

Tacit Knowledge and Semantic Theory: Can a Five per cent Difference Matter?

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Can a five per cent difference—the difference between twenty axioms and twenty-one—really matter? Put like that, the question is a bit telegraphic. Let me explain.

I

In his paper ‘Semantic Theory and Tacit Knowledge’, Gareth Evans uses a familiar kind of example in order to render vivid his account of tacit knowledge. We are to consider a finite language, with just one hundred sentences. Each sentence is made up of a subject (a name) and a predicate. The names are ‘a’, ‘b’, . . . , ‘j’. The predicates are ‘F’, ‘G’, . . . , ‘O’. The sentences have meanings which depend in a systematic way upon their construction. Thus, all sentences containing ‘a’ mean something about John; all sentences containing ‘b’ mean something about Harry; all sentences containing ‘F’ mean something about being bald; all sentences containing ‘G’ mean something about being happy; and so on. For this very simple language L, we are to consider various semantic theories. We could consider theories whose deliverances about whole sentences are of the form

\[ s \text{ means that } p \]

but we shall, in fact, consider theories which employ the familiar format

\[ s \text{ is true iff } p. \]

Nothing of substance is supposed to turn on this choice.

The first theory, T₁, is the listiform theory. It simply has one hundred axioms, one specifying the meaning (more accurately, the truth condition) of each L-sentence. The second theory, T₂, is a structured, or articulated, theory. It has twenty-one axioms: one for each name; one for each predicate; and one for the subject–predicate mode of combination. For the name ‘a’, we have

‘a’ denotes John;

for the predicate ‘F’, we have

an object satisfies ‘F’ iff it is bald;
and for the mode of combination, we have the compositional axiom

a sentence coupling a name with a predicate is true iff the object denoted by the name satisfies the predicate.

The two theories, $T_1$ and $T_2$, are not, of course, logically equivalent theories; the axioms of $T_2$ cannot be derived from the axioms of $T_1$. But they are extensionally equivalent theories, in the sense that they deliver the same truth condition specifications for whole L-sentences.

The very idea of attributing to ordinary speakers tacit knowledge of a linguistic theory has, in many quarters, been viewed as problematic. What might be called Quine’s challenge is this (see ‘Methodological Reflections on Current Linguistic Theory’). How can there ever be empirical evidence to warrant the attribution to a subject of tacit knowledge of one theory rather than another, extensionally equivalent, theory?

Quine requires that the evidence should concern the very speaker to whom the attribution of tacit knowledge is being made, and he restricts the evidence to ‘dispositions to behave in observable ways in observable circumstances’ (p. 444). Chomsky, in contrast, rejects any such a priori restriction on possible sources of evidence for attributing tacit knowledge of a particular grammar to a speaker (see Knowledge of Language, pp. 243–63). In particular:

A hypothesis about Jones’s I-language [internalized grammar] will, in nontrivial cases, be justified in part by its role in a general theory of UG [universal grammar] which will have consequences for other languages as well, and can therefore be confirmed or refuted by investigations of such consequences . . . (‘Reply to Reviews by Alexander George and Michael Brody’, p. 184).

Chomsky is surely correct on the major issue here. There is no telling in advance where relevant evidence might be found; so there is no point in imposing restrictions upon the class of admissible evidence. Given a proper constitutive account of what tacit knowledge is, any evidence for or against the empirical hypothesis that such a condition obtains in a speaker is relevant to the attribution of tacit knowledge. But still, we can attempt to meet Quine’s challenge on something like its own terms, by citing evidence which concerns the very speaker to whom the attribution is being made.

Evans’s response to this challenge is to construe the attribution of tacit knowledge of a theory as the attribution of a set of dispositions, corresponding to the axioms of the theory.

I suggest that we construe the claim that someone tacitly knows a theory of meaning as ascribing to that person a set of dispositions—one corresponding to each of the expressions for which the theory provides a distinct axiom. (p. 328)

He adds that, for the account to work as intended, the notion of a disposition must be understood ‘in a full-blooded sense’. Given such an understanding
the ascription of tacit knowledge of $T_2$ involves the claim that there is a single state of the subject which figures in a causal explanation of why he reacts in this regular way to all the sentences containing [a given expression]. (p. 330)

Thus, ascription of tacit knowledge of $T_2$ involves the attribution to the subject of twenty distinct dispositions—and twenty distinct causal explanatory states—one for each name and one for each predicate of $L$.

This constitutes an adequate response to Quine's challenge to the extent that there can be empirical evidence for or against the existence in a particular subject of such distinct dispositions or states. Evans suggests three kinds of relevant evidence: evidence from patterns of acquisition; evidence from patterns of breakdown or decay; and evidence from experiments on perceptual processing (pp. 331–3). And it is immensely plausible to claim that, if attributions of tacit knowledge are basically attributions of structures of explanatory states, then such attributions can be empirically grounded. There is nothing too mysterious about causal structure.

Crispin Wright, to whom Evans was responding in his paper, has raised three interesting objections against Evans's account ("Theories of Meaning and Speakers' Knowledge", pp. 229–33; see also 'How Can the Theory of Meaning be a Philosophical Project?). The first objection relates to Evans's characterization of the dispositions for names and predicates. Evans says that the dispositions for names and predicates 'must be inter-defined' (p. 329). Wright fears that there is a vicious circularity in the offing; and suggests that it would be better to couch the definition of tacit knowledge in terms of non-dispositional states—the underlying categorical bases of the dispositions that Evans attempts to characterize (p. 329). Wright fears that there is a vicious circularity in the offing; and suggests that it would be better to couch the definition of tacit knowledge in terms of non-dispositional states—the underlying categorical bases of the dispositions that Evans attempts to characterize (p. 329). It is not clear that Evans himself would reject Wright's suggestion. Evans says that if dispositions are taken as mere regularities then the account of tacit knowledge of an articulated theory will fail to require any genuine articulation in the linguistic competence of a speaker. And it is for this reason that he construes dispositions in a 'full-blooded' way. In any case, the accounts of tacit knowledge to be offered here will be cast in terms of underlying explanatory states, and will thus concede the force of Wright's first objection.

