Volume 49, No. 4
October 1978

THE JOURNAL OF Anesthesiology
THE AMERICAN SOCIETY OF ANESTHESIOLOGISTS, INC.

Editorial Views

Anesthesiology
49:233–236, 1978

The ASA Classification of Physical Status–A Recapitulation

IN THIS ISSUE, Owens *et al.*[1] report the inconsistency in grading Physical Status by the ASA Classification system devised in 1941. The tone of their report reflects dismay but not surprise. They expected as much. They conclude that the system "is useful but suffers from a lack of scientific precision." As I view their data, I am neither dismayed nor surprised. From replies of 235 anesthesiologists "the mean number of patients rated consistently was 5.9" of a possible 10—a result beyond my expectations. At issue then is expectations—what the classification system is supposed to do and not do. Progress requires periodic repetition to renew what is forgotten by the sliding scale of memory. So let's recapitulate and see how we got where we are.

Doctors Saklad, Rovenstine, and Taylor, as an ASA Committee, devised the original ASA Physical Status Classification. In the May 1941 issue of the Journal, Saklad[2] described the system in a paper entitled "Grading of Patients for Surgical Procedures." With exceptional foresight, his committee anticipated the problems that led to the investigation by Owens *et al.* and proposed a system that in all its essential elements is identical to the one in use 37 years later—a remarkable achievement.

In his introduction, Saklad emphasizes that this system is not an estimate of "operative risk," that it simply describes the patient's preoperative condition, that its purpose is to facilitate the "tabulation of statistical data in anesthesia," and more particularly that "it will be possible to correlate the relationship between result, the operative procedure and the patient's preoperative condition." This, then, was a contribution of taxonomy, a classification that would improve communication so that I could compare my patient population with yours for whatever purpose and we would each know whereof the other speaks. Grading of heart disease by the American Heart Association Classification and the grading of cancer in stages had the same purpose. Such taxonomic contributions have been of great importance for evaluating and recommending therapy. In the case of Physical Status, I believe the ultimate objective was the comparison of results of specific anesthetic techniques and agents.

The original classification contained 6 groups. The definitions of the first four contain the significant phrase "systemic disturbance," which was graded none, definite, severe, and extreme. Five to ten examples for each were given. Classes 5 and 6 included emergency operations only and were simply the first four classes divided into two groups. Somewhere in the transactions of the ASA House of Delegates, Class 7 was added to include a moribund patient expected to die in 24 hours with or without operation. Finally, Saklad writes with prescience, "It may be difficult, at first, for the anesthetist to classify patients with reference to their physical state alone. Subconsciously, he is apt to allow his knowledge of the contemplated surgical procedure to influence him in his grading of patients. With care, diligence, and attention to detail, he very soon will limit himself to the consideration of the patient's condition in his classification."

The system proposed by this Committee remains in its essential format in use today. From its inception the system was known as the ASA Classification of Physical Status, and unfortunately credit for its design by Saklad, Rovenstine, and Taylor became lost to memory. More unfortunately, Owens *et al.* write "In 1961, Dripps *et al.*[2] [*sic*] proposed a classification consisting of five categories." The reference cited does nothing of the kind. In a study titled "The Role of Anesthesia in Surgical Mortality," Dripps *et al.*[3] used the ASA Classification in a paraphrased form. They eliminated Classes 5 and 6 by simply placing an E for

TABLE 1. Relation between Preoperative Physical Status Class and Surgical Mortality Rates

	Surgical Mortality Rate (Per Cent)	
	Vacanti *et al.*[6] 68,388 Patients	Marx *et al.*[7] 34,145 Patients
Physical status		
PS 1	0.08	0.06
PS 2	0.27	0.47
PS 3	1.8	4.4
PS 4	7.8	23.5
PS 5	9.4	50.8

