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SHIFTING DOMAINS

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> Aber gehen sie uns beim Gebrauch des Wortes "Sessel" ab; und sollen wir sagen, daß wir mit diesem Wort eigentlich keine Bedeutung verbinden, da wir nicht fur alle Möglichkeiten seiner Anwendung mit Regeln ausgerüstet sind?
>
> Ludwig Wittgenstein, PI, 80.

It seems that the notion of basic object(s) of a theory is seductive in at least two ways. First, if a theory has basic objects which *individuate* the theory in telling us what the theory is about, then, if there is something like a universal theory of everything that there is[1], then the global theory will capture the fundamental furniture of the Universe. Second, the conception of basic objects allows a robust realist interpretation of theory change. If theories have basic objects that individuate a theory in some fundamental way, then we can try to keep the effects of much of the empirical features of theory-change to its *non*-basic ones. Keeping track of the basic objects across theory-change thus gives us a handle on theory-identity as well. However, in order to force a basic/non-basic distinction in the objects of a theory, we need first to convince ourselves that theories in fact *have* objects, that they are not mere instruments that enable systematic computation over a choice-set of symbols invented by someone. Then we could try to split this class into basic and non-basic subclasses.

DISCIPLINARY DOMAINS

It may be instructive to begin with a fairly commonsensical view of scientific theories aired recently by Jerry Fodor (1994, p.3): "empirical explanation is typically a matter of subsuming events (states, etc...) in the domain of a science under laws that are articulated in its proprietary theoretical vocabulary". The view is commonsensical in that it does not invoke a technical notion of theories as sets of sentences. The view concerns directly what theories are, not how they look like.

The view is not only commonsensical, it is also promising for the project at hand. The central notion in Fodor's conception of theories, viz., empirical explanations via laws, is that a theory is geared to a *domain*. The conception of a domain here is a hardheaded one having to do with domains of experience, domains of reality, and the like. I will settle for domains of reality because events are metaphysical entities, not epistemic ones. Thus what contains them ought to be metaphysically construed as well. So there are domains of reality out there, whether we experience events in such domains or not.

This realistic conception of domains gives us an immediate handle on the issue of individuation of theories. Theories are now individuated in terms of the events that are explained by the theory via its 'proprietary theoretical vocabulary' (= primitive vocabulary, if the theory has been formalized). This could only be if the said events could be *described* by the vocabulary of the theory. This in turn could only be if the vocabulary of the theory consists in part of terms that pick out certain objects in the domain such that interactions between these objects give rise to the events that ultimately fill the domain. A theory can now well be viewed as true *of* such events, *simpliciter*. These objects could be bodies in relation with one another, or bodies and forces, or just forces acting on each other, or maybe they are other things like mental particulars; it does not matter.

We thus get a natural fit between a domain and the vocabulary of a theory. To use a familiar metaphor, we think of domains as areas carved out from the rest of reality. Domains in that sense have a geometry; we think of objects as marking out this geometry like pillars on a field. No doubt, various kinds of objects can do the job – bushes, for example. So it is hard to think of a *given* set of objects as the only one that does the job. Yet the very geometry of the domain will impose rather severe constraints on what these objects could be. It is possible then to extract from these constraints something like the very condition of objecthood in that domain. A given theory or a set of theories thus works under an underlying generic theory that prescribes these conditions. Supposing this generic theory to be formalized, the bound variables of this theory will require values which we might very well call the "basic objects" of the domain. These objects will show up in any theory that explains events in a given domain. We thus have exactly the sort of distinction between basic and non-basic objects of a theory that explains theory-change without falling into relativism.[2]

I request you to kindly grant me all this so that we can proceed. Let us not get bogged down with questions about what these basic objects of a generic theory are, how much degree of freedom they allow in the construction of theories, whether these are always abstract objects as suggested, and the like. These questions need not detain us because from here on I am going to focus on the notion of a domain itself. In any case, I hope the following example makes it all clear.

