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Review Article

A Brief Memoir on the Early Days of the "Battered Child Syndrome"

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Abstract

The "Battered Child Syndrome" is today a widely accepted concept in Pediatric Medicine. However, there was an earlier time when this concept was still "in the air" rather than a known quantity. Recognition came slowly, impeded by intellectual, social and organizational hurdles, but aided by courageous individuals willing to make a stand. "Social entrepreneurs" had to conceptualize, fight for, and establish the concept as real and useful. First radiologists, then pediatricians, and finally everybody saw this concept as valuable and important. This case study illustrates the steps by which a "hidden event" becomes a recognized medical syndrome. The narrative shows how advocacy and information flow build on each other and thus combine to shape medical institutions. The information needed to justify special mechanisms for collection (such as protective services), come through the very creation of these same mechanisms. We are more willing to see something clearly once we feel we can do something about it.

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INTRODUCTION

Many years ago (1978-1979), I was lucky enough to spend time at the University of Edinburgh as a guest of its Science Studies Unit. While I was there, I audited a course taught by Professor John Mason called "Forensic Medicine for Lawyers." As a fan of "Sherlock Holmes" this sounded fascinating. With permission from Prof. Mason, I sat in on his course. During one lecture Mason mentioned the work of C. Henry Kempe, on child abuse. He discussed a program that Kempe had started to identify mothers who might have low bonding with their infants, putting the babies at risk for child abuse. In hospital he got nurses to identify these mothers with low bonding, and to them he offered a course about what they could and could not expect with their newborns. This course, I was told, reduced the amount of child abuse for the mothers who got it. I put this fact aside for the moment. But then, when I returned to the USA, I interviewed a number of American pioneers who had responded to early reports of child abuse. The interviewees included Dr. Kempe himself, whom I met while on another visiting appointment, at the University of Hawaii. As well there were several others who appear in the account below. The following memoir was written while in Honolulu in 1984. Since it contains some details of original research, I thought it might interest the readers of this journal.

CHILD ABUSE AS A SOCIAL PROBLEM

While child abuse was recognized as an important problem by Amboise Tardieu, a French forensic physician), in the 19th century, and also by various American reformers in the 19th century, the modern period (post WWII), begins with the persons I will discuss (There is also much material in history of foster children and adoption). My interest was awakened by the status of child abuse in the 1950's as a "hidden event," a phenomenon to which I have devoted much attention [1]. I think in fact that physical child abuse is a classic example of a hidden event. It demonstrates, as Sherlock Holmes would say, several interesting features.

First it is a valuable case study in the relationship between knowledge and action. It shows that social policy depends intimately on social perception, but also that social perception itself may depend on the growth of social intelligence organs as well as the spread of concepts. What we know may depend on organs for seeing. We did not know about the extent of child abuse until we developed social agencies for dealing with it.

Second, the battered child syndrome illustrates the dynamics of hidden events, as they pass through several stages. Anomalous events may be perceived or guessed at by a large number of social actors who exist, in relation to each other, in a state that Floyd Allport called "pluralistic ignorance [1]. The process by which these actors begin to communicate with each, and by which their uncorrelated experiences are transformed into social knowledge, is extremely important. On its dynamics may rest the protection of our society from various social, medical, and technological dangers whose appreciation requires the collation of widely dispersed and seemingly unrelated experiences [1].

Finally, the development of the BCS concept has elsewhere been ascribed to the group interests of medical radiologists [1]. This interpretation will not bear scrutiny, as this paper will show. However the failure of this narrow interpretation is instructive, as it illustrates some of the problems with simplistic ideas about groups, their "interests," and their actions.

THE HAZARDS OF HAPHAZARD INTELLIGENCE

One of the striking features of the sociology of anomalous events is the widespread belief on the part of professionals that nothing can really be hidden from them if it is important. Somehow, it is argued, people will notice the events, they will report them, and the reports will reach the proper authorities. "You certainly couldn't cover something like this up!" Yet, as case studies of anomalous events show,

1. People often fail to observe
2. When they do observe, they often fail to report and
3. Even when they report, their report may fall on deaf ears [1].

