

# Revista Română de Filosofie Analitică

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## FRANKFURT-STYLE COUNTEREXAMPLES TO INFLUENCE THEORY OF CAUSATION

MILAN Z. JOVANOVIĆ<sup>1</sup>

**Abstract:** Two prominent counterexamples to Lewis's Influence theory of causation (Schaffer 2001, Hall 2004) happen to be structurally very similar to so-called Frankfurt cases. This should come as a surprise since Lewis explicitly addresses Frankfurt cases while formulating his theory, and claims that theory deals with cases like that successfully (Lewis 2000). Hence, a good question to ask is – whether these two counterexamples are indeed plausible and valid objections despite their structural similarity to the Frankfurt cases. In this paper, I offer an analysis of two mentioned counterexamples in order to answer this question. On the one hand, in agreeing with Noordhof (Noordhof 2001), I will try to show that Schaffer's counterexample can indeed be accommodated and explained by the Influence theory. On the other hand, I will try to maintain that, even if we accept Lewis's premises, the counterexample offered by Ned Hall is still plausible – due to a certain feature that differentiates it from both: Frankfurt cases and Schaffer's counterexample. While the latter two are cases of early preemption, Hall's Smart Rock scenario doesn't exhibit that – from the perspective of Lewis's theory – convenient causal pattern in which we can find stepwise influence (which is enough for the theory to get these cases right). This result, as I believe, shows why we should regard Hall's counterexample as a better and more plausible argument (than Schaffer's counterexample) against the Influence theory.

**Key words:** Influence theory of causation, preemption, Frankfurt cases, ancestralization, counterfactuals.

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## Introduction\*

Faced with the seemingly unsolvable problem of late preemption, David Lewis had abandoned his original counterfactual analysis (Lewis 1973, 1986c) and formulated a new theory of causation (Lewis 2000). This new theory – the Influence theory of causation (ITC) – has also been strongly criticized. Interestingly, the counterexamples (CEs) offered against ITC typically involve, again, cases of preemption.

An even more interesting fact is that two prominent CEs offered against the theory (Hall 2004, Schaffer 2000) bear a striking structural resemblance to well-known Frankfurt cases (Frankfurt 1969). This should come as a surprise since Lewis explicitly addresses Frankfurt cases (FCs) while formulating his theory (Lewis 2000) and claims that the theory deals with cases like that successfully.

So, the question is whether these CEs are plausible, and Lewis is simply wrong about FCs, or, conversely, Lewis is right that ITC can explain Frankfurt-style examples, while Schaffer's and Hall's CEs are flawed? Or there is even some more refined resolution, questioning perhaps the similarity between offered CEs, or between them and FC?

Those are the questions that I am going to pursue in this paper. Before turning to them, I will present ITC and show how FCs – according to Lewis – can be accommodated within ITC. After presenting and analyzing given CEs, I will briefly point out the results and summarize the paper.

## Influence theory of causation

Belonging to the same theoretical background as its predecessor – Lewis's counterfactual theory of causation (CTC) – ITC is an analysis of the singular causal statements about actual events (and omissions).<sup>2</sup> It is

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<sup>2</sup> See Lewis 1986b for the discussion on omissions (whether they are events and should they be accepted as the causal relata).

concerned with the non-discriminatory notion of a cause,<sup>3</sup> and it is restricted to deterministic worlds.

The central notion of ITC is that of *influence*. Instead of analyzing causation in terms of simple counterfactual dependence between actual events (as in CTC),<sup>4</sup> in this new theory, Lewis proposes analysis is based on the relation of influence which is defined as a pattern of counterfactual dependences between various (actual and non-actual) *alterations* of two given events.

“Alteration” is a technical term in ITC. An alteration of a given event is an extremely fragile version or variation of that event, i.e. version of the event with maximally specified conditions (time and manner) of its occurrence. If we look *in that way* on some given actual event, with fully specified conditions of its occurrence, it is itself an alteration. But naturally, the rest of the alterations of that event are non-actualized, but merely possible events which are at least slightly different from the actualized alteration of the given event.

For example, if we take some actual throwing of the rock (call it event *a*) in some specific moment *t*, with a specific mass of the rock *m*, and force of the throw *f*, at a specific angle  $\theta$ , and so on... the alterations of *a* (alongside the alteration that is actualized) would be possible events (*a*<sub>1</sub>, *a*<sub>2</sub>, *a*<sub>3</sub>, *a*<sub>i</sub>...) which differs (at least slightly) from the actual throwing with respect to time, force or angle of the throw, or with respect to the mass or the shape of the rock, and so on.

Lewis defines influence as follows:

“Where *c* and *e* are distinct actual events, let us say that *c* influences *e* iff there is a substantial range *c*<sub>1</sub>, *c*<sub>2</sub>, . . . of different not-too-distant alterations of *c* (including the actual alteration of *c*) and there is a range *e*<sub>1</sub>, *e*<sub>2</sub>, . . . of alterations of *e*, at least some of which differ, such that if *c*<sub>1</sub> had occurred, *e*<sub>1</sub> would have occurred,

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<sup>3</sup> Meaning, it is not an analysis of *the cause, main* or the *prominent* cause, but rather a theory about what should count as *a cause*, without further pragmatic considerations.

<sup>4</sup> To be more precise, causation is in CTC defined as the causal dependence between distinct events, which is in turn defined as a counterfactual dependence between the proposition about the occurrence of those events. Also, causation is ancestral of causal dependence

and if  $c_2$  had occurred,  $e_2$  would have occurred, and so on.” (Lewis 2000 [2004], 91)

Back to the example of the rock-throwing. Let us imagine a very simple scenario: Suzy throws a rock (event  $s$ ) and breaks the bottle (event  $x$ ). Surely, there is a substantial range of alterations of Suzy’s throw such that – if those specific events (i.e. alterations) had taken place instead of her actual throw, some other alterations of bottle breaking, different than the actual one would have taken place as well.<sup>5</sup> So, if Suzy were to throw the rock earlier or at a different angle (which would constitute different alterations  $s_1$ ,  $s_2$ , and so on), the bottle would break earlier or with the glass flying off somehow differently than it actually did (the alterations  $x_1$ ,  $x_2$ , and so on).

According to the definition given above,<sup>6</sup> Suzy’s throw *does* influence the bottle breaking. And, since her throwing of the rock is clearly a cause of the bottle being broken, that is a good result if we are to analyze causation using the influence relation.

This *almost* completes ITC.

### ITC and preemption

In the introduction, I have mentioned the cases of preemption as an insurmountable obstacle to Lewis’s CTC. Let us take a closer look at these cases since they are important for the rest of the paper and, moreover, they could help us understand ITC better.

Cases of preemption are asymmetrical cases of overdetermination (or redundant causation). In all cases of redundant causation, we have more than one event (say:  $a$ ,  $b$ ,  $c$ ...) that “overdetermine” some effect, i.e. more than one event, such that each is sufficient for the effect ( $e$ ) in the absence of others. In *asymmetrical* redundant causation, we can, in addition, clearly identify one among those overdetermining causes (say:  $c$ ) as a cause, and others as merely the backups.

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<sup>5</sup> I will – for the sake of convenience – often use the term *mapping* for this counterfactual relation between the alterations of cause and the alterations of the effect.

<sup>6</sup> For now, we can leave aside the vague phrases in the definition: “substantial range” and “not-too-distant”.

The problem these cases pose to the CTC is immediately obvious – there is no counterfactual dependence between the given cause (*c*) and the effect (*e*) since it is not true that: if *c* had not occurred, *e* wouldn't have occurred. Because, it would, caused by the *b* or *a*, in the absence of *c*.

To illustrate an ordinary case of preemption, let us add to the Suzy's scenario another person – Billy, as in famous Lewis's examples (Lewis 1986c) – who throws another rock, aiming for the same bottle. So, we have Billy and Suzy throwing the rocks in order to break the bottle. Suzy's rock arrives first and breaks the bottle, just a millisecond later Billy's rock flies through space where the bottle had been. However, if Suzy's throw had been absent, Billy's rock would have broken the bottle, in almost the same time and manner.

ITC, as mentioned, was offered with the promise of solving the problem of preemption. So, how ITC works in these cases? There are clearly many alterations of Suzy's throw which could make a difference to the bottle breaking. Change the angle or the force of her throw (or the mass and shape of the rock) and, correspondingly, the effect would be different. However, there are also plenty of alterations of Billy's throw that map onto alterations of the bottle breaking – although, admittedly, not any alteration would do.<sup>7</sup> If Billy were to throw his rock earlier (enough), it would reach the bottle first and break it. Or, if he would have thrown the rock faster than he actually did, his rock would get there before Suzy's rock and would break the bottle. Moreover, every alteration of that sort (in which Billy's rock reaches the bottle before Suzy's rock), with further differences in force, mass, angle, and so on, would be mapped onto different alterations of the effect.

Should we conclude that both of these throws influence the breaking of the bottle? The answer is *no*, and the reason for that answer is present in the definition of influence. Vague phrases “substantial” and “not too distant”, which acts as the restrictions on the *type* and the *volume* of the alterations – are there to ensure that we don't get this result (that both: preempting and preempted cause have the influence on the effect).

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<sup>7</sup> We should, however, acknowledge that not any alteration of Suzy's throw will do, either. The alterations of her throw which *delays* the collision of her rock with the bottle (so that Billy's rock hit the bottle first) don't map onto different alterations of the bottle breaking.

So, in the case of Suzy and Billy, the right resolution of these explicit vague qualifications would be the one that allows as not-too-distant only those (unactualized) alterations that differ from the actualized alterations so slightly that:

- Such alterations of Suzy's throw do stand in the counterfactual relation with the different alterations of the effect.
- On the other hand, such alterations of Billy's throw do not map onto different alterations of the effect.

The central idea with this vague and case-sensitive threshold for influence rests on the observation that preempting cause has an advantage (over the preempted cause) that is relevant to influence and hence theoretically exploitable. To put it simply, preempting cause is in (somehow) a more delicate relation with the effect (than the preempted one is) and it is always possible to find really subtle "wiggles" that will be enough for the preempting cause to make difference to the effect, but not enough for the preempted cause to do the same.<sup>8</sup>

It looks like ITC *works* well with cases of preemption. And this, again, *almost* completes ITC. Just one more important thing...

### ITC and Frankfurt cases

For all we have seen in this paper, causation in ITC can simply be identified with this (complexly defined) relation of influence. But *it isn't*. Like in the formulation of his CTC, Lewis again uses the maneuver of *ancestralization*, and here defines causation as the ancestral of influence. The precise definition is: "... *c* causes *e* iff there is a chain of stepwise influence from *c* to *e*." (Lewis 2000 [2004], 91)

In other words, two events (*c*, *e*) *can* stand in the causal relation although there is no direct influence between them, provided that there is a chain of influence(s) leading from *c* to *e*, i.e. provided there are some intermediate events (say) *d*<sub>1</sub>, *d*<sub>2</sub>, *d*<sub>3</sub>... *d*<sub>*n*</sub> such that *c* influences *d*<sub>1</sub>, *d*<sub>1</sub> influences *d*<sub>2</sub>, *d*<sub>2</sub> influences *d*<sub>3</sub>..., and *d*<sub>*n*</sub> influences *e*.

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<sup>8</sup> The reader should have in mind that what I have presented is a charitable and streamlined reading of ITC. What is the right interpretation of that theory, or the most adequate one, is an open issue addressed in almost all papers discussing the theory: (e.g. Kvat 2001, Strevens 2003, Choi 2005, Maslen 2004).

But, do we need this further addition to theory? And if so – why? For which cases it would prove necessary to admit even a stepwise influence? To motivate this amendment to the theory, Lewis explicitly cites famous Frankfurt cases (FCs), as the perfect example of causation without direct, but with indirect (stepwise) influence.

In his classical paper, Harry Frankfurt offers a scenario that shows how a person could be morally responsible for some action even when she could not have acted differently (Frankfurt 1969: 835-836). The scenario involves Jones who is about to make some decision and perform some action, and Black who wants Jones to perform the exact action *e*, and who has the means of ensuring that outcome (let's say that he is a neuroscientist – as it is commonly assumed – who can control Jones's brain). Black monitors Jones in the process of deliberation and makes a decision whether or not to intervene, depending on the decision Jones had come to. Nevertheless, in the course of events, Black didn't have to do anything since Jones decided to do and did exactly what Black had wanted.

