A dozen or so years ago, most analytic philosophers would have found the following picture self-evident: Much, though by no means all philosophy involves the generation of ingenious cases about which philosophers have relatively strong and consistent intuitions; such intuitions are a significant source of evidence for philosophical analysis.

Of late this picture has come under attack. Some say 'intuition' is nothing more than a pompous word for 'belief', and that our beliefs are not—simply because they are our beliefs—a source of philosophical evidence. Some observe that intuitions are supposed to have various hallmarks—intuitions are supposed to have a particular phenomenology or issue simply from insight into conceptual structure; they complain that they are unaware of any such phenomenology and dubious about conceptual structure. Some say that since intuitions vary with the culture of their possessors, their usefulness as evidential fodder is compromised or worse.

In what follows, I will defend a version of the picture most of us used to find self-evident. That picture, I think, reflects something important about philosophy and one of the reasons it is worthwhile doing it. My plan is this. I first say what I take intuitions to be. I then say something about the idea that philosophical analysis involves (but is not exhausted by) conceptual analysis. I think there is something to this idea—something, I hasten to add, that even a Quinean could endorse. I will point out how intuitions, understood in the way I propose to understand them, obviously provide evidence for conceptual, and thus philosophical, analysis. I then compare
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the view I sketch with that of Herman Cappelen, who is no friend of the idea that philosophy needs intuitions.¹ There is, I think, not all that much distance between the view I outline here (and the views of many others who think that intuitions are philosophical evidence) and Cappelen's view. Some of the difference between Cappelen and the advocates of intuition is merely verbal. There may, however, be at least one substantive difference between Cappelen and me, one that has to do with what we can reasonably expect philosophical analysis to deliver.

I say that philosophical analysis is (in part) conceptual analysis. But I do not say that it reveals conceptual truths in a very meaty sense of 'conceptual truth'. There are, as I see it, no analyticities; nothing —well, nothing of philosophical interest --is a priori. For awhile, the idea that philosophy's project was (in part) to discover the analytic and the a priori was on the wane. But of late it has enjoyed something of a resurgence. In the last section of this discussion, I address a recent attempt to resuscitate (at least part of) the tradition that sees analytic and a priori truth at the center of the philosophical enterprise, an attempt due to David Chalmers.² There I try most directly to show that responses that Chalmers makes to Quine are unsuccessful in showing that there is a viable notion of analyticity or a priori truth. In the course of this, I also sketch a broadly Quinean view of individual learning and belief revision that I hope coheres with and reinforces the account of concepts that the first part of the paper suggests.

¹ Herman Cappelen, *Philosophy Without Intuitions*.
² What I address directly is Chalmers' 'Revisability and Conceptual Change in "Two Dogmas of Empiricism", *The Journal of Philosophy*. But what I say bears pretty directly on the project Chalmers undertakes in *Constructing the World*.

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I

I won't try to define 'philosophical analysis'. But it is the sort of thing that philosophers are doing when they offer or criticize what are meant to be illuminating accounts of the conditions under which objects have a property or relation. It's the sort of thing you find when the philosopher, after 20 or so pages of Chisholming away at various definitions triumphantly displays something of the form of

(K) S knows that p iff….

and declares the Gettier problem solved.

A case, as I shall use the term, is a description of a(n apparently) possible situation. Intuitions are things that are made manifest by strong, relatively stable inclinations to apply something predicative –a phrase or a concept –to something as described in the case.

This doesn't yet tell us what intuitions are, but it does, I think, have implications about what they are not.3 There is no need for an intuition to involve distinctive phenomenology, for example, since the strong and stable inclinations that manifest them generally do not. Intuitions presumably don't correspond to spontaneous or "snap" judgments, at least not to ones that are immediately accessible to consciousness: Confronted with a putative counter-example to an analysis of (say) x acts freely in F'ing, I may at first not know what to say about it, in part because I "go back and forth" between an inclination to think the case is a case of free action and an inclination to

3 I assume that the norm is that such inclinations are manifestations of intuitions.

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say it is not. Indeed, we have all found on occasion that our intuitions are at war with themselves: I may after reflection have a strong inclination to say that something is a case of free action (perhaps in part because it so clearly patterns with paradigms of free action) as well as a strong inclination to say that the case is not such (perhaps because it has elements that I am committed to saying are incompatible with free agency).

So what, exactly, are intuitions? There are, I think, two primary possibilities: they are psychological states—judgments or inclinations to judge—focused on propositional contents, or they are the contents of some such states. The dominant use of 'intuition', I think, identifies them with judgments about possible cases, so that (for example) my intuition about Goldman's barn case is either my making a particular judgment about it, or is the content of that judgment. Because conflicting intuitions need not issue in judgments, I don't think this is the best way to use the term. Better, it seems to me, to identify intuitions with either strong and stable inclinations to make a judgment about a case, or with the content of the judgment one is thus inclined to make. I will be non-committal about the content of the relevant judgments, though I am inclined to endorse the idea that, for example, the content of the intuition that Alvin Goldman is focused on, when he presents the case of Henry driving about fake barn country, is something like

p1: It is ('metaphysically') possible that someone be as Henry is in [here insert Goldman's description of the case] but not know that he is looking at a barn.\(^4\)

\(^4\) Ref Malmgram 2011.
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But nothing I will say turns on niceties about the content of an intuition. Goldman’s intuition is thus either his strong and stable inclination to judge p1, or it's the content of that potential judgment. Which one is it?

II

Philosophers who toil at finding biconditionals like (K) often call what they are up to conceptual analysis. I take it that those who use this monicker think that a successful philosophical analysis would among other things tell us something not just about a property or relation but about our concept of it. The idea, I take it, is that a philosopher who offered

(K’) Knowledge is reliably generated true belief

as an analysis or philosophical account means to be doing two things: (1) she's telling us that the relation of knowledge is instantiated just if the properties and relations mentioned on the right side of (K’) are instantiated in the right way; (2) she's also telling us that the concepts of being reliably generated, being true, and being a belief are "part of", or "help constitute" our concept of knowledge. The idea, that is, is that when philosophical analysis is successful, it tells us something about "conceptual structure". I hereby dub this idea CIA.

CIA needs elaboration. It is not always clear what philosophers have in mind when they speak of concepts, and it is certainly not clear what

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5 along with something that fixed the meaning of 'reliably generated'

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conceptual structure is supposed to be. And it is not totally clear why an
analysis of, say, the concept of knowledge—which one would think is
something that is in some broad sense psychological—would be helpful to
someone who was looking for illumination about the relation knowledge.

On any way of understanding the philosopher's talk of concepts,
concepts have a semantics: they (or their applications) can sensibly be said
to be true or false of objects. Some see concepts as in some sense "internal"
and thus idiosyncratic, others as in some sense "external" and common
property. On the first view, concepts are categories: enduring
psychological structures that are involved in classification, are in some way
involved in occurrent beliefs and memories, and are, in language users, in
some important way connected with the meanings of the words we use to
form their canonical names. Concepts are naturally thought of as
idiosyncratic to their possessor, as they are structures naturally individuated
in terms of knowledge and perceptual abilities that vary across individuals.
Others think of concepts as shared by different individuals, so that, for
example, normal adult humans have the same concepts of (physical) object
and agent and normal English speakers express the same concept with the
words 'accident' and 'mistake'. If you think of word meanings as one kind of
concept and think that members of a single language community typically
mean the same thing when they use everyday vocabulary items, you will be
probably be partial to this view.

