Part X
What are the priorities for future work?

Saturday morning. Session Chair: Tony Hewish

- What are the priorities for future work?
  - * What do we now seem to understand?
  - * What could we apparently learn in the foreseeable future?

The closing session of the Colloquium, chaired by Tony Hewish, consisted of five complementary reviews, three on theory by Vasily Beskin, Curt Michel and Chandra Shukre, and two on observations, by Peter McCulloch and Wolfgang Sieber. The review panelists presented a number of questions that are certain to be addressed by all of us working in this area. We hope that answers to some of the questions posed here will appear in the Proceedings *of the next meeting on pulsar magnetospheres.*

Dr. Beskin's remarks were not submitted for inclusion in the Proceedings.

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CLOSING COMMENTS: THEORY

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Abstract

I've been asked to summarize my impressions of where pulsar theory stands, in general, from the point of view of this conference. I've organized these comments from two distinct points of view: that of a prototypical observer and that of a prototypical theorist. These comments are not necessarily "fair" but have been exaggerated to make a point.

A brief history of pulsar theory (Observer's)

The dark ages

Basically, this era of total confusion ended with Sturrock's paper (1971) outlining the essentials of what are now regarded as key physical phenomena: emission of curvature radiation (γ rays) by energetic electrons near the pulsar surface and subsequent magnetic conversion of such γ-rays into electron-positron pairs.

The enlightenment

Although essentially contemporaneous with Sturrock, the Goldreich-Julian (GJ) model (1969) became the touch stone for how a pulsar magnetosphere "works": centrifugal slinging of plasma pulled from the stellar surface away to infinity, guaranteeing a dynamic system.

The golden age

This period concluded with the Ruderman-Sutherland (RS) synthesis of the above two models, crowned with PREDICTIONS.

If the literature is any guide, theory terminates with **The golden age.** It is not uncommon to read contemporary observational papers in which the sole theoretical reference is to RS (1975!). To amplify on why I use an exclamation point (!), we continue to the same history from a theorist's point of view.

A brief history of pulsar theory (Theorist's)

The dark ages

Same as for observers, with an important reservations: the acceleration voltage at the surface was seriously over-estimated, which invalidated the emission mechanism proposed (which was rather incomplete anyway).

The early false enlightenment

Again GJ, and again with important reservations: the plasma physics they used was wrong (see **Revolution**, below) and consequently their centrifugally driven wind is probably nonexistent.

The late false enlightenment

Where the Ruderman-Sutherland model would fall: an interesting step but unfortunately based to begin with on the viability of the GJ model (fatal flaw #1) with the added postulate that ions were firmly bound to the pulsar surface. The latter was needed to restore the huge accelerating fields proposed by Sturrock, but modern calculations have converged on an ion-binding energy of unimportant magnitude (fatal flaw #2), as noted by Arons earlier in this conference. It may be that the predictions of the model (so popular with observers) are in fact model-independent, but, until someone demonstrates those predictions to hold despite the two basic assumptions of the model being in unchallenged dispute, a moratorium seems prudent.

La petite revolution

Here I refer to the research that invalidated the GJ model. I call this revolution "petite" mainly because it was discussed almost a decade ago in a reasonably widely quoted review (Michel 1982) without apparently having been read by very many. A more recent discussion can be found in Michel (1990). Basically, the physics of nonneutral plasmas (the kind expected to be pulled from the pulsar surface by any $\boldsymbol{E} \cdot \boldsymbol{B} = 0$ fields) is entirely different from the physics of quasi-neutral plasmas, (*e.g.* the MHD approximation, *etc.*). Because GJ drew their intuition from the latter, they came to what

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now seem incorrect conclusions (for example, there need be no centrifugal wind because the plasma can all be confined to a finite magnetosphere, and no plasma need exist at light-cylinder distances to be ejected). Note that these comments are addressed at the models as described in the literature: actual pulsars are expected to eject a strong wind of magnetized plasma, but a model giving such a wind is uninteresting if the physics is wrong.

The heroic era

A. k. a. the present, where "heroic" refers to the efforts to breathe life into something akin to the GJ model (a dynamic aligned rotator). Figure 1 shows what I once claimed to be a plausible model for an aligned rotator. Except for minor detail, one can see similar figures on poster papers for this conference. The basic magnetic topology for a corotating MHD ($\boldsymbol{E}\cdot\boldsymbol{B} = 0$ everywhere) rotator was calculated some time ago (Michel 1972) and reconfirmed since (Mestel and Wang 1979, Beskin, Gurevich, and Istomin 1983a).