It is uncontroversial that Jones caused his consequent action.<sup>9</sup> But, an interesting question for us is – how can ITC account for this result? There is clearly no influence between his decision and the final outcome – due to the central feature of the given scenario; the outcome has no alternatives, it could not have been different, no matter what Jones had decided.

Lewis acknowledges both these claims: Jones's decision *was* a cause of his consequent action, and there is *no* influence between those two events. Nevertheless, he does not regard FCs as the CE to ITC, but rather as a clear example of *why* causation should be defined as an ancestral of influence relation.

How, according to Lewis, ancestralization helps in FCs? The explanation he gave is detailed and informative:

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<sup>9</sup> Although the scenario in question is not the most simple and clear case of causation, this is an intuitive verdict, commonly and widely accepted in literature, and it is an important thesis within Frankfurt's argument. In their seminal, textbook-like work on free will, Fischer (Fischer et al., 2014: 54-61), Pereboom (Fischer et al., 2014: 87-90) and Kane (Fischer et al., 2014: 167-170), all speak about the Jones's action using causal locutions.

“Let  $c$  be Jones's initial brain state; let  $e$  be the desired behavior. Consider a time after the neuroscientist has read Jones's brain, but before she would have seized control if the reading had been different. Let  $d$  combine Jones's brain state at that time with the neuroscientist's decision not to intervene. We have a two-step chain of influence from  $c$  to  $d$  to  $e$ . But  $c$  does not influence  $e$ .”<sup>10</sup>

So, he regarded this scenario as another case of preemption.<sup>11</sup> One way of putting this suggested causal structure into a Lewis-style diagram is given in *diagram 1*.

Circles in this diagram represent events. Simple arrows should depict influence relation; the arrow with the rounded tip marks the inhibitory connection – the one that stops some event from being actualized; the arrow with dotted line represents potential and unactualized causal relation – if the event at the beginning of that arrow had been actualized, it would cause the event on the end of it. Shaded circles represent actualized events, while the non-shaded one is unactualized (since it is inhibited).

Alongside the events Lewis explicitly mentioned, I added a few more that are implicit in his explanation, in order to have a more elaborate and informative diagram. I also added the indicated moment  $t$  with the vertical dotted line cutting

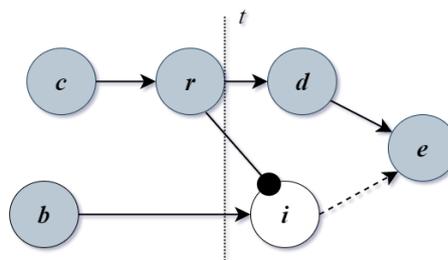


Diagram 1: Frankfurt's scenario

<sup>10</sup> One detail in this passage looks problematic. Namely, the Lewis's formulation that  $d$  combines Jones's brain state and Black's decision not to intervene, is either misleading or – in the worst case – goes against Lewis's argument here. I tried to avoid potential problems by keeping these two events simultaneous but distinct and separate. One such problem would be that if Black's decision not to intervene is on the main causal route, then it is also one of the causes of the given effect. And since it is (itself) the effect of Black's intentions and his monitoring, we then have a case of joint causation, rather than a case of preemption.

<sup>11</sup> Also, within the philosophy of free will – as the field from which this scenario originates – it is a commonplace to regard FCs as the case of preemption (see, for example, Funkhauser 2009, for the discussion on FCs and overdetermination)

through the course of events. Black with his intention and readiness to get the desired behavior from Jones is represented with *b*. Circle with *r* stands for Jones's brain state in the time of the crucial reading – reading that will determine whether Black intervenes or not. Black's intervention (on Jones's brain) that would happen if the reading had been different is depicted by *i* and it is inhibited or prevented by the actual reading.

Parallel to Lewis's explanation, in the diagram we have Jones's initial brain state (*c*) influencing his brain state at the moment of crucial reading (*r*), which in turn influences his decision (*d*).<sup>12</sup> We also have the reading (*r*) preventing Black's intervention (*i*), by deeming it redundant or unnecessary from Black's point of view. As expected, we have Jones's decision (*d*) influencing his behavior (*e*). And, finally, we have a potential causal relation between Black's intervention (*i*) and Jones's behavior (*e*), that is *merely* potential since Black's intervention wasn't needed and didn't happen.

To summarize: Lewis regards FCs, as many other philosophers do, simply as cases of preemption. Although different than worrisome case with Suzy and Billy, these cases are still accountable for within ITC, thanks to the ancestralization move (that allows stepwise influence to count as causation even without direct influence).

This completes ITC.

### Early and late preemption

Before turning to the analysis of the CEs, there is one more important distinction to be drawn. A careful reader probably noted that there is an important structural difference between the example with Suzy and Billy and Frankfurt's scenario. Although both of these cases are the cases of asymmetric redundant causation, and hence both are rightly called preemption, they nevertheless structurally differ.

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<sup>12</sup> The reader could probably see that *c* also influences *d*. That should not come as a surprise. Although influence is not transitive relation (as this exact example shows) it does not mean that it is antitransitive so that we can never have three events *a*, *b* and *c*, such that: *a* influences *b*, *b* influences *c*, and *a* influences *c*. Quite contrary, typically we would have just that.

When we take a closer look at Suzy and Billy case, we can see that what prevented Billy's rock from breaking the bottle was the sole event of bottle breaking – caused by Suzy's throw. In other words, there is nothing along the causal route leading from Suzy's throw to bottle breaking that inhibits (cuts, or stops in anyway) the causal route leading from Billy's rock to the same effect, except the occurrence of the effect itself.

Frankfurt's example is different in that respect. The event that stops Black from intervening happens early on in the scenario – certainly before the Jones has carried out his action. It is the crucial reading of Jones's brain ( $r$ ) that prevents Black from taking over the control, manipulating Jones's brain and hence causing the effect himself. And that is apparent in *diagram 1*.

On the contrary, in the Billy and Suzy case, we have two sequences of events – one starting with Suzy throwing a rock ( $s$ ) and another with Billy throwing a rock ( $b$ ) – that proceed towards the effect, without interfering with each other, until the end – i.e. until the occurrence of the effect. The shattering of the bottle itself ( $e$ ) prevents Billy's rock from breaking the bottle, by preventing some event antecedent to it, consisting of (say) Billy's rock making the initial contact with the glass of the bottle ( $g_2$ ). That can be represented as in *diagram 2*.

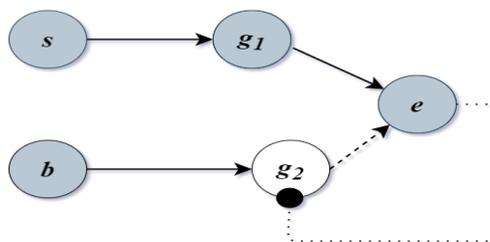


Diagram 2: Late preemption scenario with Suzy and Billy

When we compare *diagram 1* to *diagram 2*, we can see that the structural difference between them is concerned with – when this cutting of the alternative causal sequence happens. In the case of Jones and Black, it happens *early* – meaning, before the preempting causal sequence has reached the effect. Cases like this Lewis calls *early preemption*.

On the other hand, in the case of Suzy and Billy cutting happens *late*. It is the effect that functions as an inhibitor, so the cutting of the preempted causal chain happens in the moment of (or in some moment after) its occurrence. Cases of preemption with this feature Lewis calls *late preemption*.

## Two counterexamples: Button vs. Switchboard and Smart Rock

Back to the counterexamples. The literature about ITC is filled with different CEs to the theory (a list of notable ones would certainly include: Kwart 2001, Schaffer 2001, Strevens 2003, Hall 2004, Bigaj 2012). Some of them are directed towards proving that influence is not necessary for causation, i.e. that we can have causation without influence. Some others aim to prove that we can have influence between two distinct events without having causation between them. Some even aim for both.

In this section, I will present and analyze two prominent CEs, which allegedly prove that influence is neither necessary nor sufficient for causation. As it will become apparent shortly, those two CEs are structurally very similar to each other, and even more interestingly, very similar to – just discussed – Frankfurt cases. It is exactly this latter similarity that should make us suspicious with regards to their plausibility and effectiveness against the theory.

Let us start with the CE offered by Schaffer, which he conveniently called *Button vs. Switchboard counterexample* (BvS).

“The set-up: Pam is locked in a room which contains a single button. Bob is locked in a room which contains a vast switchboard. Vic is covered with electrodes and strapped to a chair. The story: Pam presses the button. Bob just watches. Vic is electrocuted.” (Schaffer 2001, 12) [Additional story] “Bob is in fact a preempted backup who will jump in if Pam delays for even a millisecond... and Pam’s wiring was only just set up at the time she actually pressed, so that had she hastened even a millisecond her button would not have worked and Bob would then have done the deed at the time and in the manner Pam actually did.” (Schaffer 2001, 15)<sup>13</sup>

The moral of the story is this: we are inclined to consider Pam’s pressing the button as a cause of Vic’s electrocution, but that event has no influence whatsoever on the effect. Take any alteration of her

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<sup>13</sup> Schaffer develops throughout the paper different CEs based on the same basic set up – that he offers at the beginning. That is why this specific CE is segmented and has two parts.

pressing the button – in which she presses the button earlier, later, harder, with right or left finger ..., or even the alteration where she doesn't press the button at all – the corresponding effect is the same, Vic is getting electrocuted (at the same time and in the same manner).

On the other hand, Bob is only a preempted back up, and we wouldn't count him as a cause of the electrocution. Nevertheless, as Schaffer claims, we would be justified in asserting that Bob's watching influences the electrocution, since he has a vast switchboard for manipulating Vic's electrocution.<sup>14</sup>

To summarize, according to Schaffer, in BvS we have a case in which a cause has no influence on the effect; and, moreover, we have the event which does have the influence on the effect but is still not a cause of that effect, but merely a preempted alternative.

Structural similarity to the Frankfurt cases should be apparent.<sup>15</sup> In both cases, we have one person (Jones/Pam) ready to perform some action, and the other person (Black/Bob) monitoring the process and ready to step in if the first person diverges from the course leading to that desired behavior. In both cases, that other person does not intervene in the end, but acts as a preempted back up, ensuring that there is no alternative to the actual effect.

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<sup>14</sup> Bob, according to the scenario and thanks to the aforementioned switchboard, can deliver Vic's electrocution in a different time and different manner: varying in the power of electricity, frequency, etc. That guarantees a very reach range of alterations (of Bob's behavior) that map onto different alterations of Vic's electrocution. It is, indeed, objectionable – as Noordhof points out (Noordhof 2001) – that those alterations are not-too-distant since we need different sorts of using the switchboard to be the alterations of Bob's *just watching*. But that is hardly an objection that would bother Schaffer. All he needs is that preempting cause doesn't have the advantage (of the kind described earlier) over preempted cause, so no resolution of vague restrictions "not-too-distant" and "substantial" can go in favor of preempting cause.

<sup>15</sup> There is, still, one important difference. In Schaffer CE we have embedded contrast between what Pam did and could have done, and what Bob did and could have done. The idea behind that is, of course, to provide Bob with a rich range of alternatives that map onto the effect, while restricting the same for Pam. There is no such strong contrast in Frankfurt cases. However, this is not relevant to the overall structure of the scenario and to the question of the applicability of Lewis's ancestralization maneuver.

Interestingly, Schaffer himself does not mention the Frankfurt cases, nor he addresses this similarity in any way. Nevertheless, that similarity poses a serious threat to the plausibility of his CE. If BvS can be framed in early preemption pattern in the same way Frankfurt cases can, then we would be right to dismiss it as an effective CE for ITC.

Before addressing this question, let us take a closer look at another CE with a similar structure – again. Namely, we are going to examine the Smart Rock CE, offered by Ned Hall, although he credits it to Steven Yablo (Hall 2004, 237). The aim is, again, twofold, but the stress in the paper is on the claim that influence is not necessary for causation. To show that, Hall (or rather Yablo) proposes a slightly altered story about Suzy and Billy:

“This time, Billy throws a Smart Rock, equipped with an on-board computer, exquisitely designed sensors, a lightning-fast propulsion system – and instructions to make sure that the bottle shatters in exactly the way it does, at exactly the time it does. In fact, the Smart Rock doesn’t need to intervene, since Suzy’s throw is just right. But had it been any different – indeed, had her rock’s trajectory differed in the slightest, at any point – the Smart Rock would have swooped in to make sure the job was done properly.” (Hall 2004, 237)

Again, we have clear intuition that Suzy throw is a cause of bottle breaking, but due to the preempted backup – that is ready to intervene and ensure the effect is exactly the same, no matter which alteration of the cause had been actualized – there is no influence between Suzy’s throw and the bottle shattering. On the other hand, one could insist that throwing the Smart Rock does influence the effect since its different settings could make a difference to the breaking of the bottle.

