

On Artigas and Analytic Philosophy

Sebastian De Haro

Trinity College, Cambridge

CB2 1TQ, United Kingdom

and

Department of History and Philosophy of Science, University of Cambridge

Free School Lane, Cambridge CB2 3RH, United Kingdom

Abstract

This essay, written on the occasion of the 10th anniversary of Mariano Artigas's death, examines Artigas's engagement with analytic philosophy in his philosophy of science. I argue that, overall, Artigas's project in the philosophy of science is one of—using his own metaphor—'building bridges' between distinct areas of knowledge. After reviewing the function of Artigas's philosophy of science as a bridge between science and philosophy, I analyse how he moved from classical to analytic philosophy. I then assess the extent to which Artigas's work conforms to reasonable analytic standards of clarity and precision, which can be expected from work in the philosophy of science. I conclude that, while Artigas's dedication and production were admirable, his work remains essentially unfinished, thus inviting further research that should develop and clarify his conception of science, of its aims, its methods, progress, and of how science leads to knowledge. I attempt to assess Artigas's philosophy of science from an objective and detached perspective. Thus the essay should be of interest to both scholars in the philosophy of science, as well as to those generally interested in Artigas-scholarship.

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Introduction

It is both an honour and a pleasure to contribute this essay for a special issue on the philosophical work of Professor Mariano Artigas. It is an honour, because I have always admired the work of this great inspirer of Spanish philosophy of science. And it is a pleasure, because the occasion has given me the opportunity to think about some issues springing forth from Artigas’ work which, I think, can be of interest to those engaging with the relations between natural science and philosophy today, as well as on the place of analytic philosophy in the Western philosophical landscape.

Given the occasion, it seems appropriate to add here a brief personal note, which will inform what I will have to say later on. I have admired Artigas’ work for as long as I recall having an intellectual life—i.e. since my teenage years, when I read Artigas’ popular articles on physics and philosophy, as well as some of his books, in particular Artigas (1984) and Artigas et al. (1984). Here was a philosopher who was also a scientist, and who wrote in an accessible and informed manner about important scientific issues bearing on philosophy. Though I never met him personally, I found in his writings a source of inspiration which exerted an influence on my professional path. I believe that Artigas has inspired many others in the same way.

In this essay I will analyse and reflect on Artigas’ engagement with analytic philosophy (for more on this notion: see Section 2). Despite Artigas’s being a philosopher trained in the Aristotelian and Thomistic tradition, he felt the need to engage with a philosophical tradition different from his own. As

I will argue in Section 1, Artigas' openness towards the analytic tradition is an important trait of his general philosophical attitude: he was, using a metaphor of his own, a 'builder of bridges'.

Analytic philosophy is nowadays increasingly prominent in areas, such as metaphysics, which lie well beyond the traditional areas of analytic philosophy, which used to be largely concerned with epistemology, philosophy of language, philosophy of science, and philosophy of mind. For instance, Glock (2008: p. 1) writes:

"Analytic philosophy is roughly 100 years old, and it is now the dominant force within Western philosophy. It has prevailed for several decades in the English-speaking world; it is in the ascendancy in Germanophone countries; and it has made significant inroads even in places once regarded as hostile, such as France."

Though it is not far from the truth, calling analytic philosophy "the dominant force within Western philosophy" may not be entirely uncontroversial. In any case, analytic methods and modes of thinking are indeed increasingly prevalent in philosophy today. It is therefore important to take notice of the way in which, and the aims for which, Artigas turned to this particular philosophical tradition in a relatively late stage of his career.

I should here make a few clarifications regarding the scope of my essay. First of all, I wish to mention that I am neither an Artigas scholar nor one of his students: and that, accordingly—though I have spoken to some of the philosophers who were his students and colleagues at the University of Navarre—my interpretation of Artigas's philosophical thought is exclusively based on his written work. Second, though I will study Artigas's engagement with analytic philosophy, the current essay does not claim (nor does it attempt!) to be a paper in 'analytic philosophy' (see Section 2.1 for my construal of this term). Though I will indeed engage in an analysis—which, given the word limit, will be necessarily brief—of some of Artigas's work, my perspective will be largely normative. I will take Artigas's work as an opportunity to reflect about how the philosophy of science could be done, or ought to be done, in light of Artigas's own history and his own philosophical project. So, it might be read as an essay in the meta-philosophy of science.

Sections 1 and 2 are introductory. In Section 1, I characterise Artigas's general approach to philosophy of science in terms of his own metaphor of 'building bridges'. For the present essay, the relevant bridge will be one built from classical to analytic philosophy. In Section 2, after clarifying what I mean by the phrase 'analytic philosophy', I explore the extent to which Artigas (2001) and, especially, Artigas (2006) can be seen to be such a bridge. In Section 3, which forms the main section of the paper, I appraise the extent to which Artigas succeeded at actually building such a bridge. The last section concludes.

My essay does not attempt to be an exposition of Artigas's philosophy of science (though I will outline some of its general aspects in Sections 1, 2.2, and 3.2), nor a defence of the relevance of his aims today, which I take to be obvious to the reader of Artigas. My aim is instead critical, and attempts to argue that Artigas's work needs revision.

1. Artigas's philosophy of science

In Section 1.1, I characterise Artigas's overall philosophical project as one of, using his own metaphor, 'building bridges': specifically, Artigas sees philosophy as a bridge between science and religion. In the same way, his own philosophy of science can be seen as a bridge between science and philosophy: and I argue this in three points, which form a bridge from philosophy to science. In Section 1.2, I introduce the theme for the rest of the essay: viz. Artigas's engagement with analytic philosophy.

1.1. Philosophy and philosophy of science as bridges

In this subsection, I will discuss Artigas' overall philosophical project in relation to science. For the characterisation which I will give, of Artigas as a builder of bridges, will also apply to the specific work in philosophy of science that I will discuss in the rest of the essay.

One of the traits which characterise Artigas, as a thinker, seems to be his interest in the dialogue between the different academic disciplines. Artigas construes such a dialogue not as an improvised exchange of ideas, or as a superficial round-table discussion on some common issues of interest, between disciplines which tend to be too far apart to understand each other's languages. Nor does he, in any way, advocate for the abolition or the transgression of the boundaries which exist between the disciplines, as if the mere removal of whatever institutional or social impediments may divide the disciplines, sufficed to bridge the deep cultural divides over method and values which are still prevalent.

