

# Scientific tractability and relevance theory

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Draft: c. 5,026 words (excluding notes and references)

## 1. Introduction

There is a widespread view that communication and language use more generally are too complex to study systematically and scientifically<sup>1</sup> (e.g. Putnam, 1978; Fodor, 1983; Chomsky, 1992b; Chomsky, 1992a; and perhaps Davidson, 1986). Here I suggest that three distinct arguments have been given for the view. The first, due to Fodor (1983), is that utterance interpretation is unencapsulated and holistic: any information may be relevant to working out what a speaker meant. We have, Fodor claims, no good theories of unencapsulated cognitive processes, and the prospects of developing any are dim.

The second argument comes from Chomsky: language use is a mystery because it is a species of human intentional action. He claims that we cannot develop naturalistic theories of such phenomena. In his view, human action is free, in the strong sense that it is not causally determined, and it is a mystery how this is compatible with its evident coherence and appropriateness to circumstances (Chomsky, 1991: 40–41). The third argument, which can be found in Putnam, Chomsky, and perhaps Davidson, is that because many or even all of our mental faculties are simultaneously involved in utterance interpretation, it is too complex to study scientifically. As the claim is sometimes hyperbolically put, a theory of language use and communication would require, *per impossibile*, “study of everything” (Chomsky, 1992b: 103).

This paper aims to examine and defuse these arguments, at least insofar as they pertain to theories of utterance interpretation and production seen as cognitive abilities. Of these, the account developed within Sperber and Wilson’s relevance theory is the most fully articulated. From the outset, relevance theory has been in dialogue with scepticism about pragmatics, particularly that of Chomsky and Fodor, thinkers with whom it shares key assumptions. It adopts from Chomsky the aim of providing a fully explicit account of a cognitive capacity (Sperber & Wilson, 1986: 94) and from Fodor the Computational/Representational Theory of Cognition (Sperber & Wilson, 1986: 71–2).<sup>2</sup> Sperber and

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<sup>1</sup> What is meant by “systematically and scientifically” will, I hope, become clear enough for present purposes in the course of the paper.

<sup>2</sup> Carston writes, “The relevance-theoretic framework developed in the 1980s by Dan Sperber and Deirdre Wilson can be seen as a response to the challenge presented by these sceptics” (2002: 2) i.e. Chomsky, Davidson and Fodor *inter alia*.

Wilson have replied to Fodor's argument explicitly (in their 1986 and 1996)<sup>3</sup>; in addition, to the extent that relevance theory's account of communication is successful, it is a challenge to all sceptical worries about the possibility of pragmatics.

While my own inclination is to think that relevance theory provides the most promising account of communication currently available, I remain as neutral as possible about that here. The arguments under consideration are meant to tell against all cognitive accounts of utterance interpretation and production, and I try to provide general, in-principle responses to them. I devote most attention to the third argument, in part because the first two have received critical attention elsewhere: the first from Sperber and Wilson (1986; 1996), Carston (2002: 1–3), Wilson (2004) and Allott (2008); the second from Kasher (1991) and Allott (2008: 198ff.).

In section 2 of this paper I set out the three arguments in more detail and discuss some connections between them. In section 3, I briefly summarise responses to the first two arguments. In section 4, I give a somewhat fuller response to the third, 'interaction effect' argument. I note, first, that it has been essential to scientific progress to abstract away from real complications in order to frame law-like generalisations. Some special reason would need to be given for thinking that this 'Galilean style' of theorising should not apply in pragmatics, but what little has been said on the topic is unconvincing. Both extant pragmatic theories and work on heuristics in cognition more generally suggest that fruitful generalisations are being found. An argument that there can be no modelling or prediction of a certain sphere of human activity is obviously in trouble if as a matter of fact such modelling and prediction is being successfully carried out. I briefly review evidence that has been accumulating in recent decades that bears out one key prediction of relevance-theoretic pragmatics, concerning the extreme context-sensitivity of even the most common pragmatic inferences, thus lending support to its assumptions, and to the general project of cognitive theories of utterance interpretation.

## **2. The arguments against pragmatics**

In this section I set out in more detail the three arguments against the possibility of systematic study of language use and communication. All three aim to show that pragmatics is intractable at present, and, most likely, in principle.<sup>4</sup>

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<sup>3</sup> They write "It seems, then, that our undertaking – and the whole pragmatic enterprise if our understanding of it is correct – should fall (and we use the word advisedly) under Fodor's First Law of the Nonexistence of Cognitive Science" (Sperber & Wilson, 1986: 66).