The analogy with the FS and BvS is, I believe, easy to spot. We have Jones, Pam, and Suzy, all three bringing about some effect, but in the complex environment which overdetermines that effect with the backup alternatives. Those backup alternatives – namely: Black, Bob, and Billy – are all idle in the actuality, but nevertheless rob their respective preempting causes of any influence on the effect.

### Problems for the two CEs?

Does this structural similarity between BvS and SR (on the one side), and FCs (on the other), constitute a problem for these two CEs? The reason for this worry is simple: since we have seen that ITC has the means to deal with FCs, and we have recently maintained that there is a relevant similarity between FCs and two given CEs, it is natural to ask whether ITC can explain away, in the same manner, BvS and SR? Or, even more precisely, isn't it a case that we could have a stepwise influence in the alleged CEs offered by Schaffer and Hall?

We can start answering this question by analyzing BvS. A quick recap of this CE: we have Pam pressing the button (which is pressable only at the given moment and cannot be pressed before or after that), we have Bob, who is monitoring the signal leading from Pam's button to Vic's electrodes, with the intention and the means of ensuring that Vic is getting electrocuted in the same time and manner that he actually was (due to the Pam's pressing of the button).

This scenario is easily adjustable to the diagram I have offered earlier for FCs. It has all the characteristics of the early preemption cases. In arguing that, I am agreeing with Noordhof (Noordhof 2001) and further support his claims by offering the diagram parallel to the one constructed for FCs.

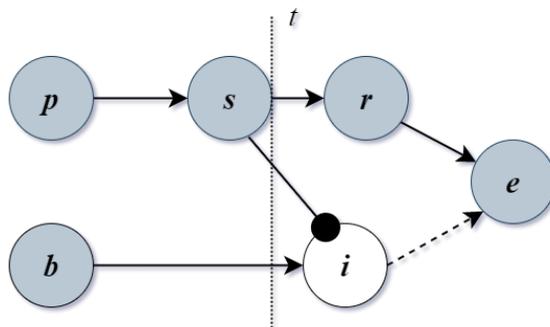


Diagram 3: Button vs. Switchboard CE

In this diagram (*diagram 3*), we have *b* standing for Bob (with his intentions) who starts monitoring what happens with the signal; then, we have *p* that marks the event of Pam's pressing the button, which causes the signal to go through the part of the wire that

Bob is monitoring (*s*). This *s* event has two further effects: on the one side, it causes the signal to continue its journey through the rest of the wire (*r*) which leads to Vic's electrocution (*e*); and on the other, it

inhibits Bob's intervention ( $i$ ), since Bob – after seeing the signal – decides not to intervene. I also added the moment  $t$ , at which Bob finishes his monitoring, and depending on it decides what to do.

If this diagram is correct and parallel to the one offered for the FCs, then we should have that  $p$  influences  $s$ . But that could seem controversial – and Schaffer would probably disagree with it (Schaffer 2001: 16). No matter how Pam presses the button (gently or hard, with her left or right hand, with the index finger or the thumb...) as long as it is at the given time, the signal is the same (and we have the same alteration of  $s$ , which is the actual alteration – call it  $s_a$ ). On the other hand, according to the scenario, since the button is only responsive in that given moment (in which Pam actually pressed it), all other potential pressings of the button, before or after that given time, simply map onto the one and the same unactualized alteration of the event  $s$  – namely, the alteration in which the signal doesn't go through the wire (let us call that alteration  $s_b$ ). Pam's not pressing the button at all – if that should count as a not-too-distant alteration of her pressing the button – also maps onto the alteration  $s_b$ .

So, bottom line, we have only two different alterations of the effect, onto which all the different alterations of the cause are mapped. Is this enough for influence? Do we have a substantial range of not-too-distant alterations of  $s$  that maps onto different alterations of the effect  $t$ ?

The answer has to be – yes. The definition of influence grants that. In it, it is explicitly stated that the alterations of the cause should map onto alterations of the effect such that *at least some of them differ*. It is the peculiarity of the case under consideration that makes the potential range of different alterations of the effect sparse. Hence, the *richness* and *variety* in the mapping that is required for influence also have to be proportionally moderate in this case. If we don't allow for this kind of reading of the vague phrases in the definition of influence, then it is easy to have even less complex CEs to ITC than the ones we have considered above.

Back to the diagram. Now, after this worrisome first step, other steps are pretty straight forward. Had the signal in the monitored part of the wire been different,<sup>16</sup> it would certainly be different later on trough

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<sup>16</sup> In analyzing the pattern of counterfactual dependence between these two events, we are free to entertain even those possibilities that are not consistent with what the

the given wire, as well. So, we have influence between  $s$  and  $t$ ; and, similarly, all the different alterations of the signal in the later part of the wire stand in the counterfactual relation with different alterations of Vic's electrocution.

To summarize: although we do not have direct influence between  $p$  and  $e$ , it could be shown – as I have tried to maintain in the passages above – that there is an indirect, stepwise influence between those two events.

Now, can we do the same for Hall's SR scenario? A quick recap: we have a regular rock thrown by Suzy, which breaks the bottle; in addition, we have Billy throwing a smart rock which monitors the trajectory of Suzy's rock, and which is able to step in at any moment and ensure that effect happens the same way it actually happened. Admittedly, this scenario looks structurally very similar to the one just analyzed. So, is SR another case of early preemption in which we do have a stepwise influence between the cause and the effect?

No, SR is not a case of early preemption at all. Although it does look similar to FCs and BvS, it still has one important feature that is different, and that feature deprives it of being an instance of early preemption.

In both cases, FCs and BvS, we hypostasized some moment  $t$  in which the monitoring happens (or rather ends), and after which – depending on the reading – backup cause can step in and ensure the effect. The events that happen on the main causal route after that moment are crucial for the early preemption since any of them can serve as an intermediate event – one which depends on the cause, and on which in turn depends effect, although there is no direct dependence between the cause and the effect.

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scenario tells us about Pam's pressing of the button. That is out of the picture now and – contrary to what Schaffer seems to think (Schaffer 2001, 16) – we don't need to ask: "well, how the signal could be there earlier if it is impossible for Pam to press the button earlier", or "how it could be stronger (the signal) when Pam only has a button without any controllers", and so on. What we would assume in doing that is the truth of some so-called backtracking conditionals (e.g. "If the signal were to be different, then it would have to be the case that Pam had a switchboard rather than simple button"). But these conditionals are peculiar and problematic – they seem to state how the things that are earlier depend on things that happened later on; Lewis denies that they can be true in the common contexts (Lewis 1979, 457-8; 1986c, 169).

But the reader should note that in the case of SR we don't have such a moment and, furthermore, we can't add it consistently to the story. Instead of an *early* moment in which the monitoring ends, and after which the potential intervention of the backup alternative happens, in this scenario, we have monitoring as an ongoing process that ultimately ends only with the occurrence of the effect.<sup>17</sup> And that is, as we have seen, a distinctive mark of late preemption.

Simply, in SR – and cases similar to it – it would be impossible to find an intermediate event, which is crucial for the implementation of stepwise influence maneuver. Hence, we cannot frame these cases into a diagram similar to those for FCs and BvS.

Consequently, with this stepwise strategy unavailable, and without direct influence between the cause and the effect in the SR scenario that Hall offers, ITC is surely in big trouble. As it turns out, SR can't be that easily disarmed (as BvS was).

### Concluding remarks

In this paper, I have analyzed two prominent counterexamples to the Influence theory of causation, with a special interest in their similarity to Frankfurt cases. As I tried to show, that similarity poses a threat to the plausibility of the given CEs.

And indeed, by insisting on the structural similarity of FCs and BvS, and hence accepting Noordhof's critique of that Schaffer's CE (together with offering a further elaboration of that critique) I aimed to show that Button vs. Switchboard CE is not a valid CE to the Lewis's latest theory of causation.

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<sup>17</sup> Somebody is maybe inclined to object that this shows that the scenario in question is not a viable one, that it simply cannot be real. The objection would proceed by stating that in order to intervene, even the Smart Rock would require some finite amount of time. So, the monitoring process should end, if not earlier, then right before the time of the effect occurrence (how much before? – the same amount of time needed for its intervention). This objection has some plausibility, but it crucially depends on the (*a posteriori*) physical restrictions and deals with the physical impossibility. That, however, is too restrictive when we seek a conceptual analysis of causation. If the analysis is successful in catching the notion of causation, it should be general enough to "work" even with different physical laws.

However, it would be wrong to conclude the same with regards to Hall's CE. Due to a slight but important structural difference between SR and BvS, the maneuver used to disarm BvS is not applicable to the CE Hall had proposed.

More precisely, Lewis's ancestralization maneuver, i.e. his introduction of a stepwise influence to the theory, was only meant for dealing with early preemption, and SR is not an instance of early preemption, as we have seen. On the other hand, although ITC was formulated with the hope of solving the late preemption problem, clearly there are still cases (different than typical late preemption cases) for which the theory doesn't give the right answers.

The analysis offered in this paper serves to show that SR is a plausible and successful CE, and, consequently, ITC is not a fully adequate theory of causation. Also, that analysis, I hope, provide us with good reason to favor Hall's CE (over the one Schaffer had offered) as the compelling argument against the theory.

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## PRACTICAL MODES OF PRESENTATION. SOME REMARKS ON THE INTELLECTUALISM VS. ANTI-INTELLECTUALISM DEBATE

MIHAI RUSU<sup>1</sup>

**Abstract:** This paper is a critical examination of the notion of *practical mode of presentation* and of related notions that have been proposed in the literature as a way to explain the propositional character of practical knowledge. If all knowledge is propositional, as intellectualists maintain, then we need an elucidation of the situations where the subject knows some true propositions about an activity without knowing how to perform that activity. Intellectualists have appealed to practical modes of presentation in order to reply to this objection and account for the apparent difference between ordinary (propositional) knowledge and the knowledge that is manifested in practical cases. While this difference is undeniable, it is not substantial according to the intellectualists. The paper proceeds as a discussion of the debate between intellectualists and their critics regarding practical modes of presentation. The controversial character of practical modes of presentation is a key issue for understanding the entire intellectualism vs. anti-intellectualism debate and, more generally, the competing accounts allow a deeper and more nuanced construal of the connection(s) between knowledge and action. The main aim of the critical discussion in this paper is rather modest, but still significant at this point of the debate: as I will show, the debate is far from over and – for all the ingenuity and complexity of extant intellectualist accounts – supplementary work needs to be done to clarify the distinctions and develop a convincing and comprehensive account of practical modes of presentation.

**Keywords:** intellectualism, anti-intellectualism, knowledge-that, knowledge-how, practical modes of presentation

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### Introduction. The sufficiency problem

Intellectualists hold that knowledge-how is propositional, i.e.  $x$  knows how to perform an activity  $F$ , according to Stanley and Williamson (2001), if  $x$  knows of a way  $w$  that  $w$  is a way for her to  $F$ . This tenet has been challenged from multiple perspectives, but one of the main lines of attack for anti-intellectualists has been the sufficiency objection, which states that, at least for some activities, knowledge of a certain proposition is not sufficient for one to possess the know-how needed to perform the activity in question (Glick 2015, 1). For instance, one may know many ways in which one could score a goal from a free kick at football (by kicking the ball in such-and-such way, with such-and-such force, etc.) and yet not know how to do it in practice.