Rather, Artigas realizes that such dialogue must be carefully crafted and requires specific tools and new forms of expression which must be developed for the task at hand. So, in Artigas (2001: p. 20), he proposes to use *philosophy* for the task of building bridges between science and religion. For Artigas, such bridges cannot be built in an ad hoc manner but must, for their fruitfulness, and given the difficulty of the task, be built *systematically*: and so, he engages in a search for 'boundary questions' between science and religion, which then bring him to the discovery of some natural places for building such bridges.

I submit that Artigas's *philosophy of science*, as a whole, can, and should, also be seen in the light of his bridge metaphor. Artigas's approach to philosophy is distinguished by his profound knowledge of both philosophy and natural science. This knowledge is evinced by, but not limited to, his holding doctorates in both physics (1968) and in philosophy (1979), both from the University of Barcelona. But Artigas's knowledge of science is best appreciated from a study of his philosophical work. His writings in the philosophy of science make essential use of the history of science, including its recent history, which he shows to know broadly, as well as in depth. Thus Artigas' philosophy of science is no erudite reflection on matters learned from books: it is a reflection on the trade which he once learned, and which he himself practiced: and it is fed by his deep knowledge of the history of science. As such (though I am not aware of his discussing this explicitly: as I will explain in the next few paragraphs, his overall project *does* exemplify this) Artigas's philosophy of science could potentially help bridge some gaps between science and philosophy.¹

There are three main reasons why Artigas's philosophy of science is, in principle, well placed to fulfil this role of a bridge between science and philosophy (focusing, for brevity, on how Artigas's philosophy of science is conversant with science and with scientists, rather than on the opposite direction: for that direction, see Section 3):

- (i) Artigas's philosophy of science is rooted in actual science, and it takes into account both the theoretical and the experimental practices of science: so, it is well conversant with the *sciences*.
- (ii) Artigas endorses a form of realism that is not naïve, yet will be pleasing to many practicing scientists because it reflects a 'common sense' view on science (more on this in Section 3) to which most practicing scientists subscribe. So, Artigas converses well with the *scientists*.
- (iii) Artigas's philosophy of science lacks the kind of technical and linguistic sophistication of other philosophical work that sometimes alienates working scientists. In other respects, though, this lack of sophistication can also be a drawback (more on this issue in Section 3.2).

Though I am not aware of an explicit declaration of intent for Artigas's philosophy of science to fulfil this bridge function between science and philosophy, it is clear from his overall philosophical project, as well as from his detailed methodology, that Artigas was, in fact, so motivated.

I have discussed the implicit bridge function which Artigas's philosophy of science could fulfil, as a bridge between science and philosophy. In the rest of this essay, I wish to focus on a second, more

¹ The theme does recur frequently, though implicitly, in Artigas's work. For he often comments on scientists who make philosophical statements, sometimes without realising that they are doing so, or without the kind of conceptual precision expected in philosophical matters.

specific, bridge function of Artigas's philosophy of science (and of Artigas as an author), viz. between *classical* and *analytic* philosophy.

1.2. A bridge from classical to analytic philosophy

In this subsection, I will apply the bridge metaphor to Artigas's philosophy of science, relative to classical (understood rather restrictively, as remarked before, as the Aristotelian and scholastic tradition) and analytic philosophy. The study of bridges between classical and analytic philosophy is of course a broad topic. Accordingly, the task of starting to build such a bridge can appear to be daunting. Furthermore, it is not a priori clear that this bridge will be of the same kind as the bridge which was discussed in the previous subsection. Philosophy of science, regarded as a bridge between science and philosophy, goes both ways: from science to philosophy, and from philosophy to science. Clearly, the philosopher of science who wishes to mediate between the two disciplines will need to talk to both of them.

Let me expand on the obvious objection to this project: for it is not immediately clear that the same reciprocity applies to the relation between classical philosophy and analytic philosophers. After all: analytic philosophers receive, as a rule—and leaving exceptions aside—training in classical philosophy: and during that training they read the main ancient Greek and Roman philosophers, at the very least. The same is not necessarily true of classically trained philosophers regarding analytic philosophy. Classically trained philosophers may have read a few of the philosophers who usually classify as 'analytic' (see Section 2), e.g. Wittgenstein, Popper, or perhaps Quine, but their knowledge of other analytic philosophers is often from secondary, rather than from primary, sources: secondary sources which have often been written by other classically trained philosophers, not necessarily from a sympathetic perspective (for more on this, see the last paragraph of Section 3.2).

This asymmetry of the two relata may in fact be true, and it might be an important consideration to take into account in a detailed attempt at developing the metaphor of the philosophy of science as a bridge between classical and analytic philosophy. But since it is *not* my aim here to systematically develop the bridge metaphor further, we will take the remarks in the previous paragraph in our stride. For my purpose is to analyse Artigas's engagement with analytic philosophy, in the forward direction: from classical to analytic philosophy, i.e. from the point of view of a classical philosopher who becomes conversant with the analytic world (rather than both ways). Indeed, this is the direction of the motion which is most visible in Artigas's writings, and it coincides with his own philosophical autobiography: being a classically trained philosopher, he retrained himself in several respects, engaging with parts of the analytic tradition relating to the philosophy of science. Thus I will leave aside the question of how Artigas's work was actually received in e.g. the English-speaking countries, as irrelevant for my purposes here (though, in Section 3, I will discuss whether Artigas accommodates to common standards of analytic philosophy, and will comment on the extent to which Artigas's work seems convincing, from an analytic perspective).

2. Artigas and analytic philosophy

In this Section, I describe in more detail the extent of Artigas's engagement with analytic philosophy. First, in Section 2.1, I will define what I mean, broadly, by 'analytic philosophy'. Then, in Section 2.2, I will analyse the motion towards analytic philosophy in some of Artigas's work, and will mention three specific points in which Artigas's work can be seen as a bridge from classical to analytic philosophy.

2.1. What is analytic philosophy?

Before we proceed, I should clarify what I mean by 'analytic philosophy': which is, of course, a broad and much disputed term. Much has been written about what, if anything, defines analytic philosophy: and whether the term, used as referring to a particular school of thought, makes sense at all. I claim that

the term indeed does make sense, as I will next briefly argue. For more details, and for a detailed argument to the effect that the term indeed makes sense, see, for instance, Glock (2008: Chapter 1).

