

THEORY AND RESEARCH IN PHONOLOGY: A QUESTION OF ALTERNATIVES

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1.0 BACKGROUND

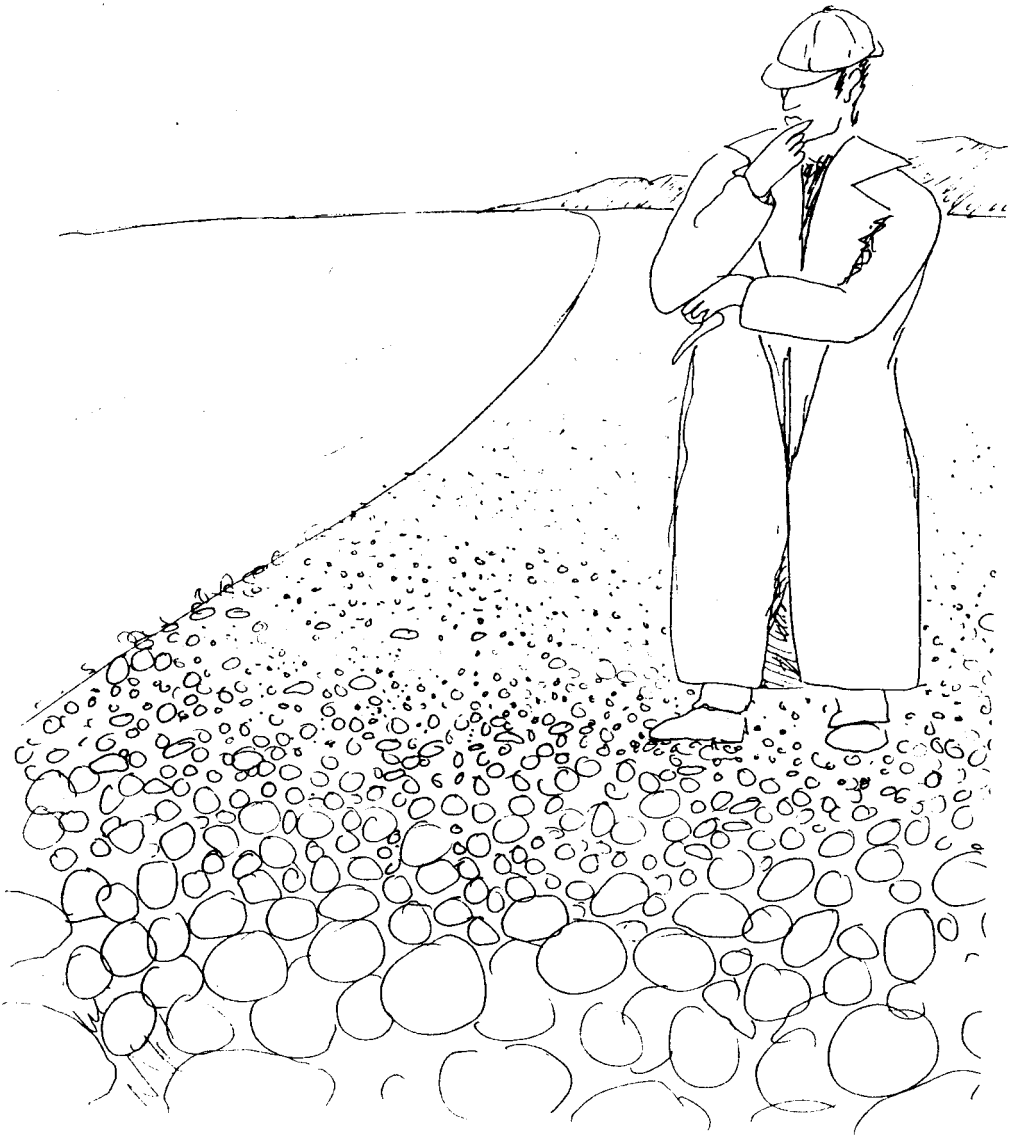
I've asked to go first because I wanted to open this discussion with some very *general* remarks about the relation between theory and research. In fact, what I propose to share with you is what I consider to be the *most important lesson* I've learned about research. I learned it many years ago, and oddly enough I didn't learn it from a linguist, philosopher, or scholar of any official strip. Actually, I learned it from a guy named Sherlock Holmes.

1.1 On Objectivity

Chroniclers of the world's first and greatest "scientific" detective", as Holmes is often described, typically make much of the fact that a chief factor in his remarkable *skill* as a criminal investigator was his unique ability to keep an open mind and to let the facts, in a sense, "speak for themselves". There is much justice in this conclusion, too, as Holmes himself repeatedly warned against pre-judging the "singular" aspects of any particular case and spoke frequently of the importance of approaching each new case "with an absolutely blank mind", so that he might be freed from prejudice "to observe and draw inferences from [his] observations". One humorist has countered this overly simplistic account, however, with the cartoon you see, which shows the great detective standing on a pebbled beach, considering the stones that lie around him: apparently there are zillions of them.