<sup>4</sup> A more careful treatment than I have space for here would more clearly distinguish claims about what is feasible now from the in-principle arguments.

### *2.1. Argument 1: interpretation is unencapsulated*

Fodor (1983) distinguishes between central and peripheral systems, where peripheral systems such as the visual system process information from sensory transducers in a rather fixed, reflex-like manner, uninfluenced by general beliefs (he calls this last property ‘encapsulation’), while the hallmark of central cognition, which includes general reasoning and decision-making, is its ability to integrate information from many sources including memory, the senses, and testimony.

Utterance interpretation appears to be a species of central belief fixation, and as such it falls under Fodor’s First Law of the Non-Existence of Cognitive Science. That is, “The more global a [...] cognitive process is, the less anyone understands it” (Fodor, 1983: 107). By ‘global’, Fodor means at least ‘isotropic’ and ‘Quinean’. A process of reasoning is isotropic if information from any domain may be relevant to the conclusion, and Quinean if the metric for a good solution is global: e.g. in deciding whether to believe a proposition one should take into account its effect on the overall simplicity and coherence of one’s whole belief system. Scientific theorising is taken as a paradigm case of central cognition (Fodor, 1983: 104; Fodor, 2005: 27). Its isotropy is demonstrated by such cases as the relevance of the rate of cooling of the sun to Darwin’s theory of evolution (Carruthers, 2003); or of rabbit designs on native American pots to the astrophysics of supernovae (Antony, 2003).

The task of working out what someone meant on the basis of what noises/gestures they made also seems to be isotropic. Just about any belief can affect the interpretation of an utterance. For example, whom one takes the speaker to be referring to with ‘he’ in an utterance of the sentence in (1) may depend on information about the prevalence of corruption in the local police force.<sup>5</sup>

(1) A policeman arrested John yesterday; he had just taken a bribe. (cf. Recanati, 2004: 32)

### *2.2. Argument 2: intentional action is a mystery*

A second argument against the possibility of scientific study of language use and communication is less well-known. Chomsky divides questions into problems – those which are currently amenable to systematic study – and mysteries, those which are not (Chomsky, 1991; Smith & Allott, 2016: 203, 368 n.18). He argues that language use is a mystery because it is a type of intentional action, and human intentional action is free in the strong sense that it is not causally determined. In his view, humans, unlike machines, are only “incited and inclined” towards certain actions, not “compelled” to perform them (Chomsky, 1991: 40), and we will therefore never be able to develop theories of human action: “theories of behaviour will always miss the crucial point: the person could have chosen to act

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<sup>5</sup> Or more accurately, information about the speaker’s beliefs (about the hearer’s beliefs etc.) about

otherwise.” (Chomsky, 1996: 17).

This is reflected in scientists’ current practice, he claims: “issues of will and choice” “are not even on the agenda”; “no one even raises the question of why this plan is executed rather than some other one, except for the very simplest organisms.” (Chomsky, 2002: 58)<sup>6</sup>

Recent work on attention provides an illustration of this point. People have some conscious control over attention:

“even without moving our eyes, we can focus our attention on different objects at will, resulting in very different perceptual experiences of the same visual field. The phrase ‘at will’ points to an area beyond serious empirical inquiry.” (Chomsky, 2002: 58–59)

That is, the *effects* of consciously shifting attention can be and have been studied, (e.g. Beck & Lavie, 2004) but such work does not aim to provide a theory of the *choice* of locus of attention. Rather, participants are typically asked to attend to one part of a screen, and the effects of their doing so are measured.

There *are* studies of the capture of attention, where the location of attention is for a period of time not under conscious control (e.g. Dalton & Lavie, 2004; Lavie & De Fockert, 2005). It is an aim of this work to discover the causes of attention capture. But this tends rather to make Chomsky’s point than otherwise, since apparently it is only when attention is *not* under conscious control that scientists try to discover the factors that govern its location.

The parallel with language use is that production of utterances is stimulus-independent: a speaker can choose to say anything or nothing (a commonplace of linguistics back to Chomsky’s famous [1959] review of Skinner).