FROM G-B TO MINIMALISM

The picture just sketched assumes, *a la* Fodor and common sense, that a science, now viewed diachronically as a cluster of theories, is individuated in terms of a domain: when domains differ, sciences differ; also, when domains overlap, sciences overlap. Is that the correct picture of an on-

going science?[3] In this part of the paper, I will examine the question just asked with respect to some recent developments in linguistic theory. That is, I am going to see if the bit of historical data I am going to look at fits with the idea that a science can be individuated in terms of its domain.

In the late '70s, Noam Chomsky proposed a theory in which the concepts of *government* and *binding* played a central role. So, in popular parlance as well as in print, the theory came to be known as "Government-Binding Theory"[4]. I will presently make an informal sketch of these concepts. However, before we do that, it is important to note the radical nature of these proposals with respect to traditional conception of linguistics. At a number of places, Chomsky has claimed that the theoretical framework in which the concepts of government and binding play a central role – called the *Principles and Parameters framework* – is a radical departure from "thousands of years" of research on language[5]. The framework departs from traditional concerns, such as Paninian grammar, in at least three significant and inter-related ways. (a) The framework concerns *knowledge* of language, i.e., states in the minds of native speakers, rather than language as an external object with properties of sound and meaning. (b) The framework attempts to identify the genetic properties of the species rather than the properties of individuals and communities. (c) The framework views notions such as Oriya, English and Sanskrit as non-theoretical and on a par with such notions as "large molecules" and "terrestrial animals". Therefore, on Fodorian grounds, current linguistic theory differs totally from traditional ones in that the domains differ sharply. The events studied by traditional grammar were speech-events taking place in the world; the events that interest Chomsky happen inside the mind of the child. So there is not even a partial overlap. How then do we understand Chomsky's statements such as the study of language is an "ancient" one that "goes back thousands of years"? In what sense are Panini and Chomsky joining hands in a common enterprise?

One could reply, plausibly in my view, that issues about the basic objects of a theory meaningfully arise only for formal theories in which its primitive vocabulary is explicitly identified. Thus, it is unfair to raise this issue *across* a formal framework and others that are largely informal commonsensical approaches on vaguely defined phenomena. In this view then we should restrict ourselves only to the Chomskyan framework and examine *its* basic objects, if any. I doubt if any empirical theory is strictly formal in the sense just demanded, even if we ignore lessons from Godel. Further, I doubt whether die-hard Paninians like B. N. Patnaik will agree that Panini's was an informal commonsensical approach, not worthy of the label of 'rigorous theory'. Let us set these doubts aside and proceed.

Returning to the theory of government and binding, the concept of government was introduced as follows. In a grammatical explanation geared, say, to the structure of a syntactic tree, various conceptions are needed to relate syntactic objects at the nodes of the tree. The central notion is that of c-command which relates syntactic objects, roughly, under maximal projection. C-command will thus relate fairly 'distant' objects, say, objects at Specifier and Complement nodes of a given phrase. However, it was observed that important syntactic generalizations could be reached much more 'locally', i.e. within 'flatter' parts of a tree. The concept of government is designed to capture such local relations, in part, by way of, roughly, *mutual c-command*, and other things. Thus, the *event* of (unexceptional) theta-role assignment, among many other events, takes place under government. For example, the relation of government obtains between a verbal head and its propositional complement.

However, another special sort of relation is needed to capture generalizations regarding the distribution of noun phrases (NPs) themselves, especially when they get co-indexed for a variety of reasons. As we saw, the relation of government, though local, is too general to capture just this information. Also, the generalizations required here need not obtain in local domains. Additional constraints on c-command give the

relation of binding between co-indexed NPs. Thus, both the relations of government and binding are required in the system.

The concepts of government and binding were thought to be so central to the linguistic enterprise that, in a famous lecture, Chomsky admonished his followers in the following words which I cite at length:

> (S)uch terms as 'government-binding theory' should be abandoned … Insofar as the concept of government enters into the structure of human language, every approach will have a theory of government … Similarly, no approach to language will fail to incorporate some version of binding theory, insofar as referential dependence is a real phenomenon to be captured in the study of language, this being a common enterprise. There are real questions about government and binding, but no tentative set of hypotheses about language has any proprietary claim to these topics.[6]

So Chomsky is suggesting that the concepts under discussion "enter into the structure of human language", a study of which is a "common enterprise". These concepts relate to "real questions" which "no approach to language will fail to incorporate". In our terms, these concepts thus signal the basic objects of the linguistic enterprise itself, not just of specific theories – "tentative set of hypotheses". They are needed to capture the events in the domain of language to which every theory of language from hereon will be geared. In sum, we get everything that we wanted of basic objects and their ability to supply disciplinary identity. This was in a lecture delivered in 1989, which was subsequently published in 1991.