Glock argues that, despite the difficulties in providing an *analytic* definition of ‘analytic philosophy’ (where, by an ‘analytic definition’, he means pointing to sufficient and necessary conditions for a philosopher to qualify as analytic), a *real* definition can and must nevertheless be given: one which applies to e.g. actual philosophers and institutions self-identifying as ‘analytic’. Glock (2008: Chapter 8) proposes to define analytic philosophy through a combination of ‘family resemblances’ (see below) and a historical, or genetic, condition.

I do not consider analytic philosophy to be a well-defined school, theory, or method, in the same way in which that may be the case for, say, analytic chemistry, or analytic mechanics. Rather, I take analytic philosophy to be a certain philosophical *tradition*, originating in a number of historical figures who developed particular approaches to philosophy: approaches which include methodological aspects and matters of style, as well as matters of content. These ‘approaches’ can be characterised by what Glock calls, borrowing a well-known term from Wittgenstein, ‘family resemblances’. For the purposes of this essay, I will basically endorse Glock’s characterisation of analytic philosophy, thus construed.

More precisely, and sidestepping the prehistory: analytic philosophy is standardly traced back to the following central philosophers. First, and very briefly, there is Frege’s revolution in formal logic, followed by Moore and Russell’s repudiation of idealism. The latter two authors contributed their own methods to philosophy: including philosophical analysis (in particular, the analysis of propositions, e.g. of propositions including definite descriptions), and logical atomism. Second, there is Wittgenstein’s *Tractatus*, in particular his picture theory, and his own doctrine of logical atomism. These authors opened what is often called the ‘linguistic turn’, in which philosophy turned to analyse language using rigorous methods which were inspired by modern logic. It was held that, in doing so, many philosophical problems could be dissolved—they could be shown to be pseudo-problems, because meaningless. Of course, many philosophical problems, specifically metaphysical problems, cannot be reduced to problems of language alone: but analytic philosophy must not be confused with linguistic reductionism either: which it indeed never was. There are many good examples of influential analytic philosophers, most notably David Lewis, for whom metaphysics was more than analysis of language. But the use of logic and the analysis of language did stick with the analytic tradition.

Third, there is the Vienna Circle (of which Schlick, Neurath, and Carnap are but three influential members), which took some of the above ideas further, combining them with empiricism, an anti-metaphysical attitude, and a focus on natural science, in particular physics. Popper—one of the authors with whom Artigas often converses—did not belong to the Vienna Circle. Popper was too much of an idealist (in the broad, not the technical sense of the word) and Popper’s ideas were too metaphysical for him to be in agreement with the Vienna Circle members; but he belongs without doubt to the analytic tradition. Finally, Quine must be mentioned, another influential analytic philosopher, partly responsible for the renaissance of ontology in the second half of the 20th century (Berto et al. (2015: p.1)), also known for his arguments against the analytic-synthetic distinction made by the logical empiricists, and for his method of regimentation of metaphysical propositions.

The above authors, among others, constitute what Glock calls ‘paradigmatic cases’ of the analytic tradition. Other analytic philosophers bear family resemblances to these paradigmatic cases: they use the same or similar methods, theories, and philosophical style. Some general traits are indeed readily recognised: there is an emphasis on the use of logic in philosophy, a centrality of the method analysis (including, but not limited to, linguistic analysis), they are often science-oriented, and their style aspires at achieving clarity and argumentative rigour. As mentioned in the Introduction, in the second half of the 20th century, analytic philosophers turned to other areas of philosophy, including metaphysics.

2.2. From classical to analytic philosophy

As I mentioned in Sections 1 and 2, Artigas was a classically trained philosopher. More specifically, his early work fits with the neo-Aristotelian tradition. His doctoral thesis, Artigas (1963), was a study of the applicability of the scholastic concept of substance to elementary particles, as described by modern physics. His early work *Introduction to Philosophy* (Artigas (1984)), too, is firmly rooted in the Aristotelian and scholastic tradition. Though Artigas engages dialectically with modern authors such as Descartes, Kant, and the Vienna Circle: his main conversation, in that early period, is with the Aristotelian and Thomistic tradition. Artigas (1984) follows closely a well-known scheme in the introduction to philosophy, resembling e.g. Jacques Maritain's "Introduction to Philosophy" (1962). It is telling that his introductory chapter on the philosophy of logic makes no reference at all to Frege or Russell, and makes only a passing reference to the Vienna Circle.

More than fifteen years after his first doctoral dissertation, he wrote a doctoral thesis on "The trustworthiness of science and its philosophical impact" (Artigas (1979)), which led to a published book on Popper, and presumably strongly influenced his later work.

Artigas's later work on philosophy of science seems to have gained in depth, compared to his early publications. I have here in mind Artigas (2001) and Artigas (2006).² Three points are apparent from these works, especially the latter (on which the rest of the essay will focus), which can indeed be seen as bridges from classical to analytic philosophy:

- (i) Artigas (2006) makes a conscious attempt at a *systematic study of science*: of its aims (Chapter 1), the kinds of activities which make up science (Chapter 2), the scientific method (Chapter 3), the relation between science, truth, and objectivity (Chapters 4-6), the progress of science (Chapter 7), and the philosophical impact of science (Chapter 8). This book could be compared, in its *aims* and its *scope*, to other classic works of philosophy of science such as Kuhn (1962) and Laudan (1978), one important difference being that Artigas assumes a realist epistemology (and, as I will argue in §3.2.5, this is one of the more problematic aspects of the book).
- (ii) Artigas' approach is systematic but also *history-based*. He illustrates each concept and point of his philosophical system with actual examples from the history of science.
- (iii) Artigas explicitly converses with the *philosophy of science in the analytic tradition*. For instance, he quotes and discusses extensively authors such as Carnap, Giere, Hempel, Kuhn, Laudan, Neurath, Lakatos, Popper, Putnam, Quine, Reichenbach, Russell, Schlick, van Fraassen, and many others.³ Thus, Artigas's philosophy of science can be seen as responding to these authors and incorporating some of their insights (cf. the last paragraph of Section 3.2).

Thus, when attempting to build his systematic account of science in his later years, we find Artigas turning to the analytic philosophical tradition for philosophical conversation. This is not to say that Artigas *became* an analytic philosopher—which he, indeed, never was, as I will argue in the next Section. Rather, Artigas combined the classical tradition with a discussion of contemporary philosophy of science. One should add here that Artigas's use of classical philosophy was never naïve or uncritical: indeed, his attempt at building a theory of contextual truth and contextual realism, applied to science, should be seen as critical of naïve correspondence theories of truth, and therefore as genuinely modern.