### 2.3. *Argument 3: use of language is an interaction effect*

The third argument is that the interpretation of utterances “cannot be studied in practice” and “is not a topic of empirical enquiry” (Chomsky, 1992b: 120) because it involves many or even all the capacities of the mind and we cannot (at present at least) study the mind as a totality. It is worth quoting Chomsky at length here:

“There is [...] a [...] problem, which we can formulate in vague terms but which cannot be studied in practice: namely to construct an ‘interpreter’ which includes the parser as a component, along with all other capacities of the mind – whatever they may be – and accepts non-linguistic as well

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such corruption.

<sup>6</sup> See also Chomsky, 1975: 25.

as linguistic input. The interpreter, presented with an utterance and a situation, assigns some interpretation to what is being said by a person in this situation. The study of communication in the actual world of experience is the study of the interpreter, but this is not a topic of empirical enquiry for the usual reasons: there is no such topic as the study of everything. [...] the proper conclusion is not that we must abandon concepts of language that can be productively studied, but that the topic of successful communication in the actual world of experience is far too complex and obscure to merit attention in empirical inquiry.” (Chomsky, 1992b: 120)

Similar views have been expressed by Putnam, and perhaps Davidson, as I explain below.

This argument is obviously related to Fodor’s argument but is, I think, logically independent. That a process of belief fixation is or should be isotropic and Quinean does not entail that the majority (or predominant part) of one’s mental faculties is/are involved in it. Nor does the involvement of the majority of one’s mental faculties in some task entail that the task they are performing either is or should be isotropic or Quinean. Likewise (and more obviously) this third argument is logically independent of the claim that free will and mental causation are mysteries.

Hilary Putnam has also claimed that language use is not currently investigable because it involves human cognition as a whole. In his view, “language speaking is [a] paradigm example” of a human ability that:

“may not be theoretically explicable in isolation. It is almost certainly impossible to ‘model’ a language speaker without modelling full human functional organization. But the latter may well be unintelligible to humans when stated in any detail!” (Putnam, 1978: 65)<sup>7</sup>

The reason for this unintelligibility, according to Putnam, is the structured complexity of the mind:

“Human functional organisation evolved over one to two million years. There is no reason it must have a description that would fit into one book, or even into the Bodleian Library.” (Putnam, 1978: 64)

### 2.3. Davidson’s view

Davidson, in his famous paper on malapropisms (1986), appears to argue against the possibility of a

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<sup>7</sup> See also Chomsky’s discussion of Putnam’s views (Chomsky, 1992a: 205ff.). While he endorses Putnam’s general conclusion: that “‘language speaking’ and other human abilities do not currently fall within naturalistic inquiry” he describes it as “understated and not quite properly formulated” (Chomsky, 1992a: 206), part of his complaint being that ‘language speaking’ and ‘human being’ are “concepts of common-sense understanding”, and scientific theories will not incorporate common-

systematic theory of utterance interpretation (as noted by Chomsky, 1992b: 108–109; and Carston, 2002: 1). However his argument (if it is successful) establishes only that interpretation depends in general on world knowledge and general beliefs as well as stable word meanings and rules of grammar. This is hardly controversial among pragmatic theorists; further argumentation would be needed to establish the conclusion that interpretation cannot be studied.

Davidson points out that speakers and hearers can coordinate on meanings even when words are used idiosyncratically – e.g. *monogamy* where ‘monotony’ was meant. His conclusion is that *if* we assume that knowing a language is sufficient for communication “we have erased the boundary between knowing a language and knowing our way around in the world generally” (Davidson, 1986: 265). He suggests giving up this conception of language.

Davidson’s conclusion is common ground with pragmatic theorists, who view interpretation as inference informed (but by no means determined) by linguistic decoding (a view implied by Grice’s work [1989] and explored in detail by relevance theory: [Sperber & Wilson, 1986: 2–15, 21–28; 1987, 697–99]). One looks in vain in Davidson’s paper for a further argument that studying such inference is impossible. He suggests the involvement in interpretation of “wit, luck, and wisdom [...] knowledge of the ways people get their point across, and rules of thumb for figuring out what deviations from the dictionary are most likely” and asserts that the process cannot be regularised, comparing it with “the process of creating new theories to cope with new data in any field” (p. 265). Presumably, then, Davidson would endorse an argument like Fodor’s, and there are also hints here of the third, ‘interaction effect’, argument, but he does not develop either.

### 2.5. Summary: What can be studied?

What is regarded as a good domain for theory construction varies across the thinkers we have been considering. Putnam’s view is the most sweeping: “in practice and [...] quite possibly in principle” we cannot study human beings “the way we study hydrogen atoms” because phenomena of our thought and behaviour involve the whole mind and this is too complex to allow for prediction or explanation (Putnam, 1978: 64).