The second objection relates to the account of tacit knowledge as a certain kind of causal structure. Suppose that a subject knows (tacitly or in the ordinary sense) what the various sentences of $L$ mean; and suppose that underlying those pieces of knowledge there is indeed a causal structure of the kind which, on Evans's account, is required for tacit knowledge of $T_2$—the articulated theory. Wright asks why such a subject would not be at least as well described by a two-part theory. The first part would be the semantic theory $T_1$—the listiform theory; the second part would be 'some appropriate hypotheses, of a non-semantic sort, about the presumed causal substructure of the dispositions which $T_1$ describes' (p. 231). Why,
in short, does mere causal structure justify articulation in a semantic theory? This objection will be faced squarely in Section IV.

Wright's third objection—and this is where the present story really begins—is this. Evans describes tacit knowledge of theory $T_2$ as involving twenty dispositions or states (p. 330); but $T_2$ has twenty-one axioms.

It is worth being clear about just why this is an objection. It is not that Evans has contradicted himself. What he requires is that there be a distinct disposition corresponding, not to each proper axiom of the theory, but to each expression for which the theory has a proper axiom. In the case of $L$, there are just twenty such expressions. The twenty-first axiom speaks for a mode of combination, not for an expression. Rather, the objection—as Wright sees it—is that a subject who has the interlocking dispositions that Evans describes—one for each of the twenty atomic expressions of $L$—will ipso facto be disposed to assign the correct meanings to whole sentences. But, in the theory $T_2$, the twenty axioms for those names and predicates are not sufficient to yield meaning (truth condition) specifications for whole sentences.

$T_2$ would be crippled without the compositional axiom, but if the brief of its axioms were merely the description of the dispositions which, on Evans's account, constitute tacit knowledge of them, the compositional axiom ought to be redundant.

(‘Theories of Meaning and Speakers' Knowledge’, p. 232).

What is more, Wright doubts that the difficulty presented by this mismatch between dispositions and theory can be easily overcome. For, on the one hand

there is in view no plausible modification of Evans's proposals concerning the dispositions relevant to singular terms and predicates which would need to be supplemented by a separate dispositional account concerning the compositional axiom.

(ibid.)

while, on the other hand,

any compositional theory of meaning for a typical natural language will incorporate something like $T_2$.

(ibid.)

The conclusion is that Evans has not shown how we are to construe an articulated semantic theory as a description of a speaker's dispositions.

Now, I am not sure that the first of these latter two points—the one about alternative dispositions—is correct. Of course, if Wright's first objection is decisive, then dispositions should not be 'inter-defined' at all. But, if dispositions are allowed to be 'inter-defined', then perhaps a disposition concerning the name 'a' can be defined in terms of both tacit knowledge of the satisfaction conditions of predicates and tacit knowledge of the significance of modes of name-predicate combination. Similarly, the disposition for the predicate 'F' would be defined in terms of tacit knowledge of the denotations of names, and of the significance of modes
of combination. And similarly again, the disposition for the only mode of combination present in L would be defined in terms of tacit knowledge of the denotations of names, and of the satisfaction conditions of predicates.

Since I hold no brief for a definition of tacit knowledge in terms of dispositions, I rest no weight upon this possible response to the first point.

As for the second point, it is surely correct that a compositional theory of meaning will incorporate something like $T_2$. The question is: how like? Let us add to our collection of theories one that is articulated, rather than listiform, but has twenty axioms, rather than twenty-one. $T_3$ contains an axiom for each name of L—indeed, the very same axiom as in $T_2$. But the axioms for the predicates are different. For the predicate ‘$F$’, we have:

a sentence coupling a name with the predicate ‘$F$’ is true iff the object denoted by the name is bald.

Theory $T_3$ is certainly extensionally equivalent to $T_1$ and $T_2$. What $T_3$ does is to parcel out the content of $T_2$’s compositional axiom among the ten predicates in the language.

As far as I can see, Wright’s third objection does not tell against the proposal that the subject with the twenty distinct dispositions or states has tacit knowledge of $T_3$. So, perhaps we should say—on Evans’s behalf—that although his account of tacit knowledge does distinguish between extensionally equivalent theories—as required by Quine’s challenge—still, it does not distinguish between every pair of extensionally equivalent theories. Perhaps—on Evans’s view—there is no distinction to be made between tacit knowledge of $T_3$ and tacit knowledge of $T_2$.

We arrive back at our starting point. Can a five per cent difference—the difference between twenty axioms and twenty-one—really matter?

II

It is a familiar thought that the attribution of tacit knowledge is fairly coarse grained. Indeed, this familiar thought is not clearly distinct from another, namely the idea that the content of a state of tacit knowledge does not have to be conceptualized by the subject whose state it is. Evans is quite explicit about this latter idea.

Such concepts as we use in specifying [tacit knowledge] are not concepts we need to suppose the subject to possess, . . .

(p. 339)

We can bring out one aspect of this coarseness of grain by introducing yet a further theory. $T_4$ has twenty-one axioms—like $T_2$—and the axioms for the ten names are just the same as in $T_2$. However, the other eleven axioms are slightly different. For the predicate ‘$F$’, for example, $T_4$ has

the extension of ‘$F$’ is the set of bald objects

and its compositional axiom is
a sentence coupling a name with a predicate is true iff the object denoted
by the name is a member of the extension of the predicate.

Suppose that the background logic of $T_4$ contains enough set theory to
ensure the derivability of the same truth condition specifications as before.

Evans's account of tacit knowledge does not distinguish between tacit
knowledge of $T_2$ and tacit knowledge of $T_4$. Roughly, the reason for this
is that, for any subject, it will be equally true of $T_2$ and of $T_4$ that the
derivational structure in the theory matches, or \emph{mirrors}, the causal
explanatory structure underlying the subject’s beliefs about the meanings
of whole sentences.