I can almost hear the linguists in the audience chuckling throughout the preceding exercise because in 1992, i.e., within three years, Chomsky circulated a paper titled "A Minimalist Program for Linguistic Theory"[7] where he proposed a framework in which, as he

summarized later (Chomsky 2000), "there's no government, no proper government, no binding theory internal to language, and no interactions of other kinds". The theoretical reasons why the minimalist program does not have government, and why binding theory has been taken away from language and has been placed elsewhere, are far too involved and technical for a quick exposition here. Roughly, the basic idea is that these concepts are no longer required since (a) the program dispenses with the notion of grammaticality which required binding theory, and (b) the program dispenses with the levels of representation, such as d- and s-structures, where the concept of government played a crucial role. In some global sense, the theory still explains the 'phenomena' covered earlier; in fact, the claim is that it covers much more. But that notion of 'phenomena' can no longer be captured in terms of the basic objects of government-binding theory. In short, the *domain* has shifted radically from the G-B framework to the minimalist one. What identifies the discipline of linguistics then?

OBJECTIONS AND REPLIES

Several objections need to be considered at this point. First, one could generally object that the sketch of the program does not show that the *phenomena* of government and binding have disappeared from view. One could still have government if one wanted to, but it is no longer required since better and more economical devices to capture the *same* phenomena have been found. There are several problems with this objection. Linguistic behavior of people is certainly the 'phenomena' that linguistic theory, in any version, tries to explain. But that, as in any science, is just a starting point since linguistic behavior is the data. What we are interested in are the basic objects of *theories*, not of 'experiences'. Which objects make a certain stretch of experience possible is exactly what a

theory tries to explain. It is obvious that if experiences came marked with their objects, no science would have been needed ever.

Second and more specifically, one could say that the historical sketch goes to show only that the chosen concepts are not basic, not that there are no basic objects. For, the concept of c-command which was central in defining both government and binding is still available in the minimalist program. So c-command is one of the basic objects (rather, relations); government and binding are not. This objection, in recent philosophical parlance, simply kicks the problem upstairs, for the 'basicness' of government and binding has been passed on to the 'basicness' of c-command.

The objection assumes that the concept of c-command remains invariant across G-B and Minimalism such that we could get back government and binding if we wanted to. This is far from the case, however. First, there is no doubt that a relation which is continued to be called "c-command" is available in MP. Let us also grant that the empirical effect of MP c-command is equivalent to GB c-command. Yet the MP c-command is *defined* in MP terms, i.e., in terms of targets and visibility[8], which are not available in GB. This has the important consequence that the notion of *mutual* c-command is no longer meaningful; hence, we cannot define government in this scheme. MP works with a primitive notion of locality which is radically different from government locality. Thus, there is no natural way of getting one from the other, and, as suggested, the exercise is not even required since the derived notion of government, if any, does not have a function in MP.

The situation with binding is somewhat different. As suggested, binding theory does not constrain operations and representations in the computational system anymore. So even if it is available, it is no longer a basic relation in linguistic theory. In fact, it is not even available despite the availability of MP c-command. Recall that binding theory requires two clauses: c-command and co-indexing. MP does not have indexing in the

first place for framework internal (= minimalist) reasons. Some version of binding theory applies to the outputs of the computational system and, hence, indexing needs to be introduced in some way. But there are several options available here, say, linking, or referential dependence which are very different from GB indexing. These notions in any case are not available as basic notions in MP.