Both Fodor and Chomsky are more optimistic, arguing that we can study some mental phenomena by “isolat[ing] coherent systems that are amenable to naturalistic inquiry and that interact to yield some aspects of the full complexity” (Chomsky, 1992a: 214). On Chomsky’s influential view, progress in the study of language requires abstracting away from the bulk of *use* of language (‘performance’) to focus primarily on the underlying grammatical ‘competence’ and secondarily on certain tightly-

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sense notions (Chomsky, 1992a: 206).

constrained performance systems that make use of it, such as the parser – i.e. the device that takes a transduced string of speech sounds as input and produces a structural representation of a sentence. Fodor’s argument suggests that the performance systems that can be studied are those that are encapsulated. He takes these to be ‘peripheral’ systems including various perceptual and motor systems and the parser, on the assumption that their operation is independent of general knowledge. More subtly, if there are top-down influences on peripheral systems, those who are in general agreement with Fodor’s argument might say that the systems can nonetheless be studied to the extent that one can abstract away from such influences.

### 3. Discussion of the first two arguments

#### 3.1. Discussion of Fodor’s argument

Fodor’s argument is certainly worth taking seriously, and it has been very influential. However there is considerable consensus that it conflates normative and descriptive considerations<sup>8</sup>. It might be true that decision making and belief fixation *ought* to be isotropic and Quinean, at least if we abstract away from limits of time and mental capacity.<sup>9</sup> But this does not at all establish whether human inferential processing in any given domain is in fact holistic, which is presumably an empirical question.

Sperber and Wilson point to two disanalogies between scientific confirmation and utterance interpretation which suggest that the latter is a great deal less open-ended (Sperber & Wilson, 1986: 66–67. See also Sperber & Wilson, 1996; Carston, 2002: 1–3). First, utterance interpretation is fast, starting during the utterance and typically concluding a few hundred milliseconds after the utterance ends. This strongly suggests that a lot of information that *might* have been considered is not used at all.

Secondly, the input is from a helpful source, a human being acting as a communicator – that is, someone who wants to be understood and will therefore devise her utterance so that it is a good clue to her intended meaning. Sperber and Wilson postulate that human beings have a comprehension heuristic, a fast and frugal mechanism which exploits this fact (Sperber & Wilson, 1986: 45; Sperber &

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<sup>8</sup> See, e.g. Wilson, 2004.

<sup>9</sup> Sperber & Wilson (1996: 531) question this abstraction: “By Fodor’s criterion of rationality, since we fail to consider all the relevant evidence, we are [...] irrational. [...] [W]ould you *want* to be rational in his sense? Do you want to consider all the (internally and externally) available evidence every time you fix a belief – which still would not guarantee that all your beliefs would be true, but would guarantee that you would fix much fewer of them? Fodor’s rationality is a purely epistemic matter: the only utility is truth, and no price is too high to pay to increase the chances that your beliefs are true.

Wilson, 2002; Wilson & Sperber, 2004). It does not search for all potentially relevant information but treats the mental accessibility of information in a context as a (fallible) indication of its significance (Sperber, 2000: 132)<sup>10</sup>. The heuristic evaluates potential interpretations in order of accessibility, and ‘locally’, i.e. as a function of immediate consequences for the belief system, not in terms of the effect on overall coherence or simplicity. (For more detail, and discussion of Sperber and Wilson’s arguments that this system is ecologically rational, see Allott, 2013.)

Note that as a response to Fodor’s argument it does not matter whether or not the details of current relevance theory (or any other pragmatic theory) turn out to be correct. Fodor has to show that nothing of this kind could work, and that is a hard case to make, particularly given the ongoing successes of work on efficient heuristics (Gigerenzer & Todd, 1999; Gigerenzer, Hertwig & Pachur, 2011) and fast ‘system 1’ thought (Evans, 2008; Kahneman, 2011) in several domains of cognition.

### 3.2. Discussion of the argument from freedom of the will

Chomsky appears to rest his argument about the impossibility of studying voluntary action on his controversial view that human actions are not causally determined. A more mainstream take on free will is that free acts must be rationally chosen and that this implies that all the relevant steps leading to the act are rationally caused. This entails some form of compatibilism, the view that the freedom of the will is compatible with causal determination of actions (Hobart, 1934; Davidson, 1970)<sup>11</sup>.

