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Making the Best of Austin's Goldfinch

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Abstract

This paper discusses Austin's goldfinch example from "Other Minds," which plays a central role in Kaplan's *Austin's Way with Skepticism*. The paper aims to clarify the obscure distinction Austin makes in connection with this example, between cases in which we know and can prove and cases in which we know but can't prove. By discussing a couple of remarks that Austin makes in passing, a view is extracted from his text that stands in conflict with Kaplan's reading at a fundamental point. The view proposed emphasizes the role of law-like generics in our practice of knowledge attribution, and brings out the disjunctivist elements in Austin's conception. It is argued that the response to skepticism that Kaplan ascribes to Austin is not fully satisfactory, since it fails to tell us what makes some challenges to our knowledge claims appropriate and others outrageous. The alternative view proposed in this paper can handle this problem without postulating the sort of general external criterion that Kaplan's Austin rightly rejects.

Keywords: Austin, disjunctivism, generics, Kaplan, knowledge, skepticism

1. Introduction

The use of Cornelius Nozeman's lovely pictures of the *Fringilla Carduelis* on the dust jacket of Mark Kaplan's *Austin's Way with Skepticism* suggests how central Austin's goldfinch example from "Other Minds" is to Kaplan's discussion. It is the example with which Kaplan

introduces Austin's criticism of the skeptical challenge (Kaplan 2019: 6), and he returns to it again and again throughout the book. Kaplan's investigation is a model of philosophical attentiveness and clarity, and his vivisection of Austin's goldfinch is extraordinarily careful and illuminating. So, it might seem unlikely that there is much to add to what he manages to bring out from it. And yet, in this paper I will identify and explain the significance of some features of Austin's example that seem to me worthy of further consideration—features that Kaplan does not discuss at any length, and which can help us identify a lacuna his reading. I will argue that this lacuna makes it impossible for Kaplan's Austin to handle the skeptical challenge in a fully satisfactory manner.

My aim in what follows is to make the best of Austin's goldfinch example. By this, I do not mean giving the most plausible account of how Austin himself conceived its significance at the time of writing "Other Minds." Nor do I mean using Austin's text as a mere Rorschach spot upon which I project my own favorite conception of knowledge. What I will actually do is to attend to some rather puzzling parts of Austin's discussion. They involve peculiar opacities and tensions—I am thinking here in particular of the suggestive but obscure distinction he makes between cases in which we know and can prove, and cases in which we know but can't prove. I will seize on certain remarks that Austin makes in passing, and that are left uncommented by virtually all interpreters, including Kaplan. I will argue that these apparently tangential remarks are more important than they seem, in that they can help us explore and find a useful way through the tensions in Austin's discussion.

Admittedly, I build a case for a reading of which there are only seeds in the actual text. However, I do think the seeds are there, and that it is rewarding to imagine the plants that would grow from them. Such imagination requires a retrospective approach—in particular, I read "Other Minds" through the spectacles of the later *Sense and Sensibilia* lectures. But I freely acknowledge that the view I ascribe to Austin is not fully stated or defended in any of

his writings. Someone might worry, then, that I preposterously claim to have understood Austin better than he understood himself. And I do not want to deny making such a claim. I even hold that kind of aspiration to be sometimes necessary in the effort to inherit a philosopher's work.

One way of gesturing *roughly* at where I am going is to say that I take Austin's conception of empirical justification to share certain structural features with what is nowadays called 'disjunctivism'. This is by itself not a new idea. Indeed, the *Sense and Sensibilia* lectures are often seen as one of the most important precursors to contemporary forms of disjunctivism. However, I hope it will become clear that my reading spells out the disjunctivist structure of Austin's conception in a novel manner. As I will also show, it differs significantly from Kaplan's reading.

Ascribing to Austin any kind of 'ism' is notoriously dangerous, since one of his overall aims is precisely to combat the schematic simplification and artificial theorization that tend to go together with philosophical isms. In the end, the label 'disjunctivism' is not important to me. I wholeheartedly agree with Guy Longworth that it may falsely foist upon Austin characteristically non-Austinian dualisms (Longworth 2019: 135-6). However, I also agree with Longworth that Austin's work is "distinctively hospitable" (2019: 132) to certain insights of a recognizably disjunctivist shape—insights, I want to add, that are better conceived as reminders of how our already familiar practice of knowledge attribution works than as descriptions made within some artificially imposed theoretical scheme.

Even if I believe that my take on Austin's goldfinch example is novel, it is deeply indebted to Sebastian Rödl's criticism of Hilary Putnam's attempt to deal with the challenge of skepticism (Rödl 2014). Several of my disagreements with Kaplan constitute direct applications of points that Rödl makes against Putnam. Intriguingly, one of Putnam's main sources of inspiration is Austin. So, my use of Rödl is also, indirectly, a matter of showing

how Austin would disagree with Putnam's Austin. Of course, Kaplan's take on Austin differs in many ways from Putnam's, and is elaborated in much more detail. However, their Austins have enough in common for both to be vulnerable to the criticism in question. My contribution, then, is to make it plausible that in this conflict, Austin's better self is on the side of the critics.