Ludwik Fleck, in his classic analysis of scientific innovation, noted these characteristics of emerging systems of medical thought:

:" What we are faced with here is not so much simple passivity or mistrust of new ideas as an active approach that can be divided into several stages. (1) a contradiction to the system appears unthinkable. (2) What does not fit into the system remains unseen. (3) Alternatively, if it is noticed, either it is kept secret or (4) laborious efforts are made to explain the exception in terms that do not contradict the system. (5) Despite the legitimate claims or contradictory views, one tends to see, describe, or even illustrate those circumstances which corroborate current views and thereby give them structure [1].

Useful collections of anomalous observations seldom emerge, then, as the by-product of routine processes. The "dynamic conservatism" that Donald Schon describes is usually sufficient to submerge them in the maze of ordinary perceptions [1].

Elizabeth Elmer, one of the true pioneers in this area, and a social worker, relates the following story. "I remember some of the tumultuous meetings in which, for example, a pediatrician would say, 'If I believed that the parent could abuse the child, I would leave pediatrics immediately!'" [1]. In another instance, she mentions a case in which "the newspapers carried the story of an infant with multiple injuries that the attending physician likened them to those that might be sustained through a all from a three-story building. The parents, in court on charges of inflicting the trauma, were acquitted because the jury believed their story that the baby had injured himself playing with a plastic rattle [1]. C. Henry Kempe, another pioneer, became Chief of Pediatrics at the University of Colorado School of Medicine in 1957. He was shocked by some of the diagnoses made to deflect recognition of child abuse.

"I was intellectually offended at first, before my better instincts took over, by the simply silly diagnoses being made by bright house staff in situations where nothing but child abuse could be the diagnosis. For example, I was shown in these first few years cases of "spontaneous subdural hematoma (a condition then described in neurosurgical texts; no more) in children who had thirved from birth and now, at six months, had these serious brain bleeds. Some of the children had other findings of abuse such as bruising. I was presented with cases of "non-specific bleeding disorders" with a family history of being "easily bruised." These children did not bruise in hospital! Their lab tests showed no bleeding problems whatever. Children were shown to me who were in coma and we showed them to be poisoned by barbiturates or vodka administered by somebody. We saw burns of palms of the hands that looked like cigarette burns, I saw burns that could come only from dunking a child's bottom in hot water with symmetrical burns, often, of the feet as well. I saw children whose tongue frenulum was torn from what I thought had to be bottle push, and thus trauma by someone. The common denominator was the denial of child abuse by these fine young doctors who simply could not imagine the facts of life [2].

One sees, then, what one is willing to see. But willingness to see is only partly conditioned by personal feelings. Each perception or observation fits into a system of social action, since what one sees has consequences for what one must do. Unless making a dramatic observation can lead to a useful consequence, the observation can only cause problems. To change the perception, then, one might well think about changing the system.

But how is the system to be changed? More specifically, who is to change it? How do some persons suddenly become "moral entrepreneurs," to use Howard Becker's term [3]. To become a moral entrepreneur requires three elements: 1) the perception that something is wrong; 2) the belief that something can be done about it; and 3) the courage to be the one who will do something about it.

Borrowing from the physiology of the nervous system, we can say that a "summation effect" occurs when two or more observations come to the attention of someone in a short enough span of time so each helps confirm the validity of the other. Seeing more than one event changes the anomalies from single "odd occurrences" to members of a class, instances of a *phenomenon*. What happened to cause the recognition of child abuse was precisely such summation effects [4]. Attention was drawn to child abuse cases because a set of physicians whose primary concern was diagnosis---pediatric radiologists---needed a unifying concept to tie together otherwise inexplicable symptoms. Once these symptoms are united by a concept, the concept could and did spread---although slowly at first---through out the medical system. Others could then begin to see and to prove to themselves that something indeed was taking place, something that would require action.

