The results of various studies performed in isolated hearts support the notion that the potential range of changes in the value of the LC in humans may be substantial, although in the recent paper by Bøtker et al. (2), a reanalysis of data previously presented by Krivokapich et al. (3), shows a less than twofold variation in the LC for various physiologic changes (with values ranging between 0.45 and 0.85). In the same report (2), estimations of the LC are made for two subjects in the fasting state by means of the kinetics of F-18 FDG (the K_1:K_1 ratio). Although this approach looks very promising, there are no reliable data presented for the magnitude and range of values for the LC in fasting humans.

Regarding the possible differences in the LC between the normal and the transplanted heart, Doenst and Taegtmeyer suggest the likelihood of an increase in the LC under conditions of increased epinephrine or ischemia (unpublished observations). In the report by Rechavia et al. (1), the possibility is raised that F-18 FDG uptake (and therefore glucose utilization) may be increased because of a state of chronic "demand ischemia." This state is proposed in the context of a substrate modifier, and in view of the modest increase in baseline workload of the hearts in these patients (53%—with a proportionate 41% increase in flow relative to that in normal control subjects), it is unrealistic to equate the status of the transplanted heart with full-blown acute myocardial ischemia.

In their letter, Doenst and Taegtmeyer suggest that they have evidence (unpublished observations) for an increase in the value of the LC with epinephrine; yet (as an illustration of the variability in the findings on this subject) they have themselves published data (4) suggesting a constant relation between the kinetic behavior of F-18 FDG and glucose during the administration of epinephrine (i.e., a constant value for the LC). Of equal importance, no indication for changes in the value of the LC were noted in the same studies during conditions of acute increase in workload. The additional finding that F-18 FDG uptake was not increased during the administration of insulin in those studies, contrary to the result of extensive experiment in humans and documented in Bøtka et al. (2), highlights the problem of extrapolating results from experimental animal models to humans.

Taken together, these findings do not support the notion that a significant difference in the value of the LC exists for the transplanted heart. It is therefore not unreasonable to interpret the finding of a threefold increase in F-18 FDG uptake in the heart transplant recipients (1) as an increase in the rate of glucose utilization. However, we do agree that there is a need for a simple and quantitative in vivo assessment of the LC, such as that proposed by Bøtker et al. (2).

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QT Dispersion as a Marker of Risk in Patients Awaiting Heart Transplantation?

We read with interest the report by Pinsky et al. (1) in a recent issue of the Journal on a possible new application of QT dispersion from the 12-lead surface electrocardiogram (ECG). The question of whether QT dispersion is a useful risk marker in patients with congestive heart failure is still under dispute (1–7). Pinsky et al. (1) reported that QT dispersion predicts death in patients awaiting list heart transplantation. In their conclusion, the authors claim that the index "may help to establish priority on a heart transplant waiting list." Obviously, such priority decisions may mean "life or death" in a given patient. The validity and objectivity of a risk stratification test in this setting are therefore of utmost importance.

Concerning the use of QT dispersion as a marker of arrhythmic events or death in patients with congestive heart failure, the contrasting results of available studies (1,4–7) indicate that the role and methodology of QT dispersion is far from settled and remains sensitive to methodologic discrepancies. We and others (2,8) have devoted research efforts to the methodology of QT dispersion that have led to entirely negative results for patients with congestive heart failure (5–7). The report by Pinsky et al. (1) raises a number of important methodologic questions.

1. QT dispersion cutoffs were determined post hoc, identifying a high risk patient group of six patients with QT dispersion >140 ms. Instead of calculating an odds ratio for such a small group, receiver operator characteristic curves could have yielded more valid statistical information. The average QT dispersion for both the event and nonevent groups was found to be significantly higher than that in published reports (4,6) and our own data (5,7). In our view, such extreme QT dispersion values must raise the suspicion of measurement errors.

2. An exact description of U wave identification was given; however, the incidence of U waves, which may have contributed to an increased QT dispersion, is not mentioned in the results section.

