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9. The problem of child sexual abuse: Response.

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The Problem of Child Sexual Abuse

THE POLICY FORUM "THE SCIENCE OF CHILD sexual abuse" by J. J. Freyd *et al.* (22 Apr., p. 501) provides an extremely important call to action to the scientific community. In 1999, James Mercy, Senior Scientist at the U.S. Centers for Disease Control and Prevention, noted the importance of viewing child sexual abuse with "new eyes" (1). The implementation of Freyd *et al.*'s policy recommendations would help us to do this. For too long, the fact that the topic makes us uneasy has caused too many of us to avert our eyes. But what if child sexual abuse were a newly discovered disease—a disease that affects up to 20% of women and 10% of men, a disease that forms a potent risk factor for developing a host of mental and

IN THE POLICY FORUM "THE SCIENCE OF child sexual abuse" (22 Apr., p. 501), J. J. Freyd and colleagues succinctly summarized an enormous amount of research and rightly highlighted the depressing bias of those researchers who "emphasized false allegations rather than false denials." It is a sad fact of life that after decades of ignoring the issue altogether, research showing the alarming prevalence of child abuse has met with a kind of "backlash."

Mental health professionals have a poor track record in this field. It was only three decades ago (1975) that the leading psychiatric textbook in the United States informed students that the rate of incest was 1 case per million (1).

If I have a criticism of the Policy Forum, it is that the list of proven effects of child sexual abuse did not include psychosis and schizophrenia. Recent large-scale studies in

abuse is both actual and reported, b be the joint frequency with which actual abuse is not reported, c be the joint frequency with which there is no actual abuse but a report of it, and d be the joint frequency with which there is no actual abuse and no reported abuse. The finding that there is "underreporting" of abuse simply states that the frequency of $b > 0$. In contrast, the statement that reporting underestimates actual abuse is the statement that $a + c < a + b$, or $c < b$. There is no way of making this inference until there is some way of knowing the joint frequency with which abuse does not occur but is reported (for whatever reason). Moreover, given that abuse (fortunately) is uncommon (according to the authors, well less than 50% of children are abused, i.e., fall in cells a and b), it is plausible to hypothesize that c might be greater than b , despite the value of $b > 0$. Of course, the most accurate way to determine c would be to survey people randomly whom we know have not been abused and then estimate how many nevertheless report having been abused, a daunting task.

		Reported abuse	
		+	-
Actual abuse	+	a	b
	-	c	d

“**But what if child sexual abuse were a newly discovered disease—a disease that affects up to 20% of women and 10% of men... a disease that... costs society over \$24 billion each year?**”

—FINK

physical problems, a disease that, according to a conservative estimate by the U.S. Department of Justice, costs society over \$24 billion each year (2)? Imagine what we as concerned scientists would do if we discovered such a disease decimating the lives of our young people?

Our response to child sexual abuse thus far "has been far from the full-court press reserved for traditional diseases or health concerns of equal or even lesser magnitude" [(2), p. 317]. We have severely underestimated the effects of this problem on our children's health. It is time to recognize that the problem is not solely a product of the action of a few sick individuals; child sexual abuse is a preventable health problem that has been allowed to spread unabated due to scientific and social neglect.

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the UK (2) and the Netherlands (3) have confirmed our smaller studies in New Zealand (4, 5) that child sexual abuse is highly predictive of these supposedly biologically based "mental illnesses."

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IN THEIR POLICY FORUM "THE SCIENCE OF child sexual abuse" (22 Apr., p. 501), J. J. Freyd *et al.* assert that "[s]urveys likely underestimate prevalence [of child sexual abuse] because of underreporting and memory failure" There is no way of making that inference.

Consider a 2x2 table in which we assess the relationship between actual abuse and reported abuse. Let a be the joint frequency with which

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THE POLICY FORUM "THE SCIENCE OF CHILD sexual abuse" by J. J. Freyd *et al.* (22 Apr., p. 501) calls for more and better scientific research on child sexual abuse (CSA). Yet it misleadingly suggests that the "[c]ognitive and neurological mechanisms that may underlie the forgetting of abuse have been identified." This statement implies that the forgetting of CSA is somehow special and cannot be explained by the sorts of principles familiar in the cognitive and neuroscientific study of memory.

The notion that individuals can develop amnesia for seemingly unforgettable traumatic events, followed by "recovery" of these memories months or years later, has been part of the folklore of psychiatry and clinical psychology for more than 100 years and has been described under such headings as "repression," "dissociation," and "traumatic amnesia" (1). But, in fact, surveys of trauma victims show that they typically remember their experiences all too well, and any forgetting is easily accounted for by organic factors or by normal memory processes such as ordinary forgetting and infantile or childhood amnesia (2).