If CIA is part of a proposal about how to best understand
philosophical practice, we should take the concepts philosophers analyze to
be shared ones. For whatever the target of such analysis may be it is

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6 By this last I mean, for instance, that there is an intimate relation between the meaning
of my word 'cat' and my concept of a cat.

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something that is public in the banal sense that when different philosophers try to give an account of knowledge, or reasons for action, or reference, or whatever, they presuppose that they are all trying to give an account of the same thing. I infer this last bit from the way in which (what is usually identified as) philosophical analysis proceeds. If you look at arguments over cases and over what our intuitions about them tell us, it is striking that we do not argue from intuitions—that is, from judgments about cases—that we do not take to be widely shared; the weight we are willing to assign to an intuition seems to be a function, not of how strong our own inclinations are, but of how widely the intuition is shared. And one just doesn't see philosophers retreating, when their intuitions are disputed, by saying that they are only trying to give an account of their own concepts. We all agree that if A's analysis of knowledge is correct and B's intuitions conflict with it, B's intuitions are messed up, even if they accurately reflect his idiosyncratic concept of knowledge.

Suppose this much is accepted. What, exactly, is the conceptual structure that analysis illuminates? What is it, for p's being true to be "part of the concept" of my knowing p?

Well, we are thinking of a concept as something that we first and foremost share with others. Many, perhaps most, of our concepts are acquired from others: We learn the rudiments of concept application from others; we work with them to decide how to apply concepts in difficult cases; when differences over application are manifest, we argue and negotiate. Even when a concept (apparently) has an innate basis—as,
presumably, the concepts object and actor do), their contours are elaborated through social interaction.

Having a concept is in part a matter of being connected to a social network, a group of people who make use of the concept in particular ways, who recognize one another as using the same concept, and who have mutual expectations about how the concept is normally deployed. Not only is it the norm, that those who can think of someone as a brother will think of someone as such only if they think of him as a male, it is the norm that we expect those who can think of someone as a brother to think in this way—we know that there is something abnormal, in need of explanation, if someone thinks not all brothers are male.

Let C be a concept that is expressed in English by c; let p be the claim made by some sentence $\Phi(c)$ in which c occurs. Say p is part of the explicit content of c if it's such that it's mutually known by those who have c that those who have c are expected to believe p. I know, and you do too, that if someone knows what a brother is, they will expect people who know what a brother is to be male. Not only do you and I know this, you and I know that you and I know it. And so on. The claim that brothers are male is part of the explicit content of the concept brother.

There is no need for the explicit content of a concept as just defined to be true. Arguably it was once part of the explicit content of the concept whale that whales were not mammals. Many of the arguments given by Putnam against traditional accounts of kind terms are examples of cases in which it turns out that part of the explicit content of a concept is false.

It seems to me that the explicit content of a concept will usually be of limited philosophical interest. There are, of course, many things that are mutual knowledge about knowledge that no one has brought to
consciousness – for example, that plankton don't know much about physics. But one doesn't design thought experiments to get at explicit content.

So what is one getting at, when one comes up with an ingenious example like Gettier's or Goldman's? Well, the explicit content of a common concept hardly exhausts its content. Take Austin's shopworn but nice examples from 'A Plea for Excuses' that illustrate the difference between doing something by mistake and doing it accidentally. Is the difference these illustrate part of the concepts' explicit content? Do all or even most competent speakers know, before they read Austin's footnote, that someone who knows what it is to do something by accident expects anyone (who knows what accidental action is) to know that in the case in which the donkey Austin aims at moves and he thus shoots the neighbor's donkey, it was done by accident, not by mistake?

I would say no, and not just because most people are unfamiliar with the Austin example. Most people who have the concepts of acting by mistake and acting accidentally have not thought very hard about them. They have picked up the concepts by seeing them applied in various cases, acquiring dispositions that more or less match those of everyone else, at least in a broad range of everyday cases. They have stable inclinations to apply the terms, ones that overlap with those of others, but they have not articulated those inclinations to themselves or to others. When the competent speaker reads the footnote and judges this one's by mistake, that one's by accident, she is not applying an explicit rule from which the judgment is an easy consequence. She is not doing something that she had a prior expectation that she (or anyone else) would do; neither did she have prior knowledge from which such an expectation is an easy consequence. This is not to say that she didn't know what it is to do something by mistake.
or by accident; it is rather to say that to have that kind of knowledge does not require very much in the way of conceptual articulation.

What is surprising is that while most people cannot articulate the difference between mistake and accident\footnote{As Austin observes, many people will say that they are the same thing.}, almost everyone immediately 'gets' the example and makes the judgments about them Austin expects. There is presumably something about our practice of labeling things as mistakes or accidents that leads to convergence here –there is some set of properties and relations, or some degree of some magnitude, or something else made manifest in the example, to which our classifications are sensitive, and which explains our convergence. When this sort of thing is true of a concept, and the something is not exhausted by its explicit content, say that the concept has implicit content.

Implicit content is implicit, and it needs to be articulated. Though pretty much everyone agrees in their judgments about Austin's cases, it is actually pretty difficult to project from the cases to an account of the difference. Such an articulation would involve claims along the lines of ones like

\[(A) \quad \text{We take such and such properties to suffice for something to fall under the concept } \textit{accident}\]

\[\text{We take such and such properties to be are necessary for something to fall under the concept } \textit{mistake},\]
where the properties in question are ones, sensitivity to which explains our converging judgments. Since implicit (along with explicit) content is what sparks application, and our common patterns of application can be erroneous, articulations of such content need to be made in this form, though of course it will often turn out that something stronger can be said, something along the lines of

\[(A') \quad \text{Such and such properties suffice for something to be an accident}
\]
\[\text{Such and such properties are necessary for something to be a mistake.}\]

As I see it, those who think that philosophical analysis involves uncovering conceptual structure think such analysis aims at uncovering implicit conceptual structure in the sense I've been trying to elucidate. Their hope is to come up with truths that look like the claims in (A); they suspect that often enough those truths will lead to truths like those in (A').

III

It seems to me *obvious* that many philosophers have understood what they were up to as conceptual analysis in more or less the sense I have been trying to isolate. Certainly Austin and other ordinary language philosophers did. Austin counsels us that at least sometimes in philosophy
…we are to proceed from "ordinary language", that is, by examining what we should say when, and so why and what we should mean by it.\(^9\)

…ordinary language … embodies … something better than the metaphysics of the Stone Age, namely…the inherited experience and acumen of many generations of men. …If a distinction works well for practical purposes in ordinary life (no mean feat, for even ordinary life is full of hard cases), then there is sure to be something in it, it will not mark nothing: yet this is likely enough to be not the best way of arranging things if our interests are more extensive or intellectual than the ordinary. …[Everday] experience has ..not been fed from the resources of the microscope and its successors….superstition and error and fantasy of all kinds do become incorporated in ordinary language…(only, when they do, why should we not detect it?).

Certainly, then, ordinary language is not the last word: in principle it can everywhere be supplemented and improved upon and superseded. Only remember, it is the first word.\(^{10}\)

Austin is suggesting here that philosophy at least begins by understanding the conceptual connections and distinctions that are explicit and implicit in the "experience and acumen of many generations", which is what I just suggested we take to be the structure of a shared concept. I would have thought that it was obvious that a good many philosophers --Plato and

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\(^{9}\) 'A Plea for Excuses', 7.
\(^{10}\) Ibid., 12.

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Moore strike me as good examples – would recognize themselves as engaged in cognate investigations.