This idea of a semantic theory mirroring the structure of speakers’
competence goes back a long way in Evans’s work. In ‘Identity and
Predication’, it is spelled out like this.

The semanticist aims to uncover a structure in the language that mirrors the
competence speakers of the language have actually acquired. This does not mean
that he aims to uncover a theory that he supposes his subjects \emph{know}, in any
acceptable sense of that word. It means merely this: if (but only if) speakers of the
language can understand certain sentences they have not previously encountered, as
a result of acquaintance with their parts, the semanticist must state how the
meaning of these sentences is a function of the meanings of those parts. He must
assign semantical properties to the parts and state the general significance of the
construction in such a way that a statement of what those sentences mean is
deductively entailed. There may be more than one way of doing this. \textit{(pp. 25–6)}

The salient structural facts about the competence of speakers are here
presented as being of the following form: speakers who understand
sentences $s_1$, $s_2$, \ldots, $s_n$ are able, without further training, to understand
sentence $s$. And the salient structural facts about a semantic theory are of
this form: the resources used in derivations of meaning specifications for
$s_1$, $s_2$, \ldots, $s_n$ are jointly sufficient for the derivation of the meaning
specification for $s$. The constraint on semantic theories which is implicit
in the passage quoted from Evans is just that these two structures should
match.

As Evans says, there may be more than one theory with the requisite
structure. For the notion of derivational structure in a semantic theory
enables us to define an equivalence relation on the class of theories
extensionally equivalent to $T_2$. Two such theories are equivalent in point
of derivational structure \emph{(DS-equivalent)} if just the same salient structural
facts hold of both; that is, if their derivational structures match the same
competence structures in speakers.

Clearly enough, $T_2$ and $T_4$ are DS-equivalent theories. What is more,$T_2$ and $T_3$ are DS-equivalent theories. For, although $T_2$ has an extra
axiom relative to $T_3$, the use of this resource in $T_2$ is constant across all
derivations of meaning specifications for whole sentences. Consequently, that extra step in the derivations makes no difference to the salient structural facts about the semantic theories.

There are problems that can be raised concerning the salient structural facts about speakers' competence. Do these facts relate to the actual history of speakers, or are they to be construed counterfactually? If the facts relate to actual history then there are two worries. First, speakers may not actually have come across many sentences; so there may not be many salient facts. Second, it may be that speakers initially worked their way into the language phrase-book fashion, but now have a highly structured competence in it. Facts about what they actually understood at the first encounter would thus underestimate the structure that is now present. So, let us suppose that the facts are to be construed counterfactually. But then there are familiar worries. For the obvious counterfactual characterization of a structured ability is typically neither necessary nor sufficient for the actual presence of that structured ability.

It would be in the spirit of Evans's full-blooded understanding of dispositions to shift to an alternative set of facts about the structure of speakers' competence. We can let the salient facts be of this form: the operative states implicated in the causal explanation of a speaker's beliefs about the meanings of $s_1, s_2, ..., s_n$ are jointly sufficient for a causal explanation of his belief about the meaning of $s$.

Armed with these notions of derivational structure and causal explanatory structure, we can frame a constraint on semantic theories, and offer a first account of tacit knowledge. The requirement on semantic theories—a crude version of the *Mirror Constraint*—is that the derivational structure in a semantic theory should match the causal explanatory structure in actual speakers. The account of tacit knowledge is this. We attribute to an individual speaker tacit knowledge of a particular semantic theory on condition that the theory meet the Mirror Constraint with respect to that speaker.

This account is arguably faithful to Evans's intentions in 'Semantic Theory and Tacit Knowledge', and it certainly addresses Quine's challenge. The salient structural facts about speakers are open to empirical investigation, especially by observation of patterns of breakdown. Also, attributions of tacit knowledge are—on this account—fairly coarse grained. The difference between theories $T_2$ and $T_4$, for example, is not significant. More importantly, on this account the five per cent difference between theories $T_2$ and $T_3$ does not matter. To that extent, the account evades the force of Wright's third objection.

This is an occasion for modest rejoicing. But doubts soon emerge. Consider, for example, the following.

Imagine two L-speakers for whom a form of the language of thought hypothesis is true. For these speakers, language comprehension—in
particular, the assignment of meanings to sentences—is a matter of
derivations in a semantic theory explicitly represented in a special-purpose
language-processing system. Suppose that speaker A conducts on his inner
blackboard derivations in theory $T_2$, while speaker B conducts derivations
in theory $T_3$. The two speakers end up assigning just the same meanings
to the same sentences. Speaker A’s language system employs a different
algorithm from speaker B’s. But, despite that fact, tacit knowledge of $T_2$
(that is, tacit knowledge of $T_3$) is equally attributable to both speakers.
For the purposes of tacit knowledge ascription, speakers A and B are
grouped together.

Christopher Peacocke has argued ('Explanation in Computational
Psychology: Language, Perception and Level 1.5') that there is an
important level of psychological explanation at which algorithms are
regarded as equivalent if they ‘draw upon the same body of information’
(p. 107), and that it is at this level that philosophers’ semantic theories
contribute to psychological explanation. The algorithms used by speakers
A and B are surely not equivalent in Peacocke’s sense. Speaker A’s
algorithm draws upon a piece of information, encoded in the twenty-first
axiom of $T_2$, that concerns every $L$-sentence; no such piece of information
is drawn upon by speaker B’s algorithm. Thus, speaker A and speaker B
are not grouped together at the level of description and explanation that
Peacocke distinguishes.

This is enough to raise a prima-facie doubt about the credentials of the
first account of tacit knowledge. It is intuitively plausible that semantic
theories should contribute to psychological explanation via the notion of
tacit knowledge. And, according to the first account, speaker A and speaker
B are grouped together at the level of tacit knowledge description. On the
other hand, Peacocke makes it plausible that semantic theories contribute
to psychological explanation at the level that he distinguishes. And, at
that level, speaker A and speaker B are not grouped together. It is bound
to be difficult to argue for recognition of both these levels of description
and explanation. What is more, quite apart from Peacocke’s proposal, it
will seem to be an unattractive feature of the first account that speaker A
and speaker B are not distinguished.

