19% very truly yours,  
Paul

Response

Dear Paul,

We admire your desire to use and not abuse research results, and to discern which research is worthy of your attention. As you have discovered, research terminology can be confusing. Many people attribute this to a lack of socialization on the part of researchers. Some have even been so unkind as to refer to researchers as "nerds," likening us to trekkies (without tape on our glasses, polyester clothing, or plastic pocket protectors). But we assure you, research terminology is not meant to exclude or confuse others.

As with professionals in any field, researchers have an "insiders' language" of agreed-upon terminology that helps us communicate clearly and concisely with each other. Imagine how much longer journal articles would be if we had to say "number of subjects" or "probability that these results could have occurred by chance" instead of "n" and "p". As you can see, these types of abbreviations save lots of time and paper, and make really good sense. We hope we can clear up some of your questions about them.

*What the heck is a "cohort"? Why is p always less than something, never more than? I prosecute deviants, but after seven years at it, the only thing I know for sure is that there is no such thing as a standard deviation.*

First off, it is important for you to understand that one study (or one journal article) is *never* the final word. Journal articles are essential to the scientific process in that they communicate findings to other scientists, and add to the body of research literature. Each published paper is a piece of the puzzle that enables other scientists to build on work that has already been done, thus advancing knowledge in the field.

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**KENDALL-TACKETT,**  
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A key component of the scientific process is the replication of results. When several researchers find the same type of result, it is more likely that the result is genuine and not caused by some peculiarity in the research design or by chance error. That is why you cannot place too great an importance on the results of one study—even an excellent one. That is also why “reviews of the literature” are especially helpful. “Literature” or research reviews survey many articles on the same topic, noting aspects of study design, data analysis, and conclusions that help readers put the findings of particular studies in perspective.

However, we understand that there are times when one study is all you have. Therefore, we have attempted to define what makes a good study. Defining “good research” is about as easy as nailing jelly to the wall. As anyone who has ever submitted an article to a journal will tell you, one person’s good research is another person’s trash. But do not despair! Some rules of thumb will help you make that judgment.

**Research Basics**

First, it helps to have a bird’s eye view of the function and purpose of research. The most basic question of research is, “How does X affect Y?” X might be sexual abuse, and Y a child’s emotional health; X a particular treatment method, and Y a perpetrator’s subsequent behavior; X a prevention program, and Y a child’s knowledge. X is called the *independent variable*: we can manipulate it—eliminate sexual abuse as a consideration, change a treatment program, refine a prevention curriculum. Y is called the *dependent variable*: we don’t directly manipulate it; rather, if it varies, it varies (hypothetically) with changes in the independent variable.

*As with professionals in any field, researchers have an “insiders’ language.” ... We hope we can clear up some of your questions.*

A study may have more than one variable (that is, independent variable: when people use shorthand and talk about a study’s “variables,” typically they are talking about the independent variable). For instance, a study might ask whether either socioeconomic status or child abuse has measurable effects on a child’s emotional health.

If we’re trying to find out the effect of X on Y (or, to turn it around, what factors cause a change in Y), we want to rule out—*control for*—the effects of other possible variables. For instance, if we’re testing the effects of a particular treatment program on

incarcerated offenders, we want to control for the possibility that something besides the treatment program—say, visits from family members—affects offending behavior. Variables that we have not adequately controlled for are aptly called *confounding variables*.

The results of research are reported in scientific articles, which are generally divided into four major sections: Introduction, Methods, Results, and Discussion. Here are some things to look for in each section.  
**The Introduction**

The Introduction states the research questions for the present study and briefly reviews relevant previous research. A good introduction describes the major studies on the topic at hand, clearly articulates the goals of the present study, and explains reasonably why past study results justify the present study. The authors should make a case for doing this particular study, stressing how it will advance the frontier of the question being asked, or clarify a major muddle. In the introduction, also, the researcher states the *hypothesis*. As you probably know from high school science, the hypothesis is the statement of expectation: “I think X is going to affect Y in these ways.” The *null hypothesis* is the statement that X will have no effect on Y. A null hypothesis sounds like a silly thing to spend time proving, but it’s important when someone claims, for instance, that a certain treatment program is highly effective, and others want to prove the person wrong.

**Methods**

The Methods section is where the researcher describes what he or she did in the study. Several aspects of the methods need to be considered.