In other words, the sufficiency problem is the claim that something else than propositional knowledge is needed for one to possess know-how, whether that something else is built on top of propositional knowledge (so we could say it is something more) or it is just different from knowledge-that, whatever we take that to be. Stanley and Williamson anticipate the sufficiency problem and claim that the knowledge present in know-how is propositional as well, only it is possessed under a practical mode of presentation (PMP). Later, Stanley (2011) replaced PMPs with *practical ways of thinking* which he analyses in a Fregean framework. The notion of a practical mode of presentation/practical way of thinking remains controversial, however. Various authors, such as Schiffer (2002), Noë (2005), and Glick (2015), claim that the introduction of PMPs is not sufficiently motivated or that PMPs are not sufficiently fleshed out from a theoretical standpoint. It does not help much that, both in Stanley and Williamson (2001) and Stanley (2011), the defense of PMPs/practical ways of thinking is made *via* a defense of the framework of modes of presentation (ways of thinking) in general. In both of these works, the authors' chief argument relies on other modes of presentation, namely indexical modes. Stanley (2011) devotes a large part of his theorizing to the elaboration of an Evansian analysis of *de se* knowledge by way of first-personal ways of thinking. About practical ways of thinking, Stanley claims that "[t]he existence of practical ways of thinking is a straightforward consequence

of the Fregean framework of individuating ways of thinking of things, one that has nothing directly to do with knowing how." (Stanley 2011, 125) While one option is to reject modes of presentation/ways of thinking framework altogether, a more charitable but also more effective strategy is to argue just against PMPs/practical ways of thinking within that framework. This is what Glick (2015) attempts to do quite convincingly.

What should be clear, nevertheless, is that notwithstanding the dialectical overshadowing of their importance in the work of Stanley (& Williamson), PMPs are a key notion in the debate between intellectualists and anti-intellectualists. In the following section, I will attempt a reconstruction of the related notions of PMPs/practical ways of thinking/practical senses as they have been construed and employed in the literature together with an assessment of the main arguments against PMPs and their implications in related controversies regarding the intellectualism – anti-intellectualism debate.

### **What are PMPs? Or what could they be?**

Imagine you are an amateur football player watching Lionel Messi play at the Camp Nou. At a certain point in the game, you witness Messi scoring a fabulous goal from a free kick. One of your friends, who is also there, leans towards you and whispers somewhat ironically: "You know, that is a way you could score a goal yourself when you take a free kick next time." Of course, having seen Messi score a tremendous goal and scoring a similar goal yourself are two different things, even though your experience at the Camp Nou has provided you with knowledge of a certain way you could score a goal from a free kick. The friend's teasing and Stanley & Williamson's analysis of know-how are not that dissimilar *prima facie*. According to Stanley and Williamson (2001, 429), if  $x$  knows of a contextually relevant way  $w$  that  $w$  is a way for  $x$  to  $F$ , then  $x$  knows how to  $F$ . This is what gives rise to the sufficiency problem. Knowing a way to score (e.g., that you should kick the ball in such-and-such way) is not always the same with knowing how to score. The solution proposed by Stanley & Williamson to this problem is the introduction of practical modes of presentation. Pavese (2016, 650) aptly

summarizes the intellectualists' view on PMPs: "For one to come to know how to  $\Phi$ , in the relevant sense, it is for one to know an answer to the question <How could one oneself  $\Phi$ ?> under a practical mode of presentation."

The notion of PMP has been challenged most notably by Schiffer (2002), Noë (2005), Stalnaker (2012) and Glick (2015). Before discussing what I regard as the most important criticisms of the idea, I will examine the first attempt of using this notion due to Stanley and Williamson. In (Stanley and Williamson 2001, 429), the authors admit that giving an account of PMPs is "quite a substantial philosophical task", in the same way that explaining first-personal modes of presentation is. To be fair, Stanley & Williamson's entire case for PMPs is piggyback riding on the case for indexical modes of presentation and the similarity between indexical modes and PMPs. While they refrain from tackling the substantial task of providing an elucidation of PMPs, Stanley & Williamson set out to give a proof of the existence of such modes of presentation. Their starting point is the general thesis that the same proposition may be entertained under distinct modes of presentation, as it appears to be clear from the demonstrative vs. first-personal distinction present in:

- (1) John believes that that man has burning pants.
- (2) John believes that he himself has burning pants.

In a situation where John sees himself in the mirror, but mistakenly believes that the mirror is actually a window, the complement clauses of (1) and (2) have the same propositional content, yet (1) and (2) have different truth values – (1) is true and (2) is false. This speaks in favour of the existence of modes of presentation, which Stanley & Williamson treat as forms of entertaining a Russellian proposition, i. e. ways under which one has an attitude regarding a proposition. Now, what Stanley & Williamson claim is that the possibility of a similar divergence in truth-value of

- (3) Hannah knows that that way is a way for her to ride a bicycle.  
and
- (4) Hannah knows how to ride a bicycle.

can be accounted for in similar terms as the difference between (1) and (2). That is, both (3) and (4) ascribe propositional knowledge to Hannah, but the former does it under a demonstrative mode of presentation, while the latter does it under a PMP. That is what explains why (3) may be true without (4) being also true. The rest of Stanley & Williamson's existence proof navigates this analogy:

Thinking of a person as oneself entails being disposed to behave in certain ways, or form certain beliefs, given relevant input from that person. Similarly, thinking of a place as *here* entails being disposed to behave in certain ways, or form certain beliefs, given relevant input from that place. Analogously, thinking of a way under a practical mode of presentation undoubtedly entails the possession of certain complex dispositions. It is for this reason that there are intricate connections between knowing-how and dispositional states. (Stanley and Williamson 2001, 429)

One of the first sustained critiques of Stanley & Williamson's intellectualism appears in Noë (2005). A concern raised by Noë is that Stanley & Williamson's existence proof begs the question against the anti-intellectualist. Why posit that the difference between (3) and (4) should be accounted for in terms of modes of presentation of the same proposition?<sup>2</sup> That implies that the knowledge present in both (3) and (4) is propositional, but an anti-intellectualist would want to deny (4)'s propositional nature. Noë argues that, in order to evade this circularity charge, Stanley & Williamson should provide us with independent reasons for the existence of PMPs, much as there are independent reasons for acknowledging the existence of first-personal modes of presentation. According to Noë, Stanley & Williamson fail to do that (see Noë 2005, 287-288).

Glick (2015, 541) has rejected Noë's criticism of Stanley & Williamson insisting that they provide independent reasons by way of

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<sup>2</sup> Glick (2015, 540) notices that the clauses in (3) and (4) express different contents. The former is a proposition, while the latter is the meaning of an embedded question. (4) is true if for some contextually relevant way *w* of riding a bicycle, Hannah knows that *w* is a way for her to ride a bicycle. Nevertheless, (3) provides us with just the kind of proposition that is needed for the quantified claim in (4) to be true. Consequently, the fact that (3) may be true, while (4) may be false still needs to be explained.

their linguistic argument for intellectualism, that is, by exploiting standard semantic theories according to which knowledge attributed by (4) has propositional content. Glick also uses the (subsequent to Noë's paper) elaboration of the intellectualist framework in Stanley (2011). In his book, Stanley replaces PMPs with practical ways of thinking which he analyses in Fregean terms as parts of propositions. I believe Noë's critique stands, even if it is not decisive against Stanley & Williamson. Regarding Glick's first point, what Noë objects to Stanley & Williamson's argument is not that they do not provide independent reasons for thinking that the knowledge attributed by (4) is propositional, but rather that Stanley & Williamson have not shown that the mode involved must be a practical one and also what it consists of. If Stanley & Williamson's other arguments are controversial (and they are), then they cannot just suppose that the knowledge present in (4) is propositional and, what is more, it is possessed under a practical mode of presentation. Now, about Glick's second point, it is worth pointing out that the weight of Stanley (2011)'s argument is again shifted to indexical, and mostly first-personal, modes of presentation. Stanley formulates a general defense of ways of thinking, rehearsing some key points about indexical modes, and – as I have mentioned before – practical ways of thinking are seen just as a straightforward consequence of adopting a Fregean ways-of-thinking analysis. While the framework is more complex (I will come back to it shortly), Noë's doubts are still noteworthy.

Glick puts forward his own critique of Stanley & Williamson's arguments. Even though he criticizes Noë, Glick's strategy is not that much different. The case is built around Stanley (2011)'s more elaborate attempt to defend practical ways of thinking. Glick focuses on Stanley's reference to a passage from Heidegger where, according to Stanley, the German philosopher "draws our attention" to practical ways of thinking by reflecting on the example of wielding a hammer. Stanley's interpretation of Heidegger's thoughts is problematic, so Glick sets upon trying to reconstruct a possible account of practical ways of thinking, in the sense of distinguishing between a hammer-wielder and an observer, *viz.*, between a supposedly practical and a non-practical way of thinking. Glick discards rapidly proprioception as a mark of the practical, because

not every know-how (e.g., solving puzzles, doing calculations) involves bodily movement and even in the case of the know-how that does so, experimental studies show there can be know-how without proprioception. Glick also rejects phenomenal concepts (concepts that one might come to have only in virtue of having a certain experience) as acceptable explanations, because the existence of such concepts is problematic, and because we should not rule out know-how for actions that do not have a phenomenology or the experience of performing them has been forgotten (e.g., one may not recollect how one rode a bicycle, yet be able to ride one when presented with the opportunity).

One of the most promising ideas explored by Glick concerns the *order* in which the various types of knowledge are formed. This argument can be framed at least initially in terms of learning-how vs. learning-that. Let us take a simple example. Some tennis coaches make their very young students play against a wall or a fence in order to shape and refine their shots and their reactions. Many actions, whether in sports or craftsmanship, are performed and repeated by the students initially and are “broken down” propositionally in explanations only later. It seems plausible that we need to learn how to perform a certain action sometimes before we can have access to any type of propositional knowledge, whether under a practical guise or in a classical conceptual countenance. In order to know that the tennis racquet is used “this way” we must first know how to use it. Therefore, even if PMPs of propositional content exist, nothing guarantees that they precede know-how in any way. Dickie (2012) proposed a similar argument to the effect that knowledge comes only after someone possesses the skill to perform a certain action. Dickie sums it up in the following way:

Consider the myriad routes to acquisition of skill. These routes include, but are not exhausted by, inborn talent; mindless repetition; unreflective imitation; hypnosis; induction from past attempts; reflection from first principles. The heterogeneity of this list generates an objection to intellectualism. For an account of propositional knowledge needs a justification component. And it is hard to see how the intellectualist can deliver the justification component of the skilled  $\Phi$ -er’s knowledge that  $w$  is a way to  $\Phi$  while respecting the variety in routes to acquisition of skill. (Dickie 2012, 741)

Now, this is a very interesting objection. What Dickie stresses is that the intellectualist needs justification for the acquisition of know-how, but this justification falls back on typical forms of justification for propositional knowledge (basically, evidential accounts) that fail to do justice to all the possible routes for acquiring know-how/skill. Yet, I think intellectualists may be able to work out at least a general version of an argument that the order of acquisition goes the desired way for them. Stanley (2011) holds that (practical) modes of presentation are connected to very complex dispositional states. Building on this, intellectualists might reply that, although we should agree that one needs to learn to shoot a bow reliably (i.e., succeed counterfactually) in order to acquire skill/know-how in shooting a bow, an agent already possesses some propositional knowledge under a PMP that allows her to learn how to shoot a bow. The archery student already knows (in a practical way) some true propositions about moving her hands, contracting her muscles, holding objects, coordinating her movements, etc., the more basic actions that compose the complex action of shooting a bow. The student does not operate in a vacuum of knowledge, but rather she uses her previous knowledge to acquire new knowledge and build up complex dispositional states on top of the previous ones so that she learns how to shoot an arrow more reliably. This rejoinder seems hard to tackle, perhaps because of its somewhat indefinite character, but the argument needs to be explored further. Anti-intellectualists might reject compositionality for complex actions and skill and maintain that there is something irreducible, Gestalt-like in at least some complex types of skill. That would ensure that some relevant (practical) propositional knowledge only comes after (even if immediately after) the development of a certain skill.

The controversy regarding the order of acquisition intersects another point of contention that is crucial for the intellectualist, that is, the question whether know-how presupposes ability or not. Although there is some ambivalence about this issue and accepting that ability is presupposed by know-how is not incompatible with their position, intellectualists typically reject the entailment. But what might be the intellectualists' reasons for rejecting this connection between know-how and ability? The critical role of this denial stems from an important

dialectical aim, that of rejecting Ryle's view. For Ryle (1949), know-how and skill appear to be one and the same – knowing how to ski means that one is skilled at skiing, which should entail that one is also able to ski. In contrast, the intellectualists should be at odds with this idea, first and foremost because it carves up the space of cognition in a very problematic way for them. The intellectualist needs to show that there is something peculiar to know-how on pain of succumbing to a Rylean collapse. Why should we suppose that there is something distinct called *know-how* when it suffices to distinguish between propositional knowledge and ability, and then posit that ability presupposes knowledge or manifests it or just is in itself knowledge? It should be clear that the concern about the order of development is related to this issue. If know-how does not presuppose ability, then some propositional knowledge under a PMP about a certain action must be present without performing that action, and therefore we have a strong reason to believe that PMPs are prior and more basic than the possession of skill.

