books

Two orthogonal views of solid-state plasmas

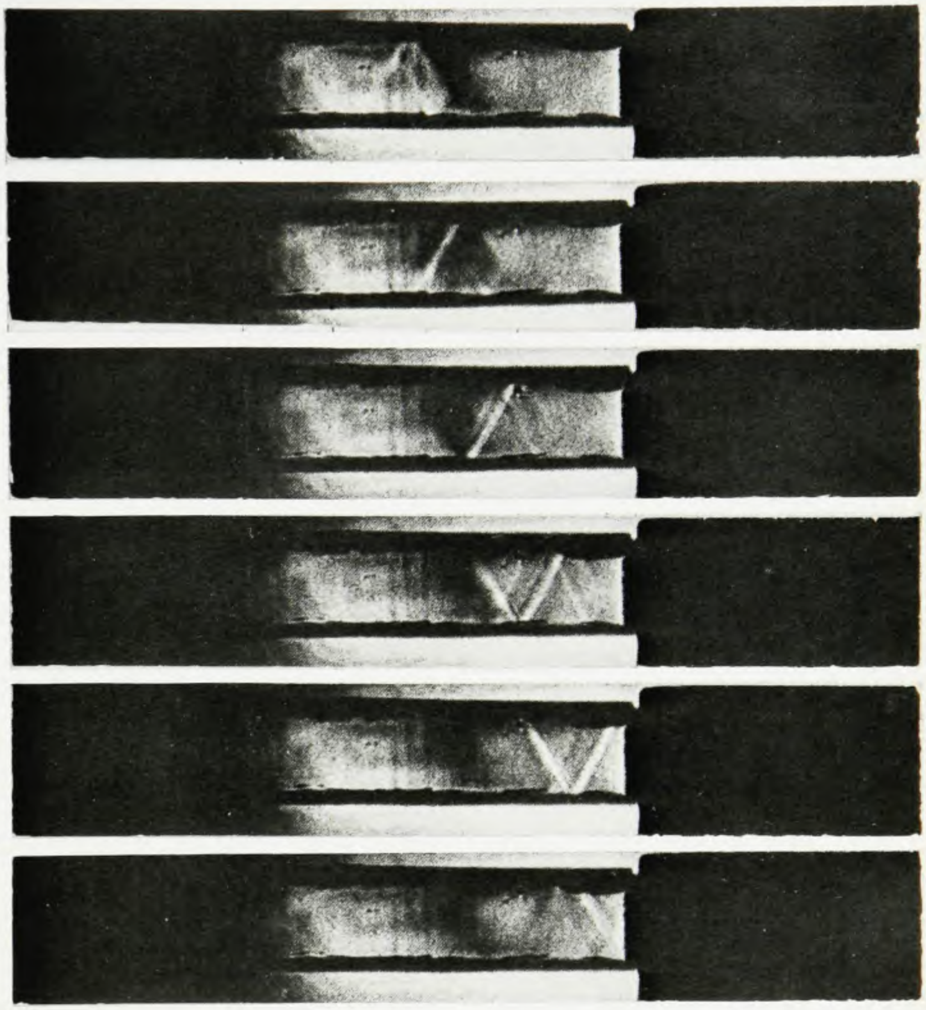
Solid State Plasmas

***By* M. F. Hoyaux**
159 pp. Pion, London
UK, 1970 £2.50

Plasma Effects in Semiconductors: Helicon and Alfven Waves

***By* A. C. Baynham and A. D. Boardman**
173 pp. Taylor and Francis
London, UK, 1971 £2.50
Barnes and Noble
New York, 1971 $8.00

Reviewed by Murray A. Lampert



Solid-state plasma. Evolution of a large-signal acousto-electric instability (domain) in an n-type single crystal of cadmium sulfide stroboscopically illuminated with the 6440 Å light from a pushed CaAsP injection laser. Photo by A. Moore, RCA Laboratories.