1.2 Holmes' Strategy #1

Clearly, there is another side to this story, and this can be readily extracted from Holmes' own expressed views. Thus, alongside the citations that I have already read to you, we also read that another and evidently equally fundamental aspect of Holmes' extraordinary success as an investigator was his great knowledge of the history of crime," since he found that there was "a strong family resemblance about misdeeds, and if you have all the details of a thousand at your finger ends [sic], it is odd if you can't unravel the thousand and first. (pp. 13-14)



Portrait of a Celebrated Detective Regretting his Rash Decision to Leave No Stone Unturned

In other words, for all his railing about the importance of *keeping an open mind*, Holmes nonetheless (and not too surprisingly) relied heavily in his work on a kind of preconceived framework in which he could make sense of his myriad past observations and conclusions: in other words, there can be no doubt that one important element in Holmes' approach to criminal investigation was that he had used and greatly benefited from a theory.

1.3 Holmes' Strategy #2

A second chief cornerstone of Holmes' methodology, however (judging by the frequency with which it appears in Watson's memoirs) was the following principle: "It is a capital offence [Holmes insists] to theorize in advance of the facts" (Doyle 8:770) because, as he says elsewhere, "It biases the judgment" (p. 18). "Insensibly [he says] one begins to twist facts to suit theories, instead of theories to suit facts" (p.179). In fact, Holmes concludes, "The temptation to form premature theories upon insufficient data is the *bane of our profession*" (p.915).

2.0 FROM HOLMES TO SCIENCE

To move now from "the science of deduction" to science in general, we find the same kind of conflict at work. It is universally accepted that theories are not only important, but that they provide an absolutely essential guide to inquiry that would otherwise quickly degenerate into random hunting and gathering of endless disordered and disconnected facts, much like Holmes searching under every pebble on that beach. To my mind, this conclusion has never been seriously challenged, nor do I wish to challenge it here.

2.1 Theory as guide vs theory as blinder: the methodological dilemma

On the other hand, however, just as we need theory as a *guide* to investigation, it is equally important to recognize the role that theory plays as a *blinder* to it, just as Holmes warned. We seem, therefore, to be faced with a *methodological dilemma* of sorts. On the one hand, theory-free data collection is at best a monstrously inefficient and essentially unproductive enterprise (if not an outright contradiction in terms). Yet, on the other hand, we are still always faced with that insidious natural tendency that we all seem to possess as human beings to see only what we want or expect to see. It is as though with too *poor* an idea of what we are looking for, we never find it, but with too *good* an idea, we *inevitably* find it - whether it is really there or not!

2.2 From Holmes to Feyerabend

Once again, however, it is the redoubtable Mr. Holmes who provides us with a way out of this apparent quandary. For him, the lesson to be learned was simply this: "[N]ever lose sight of the alternative" (p. 664). For the truly eloquent statement of this position, however, let me read to you an extended excerpt from an essay by the philosopher Feyerabend entitled, "How to be a good empiricist — a plea for tolerance in matters epistemological" which appeared in 1968:

You can be a good empiricist *only* [he says] if you are prepared to work with many alternative theories rather than with a single point of view. This plurality of theories must not be regarded as a preliminary stage of knowledge which will at some time in the future be replaced by the One True Theory. Theoretical pluralism is assumed to be an essential feature of *all* knowledge that claims to be objective. Nor can one reset content with a plurality which is merely *abstract* and which is created by denying now this and now that component of the dominant point of view. Alternatives must be developed in such detail that problems already considered 'solved' by the accepted theory can again be treated in a new and perhaps also more detailed manner. Such development will of course take time... It takes time to build a good theory...; and it also takes time to develop an alternative to a good theory. The function of such *concrete* alternatives is, however, this: They provide means of criticizing the accepted theory in a manner which goes *beyond* the criticism provided by comparison of that theory 'with the facts' [and therefore] allows for a much sharper criticism of accepted ideas. (pp. 14-15)

On the important issue of the so-called "relative autonomy of facts", i.e. whether or not facts even exist and are available independently of some particular theoretical position, Feyerabend notes that most discussions of theory validation would indeed appear to present a model in which "a single theory is compared with a class of facts (or observation statements) which are assumed to be a 'given' somehow" (p. 27). This is however, as Feyerabend puts it much too simple a picture of the actual situation. Facts and theories are much more intimately connected than is admitted by the autonomy principle. Not only is the description of every single fact dependent on some theory (which may, of course, be very different from the theory to be tested), there are also facts which cannot [even] be *unearthed* except with the help of alternatives to the theory to be tested, and which become unavailable as soon as such alternatives are excluded. (p. 27)

Feyerabend then proceeds to set up a scenario in which some investigator has decided to adopt a particular theory in his field and has *refused* to consider alternatives. [It is not at all surprising that a person should do this, either, for, after all, as Feyerabend also points out, "[you] can do only so many things at a time and it is better when you pursue a theory in which you are interested rather than a theory you find boring."] He further assumes that the pursuit of the particular theory which this investigator has selected has led to a reasonable degree of success, in that it has served to explain a number of circumstances which had hitherto been regarded as unintelligible.

