

## Reassessment of a Determination of $e/h$ , Using Macroscopic Quantum Phase Coherence in Superconductors\*

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(Received 12 February 1970)*

The discovery of a possible systematic error in the Parker-Langenberg-Denenstein-Taylor determination of  $e/h$ , using the ac Josephson effect, has led to a remeasurement together with a reassessment of the original data. A number of factors contributing to the experimental uncertainty were carefully reexamined. The result of the remeasurement is in good agreement with the result reported by Parker *et al.* A comparison of the present result with the result of a reanalysis of the data of Parker *et al.* indicates that the suspected systematic error, if present in the earlier experiment, is almost certainly less than 2 parts per million.

Recently, Parker, Langenberg, Denenstein, and Taylor<sup>1</sup> reported an accurate determination of  $e/h$ , using the ac Josephson effect in systems of weakly coupled superconductors. This measurement was the primary motivation for a least-squares adjustment of the fundamental physical constants by Taylor, Parker, and Langenberg<sup>2</sup> which resulted in revision of the values of many constants exceeding several times their previously quoted uncertainties. An important feature of this adjustment, made possible by the existence of this accurate experimental value of  $e/h$ , was a critical comparison of the present experimental values of certain quantities with the values predicted by quantum electrodynamic theory. This comparison resolved an outstanding discrepancy concerning the ground-state hyperfine splitting in atomic hydrogen and indicated the presence of remaining discrepancies between experiment and theory in the Lamb shift in hydrogen and deuterium and in the magnetic moment anomaly of the electron. Because of the central role played by the Josephson effect value of  $e/h$  in the adjustment, any error in its value exceeding its quoted uncertainty of 2.4 parts per million (ppm) could alter some of the conclusions of the adjustment. The possibility of a previously undetected systematic error conceivably as large as 7 ppm has recently been discovered. In order to determine whether this error actually did occur in the result of Parker *et al.*, we undertook a remeasurement using essentially the same equipment, procedures, and personnel. Experience gained since the completion of the measurements of Parker *et al.* has

also given us a fuller appreciation of several other sources of possible uncertainty. These too were investigated. We report here the results of this reassessment of the Parker *et al.* determination of  $e/h$ .

The source of the possible systematic error which motivated our remeasurement was a rather subtle zero-shift effect in the ND 106 electronic microvoltmeter used as a null detector in standardizing the PVP 1001 potentiometer employed in the precision voltage measurements. The shift arose as follows: The ND 106 contains a guard intended to shield the input circuit from the rest of the instrument, including the drive coil of the chopper. Because this guard was ungrounded in our instrument, some of the chopper drive signal leaked through to the input terminals. Since this leakage signal was coherent with the chopper drive signal, it was detected by the instrument and read out as a spurious dc voltage. The leakage signal was maximum when the null detector terminals were not connected to any external circuitry. In our instrument *at the time of the remeasurement*, the maximum leakage signal corresponded to an effective dc input voltage of 7  $\mu$ V. When the input terminals were connected to another instrument, e.g., the potentiometer during standardization, the stray capacitance of the connecting wires, and the circuitry in the second instrument reduced the leakage signal by bypassing it to ground, thus eliminating part of the spurious dc voltage indicated by the null detector. Now, in any application in which the real null detector input signal is reversed, leaving the source circuit-

ry otherwise unchanged, the spurious dc signal would simply be cancelled using the zero-set control and would cause no error. In the PVP 1001 potentiometer standardization procedure, however, the null detector was zeroed while it was connected to the potentiometer but not switched into the potentiometer circuit. It was then connected to the potentiometer circuitry through a "calibrate" switch on the potentiometer. The consequent reduction of the spurious dc signal (between 1 and 2  $\mu$ V in our experimental setup at the time of the remeasurement) then appeared as an opposite zero shift which was unwittingly cancelled by misadjusting the working current of the potentiometer, thus introducing a systematic error into the standardization procedure. If we could be sure that neither the sign nor the magnitude of this effect in our instrument had changed since the experiments of Parker *et al.*, we could conclude that the result of Parker *et al.* was too low and must be corrected upward by 1 to 2 ppm.

