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Segelberg on Unity and Complexity

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Complex things are things that consist of other things. Some complex things, such as e.g. sets or sums of things, cannot fail to exist once that which constitutes them exists. Call these things *collections* (Segelberg, (1999 [1945]: 47ff.). Other things, in fact most everyday things, are however such that their existence is not likewise necessitated by the existence of their constituents. Call these things the *complex unities* (ibid.). That there are both collections and complex unities and that these are entities of substantially different kinds is assumed as unproblematic (call this assumption DISTINCTION).[1] The topic for discussion is instead the surprisingly difficult question of what *makes* the difference between the two.[2]

According to Segelberg (1999 [1945]: 47), if an object *O* contains (or "is"[3]) a complex *k* (i.e. if it is a complex thing), which contains the mutually disparate constituents *x*, *y*, and *z*, then either:[4]

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[1] I will not defend this assumption (but see Maurin 2011*a* and 2012). I do, however, take it to be, on reflection, obviously true.

[2] The focus, more precisely, will be Ivar Segelberg's answer to this question. This answer he proposes and defends, first in his 1945-dissertation *Zenons Paradoxer* (*Zeno's Paradoxes*); he later returns to the issue in his 1947-book *Begreppet Egenskap* (*Properties*); and then again, in 1953, in his *Över Medvetandet och Jagidén* (*Studies of Consciousness and the Idea of the Self*). I treat Segelberg's discussions, as they appear in his different books, as if they all present and defend identical theses. I know from reading both Herbert Hochberg's thorough investigation of Segelberg's work (Hochberg, 1999), as well as Christer Svennerlind's more recent contribution in the same field (Svennerlind, 2008), that this is perhaps not entirely correct. However, the differences there are, are minor, and so arguably make no difference to the general points I wish to make here. For English versions of the relevant passages in Segelberg's work I rely throughout on the exemplary translations of Herbert Hochberg and Susanne Ringström Hochberg (1999).

[3] Although there exists an interesting and important discussion on the topic of how constitution and containment stand to identity, I will in what follows simply assume that if an object *x* is constituted by (and so "contains") objects *y* and *z*, then provided that *x* is not constituted by (does not contain) anything besides *y* and *z*, *x* **is** *y* and *z*. For the purposes of this paper, although not for the purposes of every conceivable interesting and important discussion on neighbouring topics, this will do just fine.

[4] I have opted for keeping some of Segelberg's original terminology (although I have simplified his representation of the available options). '~' stands for 'is congruent with', and 'NOT~' consequently stands for 'is not congruent with' (alternatively: 'is incongruent with'). According to Segelberg, '$x \sim y$' ≈ '*x*

1. '*O* contains *k*' ~ '*O* contains *x* & *O* contains *y* & *O* contains *z*', or;
2. '*O* contains *k*' NOT~ '*O* contains *x* & *O* contains *y* & *O* contains *z*' and there is no object *u* such that '*O* contains *k*' ~ '*O* contains *x* & *O* contains *y* & *O* contains *z* & *O* contains *u*'.

Consider a rectangle *R* that contains (or "is") a complex *k*, which contains (or "is") the three squares *x*, *y*, and *z*. Is *R* a complex thing of type (1) or of type (2)? If it is an entity of type (1), then *R* is a *collection*, i.e. it is a complex object which contains that which constitutes the complexity and so cannot fail to exist once *x*, *y*, and *z* exist. If it is an entity of type (2), on the other hand, *R* is a *complex unity*, i.e. it is something which contains a complexity without thereby (directly) containing that which constitutes this complexity. As a consequence, if given the existence of *x*, *y*, and *z*, the existence of *R* is not thereby guaranteed.

It seems clear that our rectangle is an entity of type (2), not of type (1) for if the opposite were true, then every time *x*, *y*, and *z* exist, so would *R*. It is however clearly possible that all of *x*, *y*, and *z* exist, yet this particular rectangle does not. The rectangle (*xyz*) is therefore not congruent with the collection of its constituents (*x*+*y*+*z*). Therefore, to say that '*R* contains *k*' is *not* the same as to say that '*R* contains *x*, *y*, and, *z*' in spite of the fact that it *is* true to say of *k* that it contains those very squares. This means that the rectangle is a complex unity. It also means that rectangles are among the many things about which we may ask potentially problematic questions.