A NEW FORM OF SEEING: PEDIATRIC RADIOLOGY

It was the brilliant French legal-medicine expert Auguste-Ambroise Tardieu (1818-1879), who first published in 1860 a report of what would later be called the "battered child syndrome [5]. But rather than attracting attention, the report remained buried among Tardieu's other outstanding medical treatises. An ostensible reason for this neglect was that no one translated his observations into viable social machinery, and no social movement arose that could act as a viable carrier for his

concept [6]. And then, for whatever the reasons, perhaps the first publication in the 20th Century relating to what would become the "battered child "concept was a paper by John Caffey. M.D. a radiologist, in 1946. The paper was entitled "Multiple Fractures in the Long Bones of Infants Suffering from Subdural Hematoma." I give the title in full because it gives no hint at the emotionally loaded ideas that the text contained. Even the text probed them in a manner that was, to say the least, extremely cautious [7]. In this paper Caffey explained that fractures and blood blisters were linked, i.e. they were both caused by the same agent, physical trauma. As to the social matrix that gave rise to this trauma, Caffey considered several possibilities, including negligence, unnoticed accidents, and most significantly in the light of later knowledge, "intentional ill-treatment." He also made note of two matters that today would seem ominous: a lack of caretaker-supplied "history" for the injuries and 2) caretakers' lack of affection for the infants.

To understand what this paper meant, we have to know something about the man who wrote it. John Caffey was both a pediatrician and a radiologist, and his book ***Pediatric X-Ray Diagnosis***, first published in 1945, virtually established pediatric radiology as a medical specialty [8]. For Caffey it laid the foundations for an extremely influential career with many honors. Among them was recognition as "the dean of American Pediatric Radiologists" by the ***Annales de Radiiologie*** [9], In 1946 he was 51 years old, already an authority in the tiny but growing field of pediatric radiology, and was energetically developing the field through research, writing and teaching [10]. He was very much concerned with the status and legitimacy of pediatric radiology, but even more concerned that it be both accurate and useful to pediatricians [11]. He did not see child abuse as a means by which the professional stature of pediatric radiology would be advanced, but rather as one condition that it would be dishonest for radiologists and pediatricians to ignore [12].

Exactly when Caffey first became aware of the problem of beaten children is uncertain. In a paper written in 1957, he states that he had studied the problem for more than 20 years, which would put it before 1936 [13]. In a paper delivered in 1971, however, he states that "I remember that I had hoped for several years during the 1920's and early 1930's that we would find some unknown causal factor which would disprove the guilt of the parents [14]. This would place his recognition even earlier. In the latter paper, he states that "the essential elements of the syndrome, both clinical and radiographic, had become evident by 1938, when it was clear to us, that the radiographic changes were similar when the trauma was accidental or willful,, and when admitted and denied." But doubt, that great enemy of positive discovery, must have stayed his pen, for he published nothing more on this for eight years. Then, in 1944-45, his suspicions received unexpected reinforcement. In this period, a paper by Ingraham and Matson, and films from S.T. Snedecor on traumatic bone lesions after breach births convinced him of his hunches. For both the subdural hematomas and the signs of bone healing were shown to be caused by physical trauma [15].

What had generated this trauma? Caffey suspected the child's caretakers but the evidence was incomplete and confusing. In one case, a fracture of the radius of one of his infants developed while the child was in hospital [16]. Could the parents have caused this fracture? May be at that time, however, such events were difficult to believe. Nonetheless, Caffey began at this time to communicate his suspicions orally to colleagues, and especially to his two residents, Frederic Silverman and Bertram Girdany, both themselves later to become pioneers in their own right. Alerting his two residents to the possibility of parental abuse did more than his article, whose extreme caution was such that many radiologists failed to read between the lines, and could not understand what he was driving at [17]. Caffey's 1946 article was a reference point, but he did not bang the drum [18].

