

MALTHUS AND RICARDO: TWO STYLES FOR ECONOMIC THEORY

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Abstract

We examine the most famous controversy between economists as a means of shedding fresh light on the current debate about economic methodology. By focusing on the controversy as the primary unit of analysis, we show how methodological considerations are but one of a whole set of *stratagems* strategically employed by each opponent. We argue that each opponent's preference for a particular kind of stratagems expresses his own specific *scientific style* (within the general scientific and cultural style of an age). We also describe a *dynamic dimension* of the controversy, independent of the participants' intentions. Such a dimension is analysed in a "cycle" of the controversy, which begins with a well-defined issue and expands to additional topics, without reaching a "solution" to the initial issue. The definition and re-definition of the issue(s) at stake and of the difference between both participants is an essential and recurrent feature of such cycles; the conclusion of a cycle does not imply a real "closure" of the controversy, but only that each opponent has reached a satisfying degree of self-clarification. The controversy, thus, does not yield *persuasion* -- its ostensible aim. Rather, its "benefit" seems to lie in an unintended result -- clarification and deepening of contrasting approaches to the discipline -- due to its peculiar dynamics. In so far as the history of a discipline requires a reconstruction of such contrasts, it is indispensable for it to take into account the controversies where they emerge, and to view both the positive doctrines and the methodological posture of the contenders as parts of a wider framework, that is a scientific style.

Introduction

In this paper we reconstruct a famous controversy in the history of economics in order to raise a few questions: (i) How could two classical political economists, that is, two social scientists who shared the same paradigm, disagree in a systematic and sustained way? (ii) Did the disagreement depend on something different from a contrast between truth and error, or science and prejudice? (iii) And, if the disagreement did not simply depend on the fact that one of them was all the time right and the other all the time wrong, how could it be compatible with 'science', unless the social sciences should be considered to be one more "field of endless struggles"?

Roughly, our answers to these questions are:

(i) Both Malthus and Ricardo were typically classical economists, i.e., they shared a 'paradigm' in both of Kuhn's senses: they shared a precomprehension of the field of social phenomena they set about studying, a body of positive claims, an 'exemplar' of economic theorizing, and a disciplinary matrix, including a set of basic metaphors; accordingly they may be said to share a scientific style in the sense of an general attitude of an age that allows for the formulation and treatment of certain questions while ruling out questions of a different kind.

(ii) And yet, even within this broad shared background, there was room for deep differences: in the choice of emphasizing either *states* of equilibrium or *processes* of social change, in the methodological claims professed, in the strategies adopted; all this set of factors, when taken together, amounts to a difference in the specific scientific 'styles' of both authors, reflecting an alternative that social science seems to be unable to escape from: either formalism and abstraction or realism and complexity.

(iii) A scientific controversy is a kind of affair that cannot be settled by any algorithm or well-defined decision procedure for it involves, among other things, questioning the opponent's standards of problem solving.

(iv) Although scientific controversies have no decision procedure, they are not just a matter of taste or arbitrariness. To be sure, different styles lead to emphasizing opposite aspects of a shared paradigm, but the resulting emphasis cannot go as far as one likes, since it is under constraints posed by the controversy's 'demands'.

Such answers can be substantiated only through a detailed analysis of the actual text of a controversy that pays attention both to its text and context, to the several levels through which it unfolds, to the various strategies of argumentation employed by the participants and to the fact that it is necessary to go beyond either a purely semantic analysis or a purely genetic account. Some of this substantiation has been performed elsewhere (Cremaschi and Dascal 1996, 1998; Dascal and Cremaschi, forthcoming) and is partly summarized here.

Our specific objectives here are: (a) to show that the gamut of arguments, stratagems, and strategies chosen by a contender can be understood in terms of the umbrella notion of 'style'; (b) to argue that the difference in style provides something like a propelling engine for the controversy; and (c) to argue that, even if a style governs the different choices made by both contenders, these do not end in juxtaposed scientific works each responding to its own inner criteria of plausibility, since in a scientific

controversy styles are under constraints established by the controversy's demand and by the controversy's dynamics.

2. Text, co-text, and context

Scientific works such as Ricardo's and Malthus's published writings can only be understood within the context of debates within which they function as sets of moves and countermoves. From this point of view, we can observe, in the particular case under study, significant interrelations between (i) positive claims in economic theory, (ii) methodological stances, understood as explicitly formulated (even if not ready-made) bodies of rules for the formulation of such claims, and (iii) choices of strategies, understood as different heuristics governing the conduct of the debate by each contender. It is our contention that interactions of this kind, which shape the content of a theory, can be detected and properly analysed only when the scientific works presenting such a theory are not considered as floating in a void, but as embedded in their polemical 'natural setting'.