I imagine that some will say that this a misleading picture of philosophical analysis. When, for example, Alvin Goldman reasons about the case of Henry in fake barn country, he reasons quite explicitly to an account of knowledge, not to an account of our concept of knowledge. No matter what Austin might say, he is after an account of the difference between a mistake and an accident, not an account of the difference between our concepts of these. Save when they are doing some part of philosophy of mind concerned with concepts or representations, philosophers are concerned with analyzing properties not concepts of properties.

To this, I respond that the suggestion was not that philosophical analysis was concerned solely with concepts and their structure. Of course we are as much interested in the properties and relations our concepts are concepts of as we are in the concepts themselves.

One might counter that dragging concepts into an account of philosophical practice only obfuscates it. A philosophical account of, say, knowledge or free action aspires to (something like) a true, illuminating, and (possibly) necessary account of what is necessary and sufficient for someone to know p or to act freely in doing such and such. "Applying the concept of knowledge to cases" is nothing more or less than thinking about what's necessary or sufficient for knowledge. The idea that we are thinking about our concepts when we are thinking about knowledge is about as plausible, it might be said, as the idea that we are looking at a mental image when we are looking at a barn.

One reason to think that this response is too hasty is that it ignores the possibility – a possibility that is often a probability – that philosophical
analysis may be a worthwhile enterprise in cases in which there is no property for the analysis to be an analysis of; all there is for us to analyze in many cases of philosophical interest is our concepts.

Take free action as an example. Some philosophers tell us that to act freely would be to perform an act, the performance of which was not determined by conditions over which one has no control. Others tell us that to act freely is, roughly put, to perform an act such that one could have decided not to perform it (and would not have performed it, had one so decided). Yet other accounts are on offer. There is no consensus amongst philosophers—or amongst non-philosophers, for that matter—about which of these accounts of free action we should endorse. This is in good part because all of the accounts have—I hope I will be allowed to put it this way—considerable intuitive appeal. Each of them invokes elements that are more or less central to the way we think about free action, elements that we are loathe to write out of our way of thinking of it.

Why should we think that when we use the phrase 'free action' in speech or token it in thought, it is determinate that we are picking out the property isolated by one as opposed to another of these candidate analyses of free action? I do not ask this rhetorically. I am open to being convinced that we do determinately mean something more or less co-intensive with one of these analyses, or with one that no one has been clever enough to hit upon yet. But it seems obvious that: (a) we are owed an argument for this, given that our intuitions about free action are divided and wobbly; (b) it is not at all implausible that 'free action' does not determinately denote; (c) if it does not, then all those interested in philosophical problems linked to the notion of freedom can do is to describe the varying strands in our concept of free action.
action and make recommendations, based on the interests we do or might have, as to how we might eliminate the vagueness of the concept.\footnote{I should point out that point I am making does not depend on what account of vagueness we adopt. The epistemicist about vagueness, for example, will say that if 'acts freely' is vague in the way I have suggested it might be, then while there is a fact about what property the phrase picks out, it is unknowable what that property is. But if it is unknowable, there doesn't seem to be much of interest for philosophers concerned with free action to do, beyond conceptual delineation and normative recommendations.}

I do not say that all or even most of the notions philosophy investigates suffer from the kind of indeterminacy I've suggested may infect the notion of free action. But I do say that it is very probable that some, and not improbable that many, do.\footnote{Nor do I say I am the first person to suggest that this is so. See, for example, Peter Unger's \textit{Philosophical Relativity} (Minnesota UP, 198X).}

There is a somewhat different sort of indeterminacy from which many notions of philosophical interest arguably suffer, one that gives another reason to interpret philosophical analysis as at least in part a kind of conceptual analysis.

Consider my dog, who is capable of rudimentary thought. She can, for example, think that I have thrown a ball. Many such thoughts are presumably realized by mental structures that deserve to be called concepts – structures that are used in categorization, that are invoked in memory, are implicated in the planning and rudimentary reasoning the dog engages in, and so forth. Consider, now, the project of determining the extension (or possible worlds intension) of the canine ball-concept that helps realize the dog's belief that I just threw a ball. The project is not obviously absurd, and might even be worth contemplating, if only because it raises interesting questions about interpretation and intensionality. What \textit{is} absurd is the suggestion that it is even close to determinate, for every ballish x, whether
the dog's concept is true of x. Surely my Sheltie's dispositions to behavior and her "knowledge of the world" support neither the claim that the concept mobilized when she thinks I threw a ball is true of a football the size of a car, nor the claim that the concept is false of such.

An investigation into the semantics of Sheltie thought is an investigation in semantics, and thus an investigation into what properties the dogs are thinking about, in some sense of 'property'. But the properties in question are partial. And to get a grip on the way in which they are partial, we have no choice but to look at the structure of the canine concept and its deployment. Why exactly should we think that it is any different when it comes to the analysis of human thought? It is a banal observation indeed that we have not anticipated all of the cases in which we might be puzzled as to whether a particular word or concept applies. It is a less banal, but no less correct, observation that our dispositions, world knowledge, and environmental relations do not come close to determining what our reactions to novel cases will or should be.\(^\text{13}\) It is implausible, in my opinion, that an appeal to some metric of naturalness will erase very much of the indeterminacy.

Like the dog's, our thoughts are partial in the sense that it is very often a vague matter whether their predicative elements are true or false of the things we are thinking about –there is either no fact of the matter, or there might as well be no fact, since the facts are unknowable. As in the case of Sheltie semantics, to get a grip on what we are thinking, we have no choice but to look at the structure of our concepts and their deployment. If we as philosophers are interested in what we are thinking, when we think about

\(^{13}\text{For a budget of examples see Wilson 1980, 200X.}\)

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knowledge, freedom, or the good, we have no choice but to pay as much attention to our common conceptual structure as to the properties that structure might be reaching towards.

IV

Intuitions are supposed to provide evidence for philosophical analysis. Insofar as one of the targets of such analysis is implicit or explicit conceptual structure, surely they do. When there is a widespread intuition about a case, this is sometimes best explained by supposing that that intuition reflects such structure. When we so explain the intuition, the intuition has the status of evidence for the explanation we give.

A concrete example is Goldman's discussion of Henry, who has unknowingly driven into an area where there are many fake barns and who, looking at a real barn, thinks that it is a barn. Goldman observes that if we are only told that Henry is driving around Pennsylvania, sees a barn and thinks 'that's a barn', we will be inclined to say he knows that he sees a barn; if we are also told about the fake barns, we are inclined to say that in such a case, Henry doesn't know that he is looking at a barn.

Goldman considers various accounts of knowledge that validate the intuition and settles on one of them, on which knowing p requires that one's belief that p be reliable in a particular way—the knower must be able to discriminate the actual state of affairs p from various relevant possible alternatives.\(^\text{14}\) Goldman explicitly argues for this proposal by observing that our "inclinations" to make judgments about various cases are correlated

\(^{14}\) Reference to Goldman.
with whether the case is described in such a way as to presuppose that Henry is unable to discriminate the area's fake barns from its real barns:

A person knows that p…only if the actual state of affairs in which p is true is distinguishable or discriminable by him from a relevant possible state of affairs in which p is false. …In the original description of the barn case [Case C, call it, where there is no mention of fake barns] there is no hint of any relevant possible state of affairs in which the object in question is not a barn but is indistinguishable (by Henry) from the actual state of affairs. Hence, we are initially inclined to say that Henry knows. Given that the district Henry has entered is full of barn facsimiles, there is a relevant alternative …state of affairs …[that] is indistinguishable by Henry from the actual state of affairs…. So, once apprised of the facsimiles in the district [in Case C'], we are inclined to deny that Henry knows.15

Call the property of Henry's belief that we sense to be lacking in the full blown case reliability. Part of the argument here pretty clearly involves premises like:

1. We have a stable and strong inclination to say that Henry's barn belief is knowledge, when given case C, and a stable and strong inclination to say that Henry's barn belief isn't knowledge when given Case C'.