2. Knowledge and the ability to prove

One of the many purposes for which Austin uses the goldfinch example is to distinguish between cases in which we know and can prove, and cases in which we know but can't prove. Austin ties this distinction to two different answers that might be given to a question of the form, 'How do you know ...?'. If someone challenges my assertion that there is a goldfinch in the garden by asking, 'How do you know it's a goldfinch?', I may respond: 'From its red head'. This answer, says Austin, "differ very materially" (Austin 1979: 84) from another answer that I may also give, namely: 'Because it has a red head'. According to Austin, 'From its red head' contains a sort of "vagueness" (1979: 85) that makes it akin to answers such as 'From its behavior' or 'From its markings'. What he means is that in cases like that of the goldfinch, when knowing is a matter of recognizing by seeing (or otherwise sensing) certain features which are similar to something we have noted on earlier occasions, it might well happen that we do not have the words to describe those recognized features in a more detailed and non-committal fashion. Austin compares this case to cases in which we recognize a surly look or the smell of tar but cannot describe what we recognize otherwise than as 'surly' or 'of tar', and with cases in which connoisseurs are able to recognize vintages of port or fine shades of a color without being able to specify in non-committal detail how they recognize them. If I say that I know that the bird is a goldfinch 'from its behavior', and someone asks me to be

more precise, all I am able to produce might be a pretty rough description, and one that already presupposes that the behavior is the behavior of a goldfinch. Similarly,

when I say I can tell the bird ‘from its red head’, or a friend ‘by his nose’, I imply that there is something *peculiar* about the red head or the nose, something peculiar to goldfinches or to him, by which you can (always) tell them or him. In view of the fewness and crudeness of the classificatory words in any language compared with the infinite number of features which are recognized, or which could be picked out and recognized, in our experience, it is small wonder that we often and often fall back on the phrases beginning with ‘from’ and ‘by’, and that we are not able to *say*, further and precisely, *how* we can tell. Often we know things quite well, while scarcely being able at all to say ‘from’ what we know them, let alone what there is so very special about them. Any answer beginning ‘From’ or ‘By’ has, intentionally, this saving vagueness. (Austin 1979: 85)

By contrast, Austin continues, the answer ‘Because it has a red head’ is “dangerously definite”. He explains:

When I say I know it’s a goldfinch ‘Because it has a red head’, that implies that all I have noted, or needed to note, about it is that its head is red (nothing special or peculiar about the shade, shape, etc. of the patch): so that I imply that there is no other small British bird that has any sort of red head except the goldfinch. (Austin 1979: 85)

Austin then goes on to tie this difference between answers using ‘because’ and answers using ‘from’ to the distinction between knowing with proof and knowing without proof:

In the present, very common type of case, ‘proving’ seems to mean stating what are the features of the current case which are enough to constitute it one which is correctly describable in the way we have described it, and not in any other way relevantly variant. Generally speaking, cases where I can ‘prove’ are cases where we use the ‘because’ formula: cases where we ‘know but can’t prove’ are cases where we take refuge in the ‘from’ or ‘by’ formula. (Austin 1979: 85-6)

Austin’s reasoning here is confusing. After all, if ‘proving’ means “stating what are the features of the current case which are enough to constitute it one which is currently describable in the way we have described it, and not in any other way relevantly variant,” then it would seem that a description need not be non-committal in order to offer a proof. Suppose I say that I know it’s a goldfinch ‘from its red head’, and is challenged again: ‘Plenty of other birds have red heads. For all you know, it may be a woodpecker’. And suppose all I am able to give is a blatantly committal response, such as: ‘No, what I mean is that the red spot on the bird’s head has that peculiar shape and color that are found only among goldfinches’. Even if I am unable to offer a more detailed and non-committal description, I have certainly stated what are the features of the current case which are enough to constitute it one which is currently describable in the way I have described it, and not in any other way relevantly variant. So, if proving means what Austin says it means, then I *am* able to prove what I know.

One may of course complain about circularity, but such a complaint would seem to presuppose that proving what I know by reference to what I see (or otherwise sense) is always and basically a matter of taking an inferential step from a recognized perceptual given that could in principle be fully specified in non-committal terms (had we only the adequate linguistic resources to do so) to a conclusion about the supposedly known worldly object or

fact. And in fact, the diagnosis Austin offers of why we “fall back” on the ‘from’ formula invites precisely such a conception. As we have seen, he says it is due to the “fewness or crudeness of the classificatory words in any language” that we cannot describe all the features that we perceive in a detailed and non-committal fashion. The implication seems to be that all these features could in principle be exhaustively described in a non-committal way, if we only had a suitably rich and precise vocabulary. Thus, even if I do not find non-committal words to describe the exact shape and color of the red spot, what I directly perceive would still be distinct from the goldfinch itself, and in principle fully identifiable without taking a stand on the issue of whether what I see is actually a goldfinch. This would in turn entail that even if we can know without being able to state in words how we infer what we know from what we directly perceive, this inability of ours cannot be of fundamental epistemological significance, since it would only be due to the failure of our language to fully capture what we directly perceive. The use of the ‘from’ formula rather than the ‘because’ formula would then indeed only be a matter of “taking refuge” in something we could dispense with in the ideal case. If we were only able to spell out the grounds that we actually have for our perceptual knowledge, our proof would take an appropriately non-circular inferential shape.