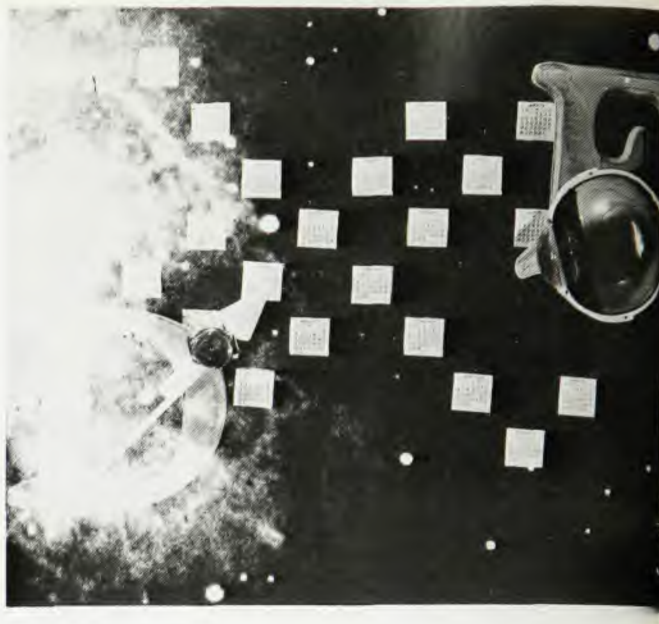
The field of plasma effects in solids is, by modern chronological reckoning, an old one, dating back to at least 1960, and has accordingly become well entrenched as a discipline within solid-state physics. As with plasma physics in general, it is extraordinarily rich in phenomena, both stable and unstable. Alas, despite intensive studies by numerous, enthusiastic and highly competent physicists working in industrial, government and university research laboratories dispersed throughout the entire Northern hemisphere, on both sides of the Iron Curtain, the field has to date yielded negligible technological returns. Despite this discouraging record the struggle for significant applications continues, and the "final" returns are not yet in—the ghost has not yet been given up. In the meantime, a formidable body of knowledge, both experimental and theoretical, has been accumulating. In recognition of this circumstance, review articles and symposium proceedings on plasma effects in solids have been appearing recently, and even more recently books have been written. In addition to the two books currently under review, there is also the recent useful book by M. C. Steele and B. Vural, *Wave Interactions in Solid State Plasmas* (McGraw-Hill, 1969), which was previously reviewed in these pages.[1]

Except for an accidental degeneracy in price, the books by M. F. Hoyaux and by A. C. Baynham and A. D. Boardman are orthogonal in almost every conceivable respect. Hoyaux's book is a once-over-extremely-lightly introduction to crystalline solid-state physics, plasma physics and finally solid-state plasmas, with about equal time for each. I'm not sure to what audience it is addressed—I suppose most obviously to those with some technical background who know very little about any of the three subjects and, for some reason, wish to learn a little bit about all three, in a relaxed manner, from one and the same book. A second audience, perhaps larger than this first one, might reasonably include plasma physicists with no background at all in solid-state who would nonetheless like to know "what gives" with plasmas in solids. The author does indeed succeed in conveying the diversity of solid-state plasma phenomena, ranging broadly from plasma oscillations, Alfvén and helicon waves, through avalanche breakdown, magnetic and thermal-pinch phenomena and the helical instability and ending with proposed applications. These various topics have been given about equal weight. For the solid-state physicist, helicon and Alfvén-wave phenomena are, because of their illumination of solid-state physics, far more important than all of the others put together. Some of the solid-state phenomena obviously related to the fusion program (for example, magnetic confinement) have managed to grab a headline or two, and even a marginal influence on fusion plasma studies. An inappropriate audience for the book is the experienced solid-state physicist looking to learn something about solid-state plasmas. The book is just too thin for his purpose.

Quite at the other extreme is the monograph of Baynham and Boardman. This is a book for workers in the field—solid-state physicists earning a living studying helicon and/or Alfvén waves in solids. For these people the monograph will be invaluable. Within the constraints the authors have set for themselves (omission of Landau quantization effects, band-structure effects and interactions of helicons with other elementary excitations, as well as only a brief foray into nonlocal effects), they have organized their presentation very methodically and with great clarity and have covered the known ground encyclopedically. The authors take great pains to derive in detail the kind of formula that is usually the starting point of a research paper, for example, the dielectric permittivity tensor (in two different approximations), and this surely will be appreciated by many readers. On and off-axis propagation are studied, and the instability-criterion problem for drifting carriers is discussed very carefully. Also much attention is paid to propagation in bounded and layered media—problems highly relevant to the elusive applications of solid-state plasma phenomena. Quite satisfactory coverage is given to experimental confirmation of theory.