Glick is also fully aware of the challenge posed to intellectualism by the relation between know-how and ability. If know-how is different from ability and can exist without it, the question is what know-how consists of. It is here that the need for an adequate account of PMPs presents itself, because PMPs supposedly contribute the extra element that differentiates know-how both from non-practical propositional knowledge and from ability *per se*. Glick attempts to shape up an account of PMPs based on Stanley and Williamson's analogy between indexical modes of presentation and PMPs. He examines the prospects of attributing three main features of indexical modes of presentation to PMPs as follows:

a. PMPs are associated with *conventional locutions*. While this might be true and useful in acknowledging the existence of PMPs, it does not tell us much about what PMPs are, *viz.*, about their role and characteristics (Glick 2015, 549).

b. PMPs involve distinctive ways of thinking of objects. Demonstrative modes of presentation are distinct from first-personal modes – John thinks of himself differently in the two cases presented by (1) and (2): in the former he thinks of himself “as that man in the mirror”, whereas in the latter he thinks of himself “as himself.” But this

does not translate well to the case of PMPs. Glick considers the case of Alice and Hannah - Alice knows how to ride a bicycle, while Hannah does not but each of them knows of a way  $w$  that that is a way for her to ride a bicycle. Both of them are physically fit and have seen someone riding a bicycle. Then, according to Glick, the difference between the two in an intellectualist framework should be that Hannah is not thinking in the right way about  $w$ , analogously to John from (1) who fails to identify himself as the man with burning pants (Glick 2015, 549-550). Glick deems this implausible, but I think that his criticism is incomplete and too one-sided. The trouble is not that it is implausible that Hannah thinks differently of riding a bicycle – it is quite intuitive that her thoughts are different from Alice’s – but rather that this does not illuminate us as to what PMPs are. So, we may accept that Hannah and Alice think differently about riding a bicycle, and we should notice here that putting the matter in terms of thinking in the right or in the wrong way seems to be partial: both of them may have correct (and also incorrect) beliefs about riding a bicycle. Moreover, success or lack of success in performing an action is not necessarily explained exclusively by adopting a certain way of thinking about that action, as Glick seems to maintain in this argument. Other factors, such as physical strength, muscle coordination, good sight, nervousness or just adopting some kind of behaviour may play an important role. However, the mysteriousness objection regarding PMPs persists: what do these differences consist in? What sets them apart? Why does one lead to practical success and the other does not? Isn’t success actually a prerequisite of having practical ways of thinking about a certain action?

c. PMPs involve distinctive dispositions. There is no account of the dispositional states that are connected to PMPs in Stanley (& Williamson)’s work. The problem with this feature is that without connecting the dispositions to  $F$  with the ability to  $F$ , it is hard to see what these dispositions might consist of. For instance, what is the difference in dispositions between someone who knows how to score a slam dunk and someone who does not know? Naturally, one would explain the difference by submitting that one has the disposition to score a slam dunk when one wants to and there are no obstacles to performing this action, while the other does not have the same disposition. But if

know-how is divorced from ability, then the disposition to  $F$  must be possessed, at least in some cases, by someone who does not have the ability to  $F$ . The question is: what is the difference in dispositional states between someone who does not know how to  $F$  and someone who knows how to  $F$  but is not able to  $F$ ? No clear answer seems to be available (Glick 2015, 552-553).

### Practical senses

Pavese (2015) has recently attempted to provide a more detailed Fregean account of practical ways of thinking as practical senses modelled on operational semantic values for programming languages. For Pavese, know-how is knowledge of a practical proposition with a practical sense. I will not delve into the intricacies of Pavese's sophisticated theory here but let me notice that for Pavese practical senses are distinguished by two features: they require *rule-following capacities* and endow their graspers with *rule-following abilities* (Pavese 2015, 10). We can construe practical senses as inferential rules whose "inputs and outputs (...) are *ways of representing the commands to be executed and the result of the execution up to a certain point*" (Pavese 2015, 13). These representations need not be linguistic, they can also be map-like or picture-like, but even basic abilities (such as ear wiggling) have a cognitive aspect, according to Pavese (2015, 14). To argue for this, Pavese uses recent research in neurosciences which has shown that cognition is important in the acquisition of very basic motor skills, such as raising one's hand or holding a tool.

According to Pavese, this theory allows the intellectualist to respond to Glick's criticism that PMPs do not involve distinctive ways of thinking by maintaining that practical senses are operational semantic values (or akin to operational semantic values) That is, practical senses are inferential rules, i. e. "inferential ways of thinking of how to perform a task" (Pavese 2015, 19) that have cognitive significance. Now, this is problematic. While Pavese's proposal is indeed substantial, in comparison to Stanley's which is more programmatic and centered on an analogy, one can argue that Pavese's Fregean theory stretches the limits of a certain

local concept (or conception, at best) and is based as well on an analogy that is limited in scope. First, we should note that while it is plausible, albeit controversial, to equate know-how for complex tasks (such as playing tennis or painting) with knowing (and in many cases applying) some rules for that activity, this is problematic in cases of knowing how to perform simple actions, such as raising your hand or chewing. Moreover, it is not sufficient to mention studies that show that the acquisition of basic motor skills has a cognitive component; the intellectualist needs to show that the exertion of these skills in every context, e.g., even long after one has learned how to raise their hand and has performed this action numerous times, retains a cognitive dimension.<sup>3</sup> Accepting Pavese's theory of practical senses means we should assent to a very broad understanding of what a rule means and what it means to follow some rule. Pavese has developed her view, in works such as (Pavese 2017) and (Pavese 2019), where she attempts to construct a theory of practical meanings as contents of motor commands, according to computational models of motor behaviour. However, it remains quite controversial that motor commands are rules (or rule-like), at least if we think of rules as always having propositional content. On the anti-intellectualist side, Fridland (2013, 2014, 2017a, 2017b) uses various empirical results to show that motor processes are intelligent in their entirety (without being necessarily propositional). Another contribution that uses this more flexible conception of embodied intelligence and knowledge is Levy (2017)'s version of the sufficiency objection, which is based on treating motor representations as intelligent, but non-propositional.

Another important aspect of Glick's criticism that Pavese answers only partially is that regarding the difference in ways of thinking between someone who has only non-practical propositional knowledge of  $F$ , someone who has know-how of  $F$  but does not have the ability to  $F$ , and someone who has the ability to  $F$  and, of course, also knows how to

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<sup>3</sup> The work of Hubert L. Dreyfus, in publications such as (Dreyfus and Dreyfus 1980) and (Dreyfus and Dreyfus 1986), contains a well-known view of expert performance according to which expertise is achieved by moving away from knowledge-guided decisions to some sort of perceptual acuity whereby the agent simply *sees* what needs to be done in each specific situation. Of course, such a perspective is incompatible with intellectualist theories.

*F*. Pavese capitalizes ingeniously on the idea of rule following, by distinguishing between knowing how to *F*, being able to intentionally *F* and being able to follow a rule to *F*. But these distinctions still cannot explain the difference in ways of thinking between someone who only knows how to *F* and someone who has the ability to *F* intentionally. In fact, Pavese holds that “knowledge how to  $\Phi$  is sufficient for the ability to intentionally  $\Phi$ ” (Pavese 2015, 17). As we have seen, this is not congenial for the intellectualist and may lead to all sorts of theoretical problems when attempting to defend an intellectualist position.

### **Concluding remarks**

I will return briefly in the end to the problem of the propositional content of rules or know-how in general. Stalnaker (2012) formulates a very sympathetic critique of Stanley (2011)’s approach, underlining the fact that Stanley’s view may be seen as a reconsideration of an entire perspective on knowledge. If “propositional knowledge is the possession of information and the capacity to use that information to guide one’s actions” (Stalnaker 2012, 755), then paradigm cases of know-how are also cases of propositional knowledge. According to Stalnaker, Stanley’s view is much closer to Ryle’s than one would think, as Ryle had a similar interest in showing that knowledge is not just inert and theoretical, but he made the mistake of identifying intelligence with intellect and thus with theoretical operations, restricting his perspective too narrowly. Further on, Stalnaker launches a criticism of Stanley’s appeal to an Evansian analysis whereby practical ways of thinking are relations between ways to *F*, subjects and times. But if modes of presentation are constitutive of propositions, the adoption of Evans’ perspective leads to the consequence that propositions are thinker and time-dependent, which is contrary to Frege’s view, obscuring the distinction between the actual content of our beliefs and the way we deploy those beliefs in action (Stalnaker 2012, 760-761). Pavese (2015, 19) claims that her theory evades Stalnaker’s concerns by analyzing practical senses as operational semantic values, i.e. independent abstract objects that determine their referents. Now, the success of Pavese’s reply

is dependent on our willingness to adopt and extend this particular programming view of semantics, but what concerns us here is that although interesting and complex, the theoretical proposals of Stanley (&Williamson) and Pavese might lead to a rather problematic extension of our view of propositions. The basic question needs to be asked and repeated: when can something (namely knowledge) be qualified as propositional? Fridland (2015) argues against treating know-how as reasoning – because behaviours are always performed in specific circumstances where one cannot use only general rules – or as conceptual, because concepts are context-independent and conceptual thought is built on a type-token distinction, whereas context-independent elements cannot get tokened in skills. Consequently, if know-how is not conceptual, it cannot be propositional either, “because propositions are necessarily constituted by concepts.” (Fridland 2015, 720-721). If we accept Fridland’s view of propositions, then it is hard to see how one can defend an intellectualist perspective such as the ones that were examined in this paper. But any view of know-how relies on the definitions and analyses that we adopt for concepts such as *knowledge*, *proposition* and *intelligence*. Hopefully, as the intellectualism – anti-intellectualism debate is still lively and innovative, many such clarifications are forthcoming.

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## ON THE ORIGINS OF THE IDEA OF A CONCEPTUAL SCHEME

BOGDAN OPREA<sup>1</sup>

**Abstract:** In 1974 Donald Davidson published *On the Very Idea of a Conceptual Scheme*, an article in which he tried to draw attention to the unintelligibility of such an idea and to the dangerous consequences of its acceptance. However, despite the influence of his criticism at the time, Davidson was never clear enough about his target. The purpose of this article is to outline a possible response to what the American philosopher has in mind when criticising the idea of a conceptual scheme.

**Key-words:** conceptual scheme, conceptual relativism, linguistic turn, historical turn, Kant, Davidson.

### The context of the discussion

In a well-known article published in 1974, entitled *On the Very Idea of a Conceptual Scheme*, Donald Davidson articulates perhaps the fiercest criticism of the idea of a conceptual scheme. Briefly, through this critique he addresses two issues: a) on the one hand he discusses the conceptual relativism, the most pernicious implication of the idea of conceptual scheme; b) on the other hand he discusses the degree of intelligibility of the idea itself (Davidson, 1991, 183-198). According to the American philosopher, the conceptual relativism – the philosophical doctrine which states that the members of two human communities can describe the world differently, or that the members of two scientific research traditions can explain natural phenomena in ways that are incommensurable – is unsustainable. This idea does not make sense whatever the context. It is not possible to talk about conceptual

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relativism in the case of a total untranslatability, because translatability is the defining feature of a language. It is not possible to talk about conceptual relativism in the case of a partial untranslatability, because there is the method of radical interpretation<sup>2</sup>. Also, in Davidson's view, the idea of a conceptual scheme – the idea that there is a conceptual component and an empirical one between which there is either an organizing relation or a fitting one – is unintelligible. In the case of the "organizing" metaphor it is not clear how a conceptual scheme can organize the world and everything it contains. In the case of the "fitting" metaphor it is not obvious what is fitted to the conceptual scheme – the experience, the data of the senses, the world itself? His main conclusion is that the idea of conceptual scheme is, in fact, a dogma – the third dogma of empiricism and we should reject it in favour of an unmediated relation between the mind and the world.

Despite the fact that throughout his career Davidson insists on rejecting this idea, discussing it in other articles such as *The Myth of the Subjective* and *A Coherence Theory of Truth and Knowledge*, he is never explicit about what he is criticising. On the one hand, he talks about a third dogma of empiricism. On the other hand, he associates this idea mainly with authors such as Feyerabend and Kuhn, two well-known opponents of empiricism. Starting from this ambiguity, in the following sections I propose to make a brief history of the idea of a conceptual scheme, in order to show that through his critique Davidson is actually considering a Kantian idea that influenced philosophers of various orientations.