What Makes The Difference?

We have found that a world in which our rectangle exists is a world in which the collection *x*+*y*+*z* exists, yet a world in which *x*+*y*+*z* exists is not necessarily one in which *R* does. Consider a world in which *R* does exist. Call this world @ (for "the actual world"). In @ it then follows that two distinct things exist:

1. The rectangle *R*, and;
2. The collection *x*+*y*+*z*

Consider, next, a world *w* in which the collection but not the rectangle exists. This world will be just like the actual world in that both worlds will contain the squares *x*, *y*, and *z*, and yet the two worlds will be different. But then what, if not *x*, *y*, and *z*, makes the two worlds different from one another? This question can be relevantly asked only if we accept something like the following (highly plausible, possibly trivially true) principle:

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is (exactly) similar to *y*' (this is an approximation, for an exact account of how Segelberg understands the notion of congruence, see his 1999 [1945]: 38-41). In what follows, whenever I use the Segelbergian notation '~', the best way to read this is as 'is equivalent to' alternatively 'means the same as'.

DIFFERENCE: there can be no difference without a difference-maker

If we accept this principle (which Segelberg does), the difference between @ and *w* must consist in a difference in content between the two worlds. What is this difference in content? It will not do simply to point out that @ contains *R* (our rectangle) and *w* does not. The question runs deeper than that. What makes it the case that *R*, although constituted by something (the collection $x+y+z$) which has its existence exhausted by the existence of x, y, and z, does not necessarily exist given the existence of this collection? Again, it will not do to say that the complex unity *just is* different from the collection it contains; that their difference is a *primitive* one. DIFFERENCE requires us to say more than that. It requires us to produce a difference-*maker*, and what that difference-maker could be in this case is not entirely clear.[5]

The Collectionist

Segelberg, throughout his work, struggles with precisely the question of what *makes* the difference between a collection and a complex unity. In this text, I consider and then criticise the way he in the end suggests that the problem be solved. I will, however, follow Segelberg's lead and, before considering the solution he suggests, consider the difference-maker proposed by what Segelberg calls "the collectionist" (Segelberg, 1999 [1945]: 52). Segelberg (supported by a famous argument from F. H. Bradley) argues that the collectionist view is essentially faulty and he consequently presents his own view as one that does not end up in the same kind of trouble. I will argue, to the contrary, that the same criticism that can be raised against the collectionist view may be raised also against Segelberg's preferred account.

On the collectionist view, the difference between @ and *w* consists in the presence in @ of a special binding or uniting relation *r*, making *r* the sought-after difference-maker. This view is however strongly criticised by Segelberg (*Zeno's Paradoxes*, 1999 [1945]: 55):[6]

> One tends to picture a connecting relation as a content in the complex unity disparate from the connected objects. If x and y are connected into the complex unity xy, one then thinks one can, in xy, distinguish a relation *R*, disparate from x and y, which connects x and y into the unity. It is obvious that a union of two objects, x and y, often contains moments which are disparate from x and y. But, it is a mistake to think that every union of x and y must contain objects disparate from x and y. Such an idea is dependent on a collectionist outlook: As the collection

[5] See also Maurin 2012.

[6] In this quote, Segelberg speaks of 'moments'. By this he means the 'things' that make up the complex thing. Moments, more precisely, are particular qualities or, as they are often called nowadays, 'tropes'. For the purposes of the discussion conducted in this paper, how you ontologically conceive of whatever constitutes the complex thing will not matter. That ontology, and in particular an ontology of tropes, could provide the sought-after key to a solution to the problem at hand, I have however argued elsewhere (2010; 2011*a*).

$x+y$ and a unity xy both contain x and y, but are not congruent, from a collectionist point of view one of $x+y$ and xy must contain a moment which the other lacks. Since the collection $x+y$, by definition, does not have a content disparate from x and y, the "differentiating" content must belong to xy, and since some such content cannot be observed in xy, an unobservable relation-moment between x and y is simulated. Collectionism seeks to understand every union of x and y as a collection of x, y and certain 'ties', relations, which unite x and y into a unity. The notion, however, is absurd. A collection of x, y and one or more relations z can never be the same as a unity xy. For, the collection $x+y+z$ exists whether x, y and z are 'dispersed' or form a unity, but the unity xy does not exist if x, y and z are 'dispersed'.