For analytical purposes, it is convenient to distinguish between the primary text of a controversy, its secondary text, and its context. The primary text consists of all the records of utterances more or less explicitly addressed by each of the contenders to the other and mutually known. Its particular importance for understanding and interpreting the contenders' claims depends on the fact that the primary text is the immediate dialogical (or dialectical) context where such claims are made, as direct reactions to the demands created by the opponent's intervention. Just as we cannot understand a 'yes' without knowing the question that precedes it, so too we cannot fully appreciate any piece of a contribution to a controversy without relating it dialectically to other pieces.

The secondary text or co-text includes all texts by the author himself or by other authors which influenced the performance of the linguistic acts that make the primary text. In a narrower sense, it includes the records of all sorts of 'meta-controversy' statements, i.e., the statements by the author himself or by others either on the subject of the controversy or on its status, or on controversies in general.

On Malthus's side a co-text in a narrower sense is scarcely documented. In the broader sense it includes those of his writings not available to Ricardo, e.g., his manuscript lecture-notes on Adam Smith and his early unpublished political pamphlet, *The Crisis*, and also works by other authors with whom Malthus was conversant -- primarily William Paley, the Cambridge theologian and whiggish political theorist who was the proponent of a version of theological utilitarianism -- that included a more explicit statement of theological and political views shared by Malthus which were never spelled out in full detail in his exchange with Ricardo.

On Ricardo's side a narrow co-text is well-documented: it includes several letters to and from James Mill and John Ramsey McCulloch with comments on his discussions

with Malthus. Ricardo's co-text at large includes: first, Thomas Belsham's *Elements*, where he first studied logic and the philosophy of mind; secondly, those writings through which James Mill tried to impart him a political education, primarily (with Mill's usual lack of modesty) his own *History of India*; thirdly, the philosophical works he read partly following Mill's advice and partly following his own inclinations, that is, Locke, Hume, Reid, and particularly the sceptic Pierre Bayle (See Cremaschi and Dascal 1996).

The context comprises, first, those macro-events in the world surrounding Malthus and Ricardo during the second and third decades of the nineteenth century which occasioned or influenced the controversy. The first component of this macro-context is the industrial revolution; the second is the shifting political scenario of Britain towards the end of the Napoleonic wars, requiring a redefinition of political allegiances, including those of Malthus and Ricardo. In addition to that, there are more specific events that are more or less directly reflected in the controversy. Among these, of particular importance were the debates of the time on matters of economic and social policies: The Corn Laws and the Poor Laws. Economic policies were debated not only in parliament, but first of all in influential British journals such as the *Edinburgh Review*, which were the channels through which opposed parties tried to influence the British public opinion; these debates at a certain stage became the immediate context and co-text of the controversy between Malthus and Ricardo, when the latter became partially a public controversy, and unavoidably also a part of those wider disputes (see Cremaschi and Dascal 1998).

3. The unfolding of the controversy

Malthus and Ricardo first met in June 1811, at a time when the former was the well-known author of the *Essay on Population*, while the latter had just started to publish articles on monetary policy in newspapers. In their first to be "the very few objections" which prevented them "from being precisely of the same opinion" (to Malthus 18 June 1811. In Ricardo, 1951-1973. VI: 23-24). In 1817 Ricardo published the first edition of his *Principles*. From this point onwards, the scope of their disagreement grew wider, and the controversy partly shifted to published writings, thus becoming a public controversy. Malthus's *Principles* of 1820 included a rejoinder to Ricardo, and Ricardo in his turn responded with his *Notes on Malthus* (originally intended for publication, even if left unpublished on James Mill's advice). The co-text contains echoes of the controversy at this stage, e.g. in Ricardo's correspondence with third parties.

The flow of written communication between them slowed down in this phase; one possible reason for this is the fact that the controversy had shifted to the public ground. Another possible reason is that they had an opportunity to meet regularly in order to discuss economic issues at the monthly dinners of the Political Economy Club in London, which were attended regularly also by Malthus even if he was in residence at Haileybury College. The most remarkable feature of this phase is that Ricardo, then working at the third edition of the *Principles*, introduced substantive modifications in his labour value theory as a result of Malthus's objections. This is one of the most noticeable effects of the controversy on published works by either, although Ricardo,

even though he modified in a decisive way his views on value (he systematically replaced "solely" by "almost exclusively" in all sentences about labour as the determinant of value) still claimed victory (a typical controversialist's attitude).

