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SIR,

Comments on "Paleothermometry by control methods" by MacAyeal and others

MacAyeal and others (1991) have introduced a type of inverse method called control methods into glaciology. They suggested that control methods are the best way of deriving information about past surface temperatures from temperature–depth profiles in polar ice sheets. Although we believe that the method has promise, we have serious reservations about the way it is used in their paper. They asserted that (1) the uncertainty of an analysis by this method "can be established quantitatively" and (2) a temperature–depth profile calculated by their method fits the profile measured at Dye 3, Greenland (Gundestrup and Hansen, 1984) more closely than the one calculated by a simpler method by two of us (Dahl-Jensen and Johnsen, 1986). We believe that the authors have not demonstrated the truth of the first statement and that the improved fit is illusory. Furthermore, their inferred surface-temperature history at Dye 3, which shows oscillations of up to 11 deg, peak-to-peak, during the last 10 000 a, is not supported by any climatic data, from Greenland or elsewhere, known to us. Some further discussion of the method seems called for.

MacAyeal and others specified the inversion problem as: to find the surface-temperature history $T_s(t)$ that will minimize the quantity

$$J = \int_0^H [T(z, t_f) - \theta(z)]^2 \, dz + \int_0^{t_f} \epsilon [T_s(t) - \eta(t)]^2 \, dt \, .$$

Here, t is time, t_f represents the present, z is depth, H is ice thickness and $\theta(z)$ is the measured temperature profile. The quantity $T(z, t_f)$ is the solution of the heat-transfer equation with surface-boundary condition $T_s(t)$, and specified basal boundary condition (constant heat flux) and initial condition. The quantity $\eta(t)$ is a "preconceived" surface-temperature history. In the present case, it is taken as a constant so that the effect of minimizing the second integral is to minimize the amplitude of the surface-temperature oscillations needed to fit the measured profile. The quantity ϵ is a weighting factor; a zero value implies that the preconceived temperature history is ignored, whereas a sufficiently high value makes the computed surface-temperature history identical with $\eta(t)$.

We have some concern about this formulation of the problem; the solution is forced to oscillate about the chosen $\eta(t)$ and this may distort or obscure some of the paleoclimatic information in the data.

The inferred surface-temperature history depends sensitively on the value of ϵ, as the authors' figure 11 shows. As the ϵ is increased, the amplitude of the inferred temperature oscillations diminishes. However, the authors gave no objective method of choosing the value. The difference between the observed and calculated temperature profiles cannot be used as a criterion because, as their figure 12 shows, a good fit can be obtained for a wide range of values of ϵ. The value chosen for the Dye 3 analysis ($2.5 \times 10^{-9}\,\mathrm{m\,s^{-1}}$) appears to be the one that reduces the amplitude of the oscillations to what the authors considered reasonable, but which we would regard as unreasonably high. Moreover, they stated that the first temperature minimum after 10 000 year BP, a value of –24.35°C at 7900 year BP and the subsequent maximum (–13.5°C at 4125 BP) are reliable. However, the next minimum (–24.1°C at 2475 year BP) is only "probably reliable" and the subsequent oscillations are "insignificant" even though they have amplitudes of several degrees. No reason for these assessments was given. The emphasis throughout the paper was on how closely a calculated temperature profile fits an observed one; the inferred temperature history was never compared with other paleoclimatic data and whether the observed history is even plausible was never discussed. The authors stated that the size of the oscillations also depends on the size of the steps in depth and time that are used in the numerical analysis. We do not understand how they can claim that the uncertainty of this analysis can be established quantitatively or even that their analysis yields any useful paleoclimatic information.

The analysis of Dahl-Jansen and Johnsen (1986) reproduced the observed temperature profile at Dye 3 to within 0.03 deg. This is also the precision of the calibration of the thermistors used to make the measurements (Gundestrup and Hansen, 1984). Further reduction of the discrepancy between calculated and observed temperatures, as achieved by MacAyeal and others (1991), seems pointless. Their inversion method is highly unusual in that it takes no account of the uncertainties in the data. Indeed, their inferred surface-temperature history (their fig. 6) looks to us like an example of overfitting, that is, fitting noise as well as the signal. The possibility of small-scale convection in the borehole fluid, as discussed by Gundestrup and Hansen (1984), makes us doubt whether measuring temperatures with a precision of better than 0.01 deg, even if feasible, would reveal further details of the paleoclimate, as MacAyeal and others claim it would.

