

## The Halfway Stage of Seminar One by Phil McShane

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There is obviously still confusion about the tasks we have set ourselves. Seriously unsuccessful efforts have not been posted and some successful efforts are not posted at the request of the members, which is quite acceptable. We have certainly learned that functional research is not easy if one is aiming at a clear instance of it and of writing clearly within the specialty. Very few, indeed, have succeeded in the latter. It is quite a novel task, and it will appear as more problematic as we go along e.g. when we come to try to write functional history. It is worth while to puzzle over this business, like running a particular leg of a 4-person relay race, say a 400 meter race. The strategies of the 4 legs are quite different, as runners would know. I suspect that not too many of us can even think in terms of writing accurately, sentence by sentence, within a given specialty. This is going to be even more evident- and difficult - when we move into the specialties - 5 to 8 - that involve direct speech. It is very difficult, so to speak, not to look back when one is very definitely suppose to be looking forward. [one I suppose to be looking forward in all specialties, but failure is not as noticeable in the first four specialties].

Indeed, it turns out to be quite difficult to write anything worthwhile, or novel, in the forward specialties. It is an interesting exercise, even at this stage, to take a look at a volume of Lonergan writings and identify which articles might tend towards being in a forward specialty. Not too many, AND not too successfully. This is a massive weakness of contemporary "Lonergan writing": certainly there is a regular compacting: one is doing not only research but venturing on to dialectic analyses that are generally confused, without lasting effect, without any serious flow towards actual implementation. It was this mess of theological effort that Lonergan faced and solved in 1965 in a sketchy heuristic that appeared first in 1969 and later, in a muddled context in 1972. The mess continues.

Now the central feature of this mess is already manifest, or being manifested, in this seminar and the larger manifestation of it is the object of the exercises recommended at this stage in our efforts. Those who found the initial exercise quite puzzling could find this effort less difficult. But it could be quite embarrassing, in that it can cause surprise when you try to write out your own basic viewpoint, personally held with some firmness. Recently I read, in the writings of one leading Lonergan scholar, that there was a solid broad consensus about our foundations in the group. This is not at all true, but in so far as it is true it is an old consensus, not terribly uplifted by Lonergan's work, even when his words are there. "First there is splintering, second autonomy [of the splinters], and third the domination of a pragmatic criterion of technique." (Lonergan, *Phenomenology and Logic*, 253). What we are moving towards learning is the gap, the "Existential Gap" (see the index in the same work) between Lonergan's achievement and the scattered achievements of his followers. Nor does it help that we are "nice" to each other.

The end-exercises of this seminar are an effort to not be nice to oneself, and in this perhaps the majority will not wish to have stuff posted, but at least you might risk running it past me. "Am I really trapped in some no-man's land of popularization, or in a halfway house of realism?" are sound questions to ask. I would suggest that the majority of present Lonergan students can answer YES to both these questions: is not that a general embarrassment? Ask yourself "Is it I, Lad?".

Back, then, to the end exercise as identifying why the task of functional research is so difficult. In a developed field, the people in the game are required to have a grip on the standard model, and indeed have. You don't get near research facilities otherwise: you settle for less than adequate teaching, if you

can get a job. But the front-liners do have that common perspective, and the first line group in actual research are pushing to find an anomaly that would nudge towards an improvement in the standard model.

In our zone, and in our seminar, that is not at all the case. So, what is an anomaly for one may already have different solutions for others within **their** view. That has appeared especially in the discussion around the diagram on page 48 of *Method*. But it shows itself generally in the broadness of various suggested anomalies. Such broad reachings occur in serious sciences only when the whole field is in a mess, when there is a serious paradigm shift emerging, as with Newton or Maxwell or Max Plank or Einstein or Schroedinger.

Here the situation is deeply different: there is a paradigm shift offered but its language has been accepted in elementary ways that do not disturb conventional mind-sets, Finding personally the gap between such conventionality and the paradigm offered by Lonergan is the second-stage task of this first seminar. It is to emerge much more soundly in the next four seminars. But light on its emergence pivots for each of us in the pause to identify what our own *acquis*, standard model, Weltanschauung, whatever, is.

The largest block here is associated with notions of theory, *theoria*. Getting such notions properly and effectively is hard work, indeed, "to say it all with the greatest brevity: one has not only to read *Insight* but also to discover oneself in oneself" (*Method*, 260). The crisis of functional collaboration, or its non-start, is not functionality but backwardness, and - more bluntly - the dodging of the challenge of doing some serious science, entering in some serious way into theory. Theory requires serious years of climbing into some respectable science, and the one respectable science that Lonergan used was physics - the simplest of the sciences. What of his later effort regarding e.g. genetic method in the last sections of chapter 15 of *Insight*? He is not benefitting from existing science: he is inventing scientific genetic method, an invention that has also been ignored. In botany and zoology *genetic* at present means primarily the vague dynamics of genes.

The word *theory* is commonly used nowadays [ apart from the phrase related to that usage, "mere theory".... it is *mere* because it is not] for nominal classifications and comparative work. This is good if you are Linneaus, but if you are a 21<sup>st</sup> century philosopher or theologian it is most likely bluff, covered up with consistent techniques of talking. It is for each of us to figure out how much of our talk is the bluffing of a shared commonsense technical language. That figuring out is vital to our moving on. It is vital to the teaching and the directing of the next generations. We are failures in our entrapment in our commonsense *acquis* only if we fail to tell the next generation not to imitate us. But first we have to tell ourselves loud and clear about this *acquis*. That is the task of this second half of the seminar. It will be helped on by the emerging of back-up essays in the FuSe series, but also of course by risky additions to the BLOG.

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