3. In praise of Artigas

My aim in this section is to praise the master. And I will do so in the best way I know of in philosophy, namely by indicating where his thoughts could lead, if developed further: perhaps where he himself would have wished them to lead him, if he had lived longer. This will entail pointing out some of the limitations and, perhaps, some of inconsistencies which one can find in Artigas's thinking: for we can

² I will not be here concerned with the personal or historical causes of this change in Artigas's thinking, about which Artigas-scholars may be better informed than I am.

³ This list includes authors discussed in both Artigas (2001) and Artigas (2006).

be sure that progress will come from overcoming those limitations. Thus the mistakes of the master will point ways forward.

In Section 3.1, I will comment on what I take to be reasonable standards of clarity and precision which apply to work in the philosophy of science, especially (but not exclusively!) in the analytic tradition, construed as in Section 2.1. In Section 3.2, I comment in some detail on Artigas (2006), and criticise it for its lack of clarity and precision.⁴

As a first attempt at contextualising Artigas’s potential influence as a philosopher, it seems appropriate to use the following quote from Whitehead (1928 [89], p. 57) about the formation of philosophical schools:

“Every philosophical school in the course of its history requires two presiding philosophers. One of them under the influence of the main doctrines of the school should survey experience with some adequacy, but inconsistently. The other philosopher should reduce the doctrines of the school to a rigid consistency; he will thereby effect a *reductio ad absurdum*. No school of thought has performed its full service to philosophy until these men have appeared.”

The first philosopher of the school tends to have a speculative, bold and creative way of philosophising; the second philosopher of the school brings the principles of the first to their logical conclusion, often requiring major modifications of the master’s doctrines in order to render them consistent. Of course, the above is (unlike Whitehead suggests) not a necessity of a philosophical school. But Whitehead’s analysis in the quote does make vivid a pattern in the history of philosophy which is easily recognisable; the pattern is particularly visible in the relation between Plato and Aristotle; but also between Socrates and Plato, and between Aristotle and Aquinas. Whitehead may have seen himself as the first philosopher of the ‘philosophy or organism’, as he called his own doctrine. The second philosopher of that school apparently has not yet appeared.

The importance of the quote for our subject is that it suggests an interesting interpretation of Artigas’s thought. Indeed, I regard Artigas as a speculative, bold and synthetic thinker whose ideas could be developed by the members of a ‘school’. His attempt at systematisation in Artigas (2006) certainly suggests this. But as I will argue in this Section, Artigas’s ways of expression are imprecise, and so the development of a ‘school’ based on his thought⁵ would depend entirely on whether the inconsistencies and vagueness in his work are exposed—and resolved.

Let me also state clearly that I have no intention of being the second philosopher myself. Thus this section should be read as an invitation.

3.1. Aiming at clarity and precision

In the preamble of this Section, I stated that Artigas could *potentially* be seen as the first philosopher of a school, whom Whitehead envisaged. I now expound on the reasons for the caveat ‘potentially’.

Remember that, on Whitehead’s conception of a philosophical school, there is the requirement of the *inconsistency* of the first philosopher of the school.⁶ In my opinion, Artigas’s work cannot be yet

⁴ One should bear in mind that Artigas (2006) is a translation of the 1989 Spanish original. Though Artigas’s thought may have developed further during that period, Artigas (2006) can still be seen as Artigas’s final word on the matters treated in his book, of which later work is further elaboration. This follows from (i) The fact that Artigas never published again such a systematic work in philosophy of science (his 2007 *Oracles of Science* book concentrates, for instance, on very different aspects of the science-philosophy relation). (ii) In the preface to Artigas (2006), he explicitly endorses everything he wrote in 1989: “After sixteen years, I still subscribe to everything I wrote in 1989... Therefore, I have decided not to change it [the text]”.

⁵ I focus, in this essay, on the possibility of developing Artigas’s *own* thinking. As I mentioned before, Artigas’s philosophy of science aims at the kind of systematization that would merit being developed by a school. Thus, my comments in the rest of the paper strictly relate to *this* specific philosophical proposal, and not to related attempts by other authors.

⁶ My use of Whitehead’s conception of, and especially his requirements on, a philosophical school, should be seen only as motivational. There is, of course, no *literal* requirement that any philosophical school must contain *two* philosophers who stand in the exact relation to each other that Whitehead envisages. Rather, I read Whitehead’s conception as a helpful metaphor: and (in so far as it can also be interpreted in a normative sense) as a practical guideline, the main thrust of which (at least, in so far as the conception is important for analysing Artigas) is the idea that every philosophical system contains some inconsistencies which are then disputed, and claimed to be resolved, within the philosophical school, once the doctrines are systematised by the followers.

qualified as ‘inconsistent’. For a doctrine can only be shown to be inconsistent once it is formulated with sufficient precision for its claims to be testable for their logical, linguistic, and conceptual consistency or coherence. But if a doctrine is vague or unclear, it is impossible to give a verdict on its consistency.

This is true even if one must admit that complete clarity and precision are never quite within reach, and that some vagueness always remains.

To illustrate the argument just made: suppose that I make a claim X, and that you are to decide whether my claim X is right or wrong. You can only reasonably be expected to decide whether I am right or wrong in claiming X, once I explain X, and my reasons for believing X, in such a way that its meaning becomes clear. If my claim X is so vague or unclear that it can be understood in widely differing ways (including ways which are right and ways which are wrong), and if my arguments for the claim X are not clearly formulated, then the question of consistency does not even arise. Clarity about the meaning of X must be reached before consistency can be even discussed.

In other words, only once the doctrines of the first philosopher of the school have been clarified, can there be a second philosopher who will be able to “reduce the doctrines... to a rigid consistency”, and thereby “effect a *reductio ad absurdum*”. The doctrines must be *clear* before they can be told *wrong*.

The interpretive *principle of charity* in philosophical discussion (see e.g. Ney (2014: p. 11)) must, of course, also be applied in this work of clarification. Given distinct ways of construing a doctrine, we should choose the most charitable interpretation—i.e. the one which renders as much of the doctrine, as possible, as true. It is not the case that the principle of charity renders just about *any* system true, especially when applied to systematic works, such as Artigas (2006), which attempt to give an overall view of an *entire field* of knowledge, viz. natural science. For the history of philosophy shows that it is very hard (if possible at all!) to find such systematic expositions which are entirely free of inconsistencies and are coherent with the history of science. So, we should give Artigas (2006) the benefit of the doubt, and assume that, if a construal of it exists which renders it approximately true, Artigas would have adopted it.