### **The idea of a conceptual scheme from Kant to Kuhn**

From a historical point of view, I think that the idea of a conceptual scheme has its origins in the distinction that Immanuel Kant makes between the form of knowledge and the content of knowledge for the purpose of investigating how synthetic *a priori* judgments are possible

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<sup>2</sup> For further information see *Radical Interpretation* by Donald Davidson (Davidson, 1991, 125-139).

(Kant, 1998, 146-149). From this transcendental dichotomy two constitutive levels of knowledge result: the level of the intuitions of the senses – the one that gives the content of knowledge – and the level of the concepts of the intellect – the one that gives shape to knowledge. So any knowledge starts with the empirical data and continues with the intellect processing them through concepts. It is possible to talk about knowledge only in the case of cooperation between receptivity and spontaneity or in the case of a correlation between intuitions and concepts. They are inextricably intertwined and they cannot be dissociated. The path of knowledge starts from the senses and ends with the intellect. In this sense, the following passages are suggestive:

“There is no doubt whatever that all our cognition begins with experience; for how else should the cognitive faculty be awakened into exercise if not through objects that stimulate our senses and in part themselves produce representations, in part bring the activity of our understanding into motion to compare these, to connect or separate them, and thus to work up the raw material of sensible impressions into a cognition of objects that is called experience?... But although all our cognition commences with experience, yet it does not on that account all arise from experience. For it could well be that even our experiential cognition is a composite of that which we receive through impressions and that which our own cognitive faculty (merely prompted by sensible impressions) provides out of itself, which addition we cannot distinguish from that fundamental material until long practice has made us attentive to it and skilled in separating it out.” (Kant, 1998, 136)

The concepts of the intellect are the conditions of possibility of any knowledge by experience. They are *a priori*, they can never be derived from the intuitions of the senses and they have objective validity given their capacity to structure the intuitions. The central idea is that any knowledge about reality is limited to how the experience is shaped by the formal structures of the subject. In this regard, I think the next fragment is highly relevant:

"The concept of a dog signifies a rule in accordance with which my imagination can specify the shape of a four-footed animal in general, without being restricted to any single particular shape that experience offers me or any possible image that I can exhibit *in concreto*." (Kant, 1998, 273)

According to Kant, the form of knowledge – the conceptual scheme – is immutable. The conditions of possibility of knowledge are unchanged, applicable for any field and time.

However, in the light of the evolution of the scientific knowledge, many authors considered that a reappraisal is needed in this regard. Ludwig Wittgenstein, Rudolf Carnap, Stephen Toulmin, Paul Feyerabend and Thomas Kuhn were among those who pointed out that it was more likely to talk about different and changing forms of knowledge – conceptual schemes. The main common point of these thinkers was that these forms of knowledge were invented and selected based on pragmatic criteria. For them, knowledge meant nothing but the processing of some particular experiences through conceptual schemes created by the human mind.

Ludwig Wittgenstein was one of the first philosophers of the early XXth century who incorporated in his work the Kantian idea that every scientific research takes place in a form of knowledge which provides us the conditions of possibility of experience. In the early period of its work, he distinguishes between meaningful and meaningless sentences. In the late period of its work he distinguishes between empirical propositions and grammatical propositions. The idea that a logical framework is what draws the boundaries of meaningful discourse and shows us what can be said and what cannot be said occupies a central place in both stages of his philosophy.

In *Tractatus Logico-Philosophicus* the Austrian philosopher tries to draw the boundaries of language and implicitly the boundaries of thought. Also he wants to show that all the problems of philosophy are in fact pseudo-problems that arise as a result of the violation of the logical form of language (Wittgenstein, 23-24). In order to reach this aim, Wittgenstein appeals to Frege's conceptual writing and to Russell's theory of definite descriptions and draws the distinction between

meaningful and meaningless sentences<sup>3</sup>. According to him, meaningful sentences are sentences of natural sciences, the only ones that tell us something about the world and can provide us knowledge. They are all that can be said about the world. The purpose of the language is to describe facts and to represent portions of reality. Meaningless sentences belong *par excellence* to logic and mathematics. They lack empirical content, thus they tell us nothing about reality. However, taking into consideration that their role is to describe the structures of the world and language, they have the task of drawing the boundaries of meaningful discourse. Their truth values are independent of how things are in the world. They are either necessarily true and admit any state of affairs – tautologies – or necessarily false and reject any state of affairs – contradictions. Meaningless sentences are the conditions of possibility of meaningful sentences. Early Wittgenstein thought in a very Kantian manner. He considered that any empirical research was shaped by a form of knowledge – by a conceptual scheme – that was independent of any experience.

Starting with *The Blue and Brown Books* (Wittgenstein, 1958a, 17-20, 77-81) and continuing with *Philosophical Investigations* (Wittgenstein, 1958b, 2-13), Wittgenstein reorients himself on the problems of common language, on the societal practices that accompany the speech and on the contexts in which the words are used. He detaches from the logical analysis of language and focuses on the description of the forms of life and language games. The spotlight is transferred towards the behavioural practices of the use of language expressions that are tacitly adopted by the members of a community. In this context, a special significance is acquired by the distinction between empirical propositions and grammatical propositions (Ambrose, 2001, 43-73). The acquaintances, the hypotheses about correlations between facts belong to the first ones. The conventions, norms and rules of language use are related with the second ones. The empirical propositions are subject to the control of experience. In this sense they can be qualified as true or false. The grammatical ones evade empirical testing. They are chosen,

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<sup>3</sup> For further information see Bertrand Russell's *Introduction to Tractatus Logico-Philosophicus* (Wittgenstein, 1922, pp. 7-19).

maintained and revised according to how well they guide human activities, including scientific research. What belongs to the grammar bears the attribute of necessity and what belongs to the facts bears the one of contingency. The existence of a framework of rules of language use that circumvents the direct control of experience supports once again the Kantian idea that any empirical research is shaped by a form of knowledge – by a conceptual scheme that has a conventional character and not an empirical one. However, contrary to Kant's claims that the form of knowledge is invariable, the grammatical rules are in some respects mutable, they may differ from one human community to another or from one scientific research tradition to another. The language games can change over time and with them the concepts can change as well. "The riverbed" of thoughts can move entailing a change of the conditions of possibility of knowledge and implicitly ruptures of communication between communities or between scientific research traditions (Wittgenstein, 1969, 15).

One of the well-known reformulations of the Kantian distinction between the form of knowledge and the content of knowledge – a reformulation that influenced many authors along the XXth century – is the one made by Rudolf Carnap in its approach to the status of abstract entities. At the beginning of the twentieth century ontology was in the midst of a bitter dispute regarding the existence and nature of such entities, between some empiricist authors. In the spotlight were questions such as: Are there numbers, properties or propositions? Are they real or abstract entities? Is it legitimate to ask what status they have? To answer such questions the German philosopher develops an original approach. He claims that in order to be able to talk about the system of numbers, the system of properties or about the system of propositions, it is necessary to construct a linguistic framework. The construction of a linguistic framework for the mathematical entities is done as follows:

"First, the introduction of a general term, a predicate of higher level, for the new kind of entities, permitting us to say for any particular entity that it belongs to this kind (e.g., "Red is a *property*," "Five is a *number*"). Second the introduction of variables of the new type." (Carnap, 1999, 90-91)

However, before constructing such a framework, it is highly important to distinguish between internal questions – those that arise inside a linguistic framework – and external questions – those concerning the linguistic framework itself (Carnap, 1999, 86). The internal questions are either logical or empirical, while the external questions are either pseudo-questions or pragmatic questions. In this context the importance of the pragmatic ones must be emphasized because they are about the choice of a linguistic framework depending on its practical consequences.

Regarding the selection of the linguistic frameworks, Carnap adopts a conventionalist position. The acceptance or the rejection of such a framework is a convention that can be assessed in terms of its effectiveness as a working tool, taking into account its success or its failure in practical use. According to him, there is nothing dogmatic or pernicious in maintaining a linguistic framework by virtue of its fruitfulness, without considering it true or false. On the contrary, the tolerance towards the free construction of linguistic frameworks and the critical examination of their practical consequences could free scientific research from prejudices that have hindered its progress throughout history.

By stipulating two distinct elements in approaching the problem of the status of abstract entities – a linguistic one and an ontological one – Carnap was obviously influenced by the Kantian distinction between the form of knowledge and the content of knowledge. By taking into consideration the possibility of inventing a linguistic framework – here with the sense of a conceptual scheme – depending on the aim of the research, he took a step forward, distancing from the Kantian perspective.

The idea that any empirical research is shaped by a form of knowledge – by a conceptual scheme that provides us the conditions of possibility of knowledge – that is constructed to achieve certain goals turned out to be of interest not only for analytic philosophers. Philosophers interested in the history of science, as well as historians of science with philosophical interests such as Stephen Toulmin, Paul Feyerabend or Thomas Kuhn pointed out that throughout history science has progressed through changes in the form of knowledge that have led to new scientific discoveries. Some of these authors argued that these changes were so profound that it is possible even to talk about

incommensurability between the conceptual schemes characteristic to the scientific research traditions.

A remarkable illustration of how the content of knowledge is shaped by a form of knowledge that is specific to a certain research tradition can be found in Stephen Toulmin's book *Foresight and Understanding*, where he deals with the evolution of the way the laws of motion were understood from Aristotle to Galileo Galilei (Toulmin, 1961, 44-83). The author argues that at the foundation of Aristotelian natural philosophy lies the unshakable belief that the rest is the natural state of bodies while the motion is the result of the action of a force on a body. To understand the Aristotelian conception of motion – and implicitly his ideal of natural order – consider the following situation. A carriage is at rest as long as the horses do not pull it, that is, as long as no force acts on it. It moves only when the horses apply a traction force, its displacement tending to be slow down by the resistance forces it encounters along the way, such as the force of friction between the carriage and the road. From this perspective, only the motion needs an explanation, the rest doesn't need it. Although later it turned out to be wrong – in the sense that the motion of a body doesn't require a force – thanks to the fact that it managed to explain and predict a diversity of aspects related to motion, the Aristotelian conception inaugurated a real research tradition. Within certain limits, Aristotle's conception managed to describe and to successfully explain many phenomena that occur in everyday life. However its decline began with John Philoponus' attempts to explain the motion of projectiles. As long as the continuous motion of bodies proved to be too much of a challenge, the Aristotelian tradition went into decline and began to be questioned. It all culminated in the XVII<sup>th</sup> century with the outline of a new conception of motion by Galileo Galilei. Starting from a single mental experiment he shaped mathematically – a ship at sea which encounters on its path only a negligible force of resistance, tends to maintain its rectilinear and uniform motion until it encounters an obstacle –, the Italian scientist foreshadowed the law of inertia and took an important step towards the mathematical science of nature. Nevertheless the completion of the new research tradition was made by Newton's statement of the first principle of motion – the law of inertia – that describes the ideal case in which a

body maintain the state of rectilinear and uniform motion as long as no type of forces is acting on it or the sum of these forces is zero.

In the completion of the mathematical science of nature, Toulmin sees the establishment of a new ideal of a natural order, of a new standard of rationality and intelligibility. What couldn't be explained by the grid of the Aristotelian natural philosophy and was considered to be an anomaly it became central element of the mathematical science of nature and was successfully explained by its grid. Changing both the goals of knowledge and the concepts needed to explain new natural phenomena represents a deep transformation in the structure of thought but a necessary one for the scientific progress. Through this example it can be seen that in Toulmin's view every scientific research is determined by the conceptual scheme in which it occurs. Accordingly, his perspective is deeply influenced by the Kantian distinction between the form of knowledge and the content of knowledge. However, regarding the fact that for British philosopher when anomalies are encountered it is necessary to make some conceptual changes in order to overcome them, his perspective moves away from Kant towards Wittgenstein and Carnap.

The year 1962 looked like *annus mirabilis* for the history and philosophy of science. The publication of Paul Feyerabend's article "Explanation, Reduction and Empiricism" and of Thomas Kuhn's book *The Structure of Scientific Revolutions* put in a whole new light the way in which the scientific knowledge has developed throughout history. Both works remain a landmark due to the idea that the conceptual schemes shared by different scientific research traditions are incommensurable.