There is another ground for reservations about the first account. For
it still faces at least one of Wright’s original objections. To be sure, we
have avoided the third objection; and the account appeals to categorical
explanatory states rather than to dispositions as such, so it concedes the
force of the first objection. But the third objection was always ancillary
to the second, and that still faces us. The problem is: why does causal
structure justify articulation in a semantic theory?

The only fair answer to this query is, I believe, that it does not. That
is to say, mere causal structure does not justify articulation in a semantic
theory. We need to distinguish, and have not yet begun to distinguish,
between causal structure that is relevant to the attribution of tacit knowledge and causal structure that is irrelevant.

III

Before commencing on that task, I should pause to clarify my rather idiosyncratic use of the notion of a theory. Officially, after all, a theory is just a set of sentences closed under deduction. Two sets of sentences—not necessarily closed under deduction—are logically equivalent if they deductively generate the same theory. And there may be many different routes for deriving a particular theorem from a given set of sentences.

As I have been using the notion, a semantic theory is not—or not merely—a theory in that official sense. A semantic theory involves a set of proper axioms, from which, given some background logic, certain 'target' sentences—the meaning specifications for whole sentences of L—are derivable as theorems. But, since those target sentences may be derivable from the axioms in many different—more or less circuitous—ways, I have also assumed that we have distinguished a 'canonical' proof procedure for deriving the target sentences from the axioms. The salient structural facts about semantic theories should really be cast in terms of canonical derivations. They should be facts of the form: the resources used in canonical derivations of meaning specifications for \( s_1, s_2, \ldots, s_n \) are jointly sufficient for the canonical derivation of the meaning specification for \( s \). And the definition of DS-equivalence should be adjusted accordingly.

We have already noted that the attribution of states whose contents do not need to be conceptualized by the subject is usually taken to be rather coarse grained. One precisification of that idea might be that such attributions do not distinguish more finely than logical equivalence. But, although that thought is plausible in some contexts, it is certainly not correct to suppose that the first account of tacit knowledge does not distinguish between logically equivalent theories.

As it happens, no two of the theories that we have considered so far are logically equivalent. But, it is clear that, since the object of tacit knowledge is being conceived as a set of proper axioms together with a canonical proof procedure for deriving theorems in a certain favoured class, the issue of logical equivalence is really irrelevant.

It might be thought that there is an alternative way of responding to the query about logical equivalence. Suppose that \( T_a \) and \( T_b \) are two logically equivalent axiomatizations of a set of meaning specifications. Then we could distinguish between two total composite tacit knowledge states by noting that one composite state has constituents which are individually states of tacit knowledge of the several axioms of \( T_a \), while the constituents of the other are individually states of tacit knowledge of the axioms of \( T_b \). Given the distinction made in that way at the level of
constituent states, we could allow that the attribution of contents to the total states need not be sensitive to the difference between the logically equivalent theories $T_a$ and $T_b$.

However, this alternative response is not legitimate in the context of the first account of tacit knowledge that we have extracted from Evans's work. For the way in which that account avoids Wright's third objection—the five per cent objection—has the consequence that it simply does not license the description of constituent explanatory states as states of tacit knowledge of individual axioms. For the account does not guarantee that axioms and explanatory states are in one-to-one correspondence. To the extent that this is now seem as an unattractive feature, it constitutes a further ground for reservation about that first account.

There is one further point of clarification about semantic theories, before we return to our main theme. I have spoken of a semantic theory as having proper axioms and a background logic. But, in fact, there seems to be no reason to rule out the possibility of proper rules of inference. Thus, for example, consider a theory $T_5$ which is like $T_3$ except that, instead of the proper axiom for each predicate, there is a proper rule of inference. For the predicate 'F', for example, the rule is

From: $N$ denotes $m$

Infer: the sentence coupling 'F' with $N$ is true iff $m$ is bald.

Theory $T_5$—given a certain obvious canonical proof procedure—is clearly DS-equivalent to $T_3$, and consequently to $T_2$ and $T_4$ as well.

IV

The principal problem that we faced at the end of Section II was that of distinguishing between causal structure that is relevant to attributions of tacit knowledge and causal structure that is not.

Let us return to one of the points we noted about the first account of tacit knowledge. The salient structural facts about speakers are open to empirical investigation, especially by observation of patterns of breakdown. Now, suppose that for a particular speaker there really is 'a single state of the subject which figures in a causal explanation of why he reacts in this regular way to all the sentences containing' the expression 'F'; and that there is another state corresponding to the expression 'a'. The first state is implicated in the explanations of the speaker's beliefs about the meanings of 'Fa', 'Fb', . . ., 'Fj', while the second state is implicated in the explanations of the speaker's beliefs about the meanings of 'Ga', 'Gb', . . ., 'Gm'. What is more, for any pair such as 'Ga' and 'Fb', or . . . or, 'Oa' and 'Fj', the operative states implicated in the causal explanation of the speaker's beliefs about the meanings of the sentences in the pair are jointly sufficient for a causal explanation of his belief about 'Fa'.
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That last condition does not, and is not intended to, guarantee that, so long as the speaker is able to understand, say, ‘Ga’ and ‘Fb’ he will be able to understand ‘Fa’. The states implicated in the explanation of the former pieces of understanding are explanatorily sufficient for the latter piece of understanding. But still, it might be that if the sentence ‘Fa’ were to be presented to the speaker those states would somehow fail to operate. That is to say that there might be all sorts of freakish circumstances that would prevent the speaker from arriving at the correct—or any—belief about the meaning of ‘Fa’. But, for all that, two conditions each of which would suffice to disrupt the speaker’s beliefs about ‘Fa’ are the decay of the state corresponding to ‘F’ and the decay of the state corresponding to ‘a’. The first condition would have the consequence of disrupting the speaker’s beliefs about all of ‘Fb’, . . . , ‘Fj’, while the second condition would disrupt the speaker’s beliefs about all of ‘Ga’, . . . , ‘Oa’. It is for this reason that evidence that loss of knowledge of the meaning of ‘Fa’ is accompanied by loss of knowledge of the meanings of other sentences containing ‘F’, or of the meanings of other sentences containing ‘a’, amounts to evidence in favour of the speaker having tacit knowledge of an articulated theory such as T2.