Unfortunately, our present determination of the magnitude and sign of this error cannot reliably be used to correct the results of Parker *et al.* *a posteriori*. There are two reasons for this. First, the leakage signal was essentially proportional to the derivative of the square wave chopper drive signal, i.e., it was a series of sharp spikes. The form of these spikes (and therefore the magnitude of the spurious dc output signal) was very sensitive to the rise and fall times of the square wave signal, and these in turn depend on characteristics of the chopper drive circuitry which are affected by component aging and vary from instrument to instrument of the same nominal type. (During our investigation of this problem, we checked three other type ND 106 instruments. Two showed total zero shifts of  $-5 \mu$ V and one showed no measurable zero shift, compared with the  $+7 \mu$ V total zero shift of our instrument.) We therefore could not be sure that the zero shift had remained constant during the two-year period since the completion of the measurements of Parker *et al.* Second, the residual shift is sensitive to the details of the circuitry connected to the null detector input, and we could not be sure of reproducing the previous conditions exactly. In order to resolve this uncertainty about the zero-shift error, we undertook a remeasurement of  $e/h$ .

The new measurements were made using essentially the same procedures and equipment used in the earlier measurements with the following improvements: (a) The ND 106 zero shift was eliminated by grounding the guard surrounding the input circuit. (b) A reversing switch was installed in series with the ND 106 in the standardizing circuit of the potentiometer as an additional

precaution against the effect of any remaining voltage offset in the ND 106. (c) The apparatus was moved into a shielded room to eliminate any possibility of rf noise interfering with the measurement. (d) The apparent temperature drift of the resistors used to synthesize the standard  $\frac{1}{10}$  ratio used in calibrating the potentiometer (actually due to temperature drift of the resistance bridge used to measure these resistors) was eliminated by running the resistance bridge under operating power for several hours before making any measurements (see Ref. 1, pp. 651-652). (e) The heating of the resistors in the potentiometer output voltbox during calibration was carefully re-examined (see Ref. 1, p. 652). (f) The stability of the ambient temperature was improved, thus reducing the drift in potentiometer calibration observed in the earlier measurements (see Ref. 1, p. 661). (g) The standard cells used to establish the local reference volt were calibrated by the National Bureau of Standards (NBS) less than one week before the measurements were undertaken and recalibrated approximately six months after completion of the measurements. (h) By placing the copper wires from the junction to the potentiometer within a copper tube reaching from below the liquid-helium level to near the top of the cryostat, we reduced the problem of the drift of thermal emfs in the measuring circuit (see Ref. 1, p. 657). (i) The treatment of unidirectional systematic errors was improved (see discussion below). Five experimental runs were made during a one-week period; the first was devoted to debugging the experimental procedures, and only the data from the last four were used. Three of the latter runs were made with Pb-I-Pb tunnel junctions and one with a Nb-Nb point contact.

TABLE I. Contributions to the uncertainty in the measured value of  $e/h$ , in parts per million (ppm). All uncertainties are intended to be 1 standard deviation.

I. Frequency	0.1
II. Voltage	
(a) Random error	0.7
(b) NBS calibration of standard cells	0.2
(c) Transportation of standard cells	0.5
(d) Standardizing potentiometer	0.5
(e) Establishing $\frac{1}{10}$ ratio, 0.3 ppm additive per decade	0.9
(f) Lead-resistance correction, 0.3 ppm additive per decade	0.9
(g) Ground loop currents	0.6
(h) Stability of operating current	0.5
(i) Self-heating during calibration	0.5
(j) Linearity of divider	0.5
(k) Temperature drift of output voltbox	1.0
Root-sum-square total	2.2

Some of the improvements noted above require further discussion. In the following they will be referred to by the item letters given in the previous paragraph. In many cases the improvements have led to changes in the corresponding assigned uncertainties. The uncertainties are listed in Table I. This table may be compared with Ref. 1, Table VII.