As for Feyerabend, his main purpose is to show that between the scientific research traditions outlined throughout history there are conceptual differences so profound that they are insurmountable. As in the case of Toulmin, in one of his examples, he focuses on how the transition from the Aristotelian conception of motion to the Newtonian one was done (Feyerabend, 52-62). In his view, for Aristotle the natural state of things was the rest while the motion was the continuous action of a "motor" on what is moving. The fact that a block of stone could be moved from one place to another by pushing or by pulling suggested to the Greek philosopher that the force is the cause of the motion. For

philosophers of nature from the beginning of the second millennium, the motion was caused by an impulse given to a body that was supposed to preserve itself until it encounters a resistance force. Finally for physicists of the XVIth and XVIIth centuries the force ceased to be seen as the cause of motion. The continuing motion of an arrow propelled by a bow or a stone thrown by a catapult, even after the force ceased to be exerted on them, could only be offered in a conceptual scheme that presupposed the inertial motion as a state and not as an effect of the action of a force.

According to Feyerabend, in order to provide good answers to the challenges appeared on the scientific research horizon, it is necessary to make conceptual adjustments whenever needed: "All these examples show that the postulate of meaning invariance is incompatible with actual scientific practice." (Feyerabend, 1962, 81) In the case discussed before, the conceptual changes were so profound that although the term "force" appears in both theories, it receives fundamentally different meanings. In Aristotle's works the force is the cause of motion. In Newton's works the force is the cause of acceleration. The development of scientific knowledge occurred through radical changes made in the conceptual schemes that shaped our research and led us to new discoveries. Through the claim that scientific research is guided by conceptual schemes, the Kantian distinction between the form of knowledge and the content of knowledge is once again in the centre of the discussion. And again, through the claim that these conceptual schemes may differ so radically, depending on the goals to be achieved, there is a distancing from Kant's perspective, towards one in the manner of Wittgenstein and Carnap.

Perhaps the well-known perspective of how the form of knowledge – the conceptual scheme – can shape the course of an empirical research is that offered by Thomas Kuhn when he talks about scientific revolutions as changes of the worldview. In *The Structure of Scientific Revolutions* the American historian of science starts from the following findings: a) after periods of normal science – science seen as puzzle solving – the emergence of some anomalies may generate periods of crisis within a certain research tradition; b) the overcome of such crisis may occur by establishing a new paradigm (Kuhn, 1970, 35-43, 52-77). The transition from the Ptolemaic system to the Copernican system, the

one from the Aristotelian physics to the Newtonian physics, the one from the phlogiston chemistry to the oxygen chemistry, and the one from classical mechanics to the relativistic mechanics are only a few cases of paradigm shifts that Kuhn approached in a very special way. However, the most striking consequence of these transitions is that the old paradigm and the new one prove to be incommensurable – partially untranslatable – in some respects (Kuhn, 1970, 144-160). The emergence of a new paradigm can produce a rupture of communication between researchers who succeed to master it and those who still follow the old paradigm. What for some is an anomaly, for others may pass as a normal thing, explicable in terms of the tradition to which they belong.

An edifying example in this respect is that provided by Kuhn in the *Preface* of *The essential tension* regarding the experience that determined the course of his research (Kuhn, 1977, xi-xii). In his attempts to understand the origins of mechanics, the Aristotelian conception of motion turned out to be the biggest challenge because of the absurdities it entailed. The fact that for Aristotle the state of a body was a quality and the motion was a change of that state seemed to him to be a blatant error. How was it possible for such a fine and penetrating researcher in the fields such as biology and politics, to make one of the most basic mistakes in describing and explaining a phenomenon such as motion? For Kuhn, the answer to this question became obvious as soon as he realized that there are several keys for reading scientific texts and that only one is the right one. The *sine qua non* condition to understand the conception of a researcher of nature from another age consists in adopting as much as possible the system of concepts that led him in describing and explaining what he observed. In the case of the Greek philosopher it becomes obvious why the motion was considered a change of the state of a body, only when his works are read starting from the assumption that for him the primary components of the universe were qualities and not material bodies. Therefore, the level at which the rupture between scientific research traditions occur is that of the language in which the questions and answers about nature are formulated. To a modern scientist, the problems formulated within the Aristotelian research tradition might seem difficult to understand because they do not use mathematical models. Similarly, to an

Aristotelian, the problems of mathematical science of nature might seem to be impenetrable because they are formulated in mathematical language.

This example highlights that Kuhn's idea that every scientific research is shaped by a conceptual scheme specific to a paradigm is obviously influenced by Kantian distinction between the form of knowledge and the content of knowledge. At the same time, it emphasizes that by the claim that when a scientific crisis is faced it is necessary to make new assumptions and change our concepts, Kuhn distances from Kant's perspective towards one inspired by Wittgenstein and Carnap.

### **Concluding remarks**

Throughout this article I tried to show that despite Davidson's labelling of the idea of conceptual scheme as the third dogma of empiricism, what he actually targets by his critique is an idea of Kantian origin. Of course, the idea of a conceptual scheme is one of the defining features of the modern empiricism, but if its history is examined, it can be seen that it also influenced historical turn – that philosophical orientation to which belong authors such as Feyerabend and Kuhn, directly mentioned by the American philosopher in his papers – and that its origins are found in Kant's philosophy. The leitmotif of the authors whose works I have briefly analysed is the distinction between the form of knowledge and the content of knowledge. All of them, from Kant to Kuhn argue that any knowledge is shaped by the knowing subject. Also, for all of them the form of knowledge bears the mark of necessity and the content of knowledge bears the one of contingency. The former always gives direction to the latter. The only difference is that while for Kant the form of knowledge is immutable, unchanging, given once and for all, starting with Wittgenstein's late philosophy and with Carnap's approach to abstract entities it can be seen that it is mutable, it can vary depending on the ideals of a human community or on a particular purpose of the research. According to them, as well as to Toulmin, Feyerabend and Kuhn, the form of knowledge – no matter what name it takes or how it is

labelled: linguistic framework, conceptual scheme etc – is a human invention. Every scientific discovery occurs in a conceptual framework that is invented in order to overcome the challenges that are sometimes encountered in research. As Feyerabend says:

“We may even say that what is regarded as ‘nature’ at a particular time is our own product in the sense that all the features ascribed to it have first been invented by us and then used for bringing order into our surroundings.” (Feyerabend, 1962, 29)

The flexibility of this framework is the very condition for the possibility of scientific progress. Without it, when a recalcitrant experience that eludes existing conceptual schemes would be encountered, the scientific research could be stuck forever.

In conclusion, when Davidson criticises the idea of a conceptual scheme, he considers the claim that it is possible to have alternative conceptual schemes through which the world can be described and explained in fundamentally different ways. However, the fact that we could live in different worlds implies a clear distinction between the form of knowledge and the content of knowledge. As this dichotomy is a landmark of Kant’s philosophy, when the ambiguities of Davidson’s critique are removed, it can be seen that he is targeting an idea of Kantian origin.

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# PHENOMENOLOGY AND ARGUMENTATION

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**Abstract:** Phenomenology and argumentation theory do not seem to be the closest of disciplines. However, there seem to be at least one exception among argumentation scholars: Charles Arthur Willard. The main focus of the second of Willard's books on argumentation, *A Theory of Argumentation* (1989) is the agent, argumentation being considered in the context of social interaction and communication, with an important emphasis given to the mundane and everyday life argumentative behavior – Willard taking a somewhat non-orthodox stance in contrast with the majority of argumentation theorists in terms of, for instance, relevance given to models such as Toulmin's. One of the influences on Willard was Alfred Schütz, who is widely known for his works in social phenomenology. In the present paper, I aim to discuss some of the specificities of Willard's view on argumentation from the angle of Schütz's influence. For example, social interactions of an argumentative kind should be considered in light of what is called 'intersubjectivity', 'joint awareness' or 'reciprocity of perspectives', these having a hierarchical and multileveled nature. Considering these, I will end the paper by discussing the possible consequences of a phenomenological import in argumentation theory.

## I. Introduction

In *The SAGE Handbook of Rhetorical Studies*, in the chapter dedicated to argumentation theory, Frans H. van Eemeren, one of the leading contemporary scholars in argumentation, writes the following when comparing Stephen Toulmin and Chaim Perelman:

"In spite of the commonalities between Toulmin and Perelman, the differences prevail. Oxbridge-bred Toulmin is much more analytic

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in the way in which he develops and writes down his ideas. Continental Perelman's intellectual proceeding comes closer to practicing phenomenology." (van Eemeren 2009, 116)

So, why is Perelman continental in argumentation? One of the chief reasons is his focus on the agent, on the arguer involved in the process of argument, his approach being maybe the most influential and developed audience theory to date. This interest in the agent seems to make many argumentation scholars raise an eyebrow since it opens the door to elements that do not seem to be of genuine interest to argumentation theory proper (e.g. values, value judgments, persuasion etc.), which should focus on the more formal or technical aspects of an argumentative interaction. In this light, it seems probable that something similar to a fear of psychologism exists in the field.

This emphasis on the agent and other connected concepts is, however, relevant to argumentation studies because it can answer questions that a formalist approach alone cannot. And a relevant question is: why Perelman's approach is similar to phenomenology? To sketch an answer to this, we should turn to the definitions of the two fields: argumentation theory and phenomenology. This way we can form an opinion about the possible points of intersection between the two fields.

There are, probably, as many definitions of phenomenology as practitioners. Nevertheless, it is necessary for the aim of this paper to present a definition. So, I will consider the following recent definition, from *The Stanford Encyclopedia of Philosophy*:

"Phenomenology is the study of structures of consciousness as experienced from the first-person point of view. The central structure of an experience is its intentionality, its being directed toward something, as it is an experience of or about some object. An experience is directed toward an object by virtue of its content or meaning (which represents the object) together with appropriate enabling conditions." (Smith 2013)

Now, if you are inclined to think that argumentation theory had a single good-for-all definition of the field and that definition was kept

until today, you'd have to think again. Actually the definition changed over and over again to accommodate new research that was considered relevant. In this case I will take the definition found in the most recent handbook on argumentation theory:

“Argumentation is a communicative and interactional act complex aimed at resolving a difference of opinion with the addressee by putting forward a constellation of propositions the arguer can be held accountable for to make the standpoint at issue acceptable to a rational judge who judges reasonably.” (Handbook of Argumentation Theory 2014, 7)

Based on these two definitions, what becomes apparent is that first of all the agent or the arguer can be the focus of both these fields, analyzed from their respective points of view. So, to enumerate more explicitly several intersection points: the experience(s) of the agent as arguer and/or audience, the intentionality of the agent as arguer/audience, interaction between agents based on their experiences, assumptions and conditions of the agent etc.

Some of these topics were already touched upon by at least one argumentation scholar: Ch. A. Willard. As we will see below, he was influenced by phenomenology through the work of the social phenomenologist Alfred Schutz.

In what follows, the structure of this paper shall be the following: I will start by summarizing Willard's view on argumentation and then I will present Schutz view on phenomenological social science in order to indicate the points where Willard seems to be influenced. Next, I will point out some problems Willard's theory has, which stem from his phenomenological influence and briefly discuss what should be taken into account in the case of a phenomenological import into argumentation theory.

## **II. Ch. A. Willard's & A. Schutz**

Charles Arthur Willard is an argumentation scholar focused on social aspects of the field. His argumentation theory is considered

“constructivist” (van Eemeren *et alii* 2014, 35) and it is more sociological and rhetorical in its features (Handbook of Argumentation Theory 2014, 233, 449). He developed this theory in three books: *Argumentation and the Social Grounds of Knowledge* (1983), *A Theory of Argumentation* (1989) and *Liberalism and the Problem of Knowledge* (1996). In what follows I will focus on the second of these.

His definition of an argument is the following:

“Argument is a form of interaction in which two or more people maintain what they *construe* to be incompatible positions.” (Willard 1989, 1, italics mine; 42, 66)

And being that, it is based on assumptions:

“An argument is a social encounter built upon the following minima: I assume that we disagree, I assume that you assume we disagree, I assume that I am arguing and that you agree that I am arguing, you assume that you are arguing and that I would agree that you are arguing.” (Willard 1989, 53)

Willard’s view is constructivist and interactionist and from this perspective: “*argument* may refer to *whatever* communications one finds in polemic conversations.” (Willard 1989, 92), argument supposing disagreement, even if imagined (Willard 1989, 12; 53; 66; 148), but at the same time is a cooperative enterprise (Willard 1989, 40; 45-46). So, based on this down-to-top way to see arguments, argumentation may be equated with a specific type of communication (Willard 1989, 12). In general, argumentation is a communicative process that is interactive, social, public (Willard 1989, p. 2, 16, 37, 53, 66, 192). It also fragile in the sense that it can change, Willard saying that it can adapt, being “chameleonlike” (Willard 1989, 2, 7, 130). As a social or public communicative interaction argument is “ubiquitous” and by studying argumentation one has the chance to reveal “the structures of our conversational system, social life, and public knowledge.” (Willard 1989, 2, 7).