The pattern of breakdown does not, of course, constitute the prior state of tacit knowledge. (How could it?) It is merely evidence in favour of there having been such a prior state. So, the account of tacit knowledge is not threatened merely by the possibility that such evidence could be forthcoming in a case where we should intuitively not want to attribute tacit knowledge of an articulated theory. However, in the context of Quine’s challenge we have attached considerable importance to patterns of breakdown as a source of evidence in favour of one, rather than another, attribution of tacit knowledge. Consequently, it will be instructive to have before us an example in which a pattern of breakdown is intuitively misleading as to the attributability of tacit knowledge. For, in the example to be given, the evidence is not obviously misleading as to the presence of some kind of causal structure. The example will thus render even more vivid Wright’s second objection: mere causal structure does not justify articulation in a semantic theory. In fact, it will force a refinement of the first account of tacit knowledge.

The example is easier to picture than to describe. Imagine a speaker, C, who is rather like the earlier A and B in having a language system which performs derivations in an explicit representation of a semantic theory. However, C is unlike A and B in that the theory explicitly represented in his language system is the listiform theory T1. Imagine that the representations of the one hundred axioms of T1 are arranged in a ten-by-ten matrix of information storage units, and that the required nutrients for these units flow in channels through the matrix. Suppose that there are two types of nutrient X and Y. As it happens, the X nutrient
flows through the matrix in ten channels $X_a, \ldots, X_j$, each of which serves
the ten storage units for the ten sentences containing a single name.
Likewise, the $Y$ nutrient flows in ten channels, $Y_f, \ldots, Y_o$, each of which
serves the ten storage units for the ten sentences containing a single predicate.
We now elaborate the example so that each unit requires both
its nutrients in order to function, and so that failure of a unit prevents
nutrient flow through at least one of the channels serving that unit.

Given the nutritional structure of $C$'s language system, damage to the
system is liable to produce a characteristic pattern of breakdown in $C$'s
semantic knowledge. In fact, the pattern of breakdown that is liable to be
shown is just the pattern that amounts to evidence for attributing tacit
knowledge of an articulated semantic theory, such as $T_2$. But, intuitively,
speaker $C$ does not have tacit knowledge of $T_2$ or of any other articulated
theory, but only of the listiform theory $T_1$.

The evidence provided by $C$'s pattern of breakdown is misleading as
to the attributability of tacit knowledge; but it is not misleading as to the
existence of some kind of causal structure. There is a causal structure in
$C$'s language system; it is a nutritional structure. We want to say that
nutritional structure is not relevant to the attributability of tacit knowledge.
But it is not absolutely clear that the first account of tacit knowledge
prevents this kind of structure from counting as constitutive of tacit
knowledge of an articulated theory.

As things stand in $C$'s language system, what is necessary and sufficient
for his arriving at the correct belief about the meaning of any sentence,
say "$Hd$", is just the flow of nutrients in the corresponding two channels,
in this case $X_d$ and $Y_d$. So, the states implicated in the explanation of $C$'s
beliefs about, say, "$Ga$" and "$Fb$"—namely, nutrient flow in channels $X_d$,
$X_j$, $Y_f$, and $Y_c$—really are jointly sufficient to explain his beliefs about
"$Fa$".

What might an advocate of the first account of tacit knowledge say in
response to this example, if he shares the intuition that speaker $C$ does
not have tacit knowledge of an articulated theory?

The example is, of course, far-fetched. An empirical psychologist can
reasonably say that—precisely because it is far-fetched—such a case is
unlikely to upset the actual attribution of tacit knowledge on the basis of
cognitive breakdown. What is more, causal structures that are relevant to
attributions of tacit knowledge are fairly consistent across subjects, while
nutritional structures tend to be variable relative to those first structures.
So, a pattern of breakdown that arises for the wrong sort of reason is
likely to be revealed as happenstance, by observation of other subjects.
This is, in fact, just what the cognitive neuropsychologist—who studies
cognitive organization by observing acquired disorders—will say. That	
theorist needs to distinguish empirically between a genuine syndrome and
a psychologically accidental co-occurrence of symptoms. And he does this
by looking for other cases in which the constellation of symptoms dissociates. The co-occurrence of a pair of symptoms is shown to be accidental if double dissociation can be discovered; that is, if each symptom can be found without the other. (For a clear statement of the cognitive neuropsychology programme, see for example Coltheart, 'Cognitive Neuropsychology and the Study of Reading'.)

This may solve the empirical psychologist's empirical problem. But it goes no way at all towards solving our philosophical problem. What the philosopher is looking for is a constitutive account of tacit knowledge; and for a constitutive account of a syndrome. He needs to be able to say what it is for a causal structure to be of merely physiological significance; and what it is for a co-occurrence of symptoms to be psychologically accidental. But it is not clear how this is to be done within the framework of the first account of tacit knowledge.

One thing that might be said is this. In suggesting that the example meets the conditions of that first account, I spoke of the necessity and sufficiency of nutrient flow as things stand in C's language system. The claim about causal explanatory structure depended upon a great deal being held constant; in particular, it depended upon holding constant the information stored in the units.

There is clearly something right about this response to the example. But, on the other hand, any claim about operative states implicated in some set of explanations being jointly sufficient for some other explanation will presume upon certain causal factors being held constant. So, what is relevant is not simply that the sufficiency claim depends upon holding certain factors constant. What is relevant is, surely, that the causal explanatory structure in the example is in no way sensitive to the information stored in the units.