Except for the difference between PS 4 and PS 5 in the series of Vacanti *et al.*,[6] the surgical mortality rate at least doubles as Physical Status increases from 1 to 5. The absolute mortality rates for PS 3, 4, and 5 are, however, vastly different in the two groups of patients. In part this can be attributed to the now well-known institutional differences in surgical death rates.[8] Another part, however, is the result of the inconsistency in grading Physical Status described by Owens *et al.*[1] In the series of Vancanti *et al.*,[6] only 9.4 per cent of patients classified PS 5 (moribund patients not expected to survive 24 hours with or without operation) died. Obviously they predicted poorly, and many of these patients should have been classified PS 3 or 4. Marx *et al.*[7] were more astute in their estimates, since the mortality rate in their PS 5 group was 51 per cent. Differences in patient populations may account for their different perceptions of severity of "sickness." The population studied by Vancanti *et al.*[6] was drawn from 11 U. S. Naval Hospitals from 1964 to 1966, with 74 per cent of their patients graded PS 1. The patients studied by Marx *et al.*[7] were drawn from a large municipal general hospital during 1965 to 1969, and only 54 per cent were graded PS 1. The distribution of "sickness" was highly skewed to PS 1 in the Naval Hospital Group.

emergency after the other five classes. For none, definite, severe and extreme systemic disturbances, they substituted normal healthy, mild, severe, and incapacitating systemic disease. They gave no examples. These revisions, which were editorial and not conceptual, were adopted by the ASA in 1962.[4] From public records, we do not know who proposed them. A further indignity occurred when the lead article of the October 20, 1977, issue of the *New England Journal of Medicine*, on the subject of cardiac risk in noncardiac surgical procedures, by Goldman *et al.*,[5] began with "The most widely used technique for the preoperative assessment of surgical risk, the Dripps–American Surgical Association, ..." In subsequent Correspondence (February 9, 1978), Goldman confesses to failing to correct the copyeditor's transposition of ASA to American Surgical Association, but is intransigent in his use of "Dripps–ASA Classification." A letter by Felts and Owens in the same issue continues to refer to "The original version proposed by Dripps." They agree with Goldman that the classification system is vague and subjective. Even if they choose to bemoan its inadequacy, they should properly credit the innovators, Saklad, Rovenstine and Taylor.

Again I return to expectations. The proposers of this simple classification system intended only to improve communication and hoped to compare results (now known as outcomes). They clearly stated this classification was not an estimate of "operative risk." Despite their denial, through use during more than three decades, this vague and subjective classification inadvertently developed into an excellent expression of "operative risk," exactly that which was not intended. When examined as a predictor, estimates of Physical Status correlate well with overall surgical mortality (Table 1). A major reason for its success is that Physical Status is the only expression of the overall preoperative condition of a patient that has been consistently recorded *before* operation for a large number of patients (92 per cent of respondents used it routinely[1]). By definition, any expression of risk must have predictive value. To test any predictor, one must estimate *before* the event; retrospective review can only suggest potential predictors. No other predictors, other than age, sex, and disease, are routinely available before operation for testing outcomes in large numbers of patients, and these would provide a basis for trivariate testing, at best. Physical Status provides a global index whose components are not specifically defined but are clearly multivariate. From the surprising correlation of Physical Status with overall surgical mortality, one must conclude the anesthesiologist, using a subjective and intuitive global index, can distinguish five grades of "sickness" related to overall surgical mortality. And the adage "the sicker the patient the more likely he is to die" is now documented.

Although mortality from anesthesia is included in overall surgical mortality, the correlation between Physical Status and anesthetic mortality is not nearly as good. We have discussed in detail the lower correlation and the many reasons why better correlation cannot be expected.[9] Despite this, Physical Status is about the only characteristic that correlates at all with anesthetic mortality. As an example, the most recently published report on postanesthetic deaths[7] identifies 47 deaths related to anesthesia, and only four of the patients who died are classed Physical Status 1 and 2. This positive correlation poses a crucial enigma. Since Physical Status concerns only preoperative "sickness" and by definition does not include risks related to anesthesia and operation, why should any correlation exist at all? Certainly Physical Status does not estimate the risk of malignant hyperthermia, inability to intubate the trachea because of deformity, inadvertent esophageal intubation, hypoxia by machine malfunction, the risk of uncontrolled hemorrhage, mismatched transfusions, or the performance of more complicated or radical operations than planned. About the only mechanism of anesthetic death that could correlate with Physical Status is "overdosage." In no review of purported anesthetic deaths is overdosage the leading cause of mortality; it is not even second. To explain the persistence of this correlation in studies of anesthetic mortality, only two postulates are possible: 1) anesthetic risk factors have been included by the myriad of anesthesiologists who grade Physical Status, or 2) those who report on anesthetic mortality are still unable to define consistently an anesthetic death as distinguished from death resulting from a patient's disease and his operation. Based on probabilities and bias, I opt for the second explanation.