Even more curious is Caffey's failure to use his own ***Pediatric X-Ray Diagnosis*** as a means of informing radiologists to be on the lookout for abusive parents. Although "Trauma" as a cause of radiologic appearances is included in the 1956, 1961, and 1967 editions, the only mention of the causes of this trauma are the following lines:

"Occasionally infants and children are under the control of psychotics who go into sudden rages or alcoholics who cause severe traumatic lesions in their victims and later have no memory of the episode, or intentionally deny it [19]."

If, as Caffey stated in 1972 that by 1946 "the evidence of parental guilt became overwhelming and conclusive" then his failure to communicate it in the form of an entry in his own handbook is odd. Yet it was not until the 1972 edition of the handbook that an article on the "parent-infant trauma syndrome" appeared. The phrase "multiple unsuspected traumata", which Girdany states that Caffey used orally, does not occur even in the 1967 (5th), edition. One may conclude, I think, that Caffey still had his doubts until a comparatively late date.

We must be careful in evaluating Caffey's actions. Pediatric radiology was just finding its way. Caffey wanted to be sure he didn't blow things up, or unleash a lynch mob. And there is a world of difference between his time and our own. We know that widespread child abuse (and neglect), is a fact. We know what to look for to identify it, what kinds of parents are more at risk, from their actions, their words, and their experiences. All of this, in Caffey's time, was guesses. That he made such guesses, and got others to make them, was supremely important. But going from conjecture to action was a different matter. The evidence was equivocal, as it often is at the brink of a discovery. One has to push aside the opposing evidence (and "common sense"), and say, as Adm. Farragut did at Mobile Bay, "Damn the torpedoes! Full Ahead!" But the author of Pediatric X-Ray Diagnosis was a leader of a different kind, a builder rather than an adventurer.

Caffey's former resident and colleague Bertram Girdany told me that Caffey was afraid of unleashing a "lynch mob," and cited the large number of false positives today as a rationale for Caffey's caution. "There were numbers of these instances. What he wanted people to understand…was that the radiologist, and everyone else for that matter, can only say injury was done, but never by whom, nor with what intent [20]." Also other medical conditions, such as copper deficiency, can give the same appearance as child abuse, since they make the bones fragile. These are very important considerations; but on the other side, there were, even in the early papers, reports like the following:

"12 mos., colored, female. Child initially terrified of all hospital

personnel, later friendly. Fractures healed well in hospital. Police investigations pointed to mother who was again pregnant and quite disturbed but no action was taken. Was dead on arrival at another hospital [21]."

Thus protection of the parents had to be weighed against the protection of the children.

Was Caffey concerned about his reputation? I doubt that this factor entered in; after all, he did say orally what he would not print. I think that the threat of printed communication was that it would involve, as it inevitably had to, public outcry and scandal. Caffey was nurturing and protecting a very important enterprise, pediatric radiology. He may have felt that this enterprise could ill afford the sort of sensationalism that was sure to arise if he made a fuss. Caffey was a very forceful person, but he was also cautious and skeptical. Perhaps banging the drum did not suit his character [22].

Caffey's resident Frederic Silverman, however, was "willing to rush in where angels fear to tread." In September 1951 he found himself presenting a paper to the American Roentgen Ray Society, which convinced many, if not all radiologists, that, lack of "history" from the parents could not be trusted [23]. If x-rays showed signs of trauma, one needed to investigate further. Silverman's paper did not portray the parents as abusive, but rather negligent and careless. In some respects it was a clarification of Caffey's earlier paper. The important contribution is the phrase "unrecognized skeletal trauma" in the title, suggested that this might be a new medical entity.

Apparently Silverman's paper created a sensation among radiiologists. Even Caffey was impressed. He told John Holt, another radiologist, that "the person who first describes something is not nearly as important as the person who convinces the world [24]. Later, to his colleague Girdany, he expressed misgivings that Silverman had not indicated how much he learned about multiple unsuspected trauma from studying with Caffey [25]. He was to suffer an even more severe disappointment later in relation to Kempe's naming of the syndrome in 1961 and his JAMA paper in 1962.