15 Cite p. 774.

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2. In case C, 'there is no hint' that Henry's belief is not reliable; in case C', there is a suggestion that it is not.

If we assume, as surely Goldman is assuming, something along the lines of

3. Nothing else about the cases is relevant to explaining the difference in our inclinations towards C and C'

we are in a position to explain 1 with:

4. Our stable and strong inclinations about cases like Henry's and about whether a belief is knowledge are sensitive to whether or not it appears to us that the belief is reliable.

But of course this pattern of explanation is one in which we explain the existence of an intuition –that is, we explain (1) –with a hypothesis about conceptual structure. If we add to (4) something else that Goldman is presumably assuming

5. When our judgments about whether something is F are sensitive to whether it is G, that is a reason, all else being equal, to think that Fs are Gs.

we are in a position to conclude that we have reason to think that knowledge requires reliability.

It seems to me that many passages in analytic philosophy of the last 50 or so years, in which philosophers make points by appeal to examples,
should be understood in much the way I'm proposing that this passage should.

V

I want at this point to compare my take on the role of intuition in philosophy with Herman Cappelen's. Cappelen is no friend of the idea that intuitions have a role in philosophy, and I am. But the distance between our views is not so great, and it is, I think, illuminating to see exactly what it is.

Acknowledging that there are different accounts of the nature and role of intuition, Cappelen focuses on the view that

A. Intuitions are:
   1. mental states with propositional content, either sui generis states ("seemings") or beliefs with a particular etiology;
   2. which have a distinctive phenomenology;
   3. that are based solely on conceptual competence;
   4. and have a special evidential status –they are in some sense "rock bottom" and need no justification.

B. Analytic philosophers rely on intuitions so characterized for evidence (at least as a source of evidence).

Cappelen is inclined to dismiss the idea that there is a distinctive phenomenology that accompanies what philosophers are voicing when they express intuitions, observing that he is unaware of any such funny feelings when he reads the work of Gettier, Goldman, or Kripke. I, too, am an

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intuition zombie. Let us agree to strike A.1 from the characterization of intuition; call the resulting job description for intuitions I.

Cappelen for the most part attacks views on which intuitions are the contents of beliefs that we give voice to when confronted with philosophical cases. For example, in discussing Goldman's barn example, Cappelen identifies the putative intuitions that Goldman would describe himself as invoking as consisting in 'two claims':

C1: In the first scenario [when fakes are not mentioned], Henry does know.

C2: In the second scenario, Henry does not know.16

On this interpretation, intuitions are beliefs *cum* contents of judgments about cases. It is not hard to show that these don't fit the job description I: As Cappelen argues, C1 and C2 not candidates for "rock bottom" evidential status, and Goldman is not well understood as thinking otherwise.

All this marks a difference between Cappelen and me, but at least some of the difference is merely verbal. I do not identify intuitions with judgments about cases nor with the contents of such judgments. Intuitions are, rather, certain psychological facts that often result in our making judgments with the relevant contents. I gave reason for thinking of intuitions in this way at the beginning of the paper: a single person's intuitions are often in conflict; we don't want to say that just because someone has conflicting intuitions, they have inconsistent beliefs.

16 *ibid.*, 171-2.

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Do the psychological facts with which I say we should identify intuitions have "rock bottom" evidential status? Well, frankly, I'm not a fan of talking that way about any evidence, being myself something of a fan of Quine's 'Two Dogmas'. But surely such facts have, for the person whose psychology they are facts about, pretty secure evidential status. You could probably convince me –with a lot of work –that I don't have a stable and strong inclination to judge that Henry doesn't know in the second scenario, just as the doctor in an old example of Keith Lehrer's manages to convince a patient that a state she reports as a pain isn't really a pain but a itch. But this doesn't mean that my knowledge about my inclinations isn't evidentially basic for me in an important sense.

This difference is related to a difference between Cappelen and me about what is going on in passages like the one from Goldman, discussed in the last section. I see the passage as involving, among other things, an abductive inference; Cappelen does not. Cappelen's main complaint about interpreting it as I do is that

in an abductive inference with $c$ as the *explanandum* and $T$ as the *explanans*, the question of whether $c$ is the case is typically not under discussion.\(^{18}\)

The point is well taken. But surely what we should conclude is *not* that the relevant passages are not best reconstructed as abductive arguments. Rather, we should conclude that the *explanandum* of the argument is the fact that the philosopher –and (the philosopher assumes) most other philosophers –make

\(^{17}\) Get reference; in *Knowledge*, if memory serves.
\(^{18}\) *ibid.*

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the relevant judgment. *That* is not up for debate; I'm sure Cappelen would not disagree.

Are intuitions in my sense thereof "based solely on conceptual competence"? Well, again, as a fan of Quine I am no fan of this way of talking. And, in any case, as I have characterized intuitions they may be result of any number of things, including contingent, collateral knowledge about the property a case is supposed to focus us on. This is why, in my reconstruction of Goldman's argument, the final premise –the one that gets us from facts about concepts to ones about properties--is hedged with a *ceteris paribus* clause. Intuitions are the result of our applying our concepts to descriptions, without "doing empirical investigation". As things stand, they are a way to get some evidence about what we are and are not sensitive to, in applying a concept. I suppose I agree with Cappelen that saying that intuitions are "based solely on conceptual competence" is a bad way of describing them. It doesn't follow that they aren't a source of evidence for what I called above conceptual structure.

Cappelen and I don't disagree that there are intuitions, characterized as I have been characterizing them. We do seem to disagree about their role in philosophical argumentation, as well as about what it is that philosophical analysis is and should be trying to do.

The disagreement is not, I think, really about the range of things that play an evidential role in philosophy. Cappelen thinks that just about anything might play an evidential role in philosophy; he believes, I think, that the idea that intuitions have a distinguished role in philosophical evidence badly –*very* badly --overestimates their importance. I agree that just about anything can play an evidential role in philosophy, and that a great
deal of what is evidence in philosophy comes not from gazing at our inner *omphalos*, but from the sciences.

I think where Cappelen I differ most fundamentally is about what philosophers are doing, when they try to give accounts of things like knowledge or free action. Cappelen's view, I think, is that they are simply trying to say something illuminating about properties and relations, and that our concepts of these properties and relations are of no particular philosophical interest. Myself, I think we have no choice in philosophically interesting cases than to proceed cautiously, open to the possibility that there is no property or relation that our words and concepts are directed on—not because those concepts are as empty of content as the concept *phlogiston*, but because they are often massively partial or painfully indeterminate. Since this is generally an open possibility, and surely sometimes how things in fact are, philosophical analysis has to be conceptual analysis, for often there is nothing else for it to be.