According to Segelberg, collectionism must be false because a collection plus a relation can never be the same as a complex unity. But why not? An answer to this question requires us to take a closer look at the argument as it was first presented by F. H. Bradley.

Bradley (and Segelberg) on the Problem of Unity in Complexity

In the beginning of *Appearance and Reality* (1908 [1893]), F. H. Bradley asks of a particular lump of sugar what it means to say of *it* that it is white, hard and sweet. I will continue as before and instead ask of our rectangle R what it means to say of *it* that it consists of three squares x, y, and z. According to Bradley, there are only three things that this might possibly mean:

1. ...that R is identical with each of x, y, and z taken separately;
2. ...that R is identical with each of x, y, and z taken collectively, or;
3. ...that R is identical with each of x, y, and z related.

If one object *could* consist of its many constituents, it would therefore have to do so in one of the above listed ways. But, Bradley argues, neither is a possible way in which one object could consist of its many constituents. Therefore, complexity (of both the complex unity- and the collection kind) is impossible.[7]

The reasons why Bradley does not think that any of the above listed answers to the question 'what does it mean to say of R that it is/contains x, y, and z?' is a possible answer, are the following:[8] the object, first of all (and obviously), cannot be identical with square x and then also with square y (and then with z), unless of course squares x, y, and z are identical, which they *ex hypothesi* are not. Nor, Bradley tells us, can the object be identical with each of x, y, and z taken collectively for then, to use a Segelbergian

[7] In Bradley's own colourful terms: (1908 [1893]: 33) "We want to take reality as many, and to take it as one, and to avoid contradiction. We want to divide it, or to take it, when we please, as indivisible; to go as far as we desire in either of these directions, and to stop when that suits us. And we succeed, but succeed merely by shutting the eye, which if left open would condemn us; or by a perpetual oscillation and a shifting of the ground, so as to turn our back upon the aspect we desire to ignore."

[8] A more thorough explication of Bradley's reasoning can be found in Maurin 2012.

term, the object would no longer be a complex unity, different from a mere collection. We are left, therefore, to consider the option according to which, when we say of *R* that it is (contains) *x*, *y*, and *z*, we mean by this that *R* is identical with *x*, *y*, and *z* related. But what does this mean? Bradley, once more, surveys the field of possibilities, and comes (on my interpretation) up with the following two options:

1. It means that *x*, *y*, and *z* are by their nature such that they are related to each other.
2. It means that *x*, *y*, and *z* are not by their nature such that they are related to each other, but that there is a relation *r* which appears with them and which relates them, and "makes" them into *R*.

The first option is ruled out because Bradley believes that it, just like the option according to which *R* is identical with each of *x*, *y*, and *z* taken collectively, fails to distinguish the mere collection of *x*, *y*, and *z*, from those three squares united. This leaves Bradley with only one more option: When we say of *R* that it is (contains) *x*, *y*, and *z*, we mean by this that *x*, *y*, *z*, and a relation *r*, exist, and that *r* relates *x*, *y* and *z*. This, of course, is the collectionist view. The trouble with this view is that it seems to lead us into a vicious infinite regress. In Bradley's own words (1908 [1893]: 21):

> The relation C has been admitted different from A and B, and no longer is predicated of them. Something, however, seems to be said of this relation C, and said, again, of A and B. And this something is not to be the ascription of one to the other. If so, it would appear to be another relation, D, in which C, on the one side, and, on the other side, A and B, stand. But such a makeshift leads at once to the infinite process. The new relation D can be predicated in no way of C, or of A and B; and hence we must have recourse to a fresh relation, E, which comes between D and whatever we had before. But this must lead to another, F; and so on, indefinitely.

