mitted in a motion which is supposed to be subject to the isothermal or adiabatic law, in which no dissipative action is contemplated."

Since that time many other researchers have studied this problem, but it was Lax [3] who discussed explicitly the conservation laws, while a broad description of the problem can be found elsewhere, e.g., [4]. These studies indicate that whether the system is linear or not, Maxwell's equations *per se* are not sufficient to ensure conservation of energy and other relevant quantities, and that entropy must also be taken into account. The mathematical theory of the relevant nonlinear PDE. has been developed (particularly in the field of fluid dynamics) into a useful method, through the concept of "weak solution" [5]. Yet the theory is not entirely free from internal inconsistencies,[2] but it does enable one to obtain solutions in agreement with observations relating to viscous fluids. However, electrical transmission lines can be made with very small losses, and the weak-solution approach leaves much to be desired.

The work of Landauer used, essentially, the results relating to the weak solution. For this reason it is necessary to include resistance in the circuit to satisfy the conditions for the particular mathematical model, but this has nothing to do with the physics of the problem. Moreover, this resistance must be larger than a certain minimum value. This is a feature of the weak solution, and, as such, does not detract from the value of Landauer's contribution. But the weak solution cannot be used to deduce the correct result for a lossless case.

It is inconsequential to say that the lost energy *can* be accounted for by such ad hoc means as radiation or resistive losses. One needs to prove it, and the literature does not provide such a proof except when conditions for a weak solution hold.

Our studies based on detailed calculations, laboratory experiments, and computer modeling [1] show that shock-wave propagation in a loss-free line (which can be dispersive if of class 1) is not accompanied by energy losses, and that for a line with small losses, a complete balance of energy also holds. But this case cannot be treated by classical methods using the weak-solution concept. More specifically, one could not account for the classical energy loss associated with a shock front using the concept of weak solution, because the time constant associated with energy dissipation is too short to ensure a detailed balance of energy. Landauer's viewpoint on this matter is not, therefore, tenable.

In (footnote 1, [2]), it is stated without proof that energy losses associated with a shock front can be accounted for by resistive elements, but the reader is left wondering what happens when the system is loss free.

In the present letter, Landauer gives further material for argument by considering discharge of a condenser through a conductor. This argument is misleading. The problem so stated is improperly formulated: the mathematical model *must* contain capacitance, resistance, and inductance. One can, in principle, balance out the resistance by the suitable addition of negative resistance and therefore consider a loss-free case, and even consider a model with a negative resistance. But one may not leave out the inductance or the capacitance from the mathematical modeling.

With the correct model one can show that as the resistance is reduced to zero, so the time taken to dissipate a given amount of energy increases without limit to infinity; the same would happen with a nonlinear lumped parameter line [1]. And it is precisely for this reason that it is not good enough to assume that resistive elements *per se* can account for the energy discrepancy associated with the classical weak solution, and worse still to assume that the energy balance would also be satisfied as the resistive elements are reduced to zero.

We cannot, therefore, agree with the reasoning contained in arguments 1) and 2) of Landauer's letter.

When dealing with a distributed system, there is a further complication in that one would need to justify the step in which one passes from the DE to the corresponding PDE. For a linear problem this can be justified, but not so readily for the nonlinear case. In this latter event, and for the loss-free case, the only limit that is justifiable is the one corresponding to the linear case. Thus the conclusions concerning the realizability as stated in our publication (footnote 1, [4]) stand.

[2] For example, the method ensures conservation of momentum and energy. However, in application, energy transformation is involved and one then evokes the heat-balance equation which predicts an increase in entropy, and, as Laudau and Liftshitz [4] state, "an increase in entropy signifies energy is dissipated," wheras the shock-wave theory was specifically devised to conserve energy while permitting entropy to increase.

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Comments on "Characterization of Microwave Oscillator and Amplifier Circuits Using an IMPATT Diode Biased Below Breakdown"

N. D. KENYON

In the above short paper,[1] the method described for the characterization of IMPATT's and their circuits is based entirely upon an assumption that the whole oscillator can be correctly described by a single-resonant circuit. This the authors have been careful to emphasize. But it is by no means clear that such an assumption is tenable for any normal circuit configurations, nor that the test described to confirm the given equivalent circuit is sufficiently stringent. Though the resonant absorption may be fairly narrow, and its variation with diode bias smooth, this is no guarantee that the circuit is single tuned, that it does not, for example, require a further series reactance giving a broad resonance elsewhere, or that the components of the equivalent circuit are not themselves functions of frequency.