**VI**

I turn now to Quine on analyticity and belief revision, and David Chalmers' responses to Quine. As I said at the outset, I have two goals. One is to show that Chalmers gives us no reason to think, in the face of what Quine says in 'Two Dogmas', that there is notion of conceptual continuity that would serve to make sense of analyticity or a meaty notion of *a priori* truth. In the course of doing this, I will say something about what a Quinean alternative, to the popular Bayesian account of learning and evidence, might look like.
Quine made at least two important points about analyticity and conceptual change in 'Two Dogmas'. One was about rational constancy in belief: Because of broadly Duhemian considerations, Quine claimed, one could rationally continue to accept a sentence "come what may". Quine's second point was that no sentence was immune to rational revision. Of course these points need to be qualified: one could, out of love for the sound of the sentence $S$, tenaciously accept it come what may by redefining its words; one might, as a lark, redefine 'or' to mean 'and' and consequently revise one's opinion about the truth of 'Bob is here or Bob is not here'. Presumably the qualification needed is something like: one may rationally accept any sentence come what may / revise belief in any sentence without there being good reason to say that one means something different by the sentence than what one formerly did.

Chalmers presents two related responses to Quine. One is a "refined" version of Carnap's response in 'Meaning and Synonymy in Natural Language'. Simplifying madly, Carnap suggested that the meaning-cum-intension of a predicate in an individual's idiolect is determined by her (potential) judgments about cases. Suppose, for example, that we describe enough of a "possible scenario" to a speaker and ask if an object therein is a unicorn. So long as the speaker doesn't make mistakes in reasoning, her answer tells us whether the intension she associates with 'unicorn' maps the individual in the described scenario to the true or the false. Chalmers' refinement of this idea is this:
...one can define the intension of a sentence $S$ at a scenario $w$, for a subject, in terms of the subject's rational conditional credence in $S$ given $D$, $cr'(S/D)$, where $D$ is a canonical specification of $w$. We can say that the intension of $S$ is true at $w$ iff $cr'(S/D)$ is high, and false at $w$ iff it is low. Here we require an idealization, so that $cr'(S/D)$ is the conditional credence $S$ would have given ideal reasoning, or something along those lines. (399)$^{19}$

This Carnapian view, of course, purports to define something like the meaning of a sentence, and so, if defensible, offers a basis for criteria for conceptual change and analyticity.

Chalmers' second proposal dispenses with talk of possible world intensions and specifications of possible worlds; it deals simply in conditional credences. Chalmers tells us that the Bayesian assumes the truth of some principle like

(CS) Let $t_1$ be later than $t_2$; let $cr_1$ and $cr_2$ assign a subject's credences (both absolute and conditional) to her sentences at $t_1$ and $t_2$ respectively. If the subject is fully rational and $E$ specifies the total evidence she acquires between $t_1$ and $t_2$, then if the content of $S$ does not change between $t_1$ and $t_2$, $cr_2(S) = cr_1(S/E)$.

If we accept that something like (CS) is true, we again have something like a criterion for conceptual change, and can hammer a criterion of analyticity

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$^{19}$ References to Chalmers are all to 'Revisability and Conceptual Change' and are indicated parenthetically.

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out of it in an obvious way. In what follows, I concentrate on the response to Quine based on (CS).

In discussing examples, Chalmers uses a criterion suggested but not entailed by (CS) to assess conceptual constancy. Consider the claim that cats are animals, a claim which seemed before Quine to be analytic and unrevisable. Doesn't the Putnamian example, in which we learn that cats are Martian robots, suggest that Quine was right? After all, we would pretty surely conclude, in that scenario, that cats turned out not to be animals. Doesn't seem like we're changing the subject in doing this, so the sentence must not have been analytic. Right?

According to Chalmers, we do not have to consider specifications of the total evidence someone might acquire over time in order to see that Putnamian arguments are less than conclusive. Let "E specify evidence confirming that the furry, apparently feline creatures that inhabit our houses are actually remote-controlled robots from Mars, while the other creatures we see are organic." (402) Suppose Sarah accepts

\[ C: \text{Cats are animals} \]

at t1, then accepts E, and consequently rejects C at t2. Let \( crl \) be Sarah's t1 credence function. If \( crl(C/E) \) is low and Sarah is rational, this, Chalmers tells us, suggests conceptual constancy on Sarah's part. And we can thus conclude that C wasn't analytic for Sarah. On the other hand, if \( crl(C/E) \) is high, then either Sarah changed her conditional credence for C given E—and so, says Chalmers, the meaning of C –between t1 and t2, or she is being

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20 We must, harmlessly, assume that Sarah considered E possible at t1.

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irrational in rejecting C. Either way, Sarah's judgment does not show that one can rationally reject C without assigning it a meaning different from the one she assigns it at t1.

There is an obvious response to this proposal, ones that anyone sympathetic with Quine's views could be expected to give.

Let us grant for argument's sake that if there is such a thing as $cr_1(C/E)$, then one may argue as Chalmers does. If this is undefined, however, it is surely not apt to say that 'cats are animals' is analytic. In this case, rationality does not demand that Sarah, if she holds the meaning of the sentence constant, accept it come what may –she has, as of t1, no opinion about what credence to assign to C given E. And if $cr_1(C/E)$ is undefined, Sarah underscores Quine's point about revisability. For it seems that Sarah can quite rationally hold the meaning of 'cats are animals' insofar as it has a determinate meaning constant and think whatever she likes about feline animality after she accepts E.

And it is in fact pretty difficult to convince one's self that $cr_1(C/E)$ will be defined, if Sarah is a normal non-philosopher. The problem is that our linguistic training and extra-linguistic opinions or knowledge often don't determine what we would –much less should –say about many possible (and even actual) cases. No one laid down rules about what to say, should we discover that the things we call 'kitty' are and always have been remotely controlled robots; who knows what we would in say about such a case? We might have a response to a detailed description of a scenario of the sort Putnam describes; but why think that prior to hearing the description there was a particular reaction we needed to have, if we were to maintain what we meant by C? This, of course, is essentially the point Putnam makes in discussing his example:

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Once we find out that cats were created from the beginning by Martians….it is clear that we have a problem of how to speak. What is not clear is which of the available decisions should be described as the decision to keep the meaning of either word ('cat' or 'animal') unchanged, and which decision should be described as the decision to change the meaning. I agree with Donnellan that this question has no clear sense.\textsuperscript{21}

Suppose that Putnam is right about this. Is there in that case any reason to think that $\text{cr}_1(C/E)$ is defined? Presumably there is nothing about the meaning of $C$ (as Sarah uses it) that would, along with a normal suite of beliefs, dictate that $\text{cr}_1(C/E)$ must be high, low, or around .5 –if there was, then neither she (nor we in her position) would have a problem about how to speak. We may fairly assume that Sarah has never considered a fanciful scenario like Putnam's and is, when she first hears it, at a complete loss about what to say about it. She will, of course, eventually come to some opinion about it –life goes on, after all. But it is hard to see why such a decision must reflect a prior, more or less determinate (constrained range of) value(s) for $S$ conditional on $E$ to which Sarah was committed.

My own feeling is that if the argument to this point is successful, then with enough ingenuity one can show that Chalmers' response to Quine does not show that Quine was wrong about rational revisability: we can accept

\textsuperscript{21} Putnam 1962, 660-1.

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what Chalmers says and still insist that pretty much no sentence is immune to rational revision (without "change of meaning").

It will perhaps be said that I am rejecting the entire Bayesian framework for understanding rational change of belief, since it can't be applied to someone unless she has a well-defined conditional credence function. For that matter, one might object, it follows from the fact that someone has a credence in C&E and a credence in E that they have conditional credence (C/E), equal to the first credence divided by the second. Surely it's not too much to assume that people have the unconditional credences in sentences they understand. Sarah, for example, presumably thinks at t1 that C is very likely, E very unlikely.

