Do Vaccines Cause Cot Deaths?

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Two studies by teams of epidemiologists headed by Marie R. Griffin represent perhaps the absolute worst I have encountered in many years of reading this literature (Marie R. Griffin, Wayne A. Ray, John R. Livengood, and William Schaffner, "Risk of Sudden Infant Death Syndrome after Immunization with the Diphtheria-Tetanus-Pertussis Vaccine." NEJM 319:10 [Sept. 8, 1988], 618-622. Marie R. Griffin, Wayne A. Ray, Edward A. Mortimer, Gerald M. Fenichel, and William Schaffner, "Risk of Seizures and Encephalopathy After Immunization with the Diphtheria-Tetanus-Pertussis Vaccine." JAMA 263:12 [March 23/30, 1990], 1641-1645). For those who are still interested I will attempt to show the reasons for my conclusion.

The first article, on "sudden infant death," was presumably written to refute the conclusion reached earlier by Alexander Walker et al.: "we found the SIDS mortality rate in the period zero to three days following DTP to be 7.3 times that in the period beginning 30 days after immunization...only a small proportion of SIDS cases in infants with birthweights greater than 2500 grams could be associated with DTP" ("Diphtheria-Tetanus-Pertussis Immunization and Sudden Infant Death Syndrome." American Journal of Public Health 77:8 [1987], 945-951).

So Walker et al. did find that the DPT shot was apparently causing "sudden infant death." And these deaths were not associated with just the first DPT shot, but with each succeeding shot.

Griffin et al. set out to refute this conclusion - not, indeed, by visiting these children and their parents but, in the new style, by leafing through computerized immunization records for children born between 1974 and 1984 in the state of Tennessee, "augmented through linkage of records with state vital statistics and Medicaid files."

The major problem with an epidemiologic study is always that of ensuring that the sample picked is representative of the larger group. It is logistically difficult to include all children, despite the availability of computerized records. Therefore, how the sample is selected is of paramount importance.

Griffin et al. found that, out of 280,000 children born in four Tennessee cities between 1974 and 1984, 180,000 had records in Public Health clinics.

Oddly enough, for over 41,000 of these 180,000 children no immunizations had ever been recorded. But instead of looking into SIDS incidence in this sizable group, Griffin et al. simply excluded them from the study.

Another 3000 children were excluded because their immunization records were confused.

This left 130,000 children in the cohort. And it is legitimate to ask if these 130,000 were truly representative of the 180,000 with public health service records. And, even more to the point, are they representative of the 280,000 children born in these same cities who did not have Public health clinic records?

Next they found that 204 children had died during days 29 to 365 of life. But they excluded 95 of the 204 because "a cause of death was listed [on the death certificate] that was clearly not SIDS." But what were these causes that were clearly not SIDS? Griffin et al. do not vouchsafe us that information, even though causes of death on death certificates are not necessarily reliable. At the very least, the chronological relationship between these deaths and a preceding vaccination should have been provided. Two of the 95 deaths had actually been coded SIDS by the attending physicians, but Griffin et al. knew better and changed the diagnoses: one baby had pneumonia (as if there is no connection between pneumonia and a vaccine reaction), while the other had heart disease (as if babies with congenital heart disease are never vaccinated).

By this time the SIDS sample has been so restricted as to be entirely unrepresentative of anything, and we are not surprised to find that Griffin et al. found the incidence of SIDS to be identical with the expected background incidence ("marginal rate of SIDS for that age group," as it is called).

As we might expect, no published references are given in support of the concept of "marginal rate of SIDS for that age group."

Griffin et al. dismiss the results of the Alexander Walker study above (7.3 times as many SIDS deaths in the first 3 days after vaccination as 30+ days after vaccination) as follows: "Since the first DTP immunization is usually given near the age when the incidence of SIDS peaks, the results of such case-series analyses are biased toward finding an apparent association between SIDS and DTP immunization." But Walker had found that SIDS was clustered not only around the first DPT shot, but around each succeeding shot. So Griffin et al. are hypothesizing that the background incidence of SIDS "peaks" every two months (!!).

It is amazing that such a study could be accepted by a reputable scientific journal. The reason was doubtless that the study was funded by the CDC and the FDA, and that two of the coauthors (Griffin and Ray) were at the time "Burroughs Wellcome Scholars in pharmacoepidemiology" (whatever that is). Burroughs-Wellcome is, of

course, a major producer of the pertussis vaccine. Have these people never heard of conflict of interest?

The second article by this same group of authors is equally typical of the kind of epidemiologic research conducted by those who work with government funding. Marie Griffin et al., "Risk of Seizures and Encephalopathy after Immunization with the Diphtheria-Tetanus-Pertussis Vaccine" is a retrospective analysis of 38,171 Tennessee children enrolled in Medicaid who received DPT immunizations during the first 3 years of life.

These constituted 29% of all children immunized in the public sector and 12% of all children born in the area during the study years, so the problem of "representativeness" of the sample is just as significant here as in the earlier study.