Why did Silverman publish the 1951 paper? First, he had been finding, soon after he arrived in Cincinnati , the same kind of cases as those to which Caffey had earlier called his attention, during his residency at Babies Hospital in New York. Being somewhat of a detective at heart, Silverman interviewed many of the parents whose x-rays showed trauma. From many such interviews, he learned facts very different from those originally offered by the parents. Frustrated by the disbelief of pediatricians that the caretakers would have accidently or intentionally have injured their children, Silverman thought that these cases needed publicity. Questioned as to his motives thirty years later, he stated:

"People had to recognize it for two points. One, not to give the kids diseases they didn't have, bring them into hospital and do all sorts of expensive and potentially hazardous diagnostic tests to come up with no answer, but also to mobilize the social forces in the community to do something about this [26]."

Caffey and Silverman were far from the only ones concerned about and publishing on abused children in the 1950's. Other radiologists, prediatricians and specialists had become interested and presented their own contributions. One particularly important paper was published by Paul Wooley and William Evans, pediatrician and radiologist respecitively, and appeared in the ***Journal of the American Medical Association*** in 1955 [27]. This paper is a model of scientific research, and at first blush it is surprising that its impact was not greater. In a study of 25 infants whose provisional diagnosis was subdural hematoma, the authors separated the injured into three groups: 1) those whose injury was an isolated accident; 2) those whose parents allowed an "injury-prone environment" through negligence; and 3) those who had been intentionally injured. This was an important elaboration of a distinction implicit in the papers of Caffey and Silverman. The authors suggested criteria for differential diagnosis, and noted that emotional disturbances, not necessarily psychosis, was typical of parents who created an "injury-prone" environment' or intentionally harmed their children. They noted that "historical data are relative and depend on the intensity of efforts at elicitation," and that bone fragililty did not seem to be an operative factor. This paper posed some serious questions, but little was done at the time to answer them.

One issue the paper addressed was the "bone fragility" argument. Medical opinion during the debate was not unanimous that unsuspected multiple traumata were caused by the children's caretakers. A paper by Roy Astley in 1953 suggested that bone fragility might be an important factor [28]. At this time, as I indicated, neurosurgical texts still referred to "spontaneous subdural hematoma," an expression that quickly went out of currency. Disbelief was passive, but still very real.

THE REPORT RELEASE EFFECT

In an earlier paper, I referred to anomalies moving from a state of "uncorrelated observations" to one of controversy. In the 1950's "unsuspected trauma" had begun to accumulate in print and in privately kept data sets. While no common phrase had developed to describe the problem, awareness of it began to percolate through occupational groups concerned with child protection. One such group was social workers. In 1957 Elizabeth Elmer, a social worker at Children's Hospital in Pittsburgh, was told by a resident about an interesting case which seemed to involve legal issues that social workers sought to address. The case involved a seriously injured 5-month old baby, whose parents did not wish to discuss how the child had been injured and left quickly. Elmer went to the infant floor, thinking the general description she had been given would be sufficient to identify the child in question. To her surprise, the nurse brought out cards of six infants, all of whom had been admitted in the middle of the night, with injuries implicating family members.

"If I had thought in such terms at all, I would have considered the original case unique, but in a few minutes revealing talk with the nurse, abuse had become a class of events with ramifications far beyond one infant, one family [29]."

But how large was this class? One of the interesting questions is what the experts thought. For the most part, they had no idea! Certainly no estimates appeared in their paper. Yet this is a very important matter, for what ought to be done, depended on what there was to do.

If I were to pick a metaphor, I would say that the social system at this point was in a half-aware, the kind of situation in which one has "sort of" realized something, but is not willing to make this something fully conscious. Unlike the situation regarding other anomalous events I have studied, there was little controversy, apart from the "bone fragility" argument. What existed instead was a gradually dawning awareness that something was deeply wrong. I imagine this is a state many of us get into when we suspect a friend of wrong-doing, but don't really want to confront the matter head-on. But this kind of half-awareness has some interesting social consequences.

