Baby Doe Rules Have Been Interpreted and Applied by an Appellate Court
Frank Clark
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Misconceptions Concerning Gastroesophageal Reflux in Children

To the Editor.—

After reading the article “Yield of Diagnostic Testing in Infants Who Have Had an Apparent Life-Threatening Event” by Brand et al.,1 we were dismayed because of the perpetuation of a myth by well-meaning but misinformed pediatricians. The authors state that a subset of tests that led to the identification of all occult causes of an apparent life-threatening event would include screening for gastroesophageal reflux (GER). To this point we agree. However, their basis for this statement comes from the evaluation of 68 children who had upper gastrointestinal (UGI) series; 36 of the 37 children in whom findings contributed to the final diagnosis of an apparent life-threatening event had GER.

On a daily basis we are deluged with requests in our radiology department to perform UGIs to “rule out GER.” In fact, the purpose of the UGI is usually to exclude upper intestinal obstruction. The finding of GER is happenstance. Cleveland et al2 and Seibert et al3 demonstrated that GER is present in a large percentage of pediatric patients who are studied for any reason and in many children whose symptoms would not suggest its presence. If we assume (based on prior studies) that the sensitivity for the UGI is 85% and the specificity is 25%, then the predictive value of the UGI is usually to exclude upper intestinal obstruction. The finding of GER is happenstance. Cleveland et al2 and Seibert et al3 demonstrated that GER is present in a large percentage of pediatric patients who are studied for any reason and in many children whose symptoms would not suggest its presence. If we assume (based on prior studies) that the sensitivity for the UGI is 85% and the specificity is 25%, then the predictive value of the positive result is ~54%, and the predictive value of the negative result is ~65%.4,5 As Leonidas astutely pointed out, “we may as well toss a coin.”4 If we examine the UGI series more closely, we can explain the poor predictive values. The infant is placed in a recumbent position (gastroesophageal junction is “under water”), is frequently strapped to an immobilization device, and is sometimes irritable (which may increase gastric pressure). The infant is then administered a dense liquid barium, usually by mouth but sometimes through a nasogastric tube (if they are uncooperative), and then turned into a variety of unphysiologic positions to demonstrate anatomy. The experience does not simulate the daily feeding experience. Moreover, the technique for performing and interpreting the examination is variable (eg, retained contrast in the esophagus from swallowing may be mistaken for GER). One must also bear in mind that up to two thirds of normal infants (<4 months old) regurgitate daily, and this finding may be of little significance.6

One might conclude from Fig 1B of the article that the UGI ranked highest in contributing to establishing the diagnosis in patients with a noncontributory history. We would look at this figure with great skepticism. As advocates for children we must be cognizant of risks with any radiographic study that makes use of ionizing radiation. The UGI potentially represents a relatively high (when compared to chest radiography) radiation exposure, particularly to vital organs like breast, liver, and bone marrow. The most effective way to reduce exposure in the population is to not do unnecessary examinations.

Although the UGI is not ideal for identification of GER, gastroesophageal scintigraphy using Tc99m sulfur colloid and the 24-hour pH probe are excellent tests. It is beyond the scope of this letter to discuss advantages and disadvantages of both tests, but suffice it to say that if one is considering a screening test for GER, the UGI is not an appropriate procedure.

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In Reply.—

Figure 1 of our article provides information about the relative yields of different diagnostic tests used in the evaluation of infants after an apparent life-threatening event (ALTE). In discussing the figure, we emphasize that the rank order of tests in the figure does not imply a simple formula for deciding which tests to order (see page 890). To illustrate this point, we later refer to the problem of gastroesophageal reflux, noting that many patients have reflux that does not precipitate an ALTE, and that the presence of reflux in a patient who has had an ALTE does not prove a cause-effect relationship (see page 892). As Drs Bisset and Frush note, this uncertainty surrounding the diagnosis of reflux in ALTE patients may be compounded when an upper gastrointestinal series forms the basis for the diagnosis. We thank them for reminding readers that this is not the best test for detecting gastroesophageal reflux, for explaining in exquisite detail why this is so, and for encouraging physicians to consider other tests for reflux when evaluating an ALTE.

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Baby Doe Rules Have Been Interpreted and Applied by an Appellate Court

To the Editor.—

I applaud the effort of Dr Kopelman1 to call attention to the disparities inherent in applying the Baby Doe rules and her courage in calling for the American Academy of Pediatrics to withdraw its apparent backing for them. I particularly appreciate her support for the continued application of the best-interests standard as being appropriate. However, I would like to correct one small misstatement.