**Sample**

The sample is the group of people studied. Sampling is a key issue, and can be a major source of discrepancies between studies. When you evaluate a study, you must consider whether the sample is appropriate to the question being asked. For example, is the sample from a clinical practice or from the larger, nonclinical community? Is it comprised of people who underwent a traumatic experience in the distant past, or more recently? At what developmental stage are people in the sample? Are race, sex, socioeconomic status, education level, etc., important to the question being asked? If so, is the sample appropriately chosen? Whom does the sample represent, and can conclusions drawn from a study of this sample be generalized to a larger group? When authors attempt to apply their results to all sexual abuse victims, for example, but have sampled only a clinical population, their conclusions are likely to be inappropriate, because sexual abuse victims who seek and receive treatment may differ from those who do not. (For more information, see the introduction of Kendall-Tackett and Simon [1987], where

this issue is discussed in agonizing detail.)

*Cohort*, to answer your question, is often simply used as another name for sample. It is more specifically used in longitudinal research (discussed below) to describe the groups the researcher will follow at point 1, point 2, and beyond. It is not the group the researcher hangs out with after work.

*The main benefit of correlational research is that you can study the effects of harmful influences without having to introduce them in an experiment. The main disadvantage is that you cannot say variable X causes Y.*

*Sample size*, to answer another of your questions, requires a judgment call. The answer to the question, “How big is big enough?” is, “It depends.” If you have a carefully controlled experiment with only one or two variables, even 20 subjects might be enough (although a larger sample would be preferable). On the other hand, if you are trying to consider many variables, and you want your results to be applicable to a broad population, your sample size may need to be in the hundreds or thousands. As a rough guideline, many researchers recommend a minimum of 10 subjects for every variable included in the design.

**Research Design**

Researchers choose from a variety of design elements depending upon the question they’re asking. One of the first questions is whether a study will be *longitudinal* or *cross-sectional*. Most studies on child abuse collect data only once, which means they’re *cross-sectional*. Studies which collect data more than once (e.g., 6 months, 12 months, 24 months, and 36 months post-abuse) are *longitudinal*. Longitudinal studies are more difficult and costly than cross-sectional studies (just keeping track of subjects can be very difficult), but they give researchers a chance to address questions about the effects of various interventions over a period of months or years.

Another major distinction is between *experiments* and *correlational studies*. (Researchers can use either longitudinal or cross-sectional data collection with either type of study.) The key distinction is whether the researcher places subjects into groups, then tries out different variables on each group, or whether the researcher studies already-existing groups (or conditions), such as abused vs. nonabused children.

In an *experiment*, subjects are randomly assigned to groups, and different variables are introduced (or there may be treatment for one group and no treatment for the other).

Experiments have the advantage of maximum control of potential biases, and allow the researcher to claim that *X causes Y*. The major drawback of experiments is that they are often artificial because the number of variables that can be explained and controlled for within the design is limited. In addition, experimental designs have recently come under fire for ethical reasons, because they mean withholding potentially beneficial treatment (such as medications and early intervention programs) from subjects in the non-treatment or "control" group. Further, experiments cannot be used to address issues where the "treatment" involves potentially serious harm to subjects. You can't randomly assign one group of children to be abused, and another to be treated well. Although experimental designs have been used very effectively in research on children as witnesses and similar topics, some questions must be examined in groups that occur naturally. That brings us to the other major type of design—the correlational study.

In *correlational research*, there is no random assignment to groups, and therefore less control of potentially confounding variables. You do not attempt to change subjects, but simply collect data from them by asking questions or using other measures to evaluate them.

The main benefit of *correlational research* is that you can study the effects of harmful influences without having to introduce them in an experiment. The main disadvantage is that you cannot say variable *X causes Y*. Predictably, cigarette manufacturers love this type of data. Because we can't ethically conduct an experiment in which one group is subjected to cigarette smoke on a daily basis for 20 years, and another is free of it, cigarette manufacturers are forever claiming that no one has ever proven that smoking *causes* lung cancer.

If cigarette manufacturers' claim is valid, how can we make any definitive statements about the effects of abuse? Aren't we caught in the same type of dilemma?

The answer is "yes and no." Although we cannot claim that child abuse causes the negative outcomes we see (e.g., aggression, depression, sexual acting out, etc.), we can describe the effects of abuse by statistically controlling for other factors that may cause the negative outcomes (the dependent variables). That is, if someone suggests that socioeconomic status, not abuse, accounts for the negative outcome, we can be sure to take the socioeconomic status of subjects into account when analyzing the data. If the effect occurs independent of variations in SES, we can be more confident that abuse, not SES, is causing it.

By controlling for several potential intervening or confounding variables, we can find out the strength of the relationship of child abuse (the independent variable) to the negative outcome we're studying (the de-

pendent variable). This is where you are likely to encounter terms such as "percentage of variance accounted for" or "independent contribution." For example, "Child abuse accounts for 45% of this negative outcome," or "Child abuse clearly makes an independent contribution to this outcome."

Although we cannot say that child abuse causes depression, we can say that child abuse is related to or increases the likelihood of depression. For further information on the subject of research design, see Philips (1971) and Rosenthal and Rosnow (1984).