It is somewhat puzzling that Austin speaks in such seemingly favorable terms of this conception. After all, he was one of the most influential *critics* of this picture of perception and perceptual knowledge—and even if his criticism is clearly formulated only a decade later, in the *Sense and Sensibilia* lectures (see, e.g., Austin 1962: 115, 138-139), it would not seem far-fetched to interpret his early interest in cases in which we know but are unable to provide a non-circular inference from given perceptual contents as premonitions of that later criticism. Indeed, if they aren’t such premonitions, then *why* does Austin make such an affair of the difference between the cases he says are associated with the ‘from’ formula and the cases he says are associated with the ‘because’ formula? If this difference is only due to the fewness or

crudeness of our classificatory words, then it seems it could not have the sort of important epistemological significance that Austin also appears to be suggesting.

I think what we should say is that the ‘from’ cases—the cases where we know but can’t offer a non-circular inference as proof—are given an interestingly unclear and awkward status in “Other Minds.” On the one hand, Austin seems to want to distinguish them from cases where a proof in the sense of a non-circular inference is involved. On the other hand, he describes them as if they also involve (or can be reconstructed as involving) such an inference, albeit a *tacit* one—the idea being that even if we lack a sufficiently rich and fine-grained classificatory vocabulary to be able to state that inference, there is nonetheless such a non-circular inferential inference to be made from what we perceive to what we claim to know about the world. So, if there is a premonition of the *Sense and Sensibilia* criticism present already in ‘Other Minds’, it is embryonic and ambivalent.

In his discussion of Austin’s goldfinch, Kaplan mentions Austin’s distinction between the sort of case associated with the ‘from’ formula and the sort of case associated with the ‘because’ formula. However, Kaplan’s focus is almost entirely on the latter, in which our knowledge requires that we are able to deliver a proof in the form of a (non-circular) inference. This is in line with the idea that the ‘because’ case is the epistemologically more interesting and fundamental one. However, I believe that there is more to be said for the idea that already in “Other Minds,” Austin is trying, albeit vaguely and inchoately, to get at a genuinely important epistemological distinction, and that his talk of the imperfections of our classificatory vocabulary should not mislead us into focusing so exclusively on the inferential structure associated with the ‘because’ formula.

3. Generics and the possibility of being wrong

Curiously enough, the best way of motivating the claim I just made is to look closer precisely at Austin's account of how the 'because' formula works in the goldfinch case. Consider again what Austin says:

When I say I know it's a goldfinch 'Because it has a red head', that implies that all I have noted, or needed to note, about it is that its head is red (nothing special or peculiar about the shade, shape, etc. of the patch): so that I imply that there is no other small British bird that has any sort of red head except the goldfinch. (Austin 1979: 85)

Why does Austin note that when I say I know it's a goldfinch 'Because it has a red head', I imply that there is no other *small British bird* that has any sort of red head except the goldfinch? After all, he has just claimed that when I say I know it's a goldfinch 'Because it has a red head', that implies that *all* I have noted about it is that its head is red. However, the added clause shows that he thinks the discussion between me and my interlocutor presupposes that I have noticed more things—such as that the creature in question is a *bird*, and that it is *small*. His point seems to be that an objection such as 'Black grouses have red heads too' would in the envisaged situation be out of place, since black grouses are so much bigger than goldfinches. That's just not the sort of mistake we make; when, in normal circumstances, we perceive a bird, we perceive its approximate size. Also, Austin presupposes that the circumstances are such that we cannot reasonably doubt that what I see is some kind of bird, rather than, say, a red-headed salamander or a gigantic butterfly.

Even more striking, however, is the predicate 'British'. It is different in function from 'small': we do not perceive the Britishness of a bird in the same sense as we perceive its size. What is the point, then? Well, consider a challenge such as the following: 'Plenty of other

birds have red heads. For all you know, it may be a bay-headed tanager'. Even if the bay-headed tanager is a red-headed bird about the same size as a goldfinch, the challenge would be misplaced if not further motivated. Why? Well, because bay-headed tanagers live in the forests of Central and South America, and not in Britain. (I am presupposing that the conversation about the goldfinch takes place somewhere in the Oxford area.)

Let us pursue this point a bit further. Importantly, the statement

(1) Bay-headed tanagers live in the forests of Central and South America, and not in Britain

functions here as a generic statement. It has a law-like character, and does not involve the sort of generality expressed by universal quantification over particulars, or any other extensional quantifier.¹ For example, it does not just say that bay-headed tanagers are much more frequent in Central and South American forests than in Britain. Rather, it says that the Central and South American forests are the bay-headed tanager's habitat. This is why it can have the function I have just gestured at, namely, that of excluding the *possibility* that the bird in my garden is a bay-headed tanager.