A perhaps more compelling aspect of Chomsky’s scepticism about theories of communicative acts is his claim that we lack understanding of how human action comes to be coherent and appropriate to circumstances across an indefinitely wide range of circumstances (Chomsky, 1991: 40–41)<sup>12</sup>. This claim could be developed into equivalents for human voluntary action of argument 1 or argument 3. Indeed Rey, commenting on Chomsky’s view, suggests that a better explanation of difficulties in studying voluntary action might be something like the third argument:

“Voluntary action may be merely a complex *interaction* effect, like the *actual* activity of a machine,

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[...] A kind of rationality worth having is one based on sound accounting principles, where not only benefits, but also costs are weighed.”

<sup>10</sup> In communication this is justified by the speaker’s interest in being understood, but Sperber and Wilson argue that in the areas for which human cognition is well-adapted, true assumptions are more likely to seem relevant than false ones (Sperber & Wilson, 1986: 116–117, read in the light of their [1995: 263–266] redefinition of relevance), and that “[i]f that is so, then Fodor’s suggestion that scientific thinking can be taken as typical of central thought processes is dead wrong.” (Sperber & Wilson, 1986: 117)

<sup>11</sup> An anonymous reviewer points out that Chomsky’s view that actions are not causally determined is compatible with compatibilism. This is correct, but I suspect that he adopted his view (in part, at least) because he takes free will to preclude mental causation.

or the precise path of a leaf in a lake, about which, indeed, general systematic theorising or prediction may be quite idle” (Rey, 2003: 130 n.16, his italics).

Even if Chomsky were right both that voluntary actions are not causally determined and that they are *therefore* beyond normal scientific explanation, it’s not clear how much of a problem this would be for pragmatic theorising, for two reasons. First, most work in pragmatics concerns interpretation of utterances. The task is to explain how a hearer works out what the speaker meant by her utterance, drawing on linguistic ability, world knowledge and beliefs about the context. Interpretation is obviously a different task from production of utterances, as Chomsky notes (1991: 40). It is not a voluntary action (and perhaps not an action at all): when you hear an utterance in a language that you understand, you generally cannot help interpreting it<sup>13</sup>.

One of Chomsky’s arguments for his views about free will is the phenomenology of voluntary action: it is, he thinks, just obvious that one could do something other than what one actually does.<sup>14</sup> This at least doesn’t apply to utterance interpretation, where the phenomenology is quite different. Chomsky’s scepticism about theories of interpretation rests instead on the ‘interaction effect’ argument (1991: 40).

One might also think that the *production* of utterances could be factored into two conceptually (if not temporally) distinct components: first, choosing what to try to convey and secondly, selection of a means to do so. Insofar as voluntary action is mysterious, the mystery might be confined to the first of these elements. Pragmatic theorists interested in production could then study the second (Kasher, 1991: 141).

#### **4. Pragmatics and the Galilean style**

The third argument is that language use cannot be studied scientifically because it is a massive interaction effect, and so a theory of language use would need to be a ‘theory of everything’. In this section I argue that this claim is in conflict with reasonable views about the role of abstraction in science.

It has been crucial to the progress of science that scientists abstract away from most of the

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<sup>12</sup> My thanks to John Collins (p.c.) for making this point clear to me.

<sup>13</sup> Sperber and Wilson take this to speak in favour of their view that there is an utterance interpretation module, that is a task-specific, autonomous mental unit whose action is triggered by stimuli that fall within its task-domain (Sperber & Wilson, 2002).

<sup>14</sup> In an interview he said, “Free will is simply an obvious aspect of human experience. I know – as much as I know that you’re in front of me right now – that I can take my watch and throw it out the window if I feel like it.” (Chomsky, 1983)

complications of actual phenomena in order to get at interesting regularities and laws. Insight into some phenomena is prioritised over coverage of all. Ironically, some of the clearest and most trenchant statements of this view are due to Chomsky, who calls it ‘the Galilean style’ (following Weinberg, 1976: Chomsky, 1978a: 9–10; Chomsky, 1978b: 14; Chomsky, 1980: 218; Chomsky, 2002: 98–100):<sup>15</sup>

“The great successes of the modern natural sciences can be attributed to the pursuit of explanatory depth which is very frequently taken to outweigh empirical inadequacies. This is the real intellectual revolution of the seventeenth century” (Chomsky, 1978a: 9–10)