It is the Bradleyan regress which Segelberg alludes to when he states that: "A collection of *x, y* and one or more relations *z* can never be the same as a unity *xy*. For, the collection *x*+*y*+*z* exists whether *x, y* and *z* are 'dispersed' or form a unity, but the unity *xy* does not exist if *x, y* and *z* are 'dispersed'" (1999 [1945]: 55). It is clear, therefore, that Segelberg accepts the Bradleyan argument. He cannot, however, accept every aspect of it. Bradley's argument condemns not only the collectionist view; it condemns the very existence of complex unities. Segelberg, on the other hand, is a firm believer in the existence, not only of complex unities, but of collections as well. But, then, which part(s) of Bradley's argument does Segelberg *dis*agree with?

As we have seen, Segelberg accepts at least the following Bradleyan claims explicitly (and I will assume in what follows that he also, albeit implicitly, accepts the claim that a complex unity cannot be identified with each of that which constitutes it taken separately):

1. A complex unity ≠ its non-relational constituents plus a relation (reason: infinite regress)

2. If there are complex unities, then the collection which now constitutes a certain complex unity could exist and not constitute that particular complex unity (DISTINCTION).[9]

But now things no longer seem to add up. If Segelberg accepts that a complex unity is not to be identified with its constituents taken separately (on pain of contradiction), and if he also accepts that it cannot be identified with its non-relational constituents plus a relation (because this leads to a vicious infinite regress), then it would seem that only one option remains: the complex unity is identical with its constituents taken collectively. But this option is *also* rejected by Segelberg. A complex unity cannot be identified with the collection of its constituents because this would contradict DISTINCTION. Segelberg seems to have run out of options.

Segelberg and States of Affairs

Segelberg, it seems, wants to eat his cake and have it too. On the one hand, he does not want to argue that the difference between a complex unity and a collection is yet another entity. He does not, after all, want to be a "collectionist" (1999 [1945]: 55):

The difference between...a unity *xy* and the corresponding collection *x+y* does not consist in the presence of certain relational moments in *xy*. There is altogether no "difference" between *xy* and *x+y*, if by 'difference' one means a content which belongs to one object but not the other.

On the other hand, he does not want to equate the complex unity with the collection of its constituents. He does not want to give up DISTINCTION (ibid.):

On the other hand, there is, of course, a difference between them in the sense of an incongruence relation.

But how can Segelberg, without contradicting himself, claim that although the collection and the complex unity are *different*, there is nevertheless nothing that a complex unity contains that its corresponding collection does not also contain, and *vice*

[9] It is easy to see that DISTINCTION is very much the driving force behind Bradley's reasoning. It is because he accepts this assumption that he rejects the alternative according to which the complex unity is (nothing but) its constituent collection, and the same assumption leads to his rejection of the alternative according to which the constituents of a complex unity are, by their nature, such that they are related. Evidence that Segelberg accepts DISTINCTION can be found throughout his writings. It is, for instance, clear that he does so from this, by now familiar, quote: "the collection *x+y+z* exists whether *x, y* and *z* are 'dispersed' or form a unity, but the unity *xy* does not exist if *x, y* and *z* are 'dispersed'." (ibid.) And when he claims that: "One can imagine several situations in which the collection *x+y+z* exists without the rectangle *xyz* being present: (a) the squares *x*, *y*, and *z* are included in *o* without touching each other; (b) the squares *x*, *y*, and *z* form a rectangle but the consecutive order between the parts is not *xyz* but *xzy* or *yxz*." (ibid.: 49). Or, again: "If the rectangle *xyz* contains *x*, *y*, *z* and an arbitrary object *u*, disparate from these, one can always imagine a collection *x+y+z+u*, incongruent to *xyz*, in which *x*, *y*, *z*, and *u* do not form a rectangle." (ibid.: 50)

versa? If there is a difference there must be something that *makes* the difference (DIFFERENCE). Can Segelberg reconcile what appears to be irreconcilable? Perhaps.

According to Segelberg, a complex unity and a collection, although not differently constituted, are nevertheless different in that, as soon as *R* exists, not only does the collection *x+y+z* that *R* contains exist, so does the following *state of affairs*:

S: That the squares *x*, *y*, and *z* lie next to each other.