One of them is that while some persons may have a more less clear, conscious appreciation of what is happening, others may suffer from an incomplete realization. That is, they suspected that an unexplained experience of their own has meaning, but can't be sure just what it means—until someone else relates a similar experience of their own. Lecturing about and publishing on abuse cases caused what I have called the "report release effect [30]."[1] This occurs when the first persons to describe publicly an anomaly become the target of reports released from other who have had similar experiences. Silverman and Wooley, for instance, both found that giving talks on child abuse to physicians later led to these same physicians button-holing them during the cocktail hour or the lobby of the hotel after their talk.

"The first three or four talks I went to out in the boondocks to talk over this---I remember one trip to San Antonio….Usually everybody gathered, and then some of the older, experienced pediatricians would sneak up to you at cocktail hour: 'You know I've seen a number of these and I wondered about it. There's just nothing written on it [31].'

Some reports (and x-ray films), came in by mail, from physicians who needed help in diagnosis, or simply moral support for opinions with which they felt uncomfortable. Just as the individuals relieved doubts and concerns by relating such observations, the experts became fortified in their belief that they were on to something. Their data bases increased [32]. Social consciousness of child abuse thus began to grow through the exchange process between generators of ideas and individuals who had experiences. Through this process, pluralistic ignorance was gradually replaced by a new state of social awareness. Child abuse was changing character from a medical diagnosis problem to a problem in social control.

Yet whose responsibility was this problem to be? The physician, especially the radiologist, could diagnose it, but treating it required non-medical intervention. The family and the community beyond the clinic were foreign territory to most physicians. Previous use of the police to protect the children had generally proven ineffective in the few cases in which it was tried. It is not surprising, then, that those hospitals successful in dealing with the problem were those that developed child-abuse teams. In Pittsburgh, Los Angeles, and Denver such teams were developing in the late 1950's. It was from the members of these teams that the breakthrough would come.

In the late fifties, then, the medical profession began to realize tat it was ineffective in dealing with child abuse. Radiologists could identify past child abuse from x-rays; some pediatricians had learned to recognize other social symptoms, such as parents' lack of interest in the child. But neither could reach beyond the hospital and examine the child's home circumstances, nor could they do much to protect the child even when they were certain abuse was taking place. Many pediatricians had difficulty seeing themselves as the advocate of the children against the parents, even when the child had been savagely beaten [33]. Thus it was indispensable for the physicians to team up with social workers, who coul examine the home situations and recommend action regarding the disposition of the child. Pfohl's argument that child abuse was an opportunity for advancing the interests of a coalition of medical specialties ignores the repeated failures and frustrations experienced by physicians who tried to protect the child without team effort. Notifying the police often proved useless unless the child was dead or social workers could recommend removal from the home. The troubled conscience of many of the pediatricians who sent children home, only to have them returned with more injuries or even dead, were even more powerful, I suspect, than any thought of how pediatrics might be advanced by creating a national "child abuse" problem. To cope with the problem, there were few good alternatives to collaboration [34]. Henry Kempe:

"I was angry that we ourselves (pediatricians) were not doing the job and also that we were not helping the social work departments who were supposed to be doing it, had been doing it for a handred years, being protective services right along. Doctors didn't help them…There were social workers over here… Social workers and doctors didn't talk until the mid-50's [35]."

The team, then, became the necessary tool in coping with the societal and familial complications of recognized child abuse cases. Teams were developed at the Children's Hospital of Pittsburgh, where Elizabeth Elmer (social work), collaborated with Bertram Girdany (radiology); at Children's Hospital of Los Angeles, under director of Services Helen Boardman; and the University of Colorado, under C. Henry Kempe. These teams became the prototypes for similar teams elsewhere, and from them came the impetus for the intellectual, legal, and social changes that would greatly enlarge society's protection of abused and neglected children.