So, for example, Galileo ignored air resistance in framing his laws of motion, and was right to do so. Any attempt at stating laws which tried to cover all the actual phenomena of the motion Galileo or anyone else observed would have been a hopeless mess. According to John Losee’s historical survey of the philosophy of science:

“much of Galileo’s success in physics may be attributed to his ability to bracket out various empirical complications in order to work with ideal concepts such as ‘free fall in a vacuum’, ‘ideal pendulum’, and the ‘frictionless motion of a ship through the ocean’.” (Losee, 1980: 59)<sup>16</sup>

This kind of abstraction may well be essential to science. At the very least, we find it at the centre of a number of striking successes of modern science from Galileo to the ideal gas law and Chomsky’s work on grammatical competence.

What Losee calls ‘ideal concepts’ are central: there is no requirement that we can ever see the law or regularity in operation in perfect isolation from the complications that we have abstracted away from. There are no ideal gases, and linguistic competence is not manifested in the absence of performance systems.<sup>17</sup>

The ontological status of the laws and other generalisations reached by abstraction is a matter of debate. Views range from Nancy Cartwright’s famous claim that they are false (albeit often useful and interesting) (Cartwright, 1983) to Chomsky’s view that “it is the abstract systems that you are constructing that are really the truth; the array of phenomena is some distortion of the truth because of too many factors” (Chomsky, 2002: 99). Similarly, the physicist Steven Weinberg claims that physicists (presumably including himself) “give a higher degree of reality [to] abstract mathematical

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<sup>15</sup> Weinberg cites Husserl (Weinberg, 1976: 28; Chomsky, 2002: 98–99). See also Botha, 1982.

<sup>16</sup> For discussion of idealisation in Galileo’s work, see also McMullin, 1985.

<sup>17</sup> Indeed, for all we presently know, “the world [could be] interaction effects all the way down, maybe even in fundamental physics” (Georges Rey, p.c.).

models of the universe [...] than they accord the ordinary world of sensation”<sup>18</sup>.

Whatever the ontological status of scientific laws, the argument that phenomena characterised by interaction effects cannot be studied seems at odds with how science has been successfully practised. Science is both more opportunistic and more various than the argument seems to give it credit for<sup>19</sup>. Scientists fasten onto whatever scientific laws, generalizations, abstractions and idealizations – of varying degrees of depth and generality – will do explanatory work.

Recall that the reason Putnam gives that “language speaking [...] may not be theoretically explicable in isolation” is that “[i]t is almost certainly impossible to ‘model’ a language speaker without modelling full human functional organization.” (Putnam, 1978: 65) It is obviously crucial what he means by ‘model’ here, and this is not entirely clear. As noted above, the intended contrast is with the way that it is possible to model hydrogen atoms. But it’s not clear why the demand should be to model a language *speaker* as a whole rather than to model some processes or system that underlie language use, or still less ambitiously, not to model them but simply to capture some regularities of their workings.

This issue is less clear in Chomsky’s comments on pragmatics, but when he says that “[t]he study of communication in the actual world of experience is the study of the interpreter” (Chomsky, 1992b: 120) he seems to define it in a way that precludes scientific abstraction as he conceives of it. Elsewhere in the same paper he argues that phenomena in the actual world are in general not the target of explanatory work because they are far too complex, even in the case of the motion of one body: “it is no part of physics to determine exactly how a particular body moves under the influence of every particle or force in the universe, with possible human intervention, and so on. This is not a topic.” (Chomsky, 1992b: 102)

Perhaps there is a way to defend the ‘interaction effect’ argument against the charge that it neglects the role of abstraction, however. One could take the claim to be that some phenomena, including pragmatics, are not underpinned by any one system and that these therefore cannot be fruitfully studied. The idea would be that idealisation is required but here abstracting away from the complications will not reveal any underlying core that is simple and law-like. It would be a considerable stretch to construe Putnam’s view this way, but it seems a more charitable way to take Chomsky’s remarks about pragmatics given his espousal of the Galilean style. It may also be a reasonable interpretation of Davidson’s scepticism about theories of interpretation.

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<sup>18</sup> For Weinberg and Chomsky, the Galilean style includes this metaphysical claim about levels of reality. I prefer to use the term more broadly as a label for ways of doing science that work on the assumption that abstraction is (often) essential to progress in science.