It seems perfectly reasonable to assume that *some* rendering of Artigas (2006) exists which will make it approximately true (and, of course, specifying the extent of ‘approximate’ truth is also part of the job to be done: see §3.2.4). For Artigas’s is, in many ways, a common-sense view, based on real scientific experience. Be that as it may: the trouble is, rather, that Artigas (2006) is often too vague for the criterion of inconsistency, as just discussed, to apply to it in a straightforward way. Indeed, for all its virtues, Artigas (2006) does not seem to stand up to common standards of analytic work in the history and philosophy of science (even admitting that complete clarity can never be attained, as remarked above), as I will illustrate in the next subsection.

3.2. Articulating the criticism

In this section, I criticise five points of Artigas (2006): his notion of natural science (§3.2.1), his conception of context (§3.2.2, §3.2.4), his use of models (§3.2.3), the role of realism in justifying objectivity (§3.2.5), and three more general points (§3.2.6).

The first problem which the reader of Artigas (2006) encounters is a severe problem of translation. Artigas (2006) is the translation of the book which first appeared in Spanish in 1989. Unfortunately, the English lacks fluency and is full of neologies and odd phrasings, which make the reading tedious.

But more importantly, the content of Artigas (2006) is often vague and unclear. Definitions and statements are frequently expounded in terms which are ambiguous, and their clarification is often left implicit: apparent in specific examples, rather than conceptually. It looks as if the reader is expected to have some intuitive grasp of the definitions, which are then further fixed in the examples.

Such a method may, of course, be suitable when teaching broad, rather than specialised, audiences. But that is not the kind of audience which Artigas had in mind, since the book was meant as a *scholarly work* contributing to philosophy of science research: “Dr Artigas’ Spanish original was addressed to the community of *philosophy of science scholars*” (Translator’s Preface, my emphasis). And even letting aside the circularity which the method, of definitions implicit through examples, may bring in (since the examples serve both for making the meaning of the concepts explicit, as well as for their testing against history). The lack of clear definitions and of a conceptually sound articulation of the different notions, makes the whole theory vague, as I will now illustrate with a few examples. It is keeping in mind Artigas’s audience, of philosophers of science scholars (and Artigas’s intent of making a contribution to the philosophy of science), that I will address my criticism.

3.2.1. The notion of natural science

In Chapter 1, Artigas gives us a definition of experimental science:

“The defining characteristic of experimental science is the demand that its theoretical contents be related *in some manner* to experimental control. If this demand is satisfied, the theoretical contents permit one to obtain knowledge of nature related to controlled domination. The primary definition of experimental science, its general goal, can be expressed in function of a *methodological* requirement. . . Science is a *knowledge-seeking activity whose theoretical contents are related in a logical and coherent manner to controllable data obtained through experimentation.*” (2006: p. 11, emphasis in the original).

Intuitively, the definition is clear enough, since it seems to agree with some of our intuitive ideas about natural science. But does the definition stand, or does it crumble, under the pressure of tough questions? On narrower scrutiny, one wishes to understand in detail the meaning of the terms in the definition, so that this notion of natural science can be put to other uses in the philosophy of science. Unfortunately, several of the words used in this conception are either intrinsically vague (‘related’ seems too generic a notion; the same goes for ‘coherent’: how strong is this a requirement?) or are used in ways which are ambiguous because not defined or used in non-standard ways (‘logic’ is here not used in its literal sense, but in a looser sense: Artigas, unfortunately, does not tell us what a ‘logical relation’, on his view, looks like). Furthermore, Artigas never told us precisely how he construes ‘knowledge’ (more on this in §3.2.2).

One would expect the rest of Chapter 1 to unpack for the reader the content of the defining notion at the beginning of the book. Unfortunately, Artigas does not do this. He assumes that the reader understands his concept of science, and goes on to illustrate it in examples. Nor do the subsequent chapters further specify the above notions more precisely.

But there is a more basic difficulty with the above definition that recurs in later chapters: it is the reference to “experimental control”, in the first sentence, and to “controllable data obtained through experimentation”, in the last sentence. First of all: many of the data, on which science is based, are not obtained through *experimentation* but through *observation*. For example, there is little cosmological experimentation which one can currently do (not to say: none at all!), whereas there is increasingly more observation available. Second, the demand for ‘control’ in the above quote is vague: in the first sentence, it refers to experimental control *of the observed system*, while in the last sentence it refers to the control *of the data* (but then: what would *uncontrolled* data look like?). (Elsewhere, Artigas seems to have in mind mostly experimental control *of the observed system*. He also uses the word ‘domination’.)

In fact, it is unclear why science should refer to experimental *control* or *domination* at all (maybe one reason for the wording is Artigas’s wishing to impose the standard requirement of reproducibility of experiments, but then his phrasing is too strong: reproducibility is different from the requirement of the availability of experimental control across the board. Furthermore, reproducibility is a requirement of a specific method: for instance, the GW150914 black hole merger cannot be reproduced, let alone controlled or dominated; yet it is a scientific discovery. One should not confuse science with one of its methods.)

This ambiguity remains unresolved in the rest of the book, and it propagates into Artigas's historical analysis. Because, on the one hand, Artigas argues that 'real', or 'fully developed', modern science, appears around 1600: so, his restriction of the notion of science, to those branches of knowledge in which one is able to carry out experiments, makes sense. But that very stipulation rules out from natural science several important disciplines (such as astronomy, geology, and parts of biology) in which: (i) observation, rather than experimentation, is the rule; and (ii) in which the system under study (e.g. a distant black hole) cannot be controlled. And so, his own definition rules out disciplines which we normally, qua natural science, think of as belonging in the same category as experimental physics.

A related problem is in the phrase 'experimental science' as the object of study of the philosophy of science. As just argued, not all of natural science is experimental: much of it is observational. (A term that captures both experimentation and observation is 'empirical'). But since all of natural science contains both empirical *and* theoretical aspects, it is misleading (one-sided) to construe philosophy of science as philosophy of experimental science. 'Natural science' would have been a more appropriate term for Artigas's object of investigation.

3.2.2. The conception of 'context'

Another vague notion, which is furthermore a key ingredient of Artigas's system and is used throughout, is that of 'context'. Contextuality is introduced in Chapter 2:

"General principles and theoretical systems provide profound explanations and permit efficacious study of a large variety of problems. The results are contextually valid, relative to contexts *of the problems and methods*. Describing the validity [of the explanations] as contextual does not imply a relativism undervaluing the cognitive reach of science. Authentic knowledge is obtained. This knowledge is partial and contextual, which only implies that its validity is judged taking into account the problems to which this knowledge refers and the conceptual and experimental means employed." (p. 51: my emphasis).