There is no need to think that the Bayesian framework is inapplicable to a person who fails to assign conditional credences to every pair of claims. Suppose a subject's conditional and unconditional probabilities are (with some idealization) defined for humdrum (and not so humdrum) claims. That doesn't imply that they will be defined for the sorts of cases that might lead to our at least considering revision of what pass for analyticities. Why should this make the Bayesean framework inapplicable to the subject? Since these later sorts of cases never come up, confining a credence function to the cases that actually might come up isn't going to hamstring us. As for the second response: Even if conditional probability is definable via the ratio rule, it is no more plausible to think that the relevant unconditional

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22 My hedge here is due to some examples of Putnam's ('a hunter hunts, or at least is disposed to', 'not every contradiction is true'), which might have something like the status of analytic truths. I hope to discuss such examples elsewhere.

23 Some things Chalmers said in response to an earlier version of this section suggest that he is at least tempted by responses along these lines –though don't hold him to this.


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probabilities exist than it is to think that the conditional ones do. Grant for
the sake of argument that if either side of the equation

\[
(E) \text{cr}1(\text{cats are animals/the robotic cat story is true}) = \\
\text{cr}1(\text{cats are animals & the robotic cat story is true}) / \text{cr}1(\text{the robotic cat story is true})
\]

is defined it's true. Since Sarah (and Putnam and you and I) have no idea
what to say about whether cats are animals if the robotic cat story is true, we
presumably don't have much of an idea about what probability to assign to
'cats are animals and the robotic story is true'. If it has a probability, it's
somewhere between zero and the probability of 'the robotic cat story is true'
—which is just another way of saying (given (E)) that the conditional
probability of cats being animals given the robotic story is something
between 0 and 1, which is effectively a way of saying that we have no idea
what the conditional probability might be. There is no good reason to think
that either side of (E) is defined.\(^25\)

Now Chalmers will, and does, respond to such objections. He
remarks that

For most \(S\) and most \(E\), the subject will have some relevant
dispositions involving \(S\) and \(E\), for example, involving her willingness
to accept various bets involving \(S\) and \(E\). In many cases, these
dispositions will line up in a clear enough way that [it will be
determinate that the probability of \(S\) given \(E\) is determinately high or

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\(^25\) I hope it is obvious enough that Chalmer's first, Carnapian proposal for assigning
intensions to sentences is subject to the same sort of objection as his second is.

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determinately low]. In other cases, the dispositions will be enough of a mix that it is hard to say. (413)

He then observes that the Quinean might well say that in this last case \( \text{cr}(S/E) \) is indeterminate, so that (even if the subject's prior probability for \( S \) was extremely high), rejecting \( S \) does not show that one is violating (CS) or being irrational. And then, so long as we can Puntamize a sentence, it will not be unrevisable.

Chalmers in essence says that such cases are not possible. We can assume that the subject, on being confronted with the robotic cat story and accepting it, will eventually come up with a (new) probability for 'cats are animals'. But

If the subject if fully rational, then the subject's dispositions to accept \( S \) on \text{supposing} \( E \) and on \text{learning} \( E \) should be the same, assuming no conceptual change. That is, if a fully rational subject rejects \( S \) on learning \( E \) and thinking things through, then if the subject were to have been initially presented with the \text{suppositions} that \( E \) and had thought things through, the subject should have rejected \( S \) conditional on that supposition. (413)

Chalmers here assumes that, if one can rationally reach a conclusion about a sentence (it's likely to be true, 50/50 that it's true, no way it's true, …), that conclusion is unique in the sense that no other conclusion could have been rationally reached by that subject. For if this isn't so, the subject might quite rationally reach one verdict when she makes a supposition relevant to the
sentence, and with equal rationality come to another verdict about the sentence when she learns what in the previous case she supposed.

Accept this for the moment for the case in which the subject's conditional credence for S given E is defined. It is very hard to see why we would want to say this when the subject's credence for S given E is undefined. Imagine that Sarah and Sonny have tokened exactly the same sentences in their lives. They have identical conditional and absolute credences, given by a function $c$ on the field of the already tokened sentences plus relatively short (no more than 10,000 word) non-bizarre sentences. (By 'bizarre sentences', I intend ones like those that describe robotic cats or telepathic vixens.) Suppose $c$ is undefined for 'cats are animals' relative to the robotic cat story. Surely there is more than one way to rationally extend $c$. One extension, $cr_1$, makes 'cats are animals' unlikely given the robotic cat sentence, another, $cr_2$, makes it likely. Surely if this much is true, then Sarah might quite rationally adopt $cr_1$ after considering the robotic cat scenario while Sonny might, with equal rationality, adopt $cr_2$ upon learning it.

Now why should we suppose that the fact that Sarah rationally came to have $cr_1$ while Sonny came rationally to have $cr_2$ implies that if their places were switched –it is Sonny who considers the robotic cat scenario, while Sarah learns it –they must come to the same conclusions? There is nothing about their beliefs or their (partial) absolute and conditional credences –which are, remember, identical --that dictates that one should adopt $cr_1$ as opposed to $cr_2$. It is not clear that there needs to be an interesting difference in their dispositions to accept and reject sentences, conditional on supposition or acquiring evidence. Perhaps both see the same considerations as relevant to the question, would cats be animals if the
robotic scenario were true? They go back and forth. They review all the evidence and are moved sometimes by some considerations to say 'on balance yes, probably', at other times they are moved by other considerations to say 'on balance, probably not.' Given that it is equally rational to go either way, there is no reason to say that they are being irrational when they are so moved; if, after a point, they are moved one way and remain there, how are they being irrational?\footnote{You may be wondering whether we should describe the decision to adopt cr1 as opposed to cr2 as \textit{rational}. Isn't it perhaps better said to be \textit{arational}, a matter of a blind leap in the dark? Even if we choose to say that the choice here is \textit{arational}, it does not follow that there is anything \textit{irrational} about what Sarah or Sonny does. In any event, the argument at the end of this paper is meant to convince you, among other things, that what Sarah and Sonny do in a case like this is perfectly rational.}

\textbf{VII}

At this point I want to shift gears and discuss the relation between whatever facts there might be about meaning and our judgments of probability. Chalmers' proposal, I think, gets the relation between the two backwards. I will give an example, and then pontificate a bit.

In the words of the poet:

\begin{verbatim}
All along the watchtower, princes kept the view
While all the women came and went, barefoot servants, too.
Outside in the distance a wildcat did growl
Two riders were approaching, the wind began to howl.
\end{verbatim}
If you are like me, you didn't find this a bit odd when you first heard it. If you are like me, you have been able to recite it for years, and it has never struck you as in the least odd. But some do find it odd. Dave van Ronk, for example, comments 'A watchtower is not a road or a wall; you can't go along it.'

If my (and your) linguistic module had already made it determinate that one can't move along something oriented perpendicularly to the earth's surface, we should find the words of the poet infelicitous. Or we should at least have a 'Lucy in the Sky with Diamonds' / 'Oh he's trying to be psychedelic' attitude towards this verse. I don't, and I suspect that you don't either.