I have one complaint, which is actually directed to the instability literature in general as well as to the Baynham–Boardman monograph in particular, and that is the highly mathematical nature of the discussion of instabilities. In some vague, intuitive sense one feels that instability phenomena are intensely "physical" in nature and origin; yet, comes the chapter on instabilities, and normally one immediately finds oneself deposited onto the complex plane. Thus, a key result reads ". . .the merging of roots in the k plane, in the pathological manner described above, leads to an absolute instability" (page 79). Now, although this result may be very powerful and very useful (I don't know), most physicists would confess that they don't perceive the physics in it. Yet, the situation is not totally hopeless in regard to this problem of acquiring some insight into what one is doing. Consider the following, extraordinarily simple conceptual scheme developed by Albert Rose[2]:

A system A, moving at *constant* velocity v_A, interacts through a mutual frictional force F with a second system B, which may consequently be at some particular velocity v_B. By Newton's law, an external agency must apply the force F to system A to maintain the constant velocity of A. This agency expends the power $P_A = Fv_A$. The power P_B expends on system B, at the given instant of time, is $P_B = Fv_B$. Energy is conserved by power dissipation P_D into frictional (zero-net-momentum) processes: $P_D = F(v_A - v_B)$. In an actual problem, it is straightforward to calculate *directly* the dissipation P_D. F is then eliminated to yield Rose's relation: $dE_B/dt = P_B = P_D v_B/(v_A - v_B)$, where dE_B/dt is the rate of change of the energy in system B. A crucial property of the above argument is that it is independent of the relative magnitude of v_A and v_B. For $v_A < v_B$ the energy content of system B decays in time; for $v_A > v_B$ it grows in time. (The apparent singularity at $v_A = v_B$ is a purely mathematical artifact. In a real problem, at synchromism there is either no interaction at all or it must simply be handled more circumspectly. In the latter case the difficulty relates to a frequency that changes by Doppler shift down to zero frequency, in which case averaging over an ac cycle is no longer a possible operation). Now let us apply the above scheme, which might appropriately by labelled Rose's "*reductio ad physicum*" scheme, to the simplest conceivable problem of a plasma-wave interaction in a solid. A very unsophisticated, 19th-century type, purely transverse, uniform plane electromagnetic wave propagates in an infinite dielectric medium ($\epsilon/\epsilon_0 \gg 1$) containing free electrons. Everybody knows that the wave will be attenuated because the electrons, in responding to the ac electric field of the wave, make random collisions with phonons or impurities or whatever. In the above scheme, let the electromagnetic wave be the system B and the aggregate of free electrons the system A. In the problem as stated thus far, $v_A = 0$, $v_B = c(\epsilon_0/\epsilon)^{1/2}$, and dE_B/dt is negative, corresponding to attentuation of the wave. Now cause the electrons to drift with a velocity $v_A > v_B$ (Cerenkov electrons). Rose's relation tells us that dE_B/dt is now positive—we have a growing wave, the source of energy being, of course, the drifting electrons. For those in doubt, this result is confirmed by a direct, elementary calculation of strictly conventional type.[2] Back around 1966 I used, not-so-innocently, to ask assorted solid-state plasma physicists whether sufficiently fast-drifting electrons could amplify a purely transverse, plane electromagnetic wave travelling in a semiconductor. I never once received the right answer (granted, my acquaintance with solid-state plasma physicists is limited). There was a natural prejudice in favor of the need to bunch the electrons, which the purely transverse wave could not do. The Rose relation has far wider application than to the purely academic, prototypical problem just discussed. For example, it leads very quickly and painlessly to the well known Hutson–White small-signal formula for the amplification of piezoelectric waves by free carriers drifting faster than the velocity of sound, leaving out the effects of free-carrier diffusion.[2] (It takes a bit more work to insert the latter effects properly.) I might take the opportunity here to note that the above-cited paper by Rose was the first of a series of five[3] by him in which he also succeeded in bringing very simple physics to bear on the important and diverse class of prob-



Two impressions of relativity. The pseudometric view of Hermann Minkowski depends on a global network of clocks (here calendars) and the transport of chronometers (left). The causal view of Revol't Pimenov (right) does not require a coordinate system, but relies solely on the causal relation between identifiable events such as the disturbance D and the observation at E. Photos by Joshua Barnes.

lems of energy loss by a fast electron in a solid by various mechanisms, a subject invariably treated by more or less formal mathematical methods, typically via perturbation theory.