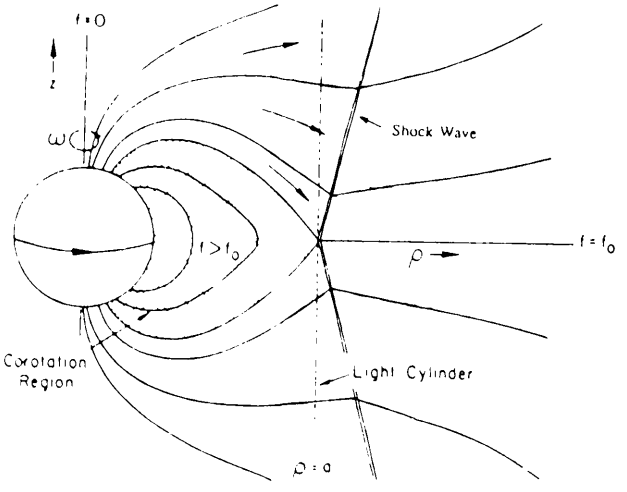

Figure 1

Where I have a "shock wave" to redirect a converging flow one now finds "closure currents" where the (relativistic) outflow electrons on the open magnetic field lines are argued to halt suddenly and cross field lines (simply because $\boldsymbol{E}$ slightly exceeds $c\boldsymbol{B}$ and/or because the corotating particles are here emitting γ-rays). Shock waves I think I sort of understand, but don't press me on how to get these massive currents across magnetic field lines. That I don't know.

Even the "return current" in figure 1 is a problem! If the return current were change separated, there would be an associated surface charge $\sigma = i/c$. This charge is huge: it corresponds to all of the outflowing charge density outside of the corotation zone being squeezed into an inflowing sheet (assuming both currents flow at c). This would be a huge electrostatic "perturbation" to the electrodynamics and is definitely not included in the modeling of the underlying magnetic field (I know, I calculated it!).

Alternatively, the current could be neutralized, but that would imply huge counter-streaming charge densities, well known to be unstable. These ideas fell from favor for just such reasons over a decade ago.

The future

Where do we go from here? There seem to be several plausible future developments:

A golden age

Here some GJ-clone has been shown to be physically viable beyond the cartoon approximation. Any discontinuities will be fully described in closed from by Maxwell's equations, *etc.*, and there will remain no hand waving claims that such-and-such "just happens" (or whatever). This is a theorist's future: of course, the RS model may still be culling 30 or 40 citations per year in observational papers.

Something new

Why the first interesting model to be suggested must necessarily somehow be the ultimately correct model defies logic. It seems amazing that everyone seems afflicted with tunnel vision, believing that at the end of the pulsar tunnel is a GJ light. I don't know if something like the pair-production avalanche I described in Dan Stinebring's session can do the job, but at least it shows how neutron stars might act as pulsars without GJ-like phenomenology.

Something old

We continue to look futilely for the keys under the street light. Pulsar data becomes equated to counting sunspots. R.I.P.

Summary

Let one just pull this together, using the following brief list:

1. Basic physics
2. Global pulsar model
3. Emission-region model
4. Observational data

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The sense of such a list is painfully, even boringly, obvious. Certainly we want a global model based on fundamental physics, certainly we expect that coherent radio emission will take place locally somewhere in this global model, and certainly we expect that the emission model will provide a reasonable account of the observational data.

But within the pulsar community, the sociology of the rewards system, such as it is, provides a significant distortion. Linkage between items 3 and 4 are deemed central while linkage between 1 and 2 are considered so peripheral as to merit little or no attention. This is basically a dangerous attitude, although one can understand the impatience of observers with issues apparently distant from observation.

Forget pulsars and imagine that a disease is sweeping the land that is 100% fatal (as indeed there is: AIDS). Item 4 becomes the disease, 3 is treatment, 2 is epidemiology, and 1 is research into the basic biochemistry. Where do we throw our finite resources? Do we treat the disease solely as an empirical problem and throw the funds at whoever has the latest snake oil with unproven but claimed efficacy? Or do we try to root it out at the base?

Once we (hopefully) succeed, the afflicted parties may have to do nothing more than take 3 pills a day for a week. But to define "success" as the ceremony of taking 3 pills a day for a week is preposterous! In the same way, pulsar observers would love to have a clear-cut prescription of how pulsars function that they could test against their data, but in a sense they have contributed to the two (plus) decades of theoretical wander by chasing after quick cures, or demanding quick cures. Pulsar theorists have handily helped in contributing approximately one model per theorist, but it has been a tough problem.

I would like to note in closing that the observer's natural preoccupation with PREDICTION is not guaranteed to be rewarded. At this point it is entirely possible that an accurate theory might simply account for known observations. It would be NICE if some new connection could be provided, but that is hardly a given. The idea that a correct theory will necessarily provide new predictions is based on how philosophers of science think science is done (*e.g.* Popper). An idea that is itself a theory without verifying predictions. For example, historically, chemists pretty well worked out the properties and systematics of chemical binding through laboratory experiments. Nowadays chemical binding is described in terms of quantum mechanics. But the belief that this theory correctly describes chemical binding stems from a host of external factors, not because verifying experimental tests were inspired within chemistry *per se*. Indeed, if chemists were forced to depend solely on *ab initio* quantum calculations, modern chemistry would largely vanish.

There are other questions to pose to a theorist besides, "Do you predict anything new?" An easier one they should be able to answer is, "Does it make sense physically?" This question can be repeated as often as necessary. Remember that some future textbook is going to cover the basic idea of how pulsars operate in a paragraph or two that smart undergraduates will be able to understand (hopefully more detailed than the existing treatments showing an oblique dipole with wiggly lines coming out of the magnetic poles). If it takes the astrophysical equivalent of smoke and mirrors (hordes of pretentious equations and accompanying cartoons to fill in the mathematical and physical gaps) to explain pulsars, it is not likely to survive long.