This notion of contextual validity is, of course, a notion to which uncritical common sense easily agrees: for most of the knowledge that we possess is, in some sense, contextual: and this is true not only in science, but in everyday life as well: thus underlining the continuity between the two kinds of knowledge. But in philosophy we are interested not just in confirming our common-sense intuitions, but in developing rigorous notions which can then succeed or fail when we apply them to some new, tough cases. Unfortunately, Artigas never goes on to define what he means by 'context' with sufficient precision.

In the above quote, 'context' seems roughly interchangeable with 'scientific problems under study and methods'. But that qualification seems both too broad and almost empty. First: Is a scientific explanation, from its very nature, not always an explanation relative to a *scientific problem* (or a given class of problems)? Second, and more importantly: it is not clear what is meant by truth that is contextual with respect to a *method*. Does contextual truth cease to be contextually true if e.g. a different method (of investigation, or of verification) is used? Third: in the inference from the first part of the quote above to the second, Artigas attempts to move from contextual knowledge to some kind of objective knowledge (which he dubs 'authentic knowledge'). But the inference does not seem to follow, at least not without further clarification. Just how does the inference, that contextual validity does *not* imply relativism, precisely work? We are not told: presumably because this step could only be justified in the context of a more general epistemology (not just a specific epistemology of science), which Artigas does not develop. (This is supposed to be the topic of Chapters 5 and 6, but the inference will not be explained there satisfactorily either: see §3.2.5).

3.2.3. Models and the scientific method

In Section 3.2, Artigas articulates his conception of the scientific method. He introduces a number of notions: some familiar, like 'theory' and 'hypothesis'; others more specific to his own account, like 'scientific object' and 'basic predicate'. (In what follows, I will often refer to the elements of Artigas's analysis of science as 'factors'). In particular, Artigas introduces the notion of 'model'. Of course, talk

of ‘models’ is widespread in the philosophy of science literature, and models figure prominently in the work of e.g. Nancy Cartwright. Models are used to fill the gap existing between theory and phenomena, so that the theory can be applied to specific phenomena. And models are usually construed as, roughly, simplified conceptual schemes which: (i) capture, i.e. represent in their essential details, specific scientific phenomena; (ii) the representation is such that it illustrates or exemplifies the theory, i.e. the theory can be applied to it: under specific physical assumptions, and using appropriate methods of approximation. Thus, on this generic conception in the philosophy of science, models mediate between theory and phenomena. In the philosophy of physics literature, the use of ‘models’ is more specific: models are solutions to the dynamical equations a theory, i.e. concrete instantiations of the theory, in terms of solutions to equations.

In order for the concept of a ‘model’ to do any philosophical work, it needs to be defined. Unfortunately, Artigas does not provide us a definition, but only examples (e.g. the corpuscular kinetic model used in thermodynamics to study gases: p. 65). He also does not seem to endorse the general notion, nor the more specific notion, just mentioned: instead, Artigas seems to assume the physicists’ informal use of ‘model’. Unfortunately, such use is insufficient for the kind of philosophical work which Artigas wants the concept to do for him: since he will later claim that models have both ‘reference’ and ‘meaning’. Furthermore, he is not consistent even relative to the physicist’s use of ‘model’, since he later talks (e.g. on pp. 167 and 170) of the ‘corpuscular model’ and the ‘wave model’ (of matter): while physicists usually talk about corpuscular and wave *descriptions* of matter. The reason why physicists nowadays talk about descriptions rather than models of matter is that, after the developments of modern quantum theory, the meaning differs from other well-known cases (like Bohr’s model of the atom).

Turning to philosophy: the precise function of models as mediators between theory and phenomena, for Artigas, remains unclear. Also, models do not seem to play any important role in subsequent sections: for instance, when, in Section 3.3, Artigas discusses the formation of theories, and in Section 3.4 the confirmation and validity of theories, he does not mention the role of models at all. So why introduce this notion in the first place? It seems that there is too much arbitrariness in the set of notions considered, and different parts of his system are not well articulated.

3.2.4. Partial truth and context: the succession of theories

In Section 6.2, Artigas uses the adjectives ‘contextual’ and ‘partial’ to refer to truth. He does not define ‘partial’, which is a vague predicate, so that the natural question arises: if truth is partial, just how right (or how wrong?) is a given instance of a partial truth? By which criteria is ‘partiality’ to be judged? Artigas does not give us any criteria here: only examples; and even the examples which he gives are not sufficiently detailed, as we will now see. His main example, in this section, is from classical mechanics:

“If one formulates new laws, such as in the theory of relativity, the object of the theory changes, so a statement true in classical mechanics may be false in relativity. Nevertheless, this statement remains true in the context of classical mechanics. If one applies classical mechanics to bodies much bigger than atoms with velocities much less than the speed of light, one can form contextually true statements and intervene experimentally with success.” (p. 142).

First of all, notice the move: as I discussed in §3.2.2, ‘contextual’ was used interchangeably, in Chapter 2, with ‘relative to certain scientific problems’ and-or ‘relative to certain methods’. Now, in Chapter 6, ‘contextual’ is apparently meant in the physicist’s sense instead: relative to a particular *theory* (classical mechanics) and, more specifically, relative to a specified *range of values of the parameters* of the theory (“velocities much less than the speed of light”). But these are conceptually different things, and the latter is not necessarily just a further specification of the former. So, which one should apply, and in which cases? In Chapter 7, Artigas will adopt yet another meaning of ‘contextual’: as referring to a conceptualisation, and to a characteristic objectification (p. 182). All of these meanings are remotely related to each other, but they differ significantly from each other. Artigas moves between those three uses without further warning. So, it seems that there is nothing here by way of a clear theory of contextual knowledge or contextual truth.