A third and more potent objection could be that, despite internal shifts in the vocabulary, the *domain*, in some global sense, remains intact. Aren't all frameworks in linguistics geared to the domain of *language*? If they are, then there must be basic objects which individuate the domain of language. Can we, for example, give up concepts like nouns, verbs, prepositions, reflexives etc. while continuing to do linguistics? A bit later, I will address the issue of whether linguistic theory, notwithstanding what it is *called*, is necessarily geared to the domain of language. For the moment, let us grant it. Even then the objection amounts to a stipulation. The domain of language is a pretheoretical conception, just as the domain of physical theory is pre-theoretical. Some array of experiences, expectations etc. no doubt give rise to such conceptions. The task of a science is to *interpret* and examine them to see whether they are valid. If their validity is taken for granted, then the issue of basic objects ceases to be an empirical issue. Basic objects, if anything, are projections of *theories* suitably formalized, *not* of expectations. If that was the case, then the growth of plants would have continued to fall within the domain of physics, as Aristotle thought, since motion – viewed as displacement over time -- is involved there. Similarly, we saw that the advent of GB signaled sharp change in the domain itself, viz., in the very notion of language. So, ultimately, we want physics and linguistics to tell us what their basic objects are, and the sense in which they are 'basic'. Domains of science are constructions, not given in advance. The interesting question under discussion is whether such constructions reach a stable core.

Turning to the more specific thrusts of the current objection, it is not even obvious that the conception of a domain for linguistics forces a basic vocabulary such as nouns, verbs and reflexives. Going back once more to the beginnings of the current linguistic enterprise, the basic reason for advancing transformational grammars against phrase structure grammars was that the latter did not give natural explanations of linguistic facts such as passive constructions. Roughly, the passive structure Mary was kissed by John ought to have deep structural connections with the active John kissed Mary. This came to be known as the 'systematicity requirement' in cognitive science (Fodor 1998). Quite obviously, one could show the structural link between these constructions by isolating their *units* of construction, rearranging them, and mapping one sequence of them to the other. The syntactic categories with which these mapping functions (=grammatical transformations) were defined happened to be things like NPs, VPs, and a host of other things. NPs and VPs themselves were built out of smaller, atomic units such as n(oun), v(erb) etc. So it might well seem that these last-named are the basic objects of the domain of language itself, rather than of a given theory.

I took pains to discuss the roles of these basic linguistic concepts in early generative grammar to bring out the (by now) familiar point that, ultimately, the identity of a concept is to be understood in terms of the theoretical role it plays. So, as in the case of c-command above, if the role varies sufficiently in a succeeding theory, then doubts arise as to the identity of the concept *across* the theories. According to Chomsky, the central difference between the Principles and Parameters framework and its ancestors is that the former dispenses with the very idea of construction-specific rules: notions such as active, passive, reflexive etc. are now treated as "taxonomic artifacts" on par with things such as pet fish and shady tree. The current systems allow only one transformational rule – Move-alpha or Affect-alpha – that is not sensitive to the *type* of syntactic category it works upon. Therefore, the classical labels such as

"nouns" and "verbs" do not play any theoretical role in either defining constructions or transformations. Thus, the chances are these are very different basic objects, if at all. No wonder then that the concept of a phrase itself differs markedly between classical transformational grammar and GB, and between GB and MP. The topic is too technical for informal exposition here. But the basic point should be clear already.

So far, we have granted that all theories of language focus upon a pretheoretical domain called "language" even if, as we just saw, we are unable to outline the geometry of this domain with a "proprietary" set of basic objects. The notion of language thus lacks stable theoretical content. Are we compelled to hold on to this pretheoretical notion, whatever it is? No doubt we *start* with some such notion. But as Richard Larson and Gabriel Segal point out, "In the process of constructing a rigorous and explicit theory, we must be prepared for elements in the pretheoretical domain to be reanalyzed and redescribed in various ways"(Larson & Segal 1995, p.8). This much is almost obvious. What is not so obvious lies buried in historical facts about the growth of a theoretical enterprise. It often happens in the historical process of "reanalyzing and redescribing" an initial domain that 'data' earlier thought to be fundamental turns out to be invalid or irrelevant on closer theoretical scrutiny.