(1) is a perfectly fine and ordinary way of explaining why the challenge, 'Plenty of other birds have red heads. For all you know, it may be a bay-headed tanager', is outrageous as it stands. By contrast, even if the number of woodpeckers in Britain decreased radically (as is already true of the lesser spotted woodpecker, the sort of woodpecker that is presumably most easily mistaken for a goldfinch), this would not mean that the challenge 'For all you know it might be a woodpecker' would be out of place. If I know, I can't be wrong—but if I am in Oxford, I *can* be wrong as long as I have not assured that the red-headed bird I saw is

¹ Rödl speaks in this connection of 'sublunary laws' (Rödl 2014: 128).

not a wood-pecker. This would be so even in a situation when wood-peckers are on the verge of extinction.

So, Austin's use of 'British' in the quotation above suggests an important difference between

(2) Plenty of other birds have red heads. For all you know, it may be a woodpecker

and

(3) Plenty of other birds have red heads. For all you know, it may be a bay-headed tanager.

(2) does identify a way in which I can be wrong. For all I know, the bird may be a woodpecker. This is the sense in which it is not sufficient for knowledge that the grounds I invoke make my knowledge claim merely probable, even if the probability is very high. By contrast, (3), as it stands, does not identify a way in which I can be wrong. Why? Well, again, because the bay-headed tanager lives in the forests of Central and South America and not in Britain. Hence the bird I see cannot be a bay-headed tanager, even if my ground for saying that it's a goldfinch is that it has a red head and bay-headed tanagers have red heads too.

Of course, the situation changes if there is a particular positive reason to think that there are bay-headed tanagers in the area. Perhaps my neighbor has imported a number of tanagers to be used as cage-birds, and yesterday they escaped and might thus be flying around in my garden. However, this is only a way of restating the difference between (2) and (3). (2) identifies a possible way of being wrong *already as it stands*—that is, even if there is no further positive reason to suspect that there are any woodpeckers in my garden. By contrast,

(3) identifies a possible way of being wrong only if there is some further, special reason to think that there are bay-headed tanagers around. The mere fact that there exist bay-headed tanagers in the world, or that it involves no formal contradiction to imagine that a bay-headed tanager has found its way into my garden, is precisely *not* sufficient to show that I may be wrong. The ‘may’ does not refer to mere non-contradictoriness, but to the sort of possibilities and impossibilities outlined in generic statements such as ‘The woodpecker is a British bird’ and ‘The bay-headed tanager is not a British bird, but lives in the forests of Central and South America’.²

Importantly, this notion of possibility is tied to the notion of *intervention* (in contrast to that of a merely unlikely event; cf. Rödl (2014: 130)). Even if woodpeckers had become very rare, the presence of a woodpecker in my garden would in no way intervene with the way things happen in the world. A British garden would still be the kind of place where woodpeckers are to be found. By contrast, the presence of a bay-headed tanager in my garden *requires explanation*: something special, an intervention, must have occurred that made it possible for the tanager to be there. Again, my neighbor might have imported the bird, after which it managed to escape from its cage. Without such an intervention, the bird could not have been in my garden.

4. The Missing ‘In Virtue’ Account

What is the significance of my discussion in section 3? Here is one important point. Suppose someone tries to challenge my claim that there is a goldfinch in the garden by saying, ‘Plenty of other birds have red heads—for all you know, it may be a bay-headed tanager’. Suppose

² I shift here between the plural and the definite singular: “Bay-headed tanagers live in the forests of Central and South America”, “The bay-headed tanager lives in the forests of Central and South America”. The possibility of this variation manifests the generic character of these statements.

also that this interlocutor offers no special reason to think that there is a bay-headed tanager in the garden. If so, his ‘challenge’ is no challenge at all, for it does nothing to show that I may be wrong. However, notice that here I can do more than simply dismiss his attempted challenge as ‘outrageous’, or ‘irrelevant’, or ‘out of place’. I can tell him why his attempted challenge fails to identify a way in which I may be wrong: The bay-headed tanager does not live here, but in the forests of Central and South America. I do not just fall back on a sense of outrageousness shared by all competent participants in the practice. I can offer a substantive reason for my dismissal.