In Appendix 1 Dr Kopelman states that the current Baby Doe rules “are untested by the courts” and then provides the text of 45 CFR §1340.15 (b)(2). This section first mandates indicated medical treatment and then provides 3 narrow exceptions to providing this treatment; it is these 3 exceptions that we generally refer to as the Baby Doe rules. Although it is true that the exceptions themselves have never been the direct target of a challenge, this section was
In Reply—

I thank Dr Clark for supporting my view1 that the American Academy of Pediatrics (AAP) should withdraw its apparent support of the “Baby Doe” rules.2 These regulations had to be adopted by states as a precondition of federal Child Abuse Protection and Treatments Act funding.3 I especially appreciate Dr Clark bringing into the discussion the Wisconsin Appellate Court’s interpretation and application of these rules in Montalvo v Borkovec.4 Even if this decision does not directly affect the criteria for withholding or withdrawing maximal treatments for infants as directly as the Supreme Court did in the Bowen decision,5 it offers an interpretation of them by an appellate court, showing unambiguously that these federal funding regulations do not allow the sort of discretion needed for individualized and compassionate decision-making for infants advocated by the AAP.6,7 Wisconsin’s Appellate Court understood these regulations as requiring maximal treatment that can only be withdrawn when an infant fails to meet the criteria. She carefully noted that this approach denied treatment to some who might otherwise survive. The advancement of technology over time has shown that this approach is not feasible. She labeled her favored approach the “individualized prognostic strategy.” As Dr Kopelman notes, it is this approach that has been adopted in guidelines published by the American Academy of Pediatrics. Singh and associates8 recently documented this practice in Chicago, Illinois. Their article also provides meaningful definitions that, for the first time, allow us to differentiate between cases in which withdrawal was undertaken because death was imminent and those cases in which quality-of-life concerns played a part in the decision.

I share Dr Kopelman’s concern that the Baby Doe rules do not allow us to discuss the competency of our leadership believed and have demonstrated that they have been strictly interpreted against our current practice of supporting parents who make a reasonable medical decision in the best interests of their infant. For those who might consider the Montalvo decision an aberration, I would suggest reading Robertson’s recent article,5 in which he promotes the restrictive interpretation of the Baby Doe rules rejected by Dr Kopelman; he asserts that all infants must receive full and equal medical treatment that can only be withdrawn when an infant fails to demonstrate any cognitive ability. His words are a direct assault on the best-interests standard that the majority of us use in clinical practice.

Our society seems poised to attempt a tectonic shift in public policy. Our current policy allows the widest latitude for decision-making and respect for values between patient, parent, and caregiver. As a matter of public policy under the standard proposed by Robertson and, as noted by Dr Kopelman, supported by the Baby Doe rules, there will be no choice but to continue maximal medical treatment in any infant with the slightest degree of conscious life. I would join Dr Kopelman in urging our professional leadership to reexamine this issue.

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and suffering or to prolong a noncomatose, minimally conscious life, many people and policy makers believe that there are sometimes worse things than dying.5,7,17–20 This attitude is reflected in the first priority of palliative care, namely, the relief of pain and suffering.17,19 If we agree that it is wrong to do to others what we would not want for ourselves and that we would not want a Baby Doe policy for ourselves (maximal treatment unless we were dying or comatose), then we should not adopt the Baby Doe policy for infants <1 year of age.

The best-interests standard is superior to the Baby Doe rules as a guidance principle because it uses the same rule for all persons lacking decision-making capacity and it permits, within socially sanctioned limits, the sort of compassionate and individualized decision-making widely recommended by policy makers,17–20 including by the AAP.6,7

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Is Homeostasis Model Assessment Better Than the Quantitative Insulin-Sensitivity Check Index and Fasting Glucose/Insulin Ratio?

To the Editor—

We read with great interest the article of Keskin and colleagues,1 who tried to establish whether fasting methods to measure insulin resistance (homeostasis model assessment [HOMA], fasting glucose/insulin ratio, quantitative insulin-sensitivity check index) are reliable and which index could be used more easily in a clinical setting. Insulin resistance is an emerging topic, because it plays a central role in developing type 2 diabetes and the metabolic syndrome, particularly in an overweight adolescent population such as the one studied by the authors.2

The markers proposed by the authors were validated recently with the euglycemic clamp technique, which represents the gold standard for the determination of insulin resistance, and a good correlation between them has been found.3,4 Moreover, the reliability of the composite whole-body insulin-sensitivity index and the insulin-sensitivity index, both deriving from the oral glucose-tolerance test, has also been validated in children and adolescents, and a strong correlation between these 2 indices and the euglycemic clamp has been found.4 On the contrary, no data on the accuracy of the sum of insulin levels during the oral glucose-tolerance test as an index of insulin resistance are available. In previous studies this index has only been used to subdivide obese patients as normoinsulinemic or hyperinsulinemic.5,6,7 In the study by Keskin et al, obese adolescents are classified as insulin resistant versus non–insulin resistant on the basis of a cutoff of this index of 300 μU/L. This is more than questionable and might have influenced the main results of the study. In fact, given the nonvalidation of this index, it is difficult to conclude that HOMA is more reliable than the fasting glucose/insulin ratio and quantitative insulin-sensitivity check index in diagnosing insulin resistance, and it is more than surprising to determine an appropriate cutoff point for HOMA for the diagnosis of insulin resistance in adolescents.

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In Reply.—

Insulin resistance is a state in which normal concentrations of insulin produce a subnormal biological response. It is difficult to distinguish between relative effects of insulin resistance and hyperinsulinemia. The degree of hyperinsulinemia and the presence of accompanying insulin resistance may form the basis for some of these conflicting effects. A variety of methods have been developed to detect the pres-
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