Taking this state of affairs into account means that in the actual world (the world in which, *ex hypothesi*, the rectangle exists) we can now distinguish between, not just two, but in fact *three* different complexes:

1. The state of affairs *S*: that the squares *x*, *y*, and *z* lie next to each other (or, more generally: that *x*, *y*, and *z*, form a unity);
2. The complex unity *R*: the rectangle consisting of squares *x*, *y*, and *z*, and;
3. The collection *C:* the squares *x*, *y*, and *z*

We can now see how *R* and *C* can be indistinguishable when it comes to that which constitutes them, yet one – *C* – may exist, even if the other – *R* – does not. The difference between a world which contains *R* (and, therefore *C*), and one which contains only *C*, is *S*. In a world which contains *R* (and so *C*), *S* obtains. In a world which contains *C*, but not *R*, *S* does not obtain. Therefore, although there is no content which belongs to the complex unity which does not also belong to the collection (and vice versa) we can still say that the two are different. The difference-*maker* is *S*, which is external to *R*. Surprisingly, therefore, it seems as if you *can* have your cake and eat it too.

States of Affairs and the Bradley Problem

Not so fast. If the difference between the actual world, which contains the rectangle, and some possible world, which contains only the collection, is *S*, we are obligated to ask (just as we once did about the complex unity): How does *S* manage to turn what is a "mere" collection into a complex unity? According to Segelberg, when *S* exists (or, obtains) there exists a higher order complex object, consisting of *x*, *y*, *z* and (surprisingly) *r*. More precisely (1999 [1947]: 221):

> A state of affairs is a complex constituted in a certain way. If one wants to discover the characteristics of such a complex which distinguishes it from complexes of the first order, one ought to compare a state of affairs with a first order complex that has, as far as is possible, the same components as the state of affairs. We have made such a comparison earlier, when we compared a rectangle H, consisting of two squares *a* and *b*, with a state of affairs S: the squares *a* and *b* lie next to each other. We found one similarity to be that H and S are complexes which have *a* and *b* as components. The crucial difference turned out to be that S contains a relation while H does not.

And again (ibid):

The essential characteristic of an object of higher order (a state of affairs) seems to be just that it contains one or more relations as components

So, the state of affairs *S*, just like *R* and *C*, consists of *x*, *y*, and *z*. With the help of *S*, we can now distinguish between a world in which only *C* exists and one in which also *R* does (in the latter, *S* obtains). But, clearly, we can only do this if we can distinguish between a world in which *S* obtains and one in which it does not. If *S* is constituted by precisely the same entities as is *C* it is (once more) unclear how a distinction can be made (and, so, it is unclear how Segelberg's view avoids both accepting and rejecting DISTINCTION). But, as we have seen, *S* is *not* constituted by precisely the same entities as is *C*. The difference between a world in which the state of affairs obtains and one where it does not is that in the former world, not only do the non-relational constituents of the state of affairs (and of the collection and the complex unity) exist: so does a (uniting) relation. But how is this not a "collectionist" answer? That is, how can Segelberg say this and not end up in vicious infinite regress?

On External vs. Internal Infinite Regresses[10]

According to Segelberg, it is *because* it is a fact that *x*, *y*, and *z* lie next to each other (R*xyz*) that *xyz* is a complex unity and not a mere collection. But then, it seems, Segelberg must admit that it is *because* it is a fact that a unifying relation relates the 'lying next to each other' relation to *x*, *y*, and *z* (R'R*xyz*) that it is a fact that *x*, *y*, and *z* lie next to each other. And so it does not seem as if Segelberg's account escapes the infinite regress. There is a difference, however, between this regress, and the regress which is generated if you are a collectionist. Or so Segelberg wants to argue. R*xyz* and R'R*xyz* (as well as the infinitely many further states of affairs that Segelberg's account commits him to) are *distinct* from one another, just as *xyz* is distinct from R*xyz*. In fact, although on Segelberg's account, infinitely many entities (states of affairs) are regressively produced, this does not mean that anything that is infinitely complex is thereby produced. Due to an *external* regress, like the one Segelberg accepts, there is no infinite complexity *in* that which generates the regress; rather, there is an infinity of "entities" (in this case, states of affairs), each of which is itself finite. In the case of the collectionist's regress the situation is the opposite: each step of the regress adds a further entity to the original ones, with the result that there is more and more entities, but no unity.