(e) As noted in Ref. 1, the potentiometer output voltbox is normally calibrated using power inputs considerably greater than the power dissipation which is present when it is used to measure an unknown voltage. The effect of the resultant heating on the potentiometer calibration depends on a variety of factors, including the sequence in which the voltbox ratios are calibrated and the duration of each step in the calibration procedure. We have studied this problem in detail and have concluded that a correction of  $0.3 \pm 0.5$  ppm should be applied to the potentiometer calibration correction when the potentiometer is calibrated and used according to our procedures. The sign of this correction is such as to *increase* the final value of  $e/h$ .

(f) Further experience with our PVP 1001 has indicated that its calibration correction on the 0.001-V range has a temperature coefficient of  $2.0 \pm 1.0$  ppm/ $^{\circ}\text{C}$ . The present experimental results have been corrected for ambient temperature drifts using this temperature coefficient, taking into account as far as possible the retarded response of the voltbox to ambient temperature changes. This temperature coefficient was not taken into account in the experiments of Parker *et al.*, where ambient temperature increases of several degrees during the course of a run were common. We have reanalyzed part of the data of these earlier experiments, using the ambient temperature data noted at the time, and find that the final mean value of  $e/h$  is changed by only about 0.1 ppm but the standard deviation of the set of measurements is noticeably decreased. The latter supports the reality of the ambient temperature drift effect, while the former results from the (fortunate) fact that in runs where only one potentiometer calibration was done, it occurred before or after the measurement with roughly equal frequency, so that the effects of temperature drift tended to average out.

(g) The uncertainty associated with NBS calibration of the standard cells has been reduced from 0.6 to 0.2 ppm in accordance with the most recent revision of NBS Report of Calibration [form NBS-532a (11-68)].

(i) We have reexamined the possible corrections due to leakage resistance to ground at various points in the measuring circuit and the resulting

ground loop currents. A careful study of the possible leakage paths indicates a *possible* systematic error of perhaps 1 ppm and that any such error would increase the measured value of  $e/h$ , so that the measured value should be corrected downward. Such unidirectional uncertainties were incorrectly treated in Ref. 1. There, Table VII simply included an estimate of the possible magnitude of the error due to leakage. A better way to treat such unidirectional systematic errors is to shift the mean value of  $e/h$  by an estimate of the mean leakage error and to include in the list of uncertainties an estimate of the standard deviation of the systematic error. Thus we shall decrease the result of the present measurements by  $0.6 \pm 0.6$  ppm.

The final value of  $e/h$  obtained from the present remeasurement is  $483.5938 \pm 0.0011$  MHz/ $\mu\text{V}_{69\text{NBS}}$  (2.2 ppm). The uncertainty is intended to be 1 standard deviation and was obtained by combining root sum square of the 0.7 ppm standard deviation of the mean of the measurements with the systematic uncertainties given in Table I. This result is 0.6 ppm higher than the result reported by Parker *et al.* (one quarter of the total uncertainty quoted by these authors). It coincides with the final value reported by Petley and Morris,<sup>3</sup> if the latter is expressed in terms of the NBS 1969 volt using the 1967 BIPM volt intercomparisons as discussed by Petley and Morris. What can we conclude from this result? First of all, our remeasured value lies 0.9 ppm higher than the Parker *et al.* value after the unidirectional leakage and heating corrections are applied to *both* measurements. This difference has approximately the magnitude and sign we suspected for the null-detector zero-shift error. However, it is impossible to attribute the difference to this source with real certainty. If all the systematic errors were assumed common to both measurements, the difference would be  $0.9 \pm 0.8$  ppm, where the uncertainty is the root sum square of just the *random* uncertainties of the two measurements. Such a difference has a probability of approximately 30% of occurring purely by chance. It therefore seems reasonable to conclude that the contribution of the suspected null-detector zero-shift error to the result of Parker *et al.* if present at all, was almost certainly less than 2 ppm. We have also encountered nothing in our reanalysis of the data of Parker *et al.* nor during our remeasurement which would alter their earlier conclusions about the uniqueness of the Josephson frequency-voltage relation. Finally, there is the question of what value of  $e/h$  we would choose to claim as the net result of our experiments and those of Parker *et al.* No objective answer is possible. The present experiments were undertaken primarily