A case in point is the very circuit cited,[1] the resonant-cap circuit much used in millimeter-wave IMPATT evaluation. It is known [1] that this circuit has a series inductance associated with the post supporting the cap, and that therefore changes in absorption frequency with diode capacitance are not so simply related as the single-tuned theory implies.

Furthermore, it is claimed[1] that other evaluation methods are handicapped by the package transformation, and yet the very existence of package parasitics makes the given equivalent circuit invalid. When a similar method [2] was applied to a circuit known to exhibit basically single-tuned properties, it was still found neces-

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[1] R. C. Tozer, R. Charlton, and G. S. Hobson, *IEEE Trans. Microwave Theory Tech.* (Short Papers), vol. MTT-22, pp. 806–808, Aug. 1974.

sary to use single-frequency data and a bilinear transformation to circumvent the problem of package and mounting geometry.

Finally, it should be noted that the method[1] does not separate out the circuit loss from the parasitic losses within the diode—indeed such a separation is not possible without substituting for the diode an equivalent susceptance of zero or known loss. Diode loss below breakdown is generally greater than that of good circuits; consequently, conclusions concerning circuit efficiency, given by the same authors in a similar paper [3], are liable to be erroneous.

It is also questionable whether an RF voltage, calculated by eliminating diode loss along with circuit loss [3],[1] can have any physical significance at the chip terminals.

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Authors' Reply[2]

G. S. HOBSON, R. C. TOZER, AND R. CHARLTON

Kenyon's objections are largely errors based on imperfect reading of our two papers [1],[2]. His comments about many identifiable circuit elements invalidating a simple equivalent circuit with two reactive components is incorrect. Foster's reactance theorem gives guidance on the form of the susceptance-frequency relationship of any circuit, however complicated, as its loss approaches zero (i.e., energy dissipated per cycle much less than energy stored). The only requirement for an equivalent circuit with two frequency independent susceptances is that the range of frequency encountered is small enough for a first-order Taylor expansion of the susceptance, B, to be sufficiently accurate in the form

$$B = B_0 + \left(\frac{\partial B}{\partial \omega}\right)_0 \Delta\omega.$$

The subscript 0 refers to the center frequency about which the expansion is taken. B_0 and $(\partial B/\partial\omega)_0$ are essentially constants of the expansion which may be expressed in terms of two constants: an inductance L, and a parallel capacity C of a simple equivalent circuit, i.e.,

$$B_0 = \omega_0 C - \frac{1}{\omega_0 L}$$

$$\left(\frac{\partial B}{\partial \omega}\right)_0 = C + \frac{1}{\omega_0^2 L}.$$

[2] Manuscript received January 22, 1975.
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Kenyon's misunderstanding appears to be of the nature of an equivalent circuit and its difference from an actual circuit. The actual circuit can contain any number of identifiable circuit elements.

Our experimental check of the validity of the first-order Taylor expansion was provided by the bias and frequency independence of the width of the resonant absorption [1]. The second check of the single tuned nature (this of course implies a two-constant description of B) is provided in the greater detail of the absorption line shape. If Kenyon had carried out experiments on multiple-tuned circuits he would have seen the vast changes in absorption line shape when a bias tunable resonance "passes through" a resonance controlled only by the inactive parts of the circuit. As additional evidence we have confirmed the shape of the absorption when multiple resonances do not overlap by calculating conductances corresponding to a given absorption line from measurements taken with several values of the ratio n[1]. These conductances were equal within a random experimental error of the same order as that shown in [2] (~15 percent) when our conditions for a simple resonance were satisfied.

The comments about package parasitics (presumably susceptive) are simply subjective and incorrect in reference to the equivalent circuit. The comments about diode parasitic losses have already been dealt with in [2], where we confined our measurements to those punched-through diodes whose loss below breakdown was independent of bias voltage.

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Correction to "The Numerical Solution of Some Important Transmission-Line Problems"

HARRY E. GREEN

In the above paper,[1] on pages 686 and 687, corrections are as follows.

1) Nowhere has the quantity called "gap ratio" been defined. In terms of Fig. 10(a) on page 686, it is the ratio s/b.

2) On page 687, (18) should read

$$C = \frac{\pi a^2 \epsilon}{2s} + 2a\epsilon \ln \frac{b-a}{s}. \tag{18}$$

3) On page 687, in the last column of Table VI (for diameter ratio 7:1) the entries in subcolumn C_1 for gap ratio 0.10 and 0.15 have been interchanged.

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