Likewise, if someone would challenge my claim to know it’s a goldfinch by saying, ‘For all you know it may be a stuffed goldfinch’, I can answer: ‘Don’t be silly. Stuffed birds are used in classrooms, museums, or sometimes as decorations in people’s houses—they are not put up in trees just like that’. Again I do not just fall back on a shared sense of silliness, but offer a genuine reason in the form a generic statement. Of course, an intervention is possible here too: Perhaps my neighbor is an eccentric who likes to break social rules, and so has decided to put up a stuffed goldfinch in my tree as a joke. But again, the need for such a special reason to think that I may be mistaken only reinforces the point just made. As long as the challenge that the bird might be stuffed is not further motivated—in this case, by reference to my neighbor’s tendencies to act in contravention of established custom—responding that people don’t put up stuffed birds in trees just like that is enough to show that the bird cannot be a stuffed goldfinch (cf. Rödl’s discussion of Putnam’s house-painting example (Rödl 2014: 127ff.)).³

³ Notice that my argument here entails a different and more straightforward response to the argument from ignorance than Kaplan’s intriguing solution (cf. Kaplan 2019: 63-77). The upshot of my argument is that when the first premise in such an argument involves an ‘outrageous’ hypothesis—as in the case of ‘You don’t know that the bird is not a bay-headed tanager (or a stuffed goldfinch)’—that

Like so many other interpreters of Austin, Kaplan bypasses in silence the occurrence of the words ‘small’ and ‘British’ in Austin’s remark. Perhaps it is not surprising, then, that he seems oblivious to the possibility of rebutting challenges such as ‘For all you know it might be a bay-headed tanager’ and ‘For all you know it may be a stuffed goldfinch’ in the fashion I have just described. Instead, Kaplan reasons as if terms of rebuttal such as ‘outrageous’, ‘out of place’, ‘silly’, and so on, must in the end stand on their own. It is true that he recognizes a possible worry at this point—namely, the worry that a satisfactory conception of knowledge must involve an account of *why* ‘outrageous’ challenges *are* outrageous, an account which does not simply fall back on a shared sense of outrageousness. *In virtue of what* can we denounce such challenges as silly or out of place? However, his answer to this worry does not seem satisfactory. Let me explain why.

As Kaplan understands the worry, it involves the idea that our practice of knowledge attribution should have as its basic foundation some ultimate criterion that is external to the practice itself. According to Kaplan, to ask with what right we treat some challenges as normal and others as outrageous is, in effect, to ask for “an account that will explain what difference there is between the hypotheses a person *does* have to rule out before she can count as knowing that P and those that she does not—where that difference is not to be explained simply by appeal to facts about what we do in ordinary practice” (Kaplan 2019: 58). Moreover, Kaplan argues that this requirement is also a requirement that the account in question be “a relatively simple, general account that will tell us by virtue of what something needs (or does not need) to be ruled out in a given case” (2019: 59). And he then rightly

premise is simply false. I do know that the bird is not a bay-headed tanager or a stuffed goldfinch, since I know that bay-headed tanagers do not live in Britain and that people don’t put up stuffed birds in trees just like that.

points out that the request for such an external, general and simple account is hard to motivate or even make clear sense of.

However, what I have argued is not that we need and can make sense of the idea of such an external, simple and general criterion. Rather, my claim is that our practice of knowledge attribution is itself richer than Kaplan imagines—that this practice already contains the resources needed to explain why certain challenges are out of place or outrageous. Importantly, those resources do not provide us with any simple and general criterion, but involves an open-ended plethora of different specific grounds for rebuttal—‘Bay-headed tanagers live in the forests of Central and South America, and not in Britain’, ‘People don’t put up stuffed birds in trees just like that’, and so on, and so on, and so on.

I entirely agree with Kaplan that we do not become competent participants in our practice of knowledge attribution by getting informed about some externally comprehensible general criterion that draws the line between appropriate and outrageous challenges. He is completely right that “[g]etting the hang of practices [...] requires socialization and acculturation. So it is with the practice of knowledge attribution” (2019: 60). However, from this insight, Kaplan apparently draws the conclusion that our practice of knowledge attribution bottoms out, as it were, in a thus inherited and shared but ultimately groundless sense of what challenges are ‘normal’, ‘appropriate’, and what challenges are ‘out of place’, ‘outrageous’. And this is where I disagree. Getting the hang of our practice of knowledge attribution involves becoming knowledgeable about *how things work in the world*—for example, that bay-headed tanagers live in the forests of Central and South America and not in Britain, that people don’t put up stuffed birds in trees just like that, and so on and so forth. Acquiring such knowledge is an absolutely crucial part of getting a hang of our practice of knowledge attribution. Kaplan’s description of our practice is too thin, and he therefore ends up with a conception that retains too much of the skeptic’s viewpoint. Thus thinly described,

the practice still seems in need of external support in order to be a practice of *knowledge* attribution.

At one point, Kaplan does acknowledge the need to say something more than that we simply share a sense of what is ‘outrageous’ and what is not. For he remarks,

Austin *has* told us why the requirements in place in ordinary life should be as they are. They work. And one shouldn’t give up something that works except in exchange for something that looks to work better. (Kaplan 2019: 60)

What, then, did Austin mean by saying that the requirements “work”? Kaplan emphasizes Austin’s observation that when I tell someone that I know that *p*, I give her my authority for saying that *p*:

[Austin] saw that we have a practice giving our word, and accepting others’ words, via making, and accepting, knowledge claims. He thought that, for that practice to work well, the criteria by which we decide what we will say we know must be such as to incorporate precaution against error, but not so strict as to preclude our being able to say that we know lots of things—not so strict as to preclude our having any trade at all in knowledge claims. And he thought that our practice of knowledge attribution works in the sense that it strikes a good balance between (on the one hand) avoiding mistaken claims to knowledge and (on the other hand) allowing us to claim in good conscience a substantial amount of knowledge of the world around us. (Kaplan 2019: 61; note omitted)