Having bravely defended the status quo of the ASA Classification, I must now concede that Owens *et al.*[1] have some legitimate complaints. Their complaint about inconsistency is legitimate; their complaint about "lack of scientific precision" is not. Except for Saklad's original presentation, in which examples of each class were given, the ASA never further described by example or category what types of patient belonged in each group. In the widely used textbook of Dripps, Eckenhoff, and Vandam,[10] the ASA definition of classes is not even presented. They further paraphrase their earlier paraphrased definitions and give examples as they believe they should be classified. Not everyone reads this text. Twenty-five per cent of the respondents to Owens *et al.*[1] had not read a definition of Physical Status in more than two years. Thus, my lack of surprise or dismay in the outcome of their study. Nevertheless, we owe them thanks for bringing this situation to our attention.

As a global index of the preoperative state, Physical Status cannot have more "scientific precision" than the validation presented in table 1. What we can do is decrease the variation in grading Physical Status, which is also illustrated in table 1. This can be achieved by convention and does not require a study of outcomes. Owens *et al.*[1] point to four characteristics that led to wide variation in grading: age, anemia, previous myocardial infarction, and obesity. If their study were repeated using perhaps 20 additional examples, other areas of uncertainty in grading might be identified. By an agreed-upon convention, this uncertainty can be eliminated and variation decreased. In this regard it is unfortunate that Owen *et al.*[1] did not present the actual frequency of selection of each Physical Status Class for each of the ten patient examples given. Had they done so, the magnitude of uncertainty would have been better appreciated. For example, the issue with Patient 7, according to the Discussion, was whether a hemoglobin of 9.5 g/100 ml was a mild systemic disease (PS 2) or normal and healthy (PS 1). Looking at outcomes (table 1), the issue in numbers of patients is negligible. It is the difference between PS 1 and 2 alone or combined and either 3, 4, or 5 that significantly alters risk in any absolute sense. By contrast, the issue of previous myocardial infarction (Patient 3) is significant. Most respondents placed this patient in PS 2, whereas I believe coronary-artery disease with previous infarction is a prime example of a constant threat to life (PS 4), even when it is not incapacitating. This can readily be settled by convention since previous myocardial infarction is only one aspect of the global index of Physical Status.

To provide "the scientific precision" called for by Owens *et al.*,[1] an effort far beyond a refinement of the Physical Status is called for. A plethora of retrospective studies of overall surgical outcomes correlated with obvious patient, disease, and operative characteristics suggested some useful predictors, most of which were never tested. Better predictors developed when the retrospective analysis was limited to a specific disease[5] or specific operation.[11] These studies generated a list of variables that correlate to various extents with outcome. Those that correlate are then weighted by degree of correlation and a weighted multifactorial index is derived. These should prove to be better predictors than anything that preceded them, but most of these have only been proposed and never tested for predictive value. Even if this were done, each of these indices is limited in scope compared with the global index of Physical Status.

It is conceivable that with sufficient effort a multifactorial index could be derived for each type of operation performed, or for each systemic disease of patients who are operated upon, or both. It is also conceivable that a multifactorial index of anesthetic risk could be designed. Factors related to the past performance of the surgeon, the anesthesiologist, the hospital, and its personnel could also be inserted into our master computer. Our computer, when then fed the appropriate information, would generate risk information with "scientific precision," giving the risk of death, the risk of each complication, the estimated hospital stay, and for some operations, estimated longevity. Here, then, would the first meaningful consent form ever generated.