A final point about the Baynham and Boardman monograph. Impelled, I suppose, by their dis-ease with the unrewarding technological situation as regards plasmas in solids, the authors come out with this sentence near the conclusion of the book, (page 161): ". . .the Gunn effect which is, of course, a form of plasma instability." It is nothing of the kind. It is a consequence of a distinctive band structure and Coulomb's law. Although indeed a subhandful of theoretical papers have cast the Gunn problem into a plasma mold, they came late in the game and have had utterly no influence on the course of Gunn and Gunn-like events. One might as well proclaim that all transistor devices are within the purview of solid-state plasmas. These are semantic ploys that just don't advance the cause. However, to redress the balance, in a book as truly valuable as that of Baynham and Boardman it is very, very easy to forgive a single sentence of exaggeration.

References

1. PHYSICS TODAY, February 1971, page 47.
2. RCA Review, **27**, 98 (1966).
3. The remaining four are also in the RCA Review; **27**, 600 (1966), **28**, 634 (1967), **30**, 435 (1969) and September 1971 (in press).

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Seminars in Mathematics of the V. A. Steklov Mathematical Institute, Vol. 6: Kinematic Spaces

By R. I. Pimenov
185 pp. Consultants Bureau
New York, 1970. $22.50

What is relativity *really* about? Minkowski makes us think we live in a space endowed with a pseudometric function. But some (such as A. A. Robb, Alexandr Alexandrov, Erik Zeeman, and E.H. Kronheimer and Roger Penrose suggest that our space is endowed with a local succession, precedence or causal relation expressing "the fundamental and general fact that each phenomenon acts upon some others" (Alexandrov). Revol't Pimenov calls such spaces *kinematic spaces.* Almost all works on relativity follow the pseudometric trail, as a result of historical circumstances. For centuries after Euclid it was believed we lived in a three-dimensional metric world, and surely this influenced mathematicians like Riemann to develop the differential geometry of metric spaces into the powerful conceptual tool that Einstein found ready for him. But now we understand Euclid's misunderstanding, and a mathematician who wishes to be relevant should give equal time to the study of spaces provided with a causal rather than pseudometric structure. Pimenov has provided us with a most comprehensive monograph on these. The only other books in the area at all are those of Robb on relativity (which covers much less ground) and H. Buseman on timelike spaces (which is less physical).

Let me explain why I resonated strongly with this approach of Pimenov's when I first saw it, and welcome this translation now, opaque as it is in patches. I think the difference between the pseudometric and causal conceptions of the world is significant. It is not merely a choice between two axiomatizations of one underlying theory. That it matters for learning is obvious, and perhaps we should try teaching relativity in causal terms. But what concerns me most is the Einstein problem:

We may picture physical reality as covered by the various domains of physical theory as the earth is by continental masses, tectonic plates. One lesson from Einstein's life is that theorectical upheavals like earthquakes come from where these masses clash: mechanics against thermodynamics (Brownian motion), electromagnetism against mechanics (special relativity), mechanics against geometry (gravitational theory). For most of Einstein's later life he wrestled with the problem of the confrontation of macroscopic physics (including the theory of gravity with microscopic (quantum theory). I call this the Einstein problem. I do not think it is closed. It seems to me we still lead a schizoid conceptual life, with neither half-world viable.

Why do I think this causal approach pertinent to the Einstein problem? In brief, because it is so much more operational. Here I try to take a second lesson from Einstein, and Heisenberg as well. In our search for the next physical theory it is important to put existing theory into the right form. Schwinger has emphasized this for quantum theory, citing how the Hamilton-Jacobi form of classical mechanics made possible Schrödinger's formal leap into wave mechanics, and it must be true for space-time theory as well. We infer from the experiences of Einstein and Heisenberg that the right form is apt to be expressible in terms of basic concepts close to operational practice. So when we develop the metric formulation fully, with its underlying topological and differential structures, and compare it with the quantum picture of the world, it becomes hard for me to believe the metric path is the right one. The causal formulation, on the other hand, makes the geometry of the world a consequence of the pattern of dynamical interactions, very close to Einstein's own operational analysis in terms of light signals. The two formulations suggest quite different paths for the future, and the causal path appears greener to me. This is a subjective view, and I do not project it upon Pimenov.

Pimenov sets up a detailed taxonomy of kinematic spaces. (The work pleads for an index, at least of definitions; in vain.) One after the other he turns