An evidential manifestation of this lack of sensitivity is that, in speaker C, the pattern of semantic revision would not follow the pattern of semantic decay. Although loss of knowledge about the semantic properties of 'Fa' is liable to be accompanied by loss of knowledge about the semantic properties of sentences with a common constituent, we do not expect that revision of C's belief about the meaning of 'Fa' would go hand in hand with corresponding revisions of his beliefs about other sentences.

The example of speaker C is not, intuitively, a case of tacit knowledge of an articulated semantic theory. If the reason for this is that the causal structure in speaker C lacks some feature of sensitivity to information, then the first account of tacit knowledge is surely not adequate. For there is nothing in that account about information, and nothing about sensitivity. Some more refined notion of causal structure is needed; but it is, as yet, an open question what the correct refinement might be.

Before considering how an improved account might go, let us pause to review the virtues and vices of the first account. Its first, and major, virtue
is that it enables the friend of tacit knowledge to meet Quine's challenge. Its second, and more parochial, virtue is that the distinction between the twenty axiom theory $T_3$ and the twenty-one axiom theory $T_2$ does not matter; the account is not open to Wright's third objection.

There is a third virtue: the difference between a theory with proper axioms, like $T_3$, and a theory with some proper rules of inference instead, like $T_5$, also does not matter. The reason why this feature of the account is a virtue may not be obvious. But, suppose that the causal structure that mirrors the derivational structure of a semantic theory is a computational structure. Then, since the relation between proper axioms and proper rules of inference is analogous to the relation between explicit representations and computational procedures, the feature just mentioned will have an attractive consequence. Tacit knowledge will not essentially be a matter of explicit representation.

So much for virtues. The first, and major, problem with the account is that it uses an undifferentiated notion of causal structure. I have acknowledged that this leaves the account open to Wright's objection that mere causal structure does not justify articulation in a semantic theory. Now, it may be that, by Wright's lights, no refinement of the notion of causal structure could ever justify the idea that a semantic theory should mirror that structure. But, to the extent that any refinement ensures that the salient causal structure can be described as an information-processing structure, this extreme view will be hard to sustain.

A second problem over the first account of tacit knowledge is that it groups together algorithms which draw upon different information. The level of tacit knowledge description is thus different from the level at which, according to Peacocke, semantic theory contributes to psychological explanation. The extent to which this problem is serious depends, of course, upon the explanatory credentials of the level that Peacocke distinguishes. But, if those credentials are good, then the fear must be that a slightly coarser level of tacit knowledge description will be excess to explanatory requirements. Any revised account that avoids this problem will not, however, share the second, parochial, virtue of the first account. It will, in short, involve a slight departure from Evans.

A third problem with the first account—briefly mentioned in Section III—is that it does not license the description of constituent explanatory states as states of tacit knowledge of individual axioms of a semantic theory. Any revised account that avoids this problem will also have to depart from Evans, and accept that a five per cent difference can matter.

We can introduce the notion of sensitivity into the account both evidentially and constitutively. On the side of evidence, the basic idea is clear enough. If we are to attribute to a speaker tacit knowledge of an articulated theory,
then we shall expect to find evidence of the following kind. If the speaker revises his belief about the meaning of, say, ‘Fa’—taking it to mean, not that John is bald, but merely that he is baldish, or less than optimally hirsute—then he likewise takes ‘Fb’ to mean that Harry is less than optimally hirsute. In short, we shall expect the speaker to keep his meaning assignments in step with each other—to preserve systematicity under revision.

As we confront Quine’s challenge, this kind of evidence from patterns of revision can be added to Evans’s list of evidence from patterns of acquisition, evidence from patterns of breakdown or decay, and evidence from experiments on perceptual processing.

There is, in fact, something of a time bomb packed away in the innocent word ‘likewise’. But, for now, let us turn to the much more fundamental matter of an improved constitutive account.

Until now, the salient structural facts about speakers have been of this form: the operative states implicated in the causal explanation of the speaker’s beliefs about the meanings of $s_1, s_2, \ldots, s_n$ are jointly sufficient for a causal explanation of his belief about $s$. But these facts have turned out to be insufficiently discriminating. The more or less obvious addition to try is this. The operative states implicated in the explanation of the speaker’s actual beliefs about $s_1, s_2, \ldots, s_n$ together with revision to the belief about the meaning of $s$ should provide an explanation of the corresponding revision in the speaker’s beliefs about $s_1, s_2, \ldots, s_n$.

The proposal is, then, that tacit knowledge of an articulated theory requires a match between the derivational structure of the theory and this more refined causal explanatory structure in a speaker. This constitutive account closely echoes the way in which the notion of sensitivity was introduced on the side of evidence for the attribution of tacit knowledge. But, of course, the actual availability of revision evidence is by no means necessary—nor, in fact, sufficient—for the condition on causal explanatory structure to obtain.

This proposed, new, constitutive account of tacit knowledge makes it clear why the example of speaker C in Section IV should not count as a case of tacit knowledge of anything other than the listiform theory.

In order for the example to meet the original, weaker, condition for tacit knowledge, we had to take the operative states implicated in the explanation of speaker C’s beliefs about ‘Ga’ and ‘Fb’ to be states of nutrient flow in four channels. But those states, together with revision of the speaker’s belief about the meaning of ‘Fa’, certainly do not provide an explanation of corresponding revisions in C’s belief about ‘Ga’ and ‘Fb’.

On the other hand, if we look for explanatory conditions that are liable to be sensitive to revisions in C’s beliefs about the meanings of whole sentences, then the only candidates are the states of storing information
about those sentences in the units of the matrix. But then, the states of that kind that are implicated in the explanation of C's beliefs about 'Ga' and 'Fb' are not jointly sufficient to explain his belief about 'Fa'.

What is wrong with focusing on the explanatory conditions constituted by nutrient flow and ignoring as a constant background condition the information stored in the units, is that one thereby ignores the only potential loci of sensitivity—the only potential causal explanatory loci of systematic revision.

The notion of systematic revision allows us to introduce the idea of sensitivity into an account of tacit knowledge. There may be other ways of achieving that same aim.