An infinite regress, Segelberg claims, can never be such that one object ends up with an infinity of components. This would be the case if we agreed that the original (first-order) complex unity contained some relation *r* (i.e., if we were collectionists). An infinity of entities of increasingly higher order, however, is not likewise a problem. Or so Segelberg wants to claim. It is not enough, however, to simply state that an external regress is unproblematic whereas an internal regress is not.

[10] For more on the difference between internal and external infinite regresses, see Maurin 2011*b*.

This is far from obvious. What reasons could Segelberg have for believing that an infinite regress that commits one to an infinity of components *in* an object is a serious problem, whereas a regress that commits one to an infinity of objects (distinct from one another) is not? Unfortunately, Segelberg does not say much more on this topic. Fortunately, others do.

One contemporary "Segelbergian" is Francesco Orilia. Orilia is interested in what makes a state of affairs an entity that exists over and above its constituents. His answer is that it is a *further* state of affairs that accomplishes this feat (2006: 229):

> What makes F*a* an entity that exists over and above F and *a* is the state of affairs E_2F*a*, understood as different from F*a*, in that E_2 [i.e. dyadic exemplification] is taken to be the really attributive constituent of the former, whereas F is taken to be the really attributive constituents of the latter.

Orilia is well aware that his proposal gives rise to an infinite regress of states of affairs (and he therefore appropriately names his suggestion *fact infinitism*). But as the regress in question is *external* he, just like Segelberg, takes it to be harmless.

Infinite Regress – Vicious or Benign?

To be able to judge whether Segelberg and Orilia have solved the problem of the unity of a complex unity (or of a state of affairs in Orilia's case), we must investigate further what distinguishes an infinite regress that is vicious from one that is harmless or benign.[11] We could, of course, simply equate this distinction with that between internal and external regresses, and hold that an internal regress is the same as a vicious regress, whereas it is always the case that an external regress is harmless or benign. This would be putting the cart before the horse, however. Whether every internal regress is vicious (and every external regress is benign), is something we will have to wait and see until we have formulated a more substantial criterion by which to make the relevant distinction.

As a first approximation, we can say that a regress is vicious if its existence somehow makes that which triggers the regress (I will call this 'the theory') impossible, or at least improbable. In this sense an infinite regress argument, featuring a vicious infinite regress as its main-premise, is a *reductio* against the position from which the regress is generated. This may sound clear-cut, but it is not. In order to decide whether or not an infinite regress contradicts the position from which it is generated it is not enough simply to look at the regress, and then at the triggering theory, to see if they match. A theory is the same as an answer to a question (albeit a very complex question). It is a solution to a problem, an account of a particular phenomenon, an explanation. In the case of an infinite regress, it is our theory (answer, solution, account, explanation) which automatically and necessarily gives rise to an infinity. This infinity contradicts, or makes highly implausible, the theory (answer, solution, account, explanation) from which it is produced if it robs this theory of its status as an answer,

[11] I discuss the distinction between a vicious and a benign regress at length in my (2013).

solution, account, or explanation. To be able to see if a particular regress in this sense contradicts the theory from which it is generated we must therefore also know what question this theory is supposed to be an answer to, or, what phenomenon it is supposed to explain. To complicate things further, to be able to judge whether or not a particular infinity demotes the triggering theory from its status as an answer – if it robs it of its 'explanatory value' – we must also decide what might constitute such a 'demotion'. This involves deciding what makes a theory explanatory, as well as what might turn an explanation into a non-explanation. Big and difficult decisions indeed! Fortunately, for the purposes of this paper, none of the really big decisions need to be made. Some smaller ones cannot be avoided, however.

In the spirit of (relatively speaking) 'small' decisions, the first thing we need to decide is whether it can *never* be the case that our triggering theory somehow depends, for its status as an answer, on whatever comes next in the regress, if the regress is to count as benign. If this is never acceptable, it follows immediately that the external regress, *pace* Segelberg and Orilia, is vicious. It is after all *because* R(xyz) obtains that xyz is a complex unity. But this criterion of viciousness seems much too strong and would be hard to justify. A more reasonable criterion is instead one which requires that *there must be some point* at which the triggering theory obtains its status as a full answer, or a complete explanation. That is, although it *could* be that the answer our triggering theory pertains to be, depends (for its status as an answer) on what comes next in the regress, it seems likely that it cannot be that its status as a full answer is never obtained because it is infinitely deferred. In other words, it *can* be that every step in the regress requires some further step to 'make' it a complete answer – but it probably cannot be that *no* step in the regress in facts furnishes the relevant completion; completion can most likely not be allowed to be infinitely deferred. If completion is infinitely deferred, therefore, the regress is most likely vicious. This gives us the following (I believe relatively neutral and reasonable) account of what distinguishes a regress that is vicious from one that is benign:

Vicious Regress	**Benign Regress**
A regress is vicious if the answer which the theory from which it is generated purports to be, is *incomplete* and requires completion in the next step of the regress, but where the answer furnished in this step is also incomplete, and requires completion in the following step, etc. *ad infinitum*	A regress is benign if the answer the theory from which it is generated purports to be, is not incomplete and so the existence of the regress does not demote the theory from its status as an answer/explanation (although it does force it to give its answer/explanation with (great) redundancy).

Do Explanations Have to Ground Out?

Now, what difference does it make if, instead of requiring that our answer in no way depends for its status as an answer on what comes next in the regress, we require that, although it could depend on what comes next, it cannot do so *ad infinitum*? In particular, does it make a difference to how we judge the regress to which Segelberg and Orilia are willing to commit themselves? No. On either account, the regress comes out as vicious.[12] That this is so is clear at least to Orilia, who in fact explicitly admits that an external regress is precisely one which defers explanation *ad infinitum*:[13]

> The externalist regress contradicts the thesis that an explanatory chain cannot go ad infinitum without reaching a bottom line.

But this is no problem. For, according to Orilia, although the viciousness of a vicious infinite regress does have something to do with explanation (a vicious regress certainly does not increase the explanatory value of a theory), it has nothing to do with not being able to come up with a *complete* explanation or a *full* answer. To the contrary, he tells us, in the case of the external Bradley regress, it is precisely the regress's postponing full explanation *ad infinitum* that counts as an *in*crease in explanation.[14] But this means that the first question we need to ask ourselves in order to be able to evaluate Segelberg's (and so Orilia's) answer to the problem of complex unities is the following:

> *Q*: Should we, or should we not, accept the assumption that explanation must ground out?

It should be obvious to all, first of all, that the assumption that explanation must ground out, and so cannot be infinitely deferred (call it GROUNDING), is highly intuitive. Orilia is certainly sensitive to this fact (2006: 232):

> ...intuitively it seems correct to say that we have an explanation for *P* only insofar as there is, so to speak, an increase in our knowledge/understanding, when we contemplate *P*. But, one could argue, if in an attempt to explain *P* I begin an explanatory task wherein at every stage I must presuppose a succeeding stage, then there is no increase. For any such increase is an approximation to the final stage and if there is no such stage, then there is no explanation. And thus there cannot be infinite explanatory chains.

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[12] That *both* external and internal regresses are vicious is argued in Hochberg (1999).

[13] In connection with the 11th *eidos* meeting in Geneva on the topic of *Bradley's* Regress (December 2008), William F. Vallicella and Francesco Orilia engaged in an interesting and clarifying discussion on the topic of the regress via Vallicella's philosophically rich blog (Maverick Philosopher). To follow the discussion, go to: http://maverickphilosopher.typepad.com/maverick_philosopher/bradley_and_his_regress/ (henceforth: 'the maverick blog') The above quote comes from that discussion. For Vallicella's views on how to solve the Bradleyan problem *cf.* also his (2004).

[14] Richard Gaskin is if possible even more positively inclined towards this sort of regress. Gaskin is interested in how to unite the different parts of a proposition so as to produce something that can carry a truth-value. He offers a solution to the problem where the unity of the proposition is catered for by *another* proposition (the proposition that the parts of the original proposition are united), and so ends up with an analogous external regress. When discussing the viciousness of this regress, he says (1995: 176): "It follows that Bradley's regress is, contrary to the tradition, so far from being harmful that it is even the availability of that regress which guarantees our ability to *say* anything at all. Bradley's regress is the metaphysical ground of the unity of the proposition. Reverting to the original terms of the problem /.../ we might say, somewhat paradoxically, that what stops a proposition from being a 'mere list' is that it is an *infinite* list (of the specified kind)." *Cf.* also his (2008).