#### **Data Collection**

Another critical issue reported in the Methods section is how the data were collected. Did researchers review case records searching for specific sorts of information? Did they interview subjects? If so, did they use a written protocol? Did researchers administer tests to measure personality traits or the potential to commit child abuse? The measures used are very important. Standardized measures are those that have gone through an often rigorous process of being used and tested by others before being published. Standardized measures are tested for *validity* (it measures what it's supposed to) and *reliability* (it is consistent). When standardized measures are used, we know more about what the results mean.

But in our field, we cannot be afraid of

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research which uses new measures. In the past, many standardized measures were developed with no sensitivity to or consideration of childhood abuse or trauma. Just as clinical assessments often neglected to ask individuals about experiences with child abuse or family violence, many standard measures of child behavior or human psychological functioning have ignored the widespread human experience of child abuse and other maltreatment. Measures still need to be developed and standardized which take these phenomena into account.

#### **Results**

The results section is where the researcher describes the results of statistical analysis. Many books have been written discussing the rigors of appropriate analysis; it is impossible to summarize them briefly enough for this already long letter. But we would like to assure you that, although there is some variation of opinion about appropriate statistical analysis, there are certain well-

established standards, and it is highly unlikely that someone just "cooked" the data to get it to say what he or she wanted. Even in the case of out-and-out fraud, the results of the "cooked" study would fail to hold up under replication, and the fraud would be quickly exposed.

When you are considering whether to trust the analysis, consider where the article was published. If the journal is reputable, then the chances of the researcher using inappropriate statistics are far fewer. In our experience, reviewers are only too happy to spot inappropriate statistics (it shows they were paying attention in their stat classes), and will jump on faulty analysis like ducks on June bugs.

But to answer some of your specific questions. . . . As we indicated earlier, *p* refers to the "probability that these results occurred by chance." By convention, something is statistically significant if  $p < .05$  (translation: it has less than a 5% chance of occurring by chance). In this case, the smaller the number, the better. If  $p < .01$ , then the result is considered "highly or very significant." Statistically significant does not necessarily mean "good," nor does it mean "socially significant." It merely means that the change (variance) in the dependent variable accounted for by the variables you are interested in is greater than the amount of error.

Which brings us to *standard deviations*. These numbers reflect the degree of heterogeneity within each group. A large standard deviation means that many individual responses varied widely from the group mean. A small standard deviation means that there were few "outliers"—that individual responses tended to cluster around the mean. Results can be significant even with a large standard deviation (especially when the sample size is large): the test of statistical significance takes the standard deviation and sample size into account.

#### **Discussion**

The final section of a scientific article is the *discussion*. In this section, the author summarizes the results of the study in words (rather than statistical symbols), and attempts to draw conclusions. The author may also describe how this study is similar to or different from previous studies. This section is generally considered one of the "softer" sections because it is open to the author's interpretation. On the other hand, it is often the most interesting to read, and may be the most important, because readers can benefit from the full experience of the author. It also provides a ready-made interpretation of the statistical results. The major danger in this section is that the author (and therefore you) will overstate his or her case and make conclusions that go beyond the study.

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**KENDALL-TACKETT,**  
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When articles appear in journals such as *Journal of Interpersonal Violence* or *Child Abuse and Neglect*, they have survived a peer-review process. This means that they have been sent to two or more reviewers (sometimes also called referees, although they rarely wear black-and-white shirts) for comments and suggestions about whether the journal should publish the paper. The author is typically required to revise the article one or more times before it appears in print.

Although the peer-review process is not perfect, it is a check for the scientific value of the article. If the article is published in a reputable journal, there is some assurance that the methods, data analysis, and conclusions are at least reasonable.

However, we would like to caution you that publication of a peer-reviewed journal is no guarantee of good science. That is where replication of findings comes in (have

we mentioned this before?). If a study was poorly done, and it somehow slipped through the peer-review process, chances are the findings will not be replicated in future studies. That is why it's important to base practice or policy decisions on the results of more than one study (to find reviews of the relevant literature).

We hope we have been able to answer some of your questions and make the findings from research more accessible. Thank you for your interest, and good luck in your work.

Sincerely,  
Kathy and Linda.  
P.S. Just who precisely is this *Lis Pendens*?

**Response**

Dear Kathy and Linda,

Thanks for your reply. I understand research better now. I write a three-paragraph letter, and a researcher needs eight pages to answer.

*Lis Pendens* hasn't meant a thing to me

since I met *Viva Voce*. I'm enclosing the plastic pocket protector you left at the APSAC conference in San Diego.

Sincerely,  
Paul

**References**

Kendall-Tackett, K.D., and Simon, A.F. (1987). Perpetrators and their acts: Data from 365 adults molested as children. *Child Abuse and Neglect*, 11, 237-245.