Genuinely traumatic events—those experienced at the time as overwhelmingly terrifying and life-threatening—are seldom, if ever, truly forgotten. Evidence to the contrary comes from methodologically weak studies that generally fail to obtain corroboration for retrospective self-reports of either trauma, amnesia, or both. Some investigators also confuse forgetting and amnesia with a simple failure to disclose a memory, or the recovery of memory with the reinterpretation of an event always remembered. The study cited by Freyd *et al.* (3) concluded that its results “do not support the existence of special memory mechanisms unique to traumatic events, but instead imply that normal cognitive operations underlie long-term memory for CSA” (p. 117).

These facts have long been known (4, 5), although they are often ignored or discounted by professionals—researchers as well as clinical practitioners—and unappreciated by the public at large. So far as the scientific evidence is concerned, traumatic amnesia appears to be a myth. Rather than searching for the cognitive and neurological mechanisms underlying a phenomenon that appears to be nonexistent, scientists and policy-makers might better focus their resources on the very real problems of CSA: its causes, correlates,

and all-too-real consequences, and the most effective means by which it can be treated and prevented.

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Response

We concur with the Letter writers that child sexual abuse (CSA) is a serious public health problem. We also agree with Read that links between CSA and adult psychosis (1) should not be overlooked.

As Dawes indicates, one must take into account both false allegations and denials in determining the prevalence of sexual abuse. However, evidence indicates that false allegations occur at rates lower than nondisclosure rates. Prevalence is underestimated (in Dawes' notation, $c < b$) whenever the likelihood that reports of abuse are false $[c/(a+c)]$ is less than the likelihood that true abuse is not disclosed $[b/(a+b)]$. Mechanisms of false allegations, such as suggestive therapy or interviewer bias, occur in a small minority of abuse reports: 2% of survey respondents claiming abuse report having recovered their memory with the help of a professional or others (2), and approximately 10% report that the abuse was disclosed to authorities, setting an upper bound on adult influences (3). On the other hand, most surveys of adults with “well-documented serious abuse or neglect” have found nondisclosure rates over 30% [(4), p. 270].

High rates of nondisclosure also speak to Kihlstrom and colleagues' assertion that sexual abuse is “seldom, if ever, truly forgotten.” Although underreporting is attributable in part to abuse victims' reluctance to disclose, Williams' (5) difficulty in eliciting abuse reports despite extensive questioning of women with documented abuse histories led

Loftus *et al.* (6) to acknowledge that “many children can forget about a sexually abusive experience from their past” (p. 1177). What Kihlstrom *et al.* call “folklore” is actually over 100 years of clinical and scientific evidence for the forgetting of trauma (7). Although the frequency and mechanisms of forgetting are

trauma” [(10), p. 428] are diagnostic of pathological posttraumatic conditions and may both reflect, in part, some common underlying dysregulation of memory processes (8). Indeed, traumatized individuals exhibit a range of memory impairments (11). Research on executive control over recall of unwanted memories (12), and research on children’s (13) and adults’ (14) encoding and memory of trauma stimuli has provided preliminary support for models of repression and traumatic amnesia. The relations among the effects of trauma on encoding, retrieval inhibition, and memory functioning are worthy of future study.

In our Policy Forum, we recommended a series of international consensus panels on scientific and clinical practice issues related to CSA, expansion of the National Child Traumatic Stress Network, and the creation of a new Institute of Child Abuse and Interpersonal Violence within the NIH that would foster research on CSA and related conditions. Claims that traumatic amnesia rarely occurs, as well as legitimate disagreements over the prevalence and accurate recall of CSA, reinforce these recommendations. Denial and underestimation of

the effects of CSA continue to be serious obstacles to ending a preventable public health problem.

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“**Claims that traumatic amnesia rarely occurs, as well as legitimate disagreements over the prevalence and accurate recall of CSA, reinforce [our] recommendations.**”

—FREYD ET AL.

not completely clear, the basic phenomenon is documented in dozens of empirical studies (8) and corroborated case studies (9).