to test the existence of the ND 106 zero-shift error. They stopped short of a full scale redetermination of  $e/h$  because a determination of  $e/h$  was then in progress using a different method promising significantly higher accuracy. We hesitate to put forward as a new value of  $e/h$  the result of such a brief series of experiments. On the other hand, our understanding and appreciation of the various error contributing factors in the present experiments *were* considerably improved over the earlier experiments. We therefore incline slightly toward our present value of  $e/h$  as the best representation of the results of our work to date. In any case, neither the value nor the associated uncertainty of the present result really differ significantly from those of the result quoted by Parker *et al.*, and none of the conclusions of Taylor *et al.*<sup>2</sup> would be appreciably modified if our present value were substituted for the earlier one.

All of this then might be characterized as "much ado about nothing"; we have expended con-

siderable time and effort and seem to have succeeded only in returning to our starting point. However, we feel the exercise was not without merit. We have laid to rest the ghost of a potentially serious flaw in previous work, and we have added substantially to the confidence which can be placed in the conclusions of that work. We also feel that the fact that a measurement of  $e/h$  can be repeated with a precision of 1 ppm in a few days portends well for the ultimate utility of the Josephson effect as a standard of emf.<sup>4</sup>

We acknowledge with thanks useful conversations about precision dc measurements with W. G. Eicke, F. K. Harris, and L. Julie. We thank W. J. Hamer for arranging the calibration of the standard cells. One of us (A. D.) thanks the National Research Council for a NRC-NAS-NAE Postdoctoral Research Associateship which he held at the National Bureau of Standards during part of the period in which the present work was done.

\*Work supported in part by the National Science Foundation and the Advanced Research Projects Agency.

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<sup>1</sup>W. H. Parker, D. N. Langenberg, A. Denenstein, and B. N. Taylor, *Phys. Rev.* 177, 639 (1969).

<sup>2</sup>B. N. Taylor, W. H. Parker, and D. N. Langenberg, *Rev. Mod. Phys.* 41, 375 (1969).

<sup>3</sup>B. W. Petley and K. Morris, *Phys. Letters* 29A,

289 (1969); B. W. Petley and K. Morris, *Metrologia* (to be published). The value with which we compare here is the final value given in the second reference; this is 0.3 ppm higher than the preliminary value reported in the first reference.

<sup>4</sup>B. N. Taylor, W. H. Parker, D. N. Langenberg, and A. Denenstein, *Metrologia* 3, 89 (1967).

## ERRATA

**Magnetic Ordering in Materials with Singlet Crystal-Field Ground State. II. Behavior in the Ordered State or in an Applied Field**, Yung-Li Wang and Bernard R. Cooper [Phys. Rev. 185, 696 (1969)]. The correct molecular field theory expression for the internal energy in the ferromagnetic regime differs by a factor of  $\frac{1}{2}$  from that given in Eq. (5.11) (and also contains a constant). The correct expression is

$$U/R = -(\Delta/4 \cos 2\theta) \tanh(\Delta/2T \cos 2\theta) - \Delta/2A .$$

Corresponding to this change, the correct molecular field values for the specific heat,  $C_V/R$ , in the ferromagnetic regime are one-half the values shown in Fig. 11. The molecular field curve for  $C_V/R$  in the paramagnetic regime and in the ab-

sence of ordering is unchanged.

Also, in the development of our expression for the pseudospin Hamiltonian, a term  $+N\mathcal{J}(0)\langle J \rangle^2$  was omitted, in transcription, from the intermediate Eq. (2.10). However, this term has been correctly included in obtaining the final expression for the pseudospin Hamiltonian, Eq. (2.13).

**Faraday Rotation in Rare-Earth Iron Garnets**, W. A. Crossley, R. W. Cooper, J. L. Page, and R. P. van Stapele [Phys. Rev. 181, 896 (1969)]. The intermediate steps in Eq. (A16) are incorrect, although the same result is obtained. In place of (A16) insert