For example, in his work on action and perception, Peacocke makes use of a notion of differential explanation (Holistic Explanation, pp. 63–89). For an explanation to be differential the invoked generalization (which need not be a fundamental law) has to specify a function (in the mathematical rather than the teleological sense) linking the explaining condition and the explained condition. In a way expressed by the function, the explained condition is sensitive to the explaining condition. To be a little more precise, the invoked generalization distinguishes, within the sufficient explaining condition, some constituent condition that is functionally related to the explained condition. It is then said to be the occurrence of the constituent condition which—given the background provided by the remainder of the explaining condition—differentially explains the explained condition.

In the case of speaker C, we shall want to say that the total explaining condition for his belief about the meaning of the sentence 'Fa' has two constituents. One is the presence in a certain storage unit of the information about 'Fa', and the other is the flow of nutrients in channels $X_a$ and $Y_f$. It is the first constituent that differentially explains C's belief about 'Fa'.

With just this much grasp on the notion of differential explanation, we can see how it could be used in an account of tacit knowledge. For, in describing the causal explanatory structure of speakers, we could focus on facts of the following form: the states which differentially explain the speaker's beliefs about the meanings of $s_1$, $s_2$, ..., $s_n$ are sufficient for a differential explanation of the speaker's belief about the meaning of $s$ (for this suggestion, see 'Meaning, Structure and Understanding', p. 160 n. 18).

We now have two possible ways of introducing the intuitive notion of sensitivity into a constitutive account of tacit knowledge. One way appeals to explanation of systematic revision; the other way appeals directly to differential explanation. My view is that a principled choice between these two ways would be answerable to larger issues than are being addressed in this paper.

Let us suppose, provisionally, that the notion of sensitivity is to be
introduced into our constitutive account of tacit knowledge via appeal to
the explanation of systematic revision. Tacit knowledge of an articulated
semantic theory requires a match between the derivational structure of
the theory—constituted by facts about canonical derivations of meaning
specifications—and the causal explanatory structure of speakers—now
constituted by facts of the following form: the operative states implicated
in the explanations of the speaker’s beliefs about $s_1, s_2, \ldots, s_n$ are jointly
sufficient for a causal explanation of the speaker’s belief about $s$; and those
first states together with the revision of the speaker’s belief about $s$ provide
an explanation of the speaker’s corresponding revisions in his beliefs about
$s_1, s_2, \ldots, s_n$. This requirement of match of structure amounts to a second
version of the Mirror Constraint. It is, in fact, the version that Wright
discusses in ‘Theories of Meaning and Speakers’ Knowledge’ (pp. 212–
13; and see Meaning, Quantification, Necessity, p. 53 and p. 78).

This revised account of tacit knowledge shares the three virtues of its
predecessor. It enables the friend of tacit knowledge to meet Quine’s
challenge. The five per cent difference does not matter. And the difference
between proper axioms and proper rules of inference does not matter.

But, as consequences of the second virtue, it also shares the second and
third vices of the first account. It leaves tacit knowledge descriptions
floating uncomfortably at a slightly coarser level of description and
explanation than the one that Peacocke distinguishes. And it does not
license descriptions of constituent explanatory states as states of tacit
knowledge of the individual axioms of a semantic theory.

The revised account is an improvement simply because it uses a more
refined notion of causal structure; it thus delivers the intuitively correct
answer about speaker C. Whether, by Wright’s lights, this improvement
is enough to justify a project in which it is required that a semantic theory
should mirror that causal structure is not so clear.

The evident advantage of mirroring causal structure in a semantic
theory is that it provides a way of giving an information-processing
description of a causal process. But, if this is to be the advantage, then
there are things to be said both for and against the revised account. In favour
of the account, we can say that no information-processing description is
ever uniquely correct, and the requirement about explanation of systematic
revision does at least warrant the description of a state implicated in
explanations of semantic beliefs about all sentences containing ‘$F$’ as a
state containing the information that it is being bald (rather than any other
property) that is associated with the predicate ‘$F$’. On the other hand,
against the account, we can say that—because of the five per cent
indifference—there is no more determinate informational description
which is definitely licensed by the account and which has the property
that the content of an explained belief follows from the information
contents of the explaining states.
VI

Suppose that we are gathering evidence in favour of the attribution to a particular speaker of tacit knowledge of an articulated semantic theory. The speaker revises his belief about the meaning of ‘Fa’—taking it now to mean that John is baldish. We expect the speaker to revise his beliefs about the meanings of other sentences likewise. So, we expect him to take ‘Fb’ to mean that Harry is baldish. But what should we be expecting to happen to the speaker’s belief about, say, ‘Gb’? Should we expect no change? Or should we expect the speaker now to take ‘Gb’ to mean, not that Harry is happy, but merely that Harry is happyish? There is no right answer. Either later belief could legitimately fall under the term ‘likewise’.

On the constitutive side of the account, we have the idea of states which mediate the explanation of corresponding revisions in a speaker’s beliefs about various related sentences. Take for $s_1, s_2, \ldots, s_n$ the sentences ‘Ga’, ‘Fb’, and ‘Gb’, and take for $s$ the sentence ‘Fa’. For tacit knowledge of an articulated theory, it is required that the operative states implicated in the explanation of the speaker’s beliefs about ‘Ga’, ‘Fb’, and ‘Gb’ be jointly sufficient for an explanation of the speaker’s belief about ‘Fa’. And it is required that those operative states, together with revision of the speaker’s belief about ‘Fa’ should provide an explanation of corresponding revisions in his beliefs about the first three sentences. But, if the revision for ‘Fa’ is to take it to mean that John is baldish, what is the corresponding revision for ‘Gb’? Is it the null revision, or the revision to take it to mean that Harry is happyish?

On both the evidential and the constitutive side of the account, there is an indeterminacy in what is required for tacit knowledge of an articulated semantic theory. This is the time bomb that I alluded to, at the beginning of Section V. For I believe that this indeterminacy threatens the stability of the revised account of tacit knowledge. (It seems that this arguably unstable position was occupied by the author of ‘Meaning, Structure and Understanding’, and Meaning, Quantification, Necessity.)