Kihlstrom *et al.* argue that trauma victims typically remember their experiences “all too well.” However, both intrusive recall and an “inability to recall an important aspect of the

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A Celebration of Ignorance

CONGRATULATIONS TO *SCIENCE* FOR SHIFTING paradigms in the 125th Anniversary issue (125 questions: what don't we know?, 1 July), not only by focusing on what leading contemporary scientists "don't know," but also by unabashedly labeling this collection of 125 important unanswered questions as a "survey of scientific ignorance." Back in 1984, based on my mentor Lewis Thomas' whimsical suggestion (1), my late husband and I brought ignorance out of the closet by creating the University of Arizona's "Curriculum on Medical Ignorance" (featuring a Summer Institute,

distinguished visiting "ignorami," and ignorance logs and exercises) to teach medical and later undergraduate and K-12 students and science teachers how to recognize and deal with ignorance—"what we know we don't know, don't know we don't know, and think we know but don't"—about a wide range of medical and scientific topics (2). Our curriculum has resulted in various ignorance-based publications, presentations, media coverage, and products, earning me the dubious title of "Ignorama Mama," mother of the global ignorance movement. Indeed, all learning and discovery do take place in the terrain of ignorance, not knowledge, and it is questions, questioning, and questioners that impel scientific advances. These mysteries and puzzles, not dry facts and pat answers, should also drive science education as well as the research enterprise. A *Wall Street Journal* editorial (3) paradoxically hailed our evolution from the Information Age to the "Age of Ignorance," where we can recuse ourselves from excessive information, admit we don't know, and humbly "google" or grope our way through what we need to know. And newly minted Nobel physicist David Gross lauded "ignorance—the most important product of knowledge"

as "lucky for science, scientists, and the Nobel Foundation" (4).

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What Are Our Research Priorities?

The advancement of science depends on what James Clerk Maxwell termed "thoroughly conscious ignorance" ("In praise of hard questions," Special Section on 125 questions: what don't we know, 1 July, p. 76). If deepening the "consciousness" of our ignorance is a prerogative of scientists, then this implies a responsibility to reflect on what to know first or the (type of) knowledge our world needs most urgently. Which hard scientific questions should become research priori-

ities if we take into consideration Kofi Annan's urgent appeal to the scientific community to improve global human welfare (1)?

It would be fascinating to see how much science could contribute to a research agenda that serves mankind by making our ignorance with respect to people, planet, and profit more conscious. Which not, yet, answered scientific questions could produce a genuine breakthrough in mankind's understanding of sustainable development?

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CORRECTIONS AND CLARIFICATIONS



Table of Contents: (22 July, p. 525). The credit for the image of parrots on page 529 was not given. It should be Roland Seitre.

Reports: "Causal protein-signaling networks derived from multiparameter single-cell data" by K. Sachs *et al.* (22 Apr. p. 523). The author names in reference (26) were incorrect. The authors are I. M.

Ong, J. D. Glasner, D. Page. The URL for the supporting online material was incorrect; it should be www.sciencemag.org/cgi/content/full/308/5721/523/DC1. There was also a reference missing in the last sentence of the last paragraph on page 527 that continues on page 528. The new reference (29) is P. O. Krutzik, G. P. Nolan, *Cytometry* **55**, 61 (2003). The acknowledgments will now be reference (30).

Technical Comment Abstracts: "Response to Comment on 'Molybdenum isotope evidence for widespread anoxia in mid-Proterozoic oceans'" (12 Aug., p. 1017). The first author should be A. D. Anbar, not D. Anbar. The name appears correctly in the full-text online version.

TECHNICAL COMMENT ABSTRACTS

COMMENT ON "Quantum State Transfer Between Matter and Light"

S. J. van Enk and H. J. Kimble

Matsukevich and Kuzmich (Reports, 22 October 2004, p. 663) claim to have produced several types of nearly maximally entangled states involving photons and atomic ensembles. We show that their experimental evidence is insufficient to support these claims, that their comparisons to a previous experiment are misleading, and that their sweeping assertions related to quantum networks are unjustified.

Full text at

www.sciencemag.org/cgi/content/full/309/5738/1187b

RESPONSE TO COMMENT ON "Quantum State Transfer Between Matter and Light"

D. N. Matsukevich and A. Kuzmich

van Enk and Kimble criticize several aspects of our study but do not challenge our main result, the achievement of quantum state transfer between matter and light. Instead, their critique focuses on the quantitative amount of entanglement present in our experiment and how the vacuum should be accounted for in these measures, both in our experiment and in others. Although a careful discussion of this topic has some value for the field, it does not alter the conclusions of our paper.

Full text at

www.sciencemag.org/cgi/content/full/309/5738/1187c

Letters to the Editor

Letters (~300 words) discuss material published in *Science* in the previous 6 months or issues of general interest. They can be submitted through the Web (www.submit2science.org) or by regular mail (1200 New York Ave., NW, Washington, DC 20005, USA). Letters are not acknowledged upon receipt, nor are authors generally consulted before publication. Whether published in full or in part, letters are subject to editing for clarity and space.