I do not see how this is supposed to alleviate the skeptical worry, however. To begin with, to the extent that this is an instrumentalist justification of our practice of knowledge attribution, it would seem that what we have here is, in effect, a sort of Humean conception: We dismiss ‘outrageous’ challenges, including skeptical ones, simply because fulfilling certain independently describable practical needs would otherwise be impossible. Admittedly, it is not clear that Kaplan thinks the needs he talks about are independently describable and that the justification is therefore instrumentalist in such a sense. Kaplan’s use of normative turns of phrase such as ‘mistaken claims to knowledge’ and ‘good conscience’ perhaps suggests otherwise. But then, one might ask how he has earned the right to use those phrases in this sort of way to begin with—for, again, if our practice bottoms out merely in a shared sense of what is ‘outrageous’ and what is not, it is not clear that the notions of ‘mistake’ and ‘good conscience’ can be robust enough to avoid instrumentalist reduction after all. It seems to me that as long as Kaplan has not recognized the need for a rich description of our practice of knowledge attribution along the lines suggested above, there is no way out of this difficulty.

5. The Disjunctivist Logic of Generics

One may think that the conception I have been arguing for is either hopeless or useless or both. For even if generic statements such as ‘Bay-headed tanagers live in the forests of Central and South America, and not in Britain’ surely play some kind of role in our practice of knowledge attribution, it may be argued that their significance cannot possibly be as crucial as I have suggested. To begin with, doesn’t the general character of these generic statements entail that they cannot play such a central role, since they must be derived from a more basic form of knowledge acquisition, namely the observation of particular matters of fact?

It is of course true that our knowledge that bay-headed tanagers live in the forests of Central and South America, or that people do not put up stuffed animals in trees just like that,

is not *a priori*. However, this is not to say that such knowledge is based on the observation of particular facts in any simple, one-way fashion. Indeed, one of the central points of my discussion has been to show how our knowledge of such generics shapes our search for knowledge by observation, with regard to how what we perceive functions as evidence, what it means to exclude possible mistakes, and so on. I take this to be a recognizably Sellarsian point: Even if there is a sense in which our knowledge of such generics is based on observation, there is also a sense in which our use of observational evidence as evidence must rely on generic conceptions of how things happen in the world.

The acculturation involved in getting the hang of our practice of knowledge attribution comprises as a central part the acquisition of knowledge of such generics. Of course, these processes are enormously complex, as they involve the reliance on parents and other authorities as well as the child's own exploration of the world, inseparably intermingled. Importantly, becoming a mature participant in this practice involves developing an ability to subject parts of what is thus inherited to critical scrutiny. Nothing I have said goes against the possibility of discovering that some of the things we thought we knew were mere prejudices. The point is rather that the scrutiny involved is not simply a matter of confronting such generics against naked observational data—for invoking the 'tribunal of experience' in each case involves relying on other generics that are *not* put in question.

Here it is important to keep in mind that generics do not involve the sort of generality expressed by universal quantification. Again, generics elude any extensional analysis, even if the universal quantifier is weakened to an 'almost all', or a 'most'. The belief that an extensional analysis *must* be possible is the semantic counterpart to the idea that the knowledge of such generics must in the end be derived by one-way generalization from observations of particular states of affairs. It is only once we abandon this presupposition that

we can appreciate the actual structure of our practice of knowledge attribution, and can do justice to it as a practice of *knowledge* attribution.⁴

However, even if one wants to agree with these Sellarsian points, one may still feel that generics cannot play the sort of role that I have been ascribing to them. Straightforwardly put, the worry may be expressed as follows:

Generics of the sort I have mentioned do not determine whether one *can* be wrong or not. For, as I have admitted, an intervention is always *possible*. And if an intervention occurs, then the relevant generic statement is, as it were, no longer in play. But then, it seems to follow that if I do not know whether an intervention has occurred, I do not know whether the generic statement is in play—and hence I do not know what I claim to know. Consider the goldfinch case: For all I know, my neighbor might have imported some bay-headed tanagers, and they might have escaped from their cage. Hence, for all I know, the situation might be such that the fact that bay-headed tanagers live in the forests of Central and South America and not in Britain does not exclude the possibility that what I see in my garden is a bay-headed tanager. Consequently, as long as I have not excluded the possibility that an intervention has occurred, I have not excluded the

⁴ The idea that an extensional analysis of generics must be possible (to the extent that such generics so much as purport to describe the world) and the idea that they must be derivable by one-way generalization from the observation of particular facts, belong together to what might reasonably be called an *empiricist* outlook. My claim that these ideas cannot even so much as begin to do justice to the structure of our practice of knowledge attribution is in its turn a variety of the point (familiar at least since Kant) that the empiricist conception of knowledge is, in effect, no conception of knowledge at all.

possibility that the bird I see is a bay-headed tanager, and I may therefore be wrong when I claim that it is a goldfinch.