<sup>19</sup> On the heterogeneity of science (and the world) see Dupré, 1993; Cartwright, 1999.

But this way of construing the objection to pragmatics recasts it not so much as an *argument* that the study of language use is hopeless, but as a *hunch* that it will turn out that way on the basis that it will turn out to lack a law-like core. No doubt hunches of this sort can be useful. Scientists are often productively guided in their activities by their gut feelings about which areas of investigation will be fruitful. But such hunches provide no objective basis to dismiss a field of study.

To summarise: I have suggested that there are two ways of construing the ‘interaction effect’ argument and that both run into problems. At base, the issue is that science is in the business of finding underlying regularities by abstracting away from complexity, and it’s hard to make an *a priori* case that there are no interesting regularities to be found in a certain area. Scientists can follow their hunches, but have to be prepared to be surprised by results.

No doubt cognition is complex and involves many interlinked systems, but the successes of work on fast and frugal heuristics and system 1 thought are relevant here (as they were to Fodor’s argument). They are, as it were, proofs of concept: cases where simple principles have been successfully factored out of the messy totality of human cognition. As noted above, relevance theory claims that such a heuristic underlies utterance interpretation and postulates principles that govern its operation. Rival, neo-Gricean pragmatic theories also propose (other) principles and heuristics. Their concern with default pragmatic inferences and those connected to the use of particular linguistic items (Sperber & Wilson, 2005: 471–472) is a different focus, but no less an abstraction.

#### 4.1. *Some successful predictions*

In recent decades the field of experimental pragmatics has emerged (from theoretical pragmatics, psycholinguistics, and the psychology of reasoning, *inter alia*) and bloomed (Noveck & Sperber, 2004; Breheny, 2011; Phelan, 2014). As in other areas of experimental psychology, predictions are made on the basis of theory and tested. Obviously its practitioners act on the basis that there are regularities in language use that can be studied.<sup>20</sup>

There has by now been far too much work to summarise here. I discuss only one high-level property of interpretation that has emerged from a number of studies: pragmatic inference is highly context-sensitive in the sense that there are apparently no default implicatures.

Grice’s ground-breaking work on conversation (Grice, 1967[1989]) is agnostic about the order of processing. Prominent neo-Gricean accounts (e.g. Gazdar, 1979; Atlas & Levinson, 1981; Horn, 1989)

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<sup>20</sup> My focus here on evidence from experiments is not meant to imply that it is the only or the best evidence. There is no obvious limit on sources of evidence in science. As in other areas of linguistics, speaker intuitions have been important in pragmatics (Drożdżowicz, 2015; Drożdżowicz, 2016). (But see Breheny, 2011: 562–563 for an argument that in pragmatics, experimental results are more reliable

predict that there are ‘default’ implicatures which are generated in interpretation where a certain word or construction is used, even when they are not speaker-intended. Hearers have to generate them and then suppress them in such cases. For example, these accounts treat (2b) as a default implicature of (2a):

- (2) a. Some of these penguins have suitcases.
- b. Not all of these penguins have suitcases.

Sperber and Wilson’s relevance theory makes opposite predictions: there are no strong defaults, and from the beginning of hearers’ processing of utterances it is sensitive to the intentions of the speaker. So, for example, in a context in which the speaker is known to have only partial knowledge, a hearer of an utterance of (2a) will not generate (2b).

Breheeny, summarising a slew of work on the topic, argues that it shows that the strong defaultist view is untenable: “even apparently common implicatures are not generated solely on the basis of linguistic context in on-line comprehension.” (Breheeny, 2011: 566) Recent work has shown that “at an earl[y] stage in processing, participants are sensitive to what the purpose of the utterance is and that they are aware of more informative alternatives” (Breheeny, 2011: 568). Of course, relevance theory is not the only possible or conceivable theory that would make these predictions. But actual rivals have either made no clear prediction or predicted the opposite.

In philosophy of science there is considerable agreement that predictive successes of a theory lend support to those of its theoretical posits that are essential to the predictions, although it is controversial what it takes for a theoretical posit to be essential<sup>21</sup>. There is no space here for discussion of which of the posits of relevance theory are supported by the experimental results mentioned above. What is crucial in the context of this paper is that regardless of their implications for particular pragmatic theories, such predictive successes shift the burden against claims that systematic investigation of communicative language use is impossible.

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than intuitions.)

<sup>21</sup> Peters (2014) provides a useful recent review of the debate.

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