This projection is not facetious, since it is entirely feasible with present technology. It is, however, a far cry from the subjective, intuitive, vague, undefined global index of Physical Status, which by its beautiful simplicity and the ease of circling one number on an anesthetic record *before* the event, provided us with only currently useful predictor of overall surgical mortality.

It is ironic that the American Society of Anesthesiologists, whose members are critical observers of surgical procedures, evolved the best index of "operative risk." Perhaps the American Surgical Association, whose members are critical observers of anesthetic procedures, will provide us with a meaningful index of "anesthetic risk."

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References

1. Owens WD, Felts JA, Spitznagel EL Jr: ASA Physical Status Classifications: A study of Consistency of Ratings. ANESTHESIOLOGY 49:239–243, 1978
2. Saklad M: Grading of patients for surgical procedures. ANESTHESIOLOGY 2:281–284, 1941
3. Dripps RD, Lamont A, Eckenhoff JE: The role of anesthesia in surgical mortality. JAMA 178:261–266, 1961
4. New Classification of Physical Status. ANESTHESIOLOGY 24: 111, 1963
5. Goldman L, Caldera DL, Nussbaum SR, et al: Multifactorial index of cardiac risk in noncardiac surgical procedures. N Engl J Med 297:845–850, 1977
6. Vacanti CJ, Van Houten RJ, Hill RC: A statistical analysis of the relationship of physical status to postoperative mortality in 68,388 cases. Anesth Analg (Cleve) 49:564–566, 1970
7. Marx GF, Mateo CV, Orkin LR: Computer analysis of post anesthetic death. ANESTHESIOLOGY 39:54–58, 1973
8. Moses LE, Mosteller F: Institutional differences in postoperative death rates. JAMA 203:492–494, 1968
9. Goldstein A Jr, Keats AS: The risk of anesthesia. ANESTHESIOLOGY 33:130–143, 1970
10. Dripps RD, Eckenhoff JE, Vandam LD: Introduction to Anesthesia: The Principles of Safe Practice. Fifth edition. Philadelphia, W. B. Saunders, 1977, pp 13–15
11. Loop FD, Berrettoni JN, Pichard A, et al: Selection of the candidate for myocardial revascularization: A profile of high risk based on multivariate analysis. J Thorac Cardiovasc Surg 69:40–51, 1975

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Halothane and Regulation of Respiration

CONDUCTING RESEARCH on man is potentially hazardous, and often impractical under modern constraints. Nevertheless, physiologic differences among species dictate that pharmacologic and physiologic information learned in laboratory animals be independently verified in man.

The report from London, Ontario, presented elsewhere in this issue of ANESTHESIOLOGY,[1] confirms in man important data gathered in dogs, first in San Francisco,[2] and later in Denver.[3] It is of some pragmatic value for the clinician to know that halothane depresses the ventilatory response to hypoxia in man. As further guidelines, codes and regulations for human research continue to evolve,[4] it may become less likely that we will have repeated opportunities to confirm, in man, the work reported by Drs. Knill and Gelb. It is, therefore, all the more important that this report, and the authors' conclusions, be examined carefully.

One may take issue with some of the methods used. Anesthesia was induced with a barbiturate, which may depress hypoxic stimulation of ventilation for a considerable period.[5] The hypotension seen in the anesthetized subjects may alter chemically induced changes in ventilation.[6,7] Repetitive hypoxic, hypercapnic, or "isocapnic" hypoxic testing may alter central P_{CO_2} for longer than the time allowed between tests.[8] These objections are relatively minor and do not qualitatively invalidate the results. It remains clear that Drs. Knill and Gelb have demonstrated in man that 0.1 MAC halothane blunts the ventilatory response to hypoxia, while 1.1 and 2.0 MAC halothane severely depress or abolish that response.

Anesthetic concentrations of halothane depress the ventilatory responses to hypercapnia–acidosis (CO_2–[H^+]) and hypoxia. It may be debated whether

Key words: Anesthetics, volatile: halothane. Receptors: chemoreceptors. Ventilation: carbon dioxide response; oxygen response; regulation.

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