Second, the inference made in the above quote is too fast. Even allowing ourselves to consider the notion of ‘contextuality’ as in Section 6.2 (i.e. as ‘relative to a certain range of physical parameters’, independently from the notions of contextuality in Chapters 2 and 7): one cannot assume that any statement in classical mechanics is automatically contextually true in special relativity. Though some version of this statement may be true, the naïve claim is contentious and requires further qualification: it has, in fact, been discussed at length in the philosophy of physics literature. For instance, the pessimistic meta-induction argument is often based on this example. Also, construing ‘contextuality’ in terms of a ‘range of parameters’ is an intricate conceptual and technical problem in itself. For example, the mathematical fact that two sequences may converge in one topology, but not in another, means that even in the context of a *single* theory, deciding whether two quantities ‘agree with each other in a given range of parameters’ is a complicated question to which no a priori answer can be given (for a recent discussion in the context of general relativity, see Fletcher (2016)). Artigas does not seem to be aware of, or be interested in, such problems. (In fact, he later shows, on pp. 167 and 171, that he *is* aware that there is a problem. But he does not solve it, nor does he explain whether it has any consequences for his reasoning: which makes things even worse, because it gives the impression that the above argument, at least without further elaboration, was a cheat!)

I am not criticising Artigas for not *solving* all of these problems in this book (which would, indeed, be an unreasonable expectation to lay on a single book). I am criticising him for: (i) not mentioning the problems and how these problems affect his work (at least, not in the key places where he *should* have mentioned them!), (ii) not indicating how, at least some of these problems, can be solved or bypassed. For it is not the task of philosophy to uncritically systematise our common-sense intuitions (not even the common-sense intuitions of physicists). And in cases like this, in which the common-sense intuitions have been challenged for good reasons, the philosopher must at least gesture at those objections, and indicate why the objections do not affect their work, if it is indeed the case that the objections do not apply to it—on pain of becoming superficial, incomprehensible, or simply incoherent.

3.2.5. Realism: from inter-subjectivity to objectivity

One of the most intricate problems in this book concerns the status of scientific realism, as a metaphysical ground for justifying the objectivity of knowledge.

Artigas has argued, in Sections 5.1-5.3, that:

“The inter-subjectivity of experimental science is achieved through agreement about theoretical and experimental stipulations, and also through the acceptance of theories.” (p. 122).

Sections 5.4-5.7 then discuss, or attempt to argue for, inter-subjective knowledge as a case of objectivity. The arguments here are not very conclusive, because Artigas does not address the obvious threat which empiricism poses to science as a source of objective knowledge. Rather, Artigas seems to ultimately retreat to the position that “these stipulations are not arbitrary, and are justified by their success, that is, by their capacity to permit reaching the objectives of the science”. This may be so, but it would require elaboration. The objectives of science included knowledge, and it is the foundation of that knowledge that we are here trying to secure. So, should the objectives of science not be the *endpoint* of the argument, rather than an instrument used to justify stipulations? The argumentative structure is here unclear and unconvincing.

Chapter 6, and in particular its sections 6.1 and 6.3, seem to be set up as an answer to the question of how an inter-subjectively construed science can give us truth (already also discussed in §3.2.2 above). Unfortunately, since contextuality did not receive a satisfactory treatment in earlier chapters, the arguments here are weak and inconclusive as well. I cannot find any substantive argument in this Chapter, which would allow us to move from inter-subjectivity to objectivity. Rather, Section 6.1.2 contains a rehearsal of arguments and examples which were already given before: a poorly worked out contextual theory of reference and meaning of terms. Artigas should at least have considered the

standard arguments against such correspondence theories (also in other places, where he discusses conventionalism). For example, there is the standard problem of under-determination of theory by data: cases in which the same experimental and observational data are consistent with different theoretical descriptions. In such cases, it is not clear that a stipulation should lead us to truth (as Artigas does seem to assume): not even if we construe this truth as limited and contextual. One reason why this does not immediately follow is that, in the face of two possible theories describing the same data, it may be the case that one is right and the other is wrong (by other criteria), so that our stipulation for one of the two options simply gets it wrong. These objections can perhaps be resolved, but a lot more work is needed to be able to argue that stipulating a ‘wrong’ theory can, in some way or another, give us truth in some limited and contextual sense—or to find some other resolution. Artigas, despite his discussion of Duhem and Poincaré’s conventionalism, does not give any signs of having given serious thought to such problems.

Only in Sections 6.6 and 6.7 does it become clear why Artigas’s arguments for the inference from inter-subjectivity to objectivity, discussed above, were ultimately unconvincing. The point is that his argument for objectivity works only on the assumption of scientific realism. But then Artigas, in somewhat of an anti-climax, declares:

“explaining scientific realism is a still unfinished work, given the special features of the procedures used by experimental science.”

First, science had been analysed (in Chapters 1-4) into a number of factors (including stipulations) whose cognitive objectivity needed to be justified. To that end, recourse was taken, in Chapter 5 and in Sections 6.1-6.3, to scientific realism. Now, at the end of Chapter 6, we are told that realism cannot be fully explained. Without a discussion of the impact, of this explanatory gap of scientific realism, on the previous arguments, which relied on scientific realism, this is worrying. Furthermore, it feels a bit like a cheat, for the following additional reason.

‘Scientific realism’ is not a kind magic powder that grounds the objectivity of science to our pleasing. ‘Scientific realism’ is just a generic phrase expressing our belief that science gives objective knowledge. Analysing science into factors, and grounding those factors on ‘scientific realism’, without giving some explanation of scientific realism itself, including *independent grounds* to be a scientific realist, is like doing nothing (unless, of course, one believes in magic powder). In other words, if we try to argue that factor F of natural science gives objective knowledge, and we do this by appealing to scientific realism, we should give independent reasons for scientific realism: for, on the face of it, scientific realism is nothing but the belief that science provides objective knowledge through factors such as F. So, the spectre of circularity looms here again. The debate over realism vs. empiricism is precisely a debate over whether, and how, factors such as stipulations, theories, etc. justifiably lead to objective knowledge. For scientific realism to have any explanatory or grounding power, it needs to be specified and argued for.

It would have been more convincing if Artigas would have simply told us clearly from the beginning that he was going to *assume* scientific realism.

Therefore, I doubt that Artigas’s argumentation will be convincing to many philosophers of science: and they are the target audience of the book, as mentioned in the quote in the preamble to this section. I do not think that they will be convinced, because Artigas is vague about *what kind of scientific realism* he has in mind (in other words, he is not clear about the meaning of scientific realism). He often uses a phrase like: one needs “a minimal dose of realism”. But this sounds like magic powder.