Before going on to the momentous changes that these groups would initiate, I would like to reflect for a moment on the effect that team formation had on the perception of child abuse by those involved with it. Previous to the existence of such teams, there was very little that could be done about abuse by physicians, so apart from intellectual honesty (Cf. Kempe's comment earlier), there was little point in recognizing it. I think it is appropriate to speculate that physicians were more willing to see child abuse, to talk about it, when they could do something about it. For when social workers were part of the team, physicians could turn the problem of legal sanctions over to them. Social workers, in turn, could be taken on the wards to see the seriousness of the injuries that parents and other caretakers inflicted, thereby becoming aware of the gravity of the problem, which could not be solved simply by admonishing the parents. The development of the team, and later the professional education of protective service personnel outside the hospital, changed the parameters of the social intelligence system by linking action with perception in

new ways. Child abuse could be better processed when there was social organization to deal with it.

But how was the problem to be brought before a wider public? How were other physicians, social workers, law enforcement, etc. to become aware of it? Elizabeth Elmer tried to alert social workers through an article published in 1960, in which she discussed what she described as a "rare hospital phenomenon that cries dramatically for attention [36]." This got some attention. C. Henry Kempe and Henry K. Silver, both pediatricians, sent a paper into the Society for Pediatric Research in 1959, but it was only "read by title," and not chosen for delivery, for reasons unknown [37]. It may have been considered too far out, or of little importance. The Pittsburgh group also attempted to solve the problem through the American Academy of Pediatrics, but apparently without success [38]. The breakthrough came almost by accident, when Kempe, who was on the national program committee of the American Academy of Pediatrics, became program Chairman in 1960. One prerogative of the Chairman is having the freedom to plan one symposium on a key morning, and so Kempe suggested "child abuse" as the topic. The entire planning group, Kempe remembered, suggested that this title would turn people off, and Kempe coined the term "battered child syndrome [39]." And so it became. On Tuesday morning, October 3, 1961, in the grand ballroom of the Palmer House in Chicago, the "Symposium—The Battered Child Syndrome" took place. It was chaired by Frederic Silverman, and included Kempe, Elizabeth Elmer, John F. Horty (Pittsburgh—Director of the Health Law Center), Brandt Steele (University of Colorado--Psychiatry), and the Honorable Benjamin S. Schwartz (Cincinnati—Judge of the Juvenile Court) [40]. Cincinnati, Pittsburgh, and Denver had all been pioneers in medical and legal action on child abuse and thus were well represented.

Kempe estimated that over a thousand people heard the panel discussion in that room. All day afterward, Kempe and the other panelists were besieged by pediatricians who came to them for advice and catharsis:

"Many were moved and came to tell us all day, long after the morning meetings about their cases and how troubled they had been, mail poured in and the Newsletter of that Academy meeting reported the Symposium well before it was published [41].

The meeting had posed the question in a form that made the issue difficult to ignore. Pediatricians had been put on notice, and it was obvious that the problems involved social workers, juvenile authorities, and many other professional groups.

At this point one should say a few words about the leader of this small revolution of thought. C. Henry Kempe had already accomplished great things by 1961, and he was going to go on to accomplish many more [42]. At age 34 he had been invited to become chairman of the Department of Pediatrics at the Colorado School of Medicine, a position to which he brought unusual talent for administration. Within a few years he had brought dramatic changes, getting grants, increasing staff, and generally making the hospital a better place for children to stay. He encouraged mothers and fathers to stay with their children in hospital, to bring or cook ethnic food for their children and themselves, and he made early use of foster grandmothers and grandfathers. Meanwhile he was playing a major role in the eradication of smallpox in India, a tax which he described as his lifework. His intellectual contributions to pediatrics would include co-authorship of the Handbook of Pediatrics and of the many editions of Current Pediatric Diagnosis and Treatment. Like John Caffey, he was an intellectual leader.