Why study argumentation? For Willard this scholarly enterprise has the following purpose:

"(...) a theory of argument can be the empirical basis of a philosophy of the public sphere. In describing actual practices, it will explain the effects of pluralism among experts discourse domains, the political implications of incommensurable epistemic claims, and thus the role of argument in public decision making. Ultimately, a philosophy of the public sphere will be a theory of criticism doubly grounded in an appreciation of the epistemic accomplishments of people and discourse domains in which they move as well as a respect for the relativity that often divides them." (Willard 1989, 10)

As we can observe, to study argument is an empirical endeavour, the main focus, if not the only one, being on actual and particular argumentative processes as they occur in everyday communicative practices.

These were the general lines of what Ch. A. Willard proposes as a theory of argumentation. In what follows, I will focus on several aspects that might have been influenced by phenomenology via A. Schutz, especially from the perspective of what is an argument as an interactive process. These relate to arguments as encounters, the relationships, and coordination between arguing parties and their reciprocal "background awareness".

Argumentative encounters are made possible by the pre-existence of a kind of relationship between the persons engaging in them. This relationship is a construct and it "describes the preconditions of subjectivity". Willard considers two important concepts here: relationships and encounters. They are circularly linked: "Relationships begin with encounters and, over time, undergo successive evolution toward refinement and greater complexity as they guide more encounters" (Willard 1989, 49). Argumentative disputes are "developmental aspects of relationships as well as circumstantial features of encounters" (Willard 1989, 83).

Arguments, as they take place, are encounters and they are actually determined by the relationships (Willard 1989, 47) and these are determined by the assumptions of the arguers. More to the point: "Encounters deal with particular matters; relationships deal with the

members' identities" (Willard 1989, 49). In this light, it is clear that the way an encounter goes about is determined by the participants' identities; as more encounters take place, they shape identities, and future encounters are shaped in turn by these newly shaped identities and so on. Relationships have rules and the relation between two arguers includes all the encounters between them (Willard 1989, 54-55) and Willard talks about a "ethnoscience move" in the study of argumentation, focusing on the arguer (as in the *rhetorical* ethos, the character of the speaker) (Willard 1989, 56). Another relevant concept here is coorientation. For Willard coorientation differs from consensus (Willard 1989, 54), as will become apparent later on. This is what he writes about coorientation, it being an "intersubjective achievement":

"There are (...) three levels of coorientation: agreement, understanding, and realization. If A and B express agreement on X, they 'agree'. If A believes (correctly or not) that B agrees, there is 'understanding.' If A believes (correctly or not) that B believes that A agrees with X, 'realization' has occurred." (Willard 1989, 49)<sup>2</sup>

This represents the multileveled nature of reciprocity.

Another concept needs to be mentioned here: "background awareness". This term is borrowed by Willard from ethnomethodology and it refers to "the assumptions behind our mutual perceptions". I take this to be intimately related to coordination and its levels or, more to the point, to what we think the other believes and assents to. Willard says that at the simplest level, this awareness is about the "formal cultural principles" of the parties (Willard 1989, 52).

Willard references to Schutz in the case of some of the aforementioned concepts. So, terms such as "intersubjectivity" seem to have been used by Schutz to refer to "joint-awareness" and the "reciprocity of perspectives" and this reciprocity has a "hierarchical or multilevel nature" (Willard 1989, 48). This seems to relate to what Willard calls coorientation in the context of an intersubjective relation.

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<sup>2</sup> It should be mentioned that this is taken from (Laing, Phillipson & Lee 1972), a work that also has found inspiration in phenomenology.

According to Schutz, in this line of thought, background awareness consists in “the tacit, taken-for-granted assumptions that lie behind our speech and action” (Willard 1989, 52).

Alfred Schutz (1899-1959) was one of the philosophers who made a phenomenological import into social science, with the purpose of offering a philosophical grounding. His views were mainly influenced by Edmund Husserl. In what follows I will refer to the following of his works, referenced by Ch. A. Willard in his book: *On Multiple Realities* (1945); *Some leading concepts of phenomenology* (1945); *Choosing Among Projects of Action* (1951); *Common-Sense and Scientific Interpretation of Human Action* (1953).

According to Schutz, human beings live in the everyday world, which in the case of Schutz is also called the “social world” or the “world of daily life”. In this world, which is intersubjective in its nature, humans have an “intersubjective experience”. Based on this experience they build-up “stock of knowledge/experience” or “knowledge at hand” which is composed of rules, norms, concepts and other mental constructs; this being what eventually amounts to “common-sense”. This has a “taken for granted” character and it offers “reciprocity of perspectives” and the presupposition of commonality in relation to the world humans live in, i.e. they “construct” a “social reality”. What is “taken for granted” underlies every human activity or experience and so it influences the “intersubjective experience” which in turn influences the way the “social world” is constructed. This construct is neither eternal, nor continuously changing but can be changed based on the shared intersubjective experience of the humans, i.e. it is a process of continuous revision of the social world. (Schutz 1945a; 1945b, 1951, 1953, *passim*).

This sketch should be enough to see that there are many similarities between Willard’s theory of argumentation and Schutz’s view on social science. For example, intersubjectivity, reciprocity of perspectives or joint awareness applies in similar fashion both to social actors engaged in social activities (Schutz) and to arguers engaged in argumentation (Willard), of course, arguers being a subtype of social actors. However, we have to keep in mind that an argument’s “causes and effects are both private and public” and this gives argumentative processes an identity that distinguishes them from psychological or

sociological processes (Willard 1989, 15). Based on this intersubjectivity social reality is constructed, in the case of Schutz by people as social actors, in the case of Willard by people as communicators (Willard 1989, 18). This intersubjectivity makes it possible for the arguers to be aware of each other, the “joint-awareness” between social actors or communicators, and this makes coorientation as a cognitive achievement on multiple levels possible, i.e. the reciprocity of perspectives taking place on multiple levels. This intersubjectivity and the intersubjective world should be continuously created and sustained by the ever-changing social actors (Schutz 1945a, 533-534), the creation being roughly equivalent to what Willard calls encounter and maintenance to what he calls “relationships” in argumentation (Willard 1989, 49). The same way a relationship from an argumentational perspective is maintained by multiple encounters, the maintenance of the intersubjective world is made possible by multiple and repeated acts of creation, i.e. of social world construction. In the case of the argumentative context considered by Willard, an argument as communication and as a repeated process has the role to do that.

What is taken for granted for Schutz, is usually related to what is constructed knowledge about the social reality (Schutz 1953, 29), this being the common sense that determines the intersubjective experience of the social actors. As mentioned above, this knowledge is made out of mental/cultural constructs. Something similar happens in the case of the arguer, who needs to have a “background awareness”. This can be different from one person to another: a “disciplined” arguer (i.e. one who knows the relevant rules) has a somewhat different background awareness than a “non-disciplined” arguer (Willard 1989, 44); this means that, since we have two types of “perceivers”, disciplined and non-disciplined, there should be (at least)<sup>3</sup> two kinds of relations between arguers, simple and complex (Willard 1989, 52). Disciplined background awareness should mean here that the agent has added to his stock of knowledge, or common-sense, the internalized norms of argumentation.

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<sup>3</sup> I say “at least” because there is a possibility of a mixed relation, between a disciplined and non-disciplined arguer. I will not follow the possible implications here.

### III. Imports

Willard's view was already criticized for its shortcomings, one of which was his attitude towards formal logic and its role in argumentation (e.g. Yoos 1991; Gilbert 1993; Johnson 2000). In what will become apparent below, it will be observed that the downplaying of normativity in argumentation is not really something to be desired and doing a phenomenological import into argumentation that will result in minimizing the importance of its normativity actually does more harm than good.

First, some context is needed. Until the middle of the 20<sup>th</sup> century, argumentation theory was more formalistic, more normative, and more theoretic. Starting with pioneering work such as that of Stephen Toulmin or Chaim Perelman and Lucie Olbrechts-Tyteca, the discipline of argumentation changed. It begins to incorporate objects of study that before were not considered as relevant to argumentation theory. Such were, for example, pragmatic issues relating to context, psychological issues relating to arguers etc. Since then, a formalistic attitude in argumentation theory might be considered a bit extremist and it is usually done by downplaying or altogether ignoring the non-formal aspects of argumentation<sup>4</sup>.

A theory such as Willard's seems to be inclined to slowly hover towards the opposite attitude and this becomes apparent when we read what he has to say about the role of formal logic in argumentation. He already criticized the use of diagrams in a paper from 1976, *On the Utility of descriptive diagrams for the analysis and criticism of arguments*. There, Willard makes it clear that by diagrams he refers to such things as Aristotle's syllogistic theory or Toulmin's argument model (Willard 1976, p. 309). This makes it clear that he refers to "models". In the book we focused on in this paper, Willard talks about the definition of argument as "claim-reason complex" – CRC (Willard 1989, 77 sqq.) as the one preferred by those who favor the uses of models in argumentation. For Willard, however, "arguments are too complex to be adequately

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<sup>4</sup> N.B.: This did not mean that argumentation theory started to ignore formal or normative issues. On the contrary, they remained an essential part of argumentation theory and even the most important part, according to many scholars.

represented by narrow models" (Willard 1989, 243) since they cannot account for non-formal aspects of argumentation such as is, for example, humor. He underlines the lack of value in what the "claim-reason complex" is capable of in order to define arguments as they happen:

"As a matter of *defining argument*, the *analytic abstraction* view is empty. Perhaps one can cull the CRCs from the messiest squabbles. But in sifting through a conversation to glean units of meaningful utterance, one may be doing something different in kind from what the arguers are doing, and one is not studying argument as it happened." (Willard 1989, 90)

Willard's model is not considered something that complements the CRC model, but something completely different (Willard 1989, 256) and it is to be expected that a normative model such as CRC should not be considered relevant to guide everyday arguments, an example of Willard being "not all situations share the explicit rule structure of legal proceedings" (Willard 1989, 74). The normative rules that the models try to impose in the argumentative practices are actually a variant of the "constraints" on "human nature" (Willard 1989, 75). An attitude of this kind can be traced back to the importance given by phenomenologists to everyday life in spite of modeling, normativity, and everything considered theoretic and objective in general. Of course, Willard cannot sustain an argument for ignoring altogether these models, but he nevertheless considers them less important in relation to aspects of everyday life argumentation.

There seem to be several problems with this view. First of all, models are not there to represent arguments as wholes, but only parts of them, which is the purpose of the model to underline. It is like the case of abstraction. A good abstraction is when it manages to avoid the extremes of identifying itself with the abstracted object or of having nothing in common with it. What use has an abstract object if it identifies itself with the abstracted objects? What use is the abstract concept of "four" if the only way you consider it relevant is as "four fingers", "four dogs" or "four trees"? Also, argumentation theory is not only about understanding arguments, it is also about understanding

why they are wrong and what we should do to correct them, i.e. the normative part should be considered essential and even principal because any theory or form of knowledge tacitly presupposes this. Even the teachable character of a discipline is profoundly linked to its normative side. The descriptive aspect is very important (and even this needs to take rules into account), and argumentation cannot be realized without it, but its importance should not be overestimated especially at the cost of inappropriately downplaying the normative side.

This aspect should be taken into consideration when we aim to import anything from phenomenology into argumentation because, if the consequence is to undermine one of the pillars of argumentation theory, such as the mentioned normative side, then an import might do more harm than good.

But what about a possible import with positive effects? If we considered what was said above, it is clear that a phenomenological take on argumentation might be useful to gain new insights in regard to the way we consider the relation between the arguing agents, especially from the perspective of how we mentally construct “the other”, his beliefs, his stances, his attitudes etc. Also, a phenomenological take might contribute to what is the descriptive side of argumentation theory, as already seen in the case of Willard, with the mention that this should not be necessarily a description of argument structures used in arguments, but of argumentative communicative behaviour which takes into account what can be called the subjectivity or intentionality of the agents.

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