The "event" monitored was the "first nonneonatal seizure or episode of encephalopathy that resulted in a Medicaid reimbursement for a medical encounter, between the first DPT immunization" and the child's attainment of 36 months of age.

Griffin et al. found that 1187 study children had a potential "outcome of interest," meaning a seizure, but hold on, we can't just throw all these cases into the hopper, as it might lead us to the wrong (right!) conclusion. So Griffin et al. started whittling down the sample.

Records were "unavailable" for 359 (30%!!), and they were excluded! Just like that! And even though half of these, in the authors' estimation, would have met their criteria for inclusion! How about some good old shoe-leather epidemiology? Sorry, that's not how we do things these days.

Of the remaining 828 children 470 more (43%!!) were excluded as not meeting the "case definition." Ultimately, only 358 of the children remained in the study - 30% of the initial number!!

The 470 excluded cases consisted of: 34 seizures in the first 30 days of life ("neonatal"), 150 cases of chronic preexisting neurological abnormality without seizures, 18 "spells" "that were not clearly seizures," 82 diagnoses of "failure to thrive," 121 other nonneurological events, and 65 miscoded records. There is no way in the world that Griffin et al. could reliably conclude that these cases were unrelated to vaccination merely by examining Medicaid records and without interviewing the families. We must take these exclusions on faith, and such faith or confidence in the conclusions reached by government-funded epidemiologic surveys of vaccine damage is today in pretty short supply.

Griffin et al. conclude: "no child had the onset of encephalopathy, epilepsy, or other serious neurological disease in the first week following DPT immunization." But this is entirely disingenuous, since the "event" of interest has been defined as a neurological illness resulting in a medical encounter. The parents would have had to take the child rather quickly to the "medical encounter" to qualify under the terms of this study. If a parent left the baby in peace for a few days, just to see what was happening, or if the parents just did not notice a seizure in the baby (seizures are not very evident in small babies), this would not qualify as an "event" worth reporting.

Furthermore, the authors seem to assume that a seizure must occur within three days after vaccination to qualify as vaccination-related. There is no evidence for this anywhere in the vaccination literature. But it allows them to ignore a few unpleasant, and even potentially disastrous, outcomes, viz.: "Four children who were previously normal and had no prior seizures developed some neurological or developmental abnormality following the index seizure. In only one was the index event a febrile seizure, and this occurred more than 30 days following immunization. The other 3 occurred after acute symptomatic seizures. An additional 11 children who were previously normal developed epilepsy. One of these children had an initial afebrile seizure in the 8-14 days following immunization; the initial seizures for the other 10 were all in the period 30 or more days after immunization." Or: "Two children were hospitalized with encephalopathy between their first DTP immunization and 36 months of age. The 2 children with encephalopathy both had their onset of illness more than 2 weeks following DPT immunization, and neither had permanent sequelae. These 2 children will not be considered further." (??) Or, "There were six febrile seizures in the 0-3 days following immunization... Other events in the 0- to 3-day interval following DTP immunization included one afebrile seizure, zero symptomatic seizures, and six potential seizures, with no evidence for an increased rate of occurrence compared with the control period of 30 or more days following DPT immunization."

Amazingly, the authors think that seizures or other neurological events occurring more than 30 days after a vaccination are unrelated to the vaccination and part of the "background incidence." Hence the period commencing 30 days after vaccination is apparently used as a "control period," allowing the authors to conclude that the incidence of afebrile seizures in the 3 days following vaccination was no greater than in the "control period."

They do find, however, that the incidence of febrile seizures (generally thought to be less serious than the afebrile ones) is 50% higher in the period 0-3 days after vaccination than in the period 30+ days following vaccination.

The inherent difficulty of making sense of this article is due in part to the authors' tendency to contradict themselves from one paragraph to the next. For instance, after stating that afebrile seizures are 50% more common in the period 0-3 days post vaccination, they then say: "Indeed, there was no significant increase in febrile, afebrile, or acute symptomatic seizures in the early post-immunization period, compared with the control period of 30 or more days following DTP immunization."

In sum, this article eliminates 70% of the cases which initially presented, without giving any justification for such elimination. The authors then excuse the neurologic illnesses and disabilities which occurred on the ground that they are part of a background incidence (whose existence and magnitude in an unvaccinated population has never been demonstrated). And this article appeared in the "peer-reviewed" Journal of the American Medical Association!

These kinds of articles bring the Public Health Service, the CDC, the FDA, the "peer-reviewed" journals, and the rest of the medical-industrial-government complex into disrepute. Physicians can swallow this garbage if they want, since they make their living from it, but parents who expect at least elementary honesty from those who call themselves "scientists," and whose children are being maimed and crippled by the very vaccines which are proclaimed innocuous by authors such as Griffin et al. are already taking steps to put this invalid out of its misery.

The relations between the public and the vaccine establishment are surely going to get a lot worse before they start getting any better.