Philips, D.I. (1971). *Knowledge from what? Theories and method in social research*. Chicago: Rand McNally.

Rosenthal, R., and Rosnow, R.L. (1984). *The essentials of behavioral research: Methods and data analysis*. New York: McGraw Hill.

*Kathleen Kendall-Tackett, PhD, is a Developmental Psychologist and Research Fellow at the Family Research Laboratory at the University of New Hampshire. She is also on the Board of Directors of the Massachusetts chapter of APSAC, and chairs MAPSAC's Research Committee.*

*Linda Meyer Williams, PhD, is a Sociologist and Research Associate Professor at the Family Research Laboratory at the University of New Hampshire. She is a member of APSAC's Executive Committee, and is co-chair of its Research Committee. She is a founding member of the Northern New England chapter of APSAC.*

*Paul Stern, JD, is Deputy Prosecuting Attorney for Snohomish County, Washington. He is also a member of APSAC's Executive Committee, and is Secretary of the Washington state chapter of APSAC.*

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The State of Montana's Attorney General's office was joined by 20 other states in asking the U.S. Supreme Court to hear the case. They argued that the Montana Supreme Court decision went against several precedents. When the U.S. Supreme Court agreed to hear the case, the Montana Attorney General's office approached APSAC for an *amicus* brief explaining the expectations of offender treatment. APSAC's Legal Committee strongly recommended to the Executive Committee that the brief be undertaken, and the Executive Committee agreed. The primary concern from APSAC's point of view was that, if the Montana decision stood, judges would have to give probation *without* treatment if probation were the choice.

Primary authors of the brief were offender treatment expert William Murphy, PhD, of the University of Tennessee, and legal scholar John E.B. Myers, JD, of University of the Pacific's McGeorge School of Law. Josephine Bulkley, JD, provided substantial written commentary on the case before the brief was undertaken. Treatment experts William Pithers, PhD, Robert Prentky, PhD, and Lucy Berliner, MSW, contributed to the writing of the brief.

The brief focuses on three aspects of offender treatment: the necessity that the offender admit the crimes he or she has committed, the importance of the court order in securing participation in a treatment program, and the effectiveness of offender treatment programs.

The Supreme Court's decision is expected this Spring, and will be reported in

*The Advisor*. APSAC owes major thanks to those professionals who responded so quickly to the request for this brief. By contributing unstintingly of their time and expertise, they have enabled APSAC to weigh in at the highest level on the critical issue of offender treatment.

Copies of the brief are available by writing to APSAC's office. Please send \$5.00 to cover costs.

**APSAC's First National Colloquium**

APSAC's Program Committee has been hard at work designing APSAC's First National Colloquium. It will be held in June 24 - 26, 1993, in Chicago. The committee decided "colloquium" was the best word to convey the intention of having small, highly interactive, intensive, day-long sessions.

Since many other national conferences do an excellent job offering 1-1/2 hour workshops, APSAC has decided to fill a different need. The first day of the colloquium will be devoted to intensive, day-long *within-discipline* institutes, the second day to intensive, day-long *across-discipline* institutes. The focus of the colloquium will be professional interaction. Participants of many institutes will be asked to send in case examples in advance, so institute leaders can incorporate participants' immediate concerns into the day's colloquium. The idea is to replace the "me lecturer, you student" format with one that stresses the energetic interaction of peers.

In addition, program designers are committed to putting into practice at this colloquium one of the major goals of APSAC's Board: to foster new leadership in the field of child abuse and neglect. Many

institute faculty teams will pair a well-established, well-known expert in the field with a less well-known expert who has a great deal to contribute. APSAC's goal in recognizing emerging expertise is to maximize the talent brought to bear on the formidable challenges confronting this field.

Please contribute your talent by submitting your ideas for topics of both within-discipline and cross-discipline colloquia. Attention your suggestions to Lucy Berliner, Chair, Program Subcommittee, at APSAC's office address.

**Nominations sought for Board election**

It's time to start thinking of outstanding professionals to nominate to stand for election to APSAC's Board of Directors. The election will be held this Fall, and nominations are due in by July 15th. You can either suggest a nominee to the Nominating Committee, or nominate someone directly. APSAC seeks nominees from all relevant disciplines, all regions of the country, and all ethnic, religious, and racial groups. For a copy of the nominating procedures, call or write the office.

**Thanks!**

Many members have called in asking for brochures to distribute at conferences, seminars, and training sessions around the country. More than 10,000 APSAC brochures have been sent to members just this Spring. By telling your colleagues about APSAC, you help build a national network of professionals that can profoundly affect the way America responds to the pervasive maltreatment of its children. Thank you for your dedication and support!