It is of decisive importance to see why this line of reasoning is invalid. The mere possibility that my neighbor has imported some bay-headed tanagers that have now escaped from their cage does not suffice to make it possible that there is a bay-headed tanager in my garden. Indeed, the very point of what I have argued is that the notion of ‘possibility’ that structures our practice of knowledge attribution is not transitive in that way. It is only an *actual* intervention that sets the generic out of play: it is only if, say, my neighbor has *in fact* imported some bay-headed tanagers who have now escaped from their cage that it is *possible* that there is a bay-headed tanager in my garden (of course other actual interventions can also have this effect). As long as such an intervention has not actually occurred—as long as it remains a mere possibility—the bird cannot be a bay-headed tanager. For such tanagers live in the forests of Central and South America, and not in Britain.

This might be said to constitute an ‘externalist’ element in our practice of knowledge attribution. Whether I may be wrong in certain ways or not depends on what in fact happens in the world, even if it happens unbeknownst to me. If my neighbor has in fact imported some bay-headed tanagers and they have in fact escaped from their cage, then my knowing that the bird I see is a goldfinch requires my excluding not only that it is a woodpecker but also that it is a bay-headed tanager. Importantly, however, this does not entail that I need to exclude the latter possibility also in a case where no such intervention has occurred. For in such a case, the bird cannot be a bay-headed tanager. So, the point is not that the possibility is still there in such a case, the difference being only that I now do not ‘need’ to exclude it, for some externally describable reason—say, the alleged need to “strike a good balance between avoiding mistaken claims to knowledge and allowing us to claim in good conscience a

substantial amount of knowledge of the world around us.” No—in such a situation, the ‘possibility’ is not just there. Why? Because red tanagers live in the forests of Central and South America, and not in Britain.

This is one way in which the view that I am extracting from Austin’s discussion has a recognizably ‘disjunctivist’ structure. There is no ‘highest common factor’ with regard to the logical role of the generic statement in the case when no intervention has occurred, and the logical role of the same statement in a case in which an intervention has occurred. In the former case, the truth of the generic statement entails that the bird I see cannot be a bay-headed tanager. In the latter case—the exceptional one when an intervention has occurred—that entailment is broken and it is quite possible that the bird I see is a bay-headed tanager (even if the generic statement is still *true*). Notice that the logical role of an extensional statement—a universal quantification, or a statement to the effect that an overwhelming majority of bay-headed tanagers lives in the forests of Central and South America, say—could not have such a disjunctivist structure. In relation to such a statement, the very notion of an ‘intervention’ has no application, and thus a distinction of the envisaged sort does not so much as get off the ground.

6. Knowing ‘from’ vs. knowing ‘because’

Let us now return to Austin’s obscure distinction between cases in which we ‘know and can prove’ and cases in which we ‘know but can’t prove’. As we have seen, Austin says the former sort of case is associated with the ‘from’ formula, whereas the latter sort of case is associated with the ‘because’ formula. I will end this paper by showing how the points I have made in the previous sections can help us get clearer about what this distinction amounts to.

Let us begin with the ‘because’ formula. You ask me how I know that what I see is a goldfinch, and I answer, ‘Because it has a red head’. Here, two sorts of challenge are

conceivable. One is exemplified by the woodpecker challenge: ‘Plenty of other birds have red heads. For all you know, it may be a woodpecker’. This challenge does not imply that any intervention has occurred. Rather, it simply states that the goldfinch is not the only small British bird that has a red head.

By contrast, the second sort of challenge involves a claim to the effect that an intervention has occurred (that it has *actually* occurred, not just that it is ‘possible’). For example, you may tell me: ‘Don’t you know that your neighbor has recently imported some bay-headed tanagers, and that a couple of them escaped yesterday? So, the bird may well be a bay-headed tanager’. Such a challenge is perfectly okay, but it differs from the woodpecker challenge in that it makes reference to an intervention.

Now, what about when I use the ‘from’ formula? You ask, ‘How do you know it’s a goldfinch?’, and I respond, ‘From its red head’. According to Austin, this is “materially different” from when I use the ‘because’ formula. But how, exactly?

Suppose you try the woodpecker challenge: ‘Plenty of other birds have red heads. For all you know, it may be a woodpecker’. According to Austin, this challenge somehow does not work here. Why? What Austin suggests is that my use of the ‘from’ formula implies that I in some sense know *more* than when I say ‘Because it has a red head’. But what does this extra knowledge consist in? Austin says that my use of the formula implies that I recognize something *peculiar* about the red head, something peculiar to goldfinches, by which I can (always) tell them. But what does *that* mean?

It is at this point that Austin starts talking about the fewness and crudeness of our classificatory words. He thereby suggests the following view: our classificatory vocabulary is too coarse to capture those peculiarities I perceive and by which I can always and safely tell goldfinches. This is why I must “take refuge” in the ‘from’ formula, or in some description that already presupposes that what I see is a goldfinch. This suggests that this formula and

those descriptions fail to do full justice to what I really see. If I only had a richer and more precise vocabulary at my disposal, I could provide a more definite description of the actual content of my experience—one that would not already assume that what I see is a goldfinch.