The University of Colorado Medical School at this time was an important intellectual center for the West. Its faculty frequently acted as consultants for physicians in the sparsely populated neighboring states that did not have their own medical schools. These physicians turned to the University of Colorado as a source for expertise and advice [43]. These outreach activities, along with his work in public health, may well have taught Kempe the value of public arousal in coping with medical problems in society.

The panel in Chicago was followed eight months later by Kempe's publication (with co-authors) of an article entitled "The Battered Child Syndrome" in the ***Journal of the American Medical Association [44]***. The enormous impact this article had on medicine and the social welfare needs explanation, since much that was in it was not new. It was not even the first article on child abuse in JAMA (Cf. the earlier work by Wooley and Evans 1955). Part of its impact was created by the intellectual context—the high level of awareness created by the Chicago panel and the previous articles on radiographic, social, and legal aspects of child abuse. The JAMA article was important in bringing all these ideas together, but there are two other key features.

The first was the inclusion of a national survey carried out at Kempe's suggestion by William Droegenmuller, then a third-year medical student. The survey, which Kempe had used to assess the prevalence of the problem, polled hospitals and district attorneys. It specifically asked each respondent how many child-abuse cases had come to their attention over the last year. Some 748 cases were reported through these questionnaires, only the tip of the iceberg but still displaying a problem that was nationwide. The survey, though, was more than simply an information-gathering device. It was a powerful form of rhetoric that could back up the authors' assertion that the problem was not only serious, but widespread. This meant that henceforth the battered child syndrome was not merely a medical problem, but a social and legal one as well.

The second feature was the name. Caffey had orally used the term "multiple unsuspected tramata" and in Silverman's unpublished paper there is a table labeled "unsuspected trauma syndrome." Neither of these terms, however, had been used in the published literature nor was in common use. Kempe's short, graphic term made the medical concept easily accessible understandable to physicians and social welfare professionals alike, to say nothing of the general public. We have already described how the term was coined. But with the publication of the JAMA article the mass media began to discuss the issue. Between 1962 and 1965 articles on child abuse appeared in ***Time, Newsweek, Life, Good Housekeeping,*** and the ***Saturday Evening Post***; during the previous decade, virtually no mention of the problem appeared in these publications [45]. It is more difficult to gauge the reactions of the newspapers. The ***New York Times*** only gradually increased its coverage during the 1960's, but by the 1970's dozens of cases might be printed in a single

year, whereas one or two cases a year had been typical of the 1950's. Television, reaching a broader audience, dramatized the problem, most notably through one episode of "Ben Casey." This single program probably had more impact on the general public than anything in print, but it was only one of several programs that emphasized abuse. Clearly, abuse had been put on the national agenda. Shortly, the forty-eight states would all pass laws mandating child abuse reporting by physicians and others [46].

COMMENTARY

Battered children are an example of a "hidden event," a term I coined for researching "things not generally known [47]. These have included, over the years, the meteorite controversy, UFOs, sea-serpents, and ball lightning. I think the best definition of a hidden event was given by a researcher of "near-death experiences," Raymond Moody, who remarked, in a passage I can no longer trace, that such events are "widely distributed but very well hidden [48]. But if they are hidden, how can society deal with them? I believe the answer in part is the history that I have traced here, relating how something hidden or only suspected, comes into the light of day. And just as one set of events is surfaced, other similar kinds of events can surface as well. Abused spouses, abused siblings, abused elders, abused pets and others come into view once we get used to the dynamics of "family secrets." Today, we are familiar with the dynamics of reporting via the many abuse scandals outed by "#MeToo" and its sequels. Although he was well aware of it early on, Henry Kempe himself held off on discussions of child sexual abuse until 18 years after he had written the original JAMA article [49]. Eventually Henry and his wife Ruth Kempe would write a book about it. ***The Common Secret: The Sexual Abuse of Children and Adolescents*** appeared in 1985.

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