3. The reader is unable to discern whether measurements were taken by hand, by digitizing pad or with or without magnifying glasses. Notably, only six measurable ECG leads were required. Apparently, a 4×3 lead ECG display was used, which does not give simultaneous information for all 12 leads. This method is susceptible to errors caused by transient changes in the RR interval and associated QT intervals and may have increased the overall QT dispersion.

4. Patients with atrial fibrillation (n = 13) were included in the analysis. Again, changing RR and QT intervals between different beats prohibits accurate determination of QT dispersion.

5. No information on the reproducibility of the measurements was provided. Accurate measurement of QT intervals and QT dispersion, which is prone to subjective operator errors, requires stringent controls by comparing data from at least two independent blinded operators.

The discussion of the report, in our view, does not adequately consider methodologic considerations or limitations. In conclusion, we

believe that priority assessment in patients awaiting heart transplantation on the basis of QT dispersion measurements is premature.

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Reply

We appreciate the interest of Zabel and colleagues in our recent report (1). We also agree that it is important to be clear regarding the interpretation of our results. We would like to respond to their comments.

1. The analysis of QT dispersion risk used two different measures of dispersion: the maximal − minimal QT interval (QTDISP) and the coefficient of variation of all QT intervals (QTCV). These variables were highly significant ($p = 0.009$ and $p = 0.001$, respectively) when analyzed as continuous variables without selection of a cutoff. The dichotomization of patients on the basis of a cutoff for QTDISP of 140 ms and QTCV of 9% presented in our report was for illustrative purposes only. The actual statistical model yields a continuum of risk based on the value for QTDISP and QTCV. The significance of these predictors remained after adjustment for other potential risk factors in a multivariate analysis and after the removal of the 13 subjects with atrial fibrillation. It can be seen in the original report that all analyses consistently showed the utility of QT dispersion as a risk factor when measured as described in the patient cohort under study (1).

Recent prospective studies (1,2) have demonstrated a significant correlation between QT dispersion and mortality in patients with heart failure, although some preliminary studies may suggest otherwise. It is reasonable to assume that patients awaiting heart transplantation represent a sicker subset of patients whose data may not be extrapolated to patients with heart failure in general. We look forward to reading the final published reports on the subject alluded to by Zabel and colleagues (3,4).

2. The issue is raised that U waves may have contributed to an increase in measured QT dispersion in the patients studied. However, U waves were not included in the analysis (1) and therefore did not contribute to increased QT dispersion in the original report.

3. In terms of electrocardiographic (ECG) analysis, the ECGs were read by two independent observers blinded as to outcome, with the aid of a magnifying lens, without a digitizing pad. The method for obtaining ECGs was, as surmized, a standard 4×3 format. We used this ECG format because it is currently the standard method for obtaining ECGs and therefore most widely applicable with existing equipment. Although transient changes in heart rate during the brief period of acquisition could have affected QT dispersion, we doubt that a sizable and systematic pattern would have occurred that would have affected its measurement.

4. We considered the possibility that patients with atrial fibrillation whose RR intervals can vary from beat to beat could have affected the interpretation of our data. In fact, not only did we consider this possibility, but we included specific data on this subject in the published report (1). Although patients with atrial fibrillation were included in most of the overall analyses, we did report specific data in which the 13 patients with atrial fibrillation were excluded from analysis (see Fig. 3 and the results and discussion sections). Indexes of QT dispersion remained significant predictors of risk both before and after the 13 subjects with atrial fibrillation were removed from the analysis.

As ardent students of the scientific process, we hope that our work serves as the springboard for additional studies in this area, so that we can ultimately know how to best select patients who are still likely to die while awaiting a donor heart.

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Echocardiography in *Staphylococcus aureus* Bacteremia

Fowler et al. (1) investigated the diagnostic and prognostic usefulness of transesophageal echocardiography (TEE) in patients with *Staphylococcus aureus* bacteremia. The authors suggest that infective endocarditis is common in patients with *S. aureus* bacteremia and is