As I see it, a reasonable guess as to the explanation of why we don't have such a reaction is this. We have all, of course, acquired 'along' by hearing it applied to movement across surfaces more or less horizontal to earth's surface, thereby acquiring a category –that is, a disposition to label some movements as movements along a surface, and other movements as not such. A first guess about the nature of this category is that it is a representation that involves something iconic –let's call it an icon --that helps categorize perception of movement on a surface as movement along it. The icon also contributes something to determining when movement is not movement along the surface. For example, movement that deviates from the surface in the way a loop-the-loop would is not movement along the surface. But the fact that only certain sorts of resemblance to the icon prompt categorization does not imply that sorts of movement that would not trigger

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27 This part of a complaint that the poet wrote carelessly when he realized his audience would allow him to. van Ronk is cited at http://en.wikipedia.org/wiki/All_Along_the_Watchtower (accessed in December 2012). © Mark Richard 2013.
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along-categorization would be miscategorized if they were categorized as movement along a surface. The category is simply silent about certain sorts of movement. In particular, it does not classify vertical surface movement as along or as not along. It has been left open, whether such movement is movement along a surface.

Suppose this is the right story to tell about the category movement along a surface. It does not quite follow that we would have no dispositions that determine a assignment of a probability to Elevators travel along buildings or the princes were keeping watch all along the watchtower relative to one or another piece of evidence. After all, if we hear someone using these sentences, we will have one or another reaction to the use, given the situation we find ourselves in. But what the story just told about the category along suggests is that what dispositions we have are not ones that are determined simply by the concept along or by the meaning of the word 'along'. In particular, one expects that how we will react to novel uses of 'along' is highly contingent on matters that have nothing to do with the meaning that we assign to the word. If our first encounter with the phrase 'along the watchtower' is the poet's confident use, we will probably conform our use to his; if our first encounter is with van Ronk's, we will find the poet's use deviant. In either case, our reaction will not be (merely) reflecting pre-existing facts about what the phrase meant in our vocabulary.

Categorization often involves a natural but underdetermined extension of an existing category. The fact that a category will naturally be applied (withheld) in a certain way in a particular situation does not show that the category would have been misused or "had its meaning changed" if it had been withheld (applied). When a case is novel – when the question does this category apply in this sort of case? has never been posed – there may be a
very natural answer to the question. The fact that an answer to the question is natural, or even more natural than any other answer, though, does not by itself imply anything about whether the category "demands" that answer, or whether deciding the case in another way would involve a change in meaning or replacing one category with another.\textsuperscript{28}

This point carries over to our actual and hypothetical judgments about conditional probability. It may be extremely natural for me at time t in situation s to judge that it is very probable that y is more or less horizontal relative to the Earth's surface, given that there are Fs all along x. It does not follow that if I assign a different conditional probability at a later time in a different sort of situation, I have "changed the meaning" of \textit{along}. What determines the credence I assign to S given E is a rather fluid confluence of factors, some linguistic, some happenstance all. Variation in one factor can shift conditional credence. Since not of all of the determinants can be thought of as aspects of meaning, shifts in credence cannot be identified with shifts in meaning.

I said above that Chalmers' proposal about the relation between credences and meaning gets the relation between them backwards; at this point, I hope it's clear why I think this. Conditional and absolute credences, in so far as they are determinate to begin with, do not determine or constitute what our sentences mean. Rather, such facts as there are about meaning \textit{along with non-linguistic facts about our psychology} determine our dispositions to do such things as accept one sentence on the supposition that another is true; these facts, in turn, determine under idealization such facts as there are about conditional credence.

\textsuperscript{28} Some of what I say here echoes claims Mark Wilson has argued for. See, for instance, 'Predicate Meets Property', \textit{The Philosophical Review} 198X.

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Now, it might be said that while this weakens the force of Chalmers' response(s) to Quine, it does not altogether eliminate their force. The observations above turn on the fact that our concepts are underdetermined – that they have, as our parents used to put it, a certain open texture. Thus, we can expect cases in which there is nothing to be said, at least nothing that what our words mean requires us to say. But it doesn't follow from this that there is no such thing as conceptual content. It does not follow that there is no change in usage or in one's assignments of conditional probability that would not mark a change in meaning.

This response has a point, but the response –and Quine's position – need to be stated carefully if we are to see it. I don't think it was ever Quine's intention to say that no change in use could be a change in meaning. Quineans –this Quinean, at least –agree that if we this minute decide to stop applying 'duck' to ducks and instead this minute decide to apply it to larks, we have changed the meaning of 'duck'. Quine's point –one of them, anyway --was that no matter what transformation T in usage of an expression e one might envision –for example, from using 'duck' for ducks to using 'duck' for larks –there is nothing about the meaning of e that makes it impossible for e's use to undergo transitions that would eventually result in that usage undergoing T "while meaning is preserved" from point during the transition.

To be a bit more precise: Let us speak of patterns of usage of an expression at a particular time. Such patterns are made manifest by
dispositions to verbal behavior (as such occur in a particular environment).\textsuperscript{29}

Quine's (in my opinion) sound point was \textit{not} that there is no principled reason to think, of \textit{any} transition from one pattern of usage to another, that it should be thought of as change in meaning as opposed to change in belief. Rather, Quine's point as I read him was that for (just about) any patterns of usage $u_1$ and $u_j$, there could be a temporally extended sequence of abutting patterns of usage $u_1, u_2, u_3, ..., u_j$ which are such that: for no $i$ is there good reason to say that the meaning of 'duck' changes from $u_1$ to $u_{i+1}$\textsuperscript{30}; in $u_1$ the claim that 'duck' is true of (just) the ducks gives part of a good account of 'duck' 's usage (and there is no better one to be given); in $u_j$, the claim that duck' is true of (just) the larks gives part of a good account of 'duck' 's usage (and there is no better one to be given). But we would not want to say that the meaning of 'duck' in $u_j$ is the same as the meaning of 'duck' in $u_1$. We should conclude (as I am parsing Quine) that, contrary to tradition, there is no theoretically interesting notion of continuity of meaning or conceptual constancy: we can legislate that certain changes in usage in a sequence like that from $u_1$ to $u_j$ are to be said to be changes in meaning, not changes in belief; but we must remember that if we do this, we are legislating, not theorizing about linguistic practice. Call this Quine's transformation argument.\textsuperscript{31}

\begin{footnotesize}
\begin{enumerate}
\item The parenthetical remark is something of a departure from Quine. In so far as meaning is determined by (patterns of) use(age), I would want to wire into (such patterns of) use things like "causal connections" with the environment and sub-personal facts that determine such things as syntactic structure and semantic evaluation. If this makes me something of a flaccid Quinean, so be it; sometimes we have to go beyond rigidity.
\item Indeed: for each such $i$, there are good reasons to say that the meaning of 'duck' stays the same from $u_1$ to $u_{i+1}$.
\item To be clear: the point of the argument is that it is utterly arbitrary to say that an incremental change in use is a change in meaning as opposed to a change in belief. Though incremental changes in use can add up to what is intuitively a change of
\end{enumerate}
\end{footnotesize}

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One might reasonably say that the points in previous sections about indeterminacy don't show that Chalmers has no response to this sort of argument. Chalmers, as I understand him, could concede something like the point about possible transformations in use but insists that it doesn't show that there is no such thing as conceptual constancy / continuity in meaning. Let us think of actual and possible cases of conceptual change or constancy as sequences of usage $u_1, u_2, u_3, ..., u_j$ by a particular individual $U$. During each $u_i$, $U$ acquires some collection of evidence $e$, which we may assume to be specified by some sentence $E_i$ of $U$'s language. Chalmers' response to Quine might be put so:

According to Quine, for any sentence $S$ and individual $U$, there is a possible sequence of usages $u_1, u_2, u_3, ..., u_j$ of $S$ by $U$ that begin with $U$'s current usage of $S$ such that $U$'s credence in $S$ in $u_1$ is high, her credence in $S$ in $u_j$ is low, $U$'s shift in credence in the sequence is not irrational, and there is no good reason to say that the meaning of $S$ shifts between $u_i$ and $u_{i+1}$ for any $i$ between 1 and $j-1$.