Intuitions may be misleading however, and only a little bit of reflection is required to convince Orilia that GROUNDING is an assumption that should be given up (ibid.):

> But in fact good motivations in favour of such chains can be offered. That at any given stage we can continue the explanatory task does not show that no knowledge or no understanding is provided at any stage. It merely shows that at no stage do we know/understand everything that there is to know/understand about the *explicandum* which gives rise to the explanatory chain. And noting that the *explanandum* in question gives rise to such an infinite chain may be considered part of our understanding of it.

To argue for or against fundamental methodological principles is always a tricky thing. After all, methodological principles are the framework against (and with the help of) which we measure our theories. They are not, or are at least very rarely, explicitly introduced as substantial theses up for evaluation in and of themselves. But even so, if asked to choose between two opposing (or mutually excluding) methodological principles, there are some things we value more than other things, reference to which can make our decision at least reasonable. Intuitiveness is of course one such value, and it is clear that intuitiveness is on the side of keeping rather than rejecting GROUNDING. Orilia is however right to point out that intuition is very often misleading and so can certainly not be our only reason for making that choice. Another important value, of relevance here, is however intelligibility. If faced with two options, where one makes sense, whereas exactly what the alternative entails is far from clear, it seems as if we ought (pending more information) to go for the comprehensible over the incomprehensible or the less comprehensible option. That being said, it is hard to understand how "that the *explanandum* in question gives rise to such an infinite chain may be considered part of our understanding of it." If this means anything at all, it means that Orilia measures explanatory value in terms of our understanding. But surely, to find out that something has *no* explanation could also increase our understanding of it. Explanation and understanding, although perhaps related to each other, are therefore separate, and they should be treated as such. Moreover, if the external regress is benign, it is hard to understand what that which triggers the regress really *is*. It is, after all, our answer to that very question which is being infinitely deferred. For reasons of intelligibility, therefore, we should prefer to keep GROUNDING over rejecting it. Whether there can be explanation even where the explanatory chain never "grounds out", and exactly what that means for that matter, remains to be explained. And the burden of proof is on whoever wishes to reject a perfectly comprehensible and deeply intuitive principle, not on the one who wishes to keep it. Orilia is simply *saying* that there *can* be infinitary explanatory chains. This does not constitute the requisite defence.[15] We should therefore (pending arguments able to convince us otherwise)

[15] Cameron (2008) also thinks that we should keep GROUNDING. His reason is however different from mine. He argues (2008: 12): "I am denying that the intuition can be justified by any more basic metaphysical principle, and so it is a mistake to attempt to justify it in any way. I suggest trying instead to justify it by appeal to theoretical utility. If we seek to explain some phenomenon, then other things being equal, it is better to give the same explanation to each phenomenon than to give separate explanations of each phenomenon. A unified explanation of phenomena is a theoretical benefit. This seems to provide

accept GROUNDING as a matter of 'methodological' law. But then Segelberg as well as Orilia fail to solve the problem at hand.

Taking Stock

Segelberg wants to claim that there are both collections and complex unities, and that the difference between the two consists in the presence in worlds in which the complex unity (and not just the collection) exists of an infinity of states of affairs of increasingly higher order. Segelberg's suggestion is interesting, but problematic. I have argued that it can succeed only if we give up a fundamental and deeply intuitive assumption: GROUNDING. As it is not clear what might replace this assumption, it is not clear in what sense Segelberg's account actually manages to solve the problem at hand. For this reason his suggestion should be rejected. Not even Segelberg gets to eat his cake and have it too.

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some evidence for the intuition under discussion. For if there is an infinitely descending chain of ontological dependence, then while everything that needs a metaphysical explanation (a grounding for its existence) has one, there is no explanation of everything that needs explaining." I, however, do not agree that, in the objectionable kind of infinity "everything that needs a metaphysical explanation (a grounding for its existence) has one". On the contrary, I believe that in the objectionable kind of infinite regress, nothing is really explained (because for a real explanation, we need at some point to have a full explanation). I also suspect that Cameron's argument might be an instance of the kind of fallacy which Russell once accused Copleston of committing when arguing for the existence of God: that of conflating the properties of the parts, with those of the whole (*Cf.* Russell (1948)).

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