More significantly, during the same process, new data begin to be drawn in that were not even visible earlier. This requires proposals for new theoretical tools, and the entire theoretical machinery needs to be redesigned to accommodate these new facts coherently. Thus as the phenomenal field changes, the domain shifts gradually. We have seen several examples of such domain shift. The logical consequence of these incremental shifts is that there comes a point when so much of the earlier domain has shifted out of view, and so much of a new one has got into focus, that the unity of the changed theory can only be understood in terms of a fresh domain, leading to a new discipline. The exclusion of growth of plants, and inclusion of planetary motion in the domain of

physical explanation, led to the separation between biology and physics, as we knew them until this century. By parity of reason then, there is no more basis for thinking of *current* domains of physics and biology as absolute. There are many examples in the history of science which exemplify the point. In fact, without such a dynamic, it is difficult to understand the proliferation of disciplines and subdisciplines, as we find them today.

With this general historical scenario in mind, let me turn briefly for the last time to some features of the current minimalist program to address the issue of the domain for linguistics. We saw that no interesting sense can be given to the notion of 'proprietary' vocabulary to identify the domain called "language", although it is beyond dispute that linguists study things like English, Oriya and Japanese for whatever theoretical goals they have in mind. Yet, as the enterprise progresses, *aspects* of these pretheoretical entities, that are brought under the scope of a theory, keep changing. Thus, for linguists working within the principles and parameters framework, "Oriya" means a combination of parametric values instantiated in the mind of a child. In MP, these values are located essentially in a small discernible part of the lexicon. The rest of the system, viz., the computational system, is entirely universal and is immune to the differences between pretheoretical objects. Arguably then the computational system, whose architecture is the central focus of MP, could very well apply to objects outside the *entire* pretheoretical set. Suppose it applies to some aspects of the domain of music.[9] That scenario will closely resemble the branching of biology and physics. What is the domain of this new theory? The only legitimate answer is that, it is *neither language nor music* when conceived pretheoretically. In effect, the highly abstract theoretical vocabulary defines its own universal domain, *for the time being*.

A FINAL OBJECTION

There is a fourth objection to the historical example discussed in this paper. It needs to be treated separately because the objection has to do not with some aspects of the example, but with the validity of the example itself. One could argue that a meaningful discussion of the basic objects of a scientific theory ought to focus on matured and hard sciences, not on theoretical enterprises in their infancy. A beginning science is naturally unstable, and it has to go through several 'upheavals' before it settles down to a coherent picture of reality it projects. This response is interesting because it takes seriously the idea of a science delinking itself from pretheoretical conceptions. Recall that much of the preceding discussion was also based on this idea. So here the point of the objection is that until some time has passed to allow a science to find its own nest, so to speak, we cannot legitimately talk about its basic objects. The short history of formal linguistics violates this condition.

Again there are several difficulties here. First, delinking a science from pretheory only robs the science of a domain it can cling on to; the delinking *does not* entail that the science finds its own *sustainable* domain. Quite the contrary, as we saw. Since we failed to find any stable notion of a domain within the course of the formal theoretical enterprise, we appealed to the pretheoretical conception as a last resort, to allow the greatest possible room for the issue to maneuver. Having failed in that move, we could conclusively reject any coherent notion of a stable domain. Delinking, thus, is not a move towards stability; just the contrary, in fact.

Moreover, granting that we can form some conception of suitable "time" to have elapsed before we talk about basic objects, it is not clear at all what contribution does the passage of time make on this issue. In fact, one could argue, plausibly in my view, that the study of basic objects ought to be focussed, if at all, to the *early* phases of formal theorizing for the best results. It is well known that metaphysical battles are fought in

science only at the early phases, and at a very late phase when faced with a crisis. Since the latter phases in fact cast doubt on the very availability of basic objects, it follows that the search could be successfully conducted only in the early phases. In the beginning, theories tend to be innocent, self-critical and open to radical reformulation. The events of their tortuous delinking from pretheory are still fresh in the collective memory of the enterprise. The theory is still simple enough for us to look at its foundations with sufficient accuracy and coverage. No wonder biologists prefer baby mice. The trouble is that it is hard to find a case where an enterprise is sufficiently formalized *and* is in its infancy. Contemporary linguistics provides that rare opportunity.