Now, I do not deny that *some* cases might be like that. Certainly, we often find that we lack the words to fully describe what we perceive. As I have already argued, however, if this is the only sort of case Austin has in mind, then it does not seem that the difference between the ‘from’ formula and the ‘because’ formula is very material. In fact, it would seem that epistemologically speaking, it must be the case associated with the ‘because’ formula (where we know on the basis of an inference) which is the fundamental one—since our incapacity to state the inference in other cases is due only to the poverty of our classificatory vocabulary.

However, there is another way of understanding ‘from’ cases which takes more seriously the notion that in such cases the knowledge we gain from our senses is not inferential. This conception does not blame our incapacity to state any such inference on the poverty of our classificatory vocabulary. Rather, the idea here is that the reason why no such inference is made is simply that what I knowingly see is the goldfinch itself. What is ‘peculiar’ about the red head is not that its specific nuance of red is one for which there is no word, or that it has some irregular geometrical shape that I cannot describe in precise detail – no, it is rather that it’s *the red head of a goldfinch*. This is the most full and precise description at my disposal, not because I lack more precise words, but because it already captures what I see.

In such a case, if you raised the woodpecker objection, I would reject it by answering, ‘no, no, it has the kind of red spot that is only found among goldfinches’. More generally, no challenge of the woodpecker sort—that is, no challenge that does not involve a claim to the effect that an intervention has occurred—would cut any ice against my claim to know that it’s

a goldfinch. The bird has that characteristically goldfinchy red spot on its head, so the case is settled—it cannot be anything else than a goldfinch.

But now, suppose instead that you issued a challenge of the *second* sort—that is, suppose you told me that some sort of intervention has occurred, and that I therefore do not really know that what I see is a goldfinch. The bay-headed tanager case does not work very well here, since bay-headed tanagers do look quite different from goldfinches. However, suppose you told me that my neighbor's eccentric hobby is to put up really well-made stuffed birds in the trees of other people's gardens, or that the new medication I have just started has as one side-effect that one's color perception is occasionally disturbed. Wouldn't an objection of this sort undermine my claim to know that it's a goldfinch 'from its red head'?

There are two cases here that need to be distinguished. One is that in which what you tell me about the intervention is true—my neighbor really has this eccentric hobby, my medication really has this strange side-effect, etc. In such a case, your challenge does indeed undermine my claim to knowledge. I would then have to make further investigations before I could rightly claim to know that it's a goldfinch that I am seeing.

The other case is that in which what you tell me about the intervention is false. My neighbor does not have this eccentric hobby, my medication does not have this strange side-effect, and so on. In that case, my knowledge claim is not undermined. No intervention has occurred, so I can't be wrong—what I see is a goldfinch, the case is settled. Notice that this is so, even if you give me some reasons to believe that an intervention has occurred. For example, suppose you are a doctor, so I trust your words about my medication. But unbeknownst to me, you are lying. In this case, I may withdraw my knowledge claim—but wrongly, for in fact I do know, via my senses, that there is a goldfinch in my garden. What I see is still a goldfinch, and I can't be wrong. No further investigation is needed (even if I mistakenly think so).

Again, we encounter a characteristically disjunctivist structure of our practice of knowledge attribution. With regard to what I know via my senses, there is no highest common factor between the case in which an intervention has occurred, and the case in which an intervention has not occurred. If an intervention has occurred, I do not know that it's a goldfinch. Further inquiries are required to settle the matter. In this case, my self-assured claim that what I'm seeing must be goldfinch is suddenly under unexpected pressure, and I need to look closer in order to determine what it is that I actually perceive. Of course, it may turn out that what I saw was a goldfinch after all. But since an intervention has occurred, this is something I need to make sure before I can claim to know that it's a goldfinch. By contrast, in the case where no intervention has occurred, my senses provide me with full knowledge: I cannot be mistaken, what I see is a goldfinch and I know it.

Compare this disjunctivist structure with the *non*-disjunctivist structure of the 'because' case when my proposed knowledge is inferential and you come up with the woodpecker challenge. What the woodpecker challenge reveals is that in this case, the perceptual knowledge I do have is not enough to constitute knowledge that what I see is a goldfinch. This is so, irrespectively of whether the creature actually is a goldfinch or not. So here we do have a highest common factor present both in the case in which what I have before me is a goldfinch and in the case in which what I have before me is something else (a woodpecker, say). In both these cases, what I know is *not* that it is a goldfinch, but something more limited: I know that it is a small British bird with a red head. If the only small British bird that existed were the goldfinch, then this perceptual knowledge could serve as a premise in an inference to the valid conclusion that what I see is a goldfinch. This would then be something I knew. What the woodpecker challenge shows is that the inference does not go through, and that therefore I do not know that the bird is a goldfinch—even if it happens to actually be one.

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