Fair enough. But $U$ is supposed to be rational. Let $c_i$ name $U$'s conditional credence function (for the sentences of her language) during period $u_i$. Suppose that $c_1(S/E)$ is defined for the following values of $E$: $E_1; E_1&E_2; E_1&E_2&E_3; ...; E_1&E_2&...&E_{j-1}&E_j$. Since $U$ is supposed to be rational, $U$ updates her beliefs, between $u_i$ to $u_{i+1}$, by conditionalizing. So $c_{i+1}(S)$ will be $c_i(S/E_1&E_2&...&E_{j-1}&E_j)$. [Footnote: Meaning, the conclusion to draw is that the intuitive notion of meaning doesn't underwrite notions like analyticity or a priority, which (apparently) demand fine distinctions between change of meaning and change of belief.]

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Ei-1). But this means that from the outset what U meant by S involved (as we might put it) the thought that given E1&E2&…&Ei-1, S's probability would be cri(S/ E1&E2&…&Ei-1). So we have good reason to say that there is no change of meaning here. The fact that use can rationally change in the way Quine thinks it can just doesn't show that there isn't such a thing as conceptual constancy.

In so far as this is Chalmers' response to Quine, one might say, he could concede that there is (considerable) indeterminacy of meaning of the sort noted above\textsuperscript{32}. He can even concede (though I doubt that he would) that considerations about indeterminacy and open texture show that pretty much no sentence is analytic, since it is pretty much never determinate what to say about the sort of bizarre cases that pre-occupy philosophers. He could concede all this but still insist that conditional credence provides a crisp criterion for conceptual change and the basis of a response to Quine.

I don't think it does. The basic problem is that the view Quine is sketching in "Two Dogmas" is one on which Bayesianism, while a good first approximation to the truth about confirmation and rational belief revision, is not quite the right story to tell. And when one makes the modifications in Bayesianism Quine's account suggests, one sees that Chalmers' suggestions about what constitutes conceptual constancy can't do the work Chalmers wants them to.

Recall Quine's Duhemian reflections in 'Two Dogmas': "A recalcitrant experience….can be accommodated by any of various alternative reevaluations in various alternative quarters of the total system\textsuperscript{32} such indeterminancy being of a quite different sort than the sort of indeterminacy Quine argues for in Word and Object!"

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One way to state what Quine has in mind here is in terms of what ways of revising one's beliefs – thought of as assignments of conditional and unconditional credences – are rational. Quine's idea is quite simply that there will not be a single rational way to react to an experience, recalcitrant or otherwise. At any time a rational person has open to her a number of ways of changing her assignments of probabilities to claims. Her dispositions to react to and evaluate evidence and then change belief determine these strategies, but the determination is one-many.

How do we choose among them, if we are rational? Well, essentially in terms of pragmatic factors: for example, "our natural tendency to disturb the total system as little as possible" has a tendency to make us choose ways of updating our beliefs that ceteris paribus leave the absolute probabilities of certain claims (that there are brick houses in North Carolina, that cats are animals, that 1+3=4) more or less as they were. Of course other factors can play a role here – our desire for simplicity, feelings that one explanation is "more satisfying" (for example, it "unifies the sciences" better) than another, convenience in computation, efficiency as a predictive device, etc., etc.

Let X be a rational agent; let \(t_1, t_2, \ldots, t_i, \ldots\) be the times at which she is holding and updating her beliefs. Let \(a_1, \ldots, a_i, \ldots\) be her assignments of absolute credences to (as many of) her sentences (as she does make assignments to) at the various \(t_i\)'s. Let \(E_1, \ldots, E_i, \ldots\) be specifications in X's language of the total evidence X acquires from \(t_0\) to \(t_1\), from \(t_1\) to \(t_2\), \ldots, from \(t_{i-1}\) to \(t_i\), \ldots. Say that a sequence of conditional credence functions \(c_1, \ldots, c_i\) characterizes X in the interval from \(t_1\) to \(t_i\) provided that for each \(j\) from 1 to \(i\), and each \(S\) to which X assigns an absolute credence at \(t_j\), \(a_{j+1}(S) = c_j(S/E_j)\). And say that a credence function \(c\) characterizes X at a time \(t\).
provided that it is at the tail end of a sequence of functions that characterizes X from the time she began thinking about the world up to t. On the picture I am claiming is implicit in Quine, for each t at which X is forming beliefs there will be a set of Rt of strategies, thought of as conditional credence functions, that are available to X for (rationally) learning from experience; this set will be a subset of the set of conditional credence functions that characterize X at t. Rt is, as suggested above, determined by X's dispositions to react to and evaluate evidence. It will be constrained by the pragmatic factors mentioned at the end of the last paragraph.

Being determined by X's dispositions towards possible evidence, the members of Rt can be expected to more or less agree about the conditional probabilities of sentences relative to pieces of potential evidence that don't go beyond the bounds of what X (currently) takes to be more or less likely. Thus, the members of R can be expected to tend to yield more or less the same credence functions when one conditionalizes them on E's that summarize total evidence accumulation over short periods of time. These facts form the basis of a justification for the claim that Bayesianism provides a decent first approximation to the truth about confirmation and rational belief revision. For they suggest that in the short term and in the case of "normal science", the Bayesian paradigm may give a decent description of rational belief revision and evidentiary relations.  

33 Conceivably we should put some further constraints on what it takes for a sequence of c's to characterize X. But the above should get the ball rolling.

34 A complicating factor is that almost no serious Bayesian thinks (it is even a reasonable idealization to say) that people assign crisp conditional or unconditional probabilities to many or any sentences beyond logical truths and untruths. The standard view amongst Bayesians is that we can assign a range of credence functions to an individual at a time; the facts about her credences are those claims about her credences that are true relative to any function in the range that represents her. This sort of multiplication of the individual's credence functions is of a quite different sort than the one I suggesting Quine
As I am reading Quine, he thought it was nonsense, and the root of much nonsense, to think that a particular member or even a small set of members of a person's Rt (the set of strategies at t she might rationally adopt for belief updating) were the only strategies the use of which would preserve the meanings of one's sentences. The facts about what one means by one's sentences are given by the ways in which one might rationally revise one's beliefs in the face of recalcitrant evidence.

I take it that it is sufficiently obvious that on the picture I am ascribing to Quine, there might be a very wide divergence indeed among the ways in which one might rationally revise one's credence in, say, 'cats are animals', given that one comes to think that the furry pets that go 'meow' are robotic. That and kindred facts substantiate Quine's transformation argument – at least if we accept the general outlines of the picture of rational belief revision I just sketched. It is not as if accepting it means that we have to abandon the Bayesian framework for thinking about evidence and learning. We have to modify it in various ways; but that, I think, has been obvious for quite some time now.

It seems to me that the picture I have ascribed to Quine has considerable intuitive appeal. In particular, it sits well with (what I take to be) the obvious point that what it is rational for us to think as we make our way through the world is a joint function of the variety of ways we can see to make sense of the evidence we acquire and more or less practical constraints on how we might theorize, given our interests and abilities. Indeed, I think the broadly Quinean picture just sketched sits far better with this point than does the rigid Bayesianism that is required to get Chalmers'...
enterprise off the ground. For this reason alone, Quine's view of meaning is much more likely than Chalmers' to be close to correct. And if that is so, Chalmers does not have much of a response to Quine.

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