As the science matures over centuries and gets 'harder', the theoretical edifice becomes enormously complex with many layers and practices filling the space. Some of these acquire enough autonomy of research to resist almost any 'outsider' attempt to look at its foundations; the 'insiders' had by then developed enough vested interests in the life of the sector they occupy.[10] A mature science then is a breeding ground for dogma. Centuries of success give rise to the illusion that the science has finally hit the 'truth', that its basic objects are inviolable. The intimate details of subtle shifts of research strategies that lead to shifting domains are lost from the memory of the enterprise[11]. Thus, when a crisis hits the enterprise, it gets difficult to retrieve the track record from the archives.

Finally, it is not even clear that the central features of the picture that we sketched for the linguistic enterprise do not apply to the more advanced sciences. For a quick example, consider some of the recent remarks by Roger Penrose (1998)[12]. Discussing the measurement problem in quantum theory, and the prospects for its unification with the theory of relativity, Penrose suggested aiming for a "new physics". In this new physics, quantum theory and, say, gravitation will be properly unified, i.e., we will expect gravitational effects at the quantum scale. For that to happen though, the current scales of both quantum theory and relativity

theory are insufficient. If we take a free electron (current scale of quantum theory), we get the relevant quantum effects, but the gravitational effects are too small. If we take a cat (current scale of relativity), quantum theory produces paradoxes. So we settle for an intermediate scale, say, the scale of 'a speck of dust'.

> With a speck of dust you can start to ask the question, 'could a speck of dust be in this place and in that place at the same time'? My arguments would say that, at a certain level, you will start to see differences from the quantum procedure, and, at this level, you can actually compute on the basis that this is a *gravitational effect*, that somehow it is part of this union between, on the one hand quantum mechanics, and, on the other hand, Einstein's general relativity.

The point to notice here is, by now, the familiar one: a speck of dust is not in the domain either of quantum theory or relativity theory, as these are currently conceived. The effect sharpens if we include, as Penrose suggests, *consciousness* to fall within the scope of new physics. If Penrose is right, we ought to retrain our minds to conceive of a domain that has consciousness as well as a tiny speck of dust. That ought to take some shifting.

NOTES

[1] I am leaning on a conception of *universal* theories. Examples include exactly Newtonian mechanics and Relativity theory, nothing else. Einstein suggested that quantum theory is not universal in this sense. I am *not* thinking of *unified* theories whose existence is in dispute in any case.

[2] Notice that the preceding line of thinking does not give us any independent hold on *theory*-identity. As Fodor correctly noted, the notion of a domain attaches to a given *science*, not really to the theories within that science. So the most we have got is some conception of basic objects of a given science. With this I have given up the idea of a basic object of a theory and have settled for basic objects of a science although, no doubt, the basic objects will be *expressed* by the primitive vocabulary of a given theory at a time; how else? I guess that's the most we get, if at all.

[3] "On-going science" is supposed to be read as a rigid designator; otherwise, we confront the unwholesome notion of a dead science.

[4]Chomsky (1981, 1982).

[5] See, for example, Chomsky (1991, pp. 23) for one such statement among many elsewhere.

[6] Chomsky, *ibid.*, p. 1-2.

[7] Included as Chapter Three in Chomsky (1995).

[8] These are technical terms which relate to the MP operation MERGE, not present in GB. The basic idea is that, as syntactic objects are put together by MERGE, relations obtain between sites denoting the new object and the sites where old objects, no longer visible, were merged. Obviously the relation is asymmetric.

[9] For some arguments and speculations to that end, see Mukherji (forthcoming, Chapter Four).

[10] I have actually heard computer scientists proclaim that those who question the foundations of the enterprise are jealous of the funding. See,

Roger Schank, Presidential Address, Society for Philosophy and Psychology, Annual Meeting, Toronto, October 1987.

[11] Or, they become too scattered for a collective view.

[12].See Penrose (1998). Notice that it is always in the context of a projected 'new' science that motivations for radical domain-shifts typically and dramatically arise. But then each paper in scientific journals is a step in that direction.

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