This threat might not be absolutely decisive. I do not deny that one could frame a definition of tacit knowledge by taking a permissive attitude to the indeterminacy. One could simply require, evidentially, that there be some pattern of revision deserving of the title ‘likewise’, and constitutively, that explanations be provided of some pattern of corresponding revisions.

But, it is a striking fact that what we regard as corresponding revisions goes in step with which articulated theory we are considering. The revised account, like the first account, draws no distinction between tacit knowledge of $T_2$ and tacit knowledge of $T_3$. But, if we look at the semantic properties of language $L$ through the grid of theory $T_2$, then we see more possible patterns of corresponding revision than if we look at the semantic properties of $L$ through the grid of $T_3$. 
The reason for this is simple. Each proper axiom—or proper rule of inference—in a semantic theory constitutes a locus of content sensitivity. A small change in the semantic property assigned to a sentence constituent by a proper axiom results derivationally in systematic changes in the meanings assigned to whole sentences. If we consider $T_2$, then there are two possible changes to the proper axioms, each of which would result in 'Fa' being assigned the meaning that John is baldish. A change in $T_2$'s axiom for 'F' has one pattern of consequences; a change in the twenty-first axiom—the compositional axiom—has a different, and more extensive, pattern of consequences. If we consider $T_3$, on the other hand, then there is only one possible change to the proper axioms which would result in 'Fa' being assigned the meaning that John is baldish, namely a change in $T_3$'s axiom for 'F'.

So we are in the following, rather paradoxical, position. We attribute tacit knowledge according to an account which does not distinguish between tacit knowledge of $T_2$ and tacit knowledge of $T_3$. There is an indeterminacy in the account, over exactly what is required for the correct attribution of tacit knowledge of $T_2$, that is, of $T_3$. A permissive interpretation of the account may be possible. But, the indeterminacy can be resolved according as we look at the semantic properties of the language in question through the grid of one theory rather than the other.

Clearly, having moved from the first account of tacit knowledge to the revised account, we now have all the resources needed for a further revision—one that will result in a final account that makes slightly finer distinctions. To make the point vivid, let us consider again the speakers A and B. Both these speakers meet the original condition for tacit knowledge of $T_2$, that is, of $T_3$. And let us suppose that the processes of theorem derivation on inner blackboards also facilitate feedback of revisions from the theorems to the axioms. Then, both speakers will also meet the revised condition for tacit knowledge, under its permissive interpretation. But, just as theory $T_2$ with its twenty-one axioms provides an extra locus of content sensitivity over theory $T_3$ with its twenty axioms, so the causal explanatory structure in speaker A provides an extra locus of systematic revision over the causal explanatory structure in speaker B. So, not altogether surprisingly, it is speaker A—conducting inner derivations in theory $T_2$—who meets the condition for tacit knowledge if the indeterminacy is resolved by looking at the language through the grid of $T_2$. And it is speaker B who meets the condition if the indeterminacy is instead resolved by looking at the language through the grid of theory $T_3$.

In the language systems of speakers A and B, derivations are carried out in an explicitly represented semantic theory. But it is not the fact of explicit representation that permits a finer-grained attribution of tacit knowledge. The final account that is envisaged will not require explicit representation of the axioms of a theory. What it will require—in addition
to what is required by the second account—is this. For tacit knowledge of a particular articulated theory, there should be, within the causal explanatory structure in the speaker, an explanatory locus of systematic revision corresponding to each proper axiom or rule of the theory. For each axiom or rule, the required notion of systematic revision can be spelled out in a quite determinate way.

This final account of tacit knowledge retains the first virtue of the earlier accounts, namely that it enables us to meet Quine's challenge. It also retains the third virtue. Although it is a more finely discriminating account, it still does not distinguish between $T_3$ and the theory $T_5$ which has a proper rule of inference for each predicate. Nor, of course, does it distinguish between, for example, the twenty-one axiom theory $T_2$ and the theory $T_4$ which uses the notion of extension instead of that of satisfaction.

However, the final account forgoes the second virtue of the earlier accounts. The five per cent difference between $T_2$ and $T_3$ really does matter. So here we part company with Evans. His man with twenty distinct dispositions, and twenty distinct explanatory states (each of which, we may suppose, constitutes an explanatory locus of systematic revision) does not have tacit knowledge of the twenty-one axiom theory $T_2$, but only of its twenty axiom cousin $T_3$.

By forgoing this parochial virtue, the final account is better placed to avoid vices. It certainly avoids the third vice, for it does license the description of constituent explanatory states as states of tacit knowledge of the individual proper axioms or rules of a semantic theory. There is a measure of indeterminacy in these descriptions; there is, for example, nothing to choose between tacit knowledge of an axiom of $T_2$, say,

\[
\text{an object satisfies } 'F' \text{ iff it is bald}
\]

and tacit knowledge of the corresponding axiom of $T_4$

\[
\text{the extension of } 'F' \text{ is the set of bald things.}
\]

But each determinate tacit knowledge description does have the property that the content of the explained belief follows from the contents of the explaining states.

As for the second vice, I shall say only that I know of no obvious case in which the final account would classify two algorithms as realizing the same states of tacit knowledge, while Peacocke would distinguish the two algorithms as drawing upon different information.

The first vice of the original account was that it used an undifferentiated notion of causal structure, and was thus wide open to Wright's second objection. The revised account was already an improvement in this respect. But, in terms of the basic idea of giving an information-processing description of a causal process, there were things to be said against, as
Beginning from Evans's proposal, I have discussed three accounts of tacit knowledge. The choice between the three is clear. The first account gives wrong answers; for example, in the case of speaker C. And once the notion of sensitivity is introduced to yield the revised account, we have everything we need for the final account.

That final account is intended to meet Wright's original three objections, and to provide a form of description of subjects that can be deployed in information-processing psychology. I have had to part company with Evans over one point—the five per cent difference. But for all that, the form of Evans's original proposal shines through.¹

References

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