The large oscillations in the inferred surface temperature may arise partly because the heat-transfer equation is difficult to solve in the space and time domain that the authors used (constant depth intervals of 50 m and time step 25 a). A transformation of the time variable might be an improvement. Reduction of the depth interval in the upper part of the profile might also help, although this change is constrained by the fact that the temperature was measured only every 25 m.

The authors tested their method with synthetic data. They calculated a temperature–depth profile by solving the heat-transfer equation with a surface-boundary condition consisting of two cycles of a sinusoidal temperature oscillation of period 2500 a and amplitude 5 deg. They then saw how well their method recovers this oscillation from the profile. Their figure 2 shows discrepancies of up to 2 deg, which is 40% of the amplitude. The authors never mentioned this large discrepancy, let alone discussed possible reasons for it. Because their analysis of the Dye 3 data covered the past 10 000 a, running this test for four cycles, rather than merely two, might have been enlightening because the error increases with time before the present.

There is an extensive literature about deriving paleoclimatic information from temperature–depth profiles in rock. Since the classic paper by Birch (1948), a variety of forward and both Bayesian and non-Bayesian inverse methods has been used. Wang (1992) has recently summarized these. This is a simpler problem than the interpretation of ice-sheet temperatures; conduction is the only means of heat transfer and so there is no need for assumptions about past changes in precipitation rate and how the vertical velocity component varies with depth. If the authors wish to develop their method, applying it to some of these data, and comparing their results with those obtained by other methods, would be a useful first step.

In their final paragraph, the authors suggested that temperature–depth profiles can provide a check of paleotemperature records derived from oxygen-isotope ratios measured in ice cores, as proposed by Robin (1976). They then tentatively identified the cold period from 10 000 to 7500 year BP in their inferred surface-temperature history with the Younger Dryas event. They pointed out that this differs from the record of this event in the oxygen-isotope profile at Dye 3, namely a cold period lasting less than 1000 a immediately preceding the end of the glaciation at about 11 000 year BP. We would ascribe the discrepancy to the defects in their analysis discussed above, combined with the use in their calculations of an accumulation rate of only 75% of the present value. The amplitude, timing and duration of the cold event depend sensitively on the accumulation rate, as their figure 10 shows. The authors, on the other hand, took the discrepancy as support for the suggestion of Fairbanks (1989) that the low (i.e. highly negative) values of δ^{18}O in the Younger Dryas section of the Dye 3 record result, not so much from low temperatures in Greenland as from the presence of a surface layer of glacial meltwater in the North Atlantic, in the source region of Greenland precipitation, at that time. The low values of δ^{18}O are, however, accompanied by the high concentrations of wind-blown dust and also chloride and sulphate, which come mainly from the ocean, characteristic of a glacial period (Hammer and others, 1985; Herron and Langway, 1985; personal communication from M. M. Herron to W. S. B. Paterson); a surface-meltwater layer cannot account for these features. Furthermore, deuterium-excess data (Dansgaard and others, 1989) excluded Fairbanks' explanation because evaporation from mid- to high-latitude source regions would result in much lower excess values (Johnsen and others, 1989) than observed. Again, Lehman and Keigwin (1992) have recently shown that the meltwater peaks coincide with the high rather than the low δ^{18}O parts of the Dye 3 record (Lehman and Keigwin, 1992, fig. 3c and d). For these reasons, we prefer the straightforward explanation that the low values of δ^{18}O in the Younger Dryas sections of the Dye 3 (and Camp Century) cores do indeed reflect low temperatures in Greenland at the time.