This gives *empirical support* to an idea which to start with seemed to possess only this advantage: it was interesting and intriguing. The concentration upon the theory will not be reinforced, the attitude towards alternatives will become less tolerant. Now if it is true, as has been argued, that many facts become available only with the help of such alternatives, then the refusal to consider them will result in *the elimination of potentially refuting facts*. More especially, it will eliminate facts whose discovery would show the complete and irreparable inadequacy of the theory. Such facts having been made inaccessible, the theory will appear to be *free from blemish* and it will seem that 'all evidence points with merciless definiteness [to the essential correctness of the accepted theory].' This will further reinforce the belief in the uniqueness of the current theory and in the complete futility of any account that proceeds in a different manner, ... popular science books will spread the basic postulates of the theory; applications will be made in distant fields. More than ever the theory will appear to possess tremendous empirical support. The chances for the consideration of alternatives are now very slight *indeed*. The final success of the fundamental assumptions [of the theory] ... will seem to be assured. At the same time it is evident...that this *appearance of success* cannot in the least be regarded as a sign of truth and correspondence with nature. Quite the contrary, the suspicion arises that the absence of major difficulties is a result of the *decrease of empirical content* brought about by the elimination of alternatives, and of facts that can be discovered with the help of these alternatives only... such a system will of course be very 'successful' not, however, because it agrees so well with the facts, but because no facts have been specified that would constitute a test and because some facts have been removed. Its success [in other words] is *entirely man-made*. It was decided to stick to some ideas and the result was, quite naturally, the *survival* of these ideas. If now the initial decision is forgotten, or made only implicitly, then the survival itself will seem to constitute independent support, it will reinforce the decision, or turn it into an explicit one, and in this way close the circle. This is how empirical 'evidence' may be *created* by a procedure which quotes as its justification the very *same* evidence it produced in the first place. At this point an 'empirical' theory of the kind described...becomes almost indistinguishable from a myth. (pp. 30-31).

I know this is correct, because I have experienced the effect myself. Interestingly, later on in the same book where I first laid out these arguments over twenty years ago, the following statement also appeared (p. 197), in connection with the phenomenon of e-epenthesis in Spanish: "It is a *blatant contradiction* [I wrote] to speak both of Initial Epenthesis as a *rule* and of morpheme-initial /e/ as a *lexical* representation."

Now what in the world possessed me to have ever written such a ridiculous thing as that? It was very simple, really. I was raised on and totally imbued with classical generative phonology, and all the presuppositional baggage that came with it. Thus, even in a book that was largely written as a criticism of the theory, I still had only one concept in mind as to what the *function* of a rule in a phonological system might be. This was the function it had within that generative

framework: to express lexical redundancies, and thus to *simplify* the lexicon. At the time, therefore, I could not conceive of any alternative view, because I had not yet *looked* for one. Soon, I found one, of course (e.g. Venneman's idea that the function of rules is to express generalizations *about* the lexicon, not to *supply redundancies* left out), and at that point the absurdity of my own statement became blatantly apparent.

To give just one other quick example, in later work I became heavily involved in a series of experiments over several years, designed to answer the question, "which rule or rules" are learned to account for the regular or phonologically conditioned variation that we see in connection with such cases as the English plural and past tense inflections. And though my studies seemed quite sensible and logical at the time (indeed they attempted to systematically weed out a half dozen or so *alternative theoretical accounts* on the basis of *critical experimental tests*), they were all at the same time predicated on the unchallenged underlying assumption that 'rules' of a familiar kind were involved. Today, however, we can see that even that notion is no longer sacrosanct, as alternative models have been developed both along connectionist and analogical lines that strive to account for linguistic productivity without resort to rules at all. As a result, a good part of this past work of mine has suddenly become quite irrelevant and the focus of the effort has been forced up on an entirely new plane of activity altogether. Once again, I'd been "caught with my theories down" so to speak.

3.0 CONCLUSION

My embarrassing conclusion is that even though thoroughly forewarned of the dangers before I started, I nonetheless got sucked in: I have myself fallen into the trap of premature commitment to an overly narrow theoretical conception on at least two big occasions. To help reduce the chance of that happening again, I now refer often to a card that encapsulates the essence of the problem in a few brief words: "Those who have an excessive faith in their own ideas are not well fitted to make discoveries." An open mind is required to do science, and for a mind truly to be open, it must make a serious commitment to more than one theoretical position. It's just that simple — and just that hard. Unless we, as individual researchers are *each* willing to work *comfortably* with at least two *incompatible* theories, the odds are that we're just fooling ourselves. I know I was.

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