A more convincing argument, using scientific realism to justify the objectivity of (specific factors in) scientific knowledge, would have approximately the following structure:

- (a) Provide *independent reasons* for why one is a scientific realist (these reasons may be of whatever kind: e.g. a no-miracles kind of argument, a Bayesian analysis of scientific success, etc.: but those reasons must be independent from the concrete factor which one wishes to justify).
- (b) Based on these reasons, specify just *what kind of scientific realism* is needed, i.e. what kind of scientific realism the reasons (a) require. Thus answering the question: what does (a) require us to be scientific realists about?
- (c) Confront the scientific realist position, constructed in (b), with the conceptual analysis of science (i.e. with science analysed into a number of factors), which one has obtained by an *independent analysis*.
- (d) Based on (c), assess whether the version of scientific realism, constructed in (b), renders the knowledge provided by science objective (e.g. assess whether stipulations are justified as leading to objective knowledge).

If (d) does not render scientific knowledge objective, then one needs to either go back to (a) and rethink the arguments for realism, or else redo the conceptual analysis of science.

Artigas seems to skip several of the steps above: he begins with (d), i.e. with the analysis of the objectivity of the knowledge provided by science, but without having a proper notion of scientific realism. He then introduces scientific realism as a justification for that objectivity. But he does not independently construct his form of scientific realism (b) from the reasons (a). So, one gets the impression that, as a result, scientific realism is very much like magic powder.

3.2.6. Epistemology, rigour, and analytic philosophy

Let me end with three further, more general, points in which Artigas, though engaging with analytic philosophy of science, does not seem to do full justice to what an analytic approach to the philosophy of science would entail.

First, as I have already argued above, Artigas seems unaware of the existence of basic philosophical problems underlying some of the philosophical questions with which he is concerned. Knowledge and truth are leading themes of Artigas (2006): yet he nowhere gives us a construal of these notions, nor does he seem aware of the existence of a basic Gettier problem about the tripartite conception of knowledge, or of other analytic work on contextual notions of truth.

Second, Artigas seems to despise the mathematical rigour and axiomatic approaches which are characteristic of some analytic approaches (and, by the way, are also characteristic of a number of important contemporary approaches to theoretical physics: see e.g. developments in category theory applied to quantum field theory). His negative verdict here is partial, and possibly influenced by an idiosyncratic understanding of physics. Notice that axiomatic approaches to theories are, in reality, attempts at attaining technical and conceptual clarity and can be very fruitful, leading to new insights. For instance, Artigas's discussion of realism, reference, and semantic aspects of theories seems blissfully unaware of two important problems: (a) the problem of categoricity of formal models (originating, in its simplest form, in the Löwenheim-Skolem theorem in first-order logic, giving rise to non-intended or non-standard interpretations of the same formal structures), and (b) the under-determination of theory by data (which Artigas briefly mentions, but puts to no use, in connection with Duhem): discussions which, especially (a), can only be had once a theory has been axiomatised, or at least formulated on sufficient level of rigour.

Third, and perhaps more importantly, Artigas's discussion of the analytic literature is, in my opinion, unnecessarily critical on the whole. Save, perhaps, the case of Popper, whom Artigas deeply admires, there seems to be nothing in logical positivism that might be of any use to Artigas. But I just gave examples of topics in logic and analytic epistemology which could have served Artigas well: the Gettier problem and its proposed solutions (including the contextual positions on truth), the problem of categoricity, and the problem of under-determination. Thus, though Artigas engages with analytic

philosophy, his engagement is rather one-sided: usually only aimed at criticism. This seems unproductive, also in light of the increasing prominence of analytic philosophy in Western philosophy, as evinced, for example, by the quote from Glock (2008) given in the Introduction.

Let me address an important general objection to the criticism in this Section: namely, the contention that Artigas does not attempt to achieve (is not interested in!) the kind of dialectical rigour which analytic philosophers do aim to achieve, and so that we should not judge him by such standards. But this objection is misguided: for being an ‘Aristotelian or Thomistic philosopher’ should not be an excuse for allowing vagueness and inconsistency in philosophy. From Plato’s Socrates to Aquinas, the best of the classical tradition has always aimed at conceptual clarity and rigour: and so, it is a mistake to think that clarity and precision are exclusively the demand of the analytic philosopher. The dialectical practices of the scholastic tradition may, in fact, be yet another bridge between classical and analytic philosophy!

Conclusion

In this essay I have explored Artigas’s engagement with analytic philosophy, especially in Artigas (2006). Artigas gives an original analysis of natural science based on scientific realism, and his conclusions seem, in several ways, to be in agreement with scientists’ intuitions about the truth of their theories and about how science describes nature. Artigas’s synthesis is articulated enough that it deserves further study. Thus I have argued that Artigas has the *potential for becoming* the ‘first philosopher’ of a philosophical school, on a specific conception of Whitehead’s.

But I also argued that, on Whitehead’s description of the first philosopher of the school, Artigas’s work, in its present state, does not yet satisfy reasonable requirements of clarity and precision: containing too much vagueness, it cannot yet be shown to be inconsistent.

First, a systematic work of clarification is needed. I analysed in detail several substantive problems of Artigas (2006) which, in my opinion, render several central aspects of this work rather inconclusive and unconvincing, by common standards in the philosophy of science.

Second, while Artigas (2006) is potentially interesting as a synthesis of a philosophical theory of natural science, the system lacks coherence: it is unclear why the system contains the elements it does, and not others. I illustrated this in the case of the notion of a ‘model’ (and there are many more examples): it is unclear what philosophical role models really fulfil within Artigas’s overall system: and it looks as if they could be eliminated without essential loss of information.

Third, the conceptual clarity of Artigas (2001, 2006) seems to be marred by Artigas’s undeniable erudition: specifically, by the large number of historical examples which he gives, and by the somewhat dubious use of the history of science in his argumentation: both as part of the (extended) definition of a concept, as well as a test case for the concept. As per the first point mentioned above, better conceptual definitions would be needed.

A further issue on which I have not touched upon in this essay, but which seems unavoidable, is that turning Artigas (2006) into a competitive construal of natural science will require developing its metaphysical underpinnings in conversation with analytic metaphysics and epistemology.

We can thus readily conclude from the three points above that Artigas’s acquaintance with analytic philosophy was incomplete and somewhat one-sided. He engaged in a debate with analytic philosophers but, ultimately, he did not himself apply the analytic methods, as one would expect from a work in philosophy of science. The result is unfinished work: a premature synthesis laid down as an interesting set of ideas, many of which can probably be rendered true upon suitable development, but which are not specific enough for Artigas to qualify, at this time, as the first philosopher of the school, in Whitehead’s sense.

That the above criticism is meant in praise of Artigas, follows from the general principle that only that which is highly valuable is worth criticising in some length, and in print. Artigas's system, if further developed, could potentially be very fruitful. Further work by other philosophers is needed in order to finish what Artigas only began.

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