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SIR,

Paleothermometry redux

In "Paleothermometry by control methods" (MacAyeal and others, 1991), we presented a mathematical method for estimating past surface-temperature history from ice-sheet borehole-temperature profiles (the paleothermometry problem). Dahl-Jensen and others (1993) have suggested that our solution of the paleothermometry problem fell short of what is needed to achieve meaningful paleoclimatic inference. Naturally, we focused in our paper primarily on the virtues of the control method, perhaps to the detriment of a sufficient discussion of the vices. Therefore, we appreciate Dahl-Jensen and others' comments for drawing attention to questions which our paper left unanswered. Answers to many of these questions, and further analysis of the Dye 3 demonstration test we presented in our paper, are provided in Firestone (1992).

The two main points we shall address in this letter are: (1) that our paper fails to demonstrate that a quantitative estimate of surface-temperature uncertainty is possible (let alone satisfactory in the examples presented in our paper); and (2) that the improved fit between calculated and observed temperature profiles that our method facilitates is illusory (i.e. deceptive). We agree with both criticisms. We disagree, however, with what we believe Dahl-Jensen and others imply: first, that failure to quantify uncertainty results from the inadequacy of our particular method alone; and secondly, that our method cannot avoid "overfitting" the data.

With respect to the first implication, we believe that *all* inverse methods are inadequate in quantifying uncertainty in the paleothermometry problem. A notorious difficulty of this problem is that there exists a class of possible surface-temperature histories which has no measurable effect on the borehole temperatures. The diurnal temperature cycle prior to 5000 years ago is an extreme example of one such history. It is therefore impossible for *any* mathematical analysis of a borehole-temperature profile, and in particular a least-squares analysis such as ours, to distinguish between these histories. It is in this sense that we agree with Dahl-Jensen and others; our method, indeed *all* methods, fail to quantify uncertainty.

In retrospect, we recognize that we misstated one of the conclusions in the abstract of our paper; namely, that the *uncertainty* of our method "can be established quantitatively". We correct this misstatement by replacing the term *uncertainty* with the term *resolution* which, as shown below, is a more specific measure of uncertainty. Figure 9 of our paper, and the discussion surrounding equations (44) and (45), present the quantitative assessment of *resolution* we intended to highlight in our abstract.

With respect to the second implication, we reiterate what we stated in our paper; the control method *does* allow for a trade-off between fitting noisy borehole data and satisfying independent performance constraints such as estimated climate histories. The transformation of equation (7) into equation (8) demonstrates this trade-off. The point Dahl-Jensen and others made, and with which we agree, is that this trade-off should be carefully engineered to restrain the method from interpreting unmeaningful measurement noise in the borehole profile. We shall outline how this can be done.

Insofar as other points have not been discussed in sufficient detail to satisfy Dahl-Jensen and others, and conceivably other readers as well, we shall re-visit the paleothermometry problem in sufficient detail to diagnose the unsatisfactory results in the demonstration tests of our paper. The lengthy analysis that follows reflects our continuing interest in applying inverse methods to glaciological problems. In particular, we believe that the paleothermometry problem serves as a metaphor for a large class of glaciological inverse problems which are burdened by imprecise methods. An example is the problem of deducing basal traction from measurements of velocity at the surface of a glacier. The surface velocity is analogous to the borehole-temperature profile, and the basal-traction field is analagous to the surface temperature history. As demonstrated by Bahr and others (1992), this problem is ill-posed in the same sense as is the paleothermometry problem. Techniques developed here may therefore have applications that extend beyond the narrow subject of borehole-temperature analysis.

We set forth several goals to accomplish in this letter. First, we wish to show that unsatisfactory aspects of our demonstration tests do not stem from the control method, but rather from the way in which we defined our particular performance index (i.e. the way in which we defined the paleothermometry problem). Second, we wish to develop an integral-equation approach using continuous variables as a means of separating the fundamental properties of the problem from the details associated with finite-difference discretization. Third, we wish to derive a formal correspondence between the control method and other least-squares methods in common use (e.g. Anderssen and Saull, 1973; Wang, 1992). Fourth, we wish to cut through the exoskeleton of mathematical formalism that may have left some readers of our paper mystified as to what the paleothermometry problem is and how it can be solved; we re-develop our method using a familiar eigenfunction (or eigenvector) approach.