In the last months of Ricardo's life, from April 1823, the correspondence had a new start, confined to the issue of the measure of value. In this phase Ricardo resorted to a different tactics: at some point he acknowledged his own defeat and tried to extract a similar acknowledgment from Malthus. The controversy was brought to an end by Ricardo's death in September 1823¹. Its traces in the second posthumous edition of Malthus's *Principles* are not easy to assess. There are indeed a number of important deletions (without substitution) of passages attacked in Ricardo's *Notes on Malthus*, e.g. the pages on the productivity of labour, but, since the editing was carried out by John Cazenove on the basis of Malthus's manuscript notes, no final conclusion can be reached on how far Malthus intended to take Ricardo's objections into account.

We propose to single out nine cycles in the controversy, each consisting of a set of letters and publications. Each cycle is triggered by a more or less specific topic and can be identified by certain features such as a pattern of intensification in the frequency of the letters exchanged and in the number of topics discussed in the various phases of the cycle. Although each cycle is typically under the aegis of one topic -- almost in all cases a point of positive doctrine in economic theory -- in most cycles the discussion of positive claims is accompanied by discussion of methodological and meta-scientific issues. For convenience, we identify the cycles in terms of their main topics:

1. On the influence of the currency upon foreign exchanges and the possibility of a "general glut" (June 1811 - February 1812).
2. On corn laws (August 1813 - January 1815).
3. On rent (February 1815 - April 1815).
4. On the growth of wealth, multicausality, temporary and permanent effects (April 1815 - January 1817).
5. On wages and profits (1817-1820).
6. On the determinants of value (1817-1820)².
7. On language and definitions, theory and practice, the 'value-free' character of political economy (May 1820 - November 1820).

8. On the determinants of value in a pre-capitalist economy and on machinery (1821)³.

9. On the possibility of an invariable measure of value and on various proposals for the best measure (April 1823 - August 1823).

4. Methodological and meta-scientific divergences

It is noteworthy that, throughout the controversy, a number of methodological and meta-scientific divergences between the two contenders recur and are invoked in sustaining this or that particular positive claim. In general, one contender's allegiance to one methodological position does not imply that he will uniformly apply it. Nevertheless, there is sometimes an elective affinity between methodological statements and positive claims, although occasionally the same methodological statements are used for opposite purposes.

The first issue on which there is permanent disagreement is the definition of political economy: for Ricardo, it is the science of the "laws" regulating distribution; for Malthus, it is the science of the "causes" of the growth of wealth. Presumably, this difference can be traced to deeper epistemological commitments. Malthus sticks to Adam Smith's usage -- a choice that is associated with his adhesion to the semi-inductivist methodology deriving from Dugald Stewart's epistemology of belief. Ricardo, on the other hand, had in mind a concept of law not in the Physiocrats' sense of a "natural and essential" order of society, but as expressing mathematical relations; for him such laws are probably to be contrasted with the (unknowable) causes of those states of affairs for which the laws hold.

The second recurrent issue is mono- vs multi-causality. Here Malthus stresses: (i) the importance of multiple interacting causes; (ii) the importance of temporary, as contrasted with permanent, causes; (iii) the existence of final causes as preconditions for effectual demand. Ricardo on the contrary argues that there is generally one prevailing cause, whereas the others may, at least *prima facie*, be ignored; hence, there are a few permanent causes, which sooner or later generate permanent states.

The third source of disagreement of this kind is the distinction between questions of fact and questions of principle, or the issue of "strong cases". Ricardo sees economic science as the construction of abstract models depicting ideal *states* of equilibrium; Malthus seems to believe in a semi-historical character of political economy, whose subject matter consists primarily of *processes*, where disequilibrium is a rule, and equilibrium the exception.

The fourth point is the oversimplification issue. While Malthus insists on the need to keep complexity in mind, Ricardo favours simplification. His reasons are the same that lead him to favour abstraction and mono-causality, namely alleged impossibility of *keeping* so much complexity *in mind*, as well as the alleged *unknowability* of the

complex causes at work.

The fifth is the issue of vagueness vs precision of language. Each of them accuses the other of inconsistency in language usage. Presumably both appeal to shared standards of linguistic propriety. However, whereas Ricardo constantly *presumes* that proper use of language is tantamount to *precision* and *univocity*, Malthus always *presumes* that propriety is tantamount to *explicit definitions* and conformity to the "usage of the best educated part of society".

5. Beyond semantic analysis

Usually, the history of economic thought focuses on the contents of the theories and contrasts them on the basis of divergences regarding their positive claims. At best, some attention is paid to the methodological and meta-scientific divergences as well, and an attempt is made to show how they relate to the differences in doctrines. There are good 'semantic' reconstructions of the Malthus-Ricardo controversy, and we have ourselves (Cremaschi and Dascal 1996) reconstructed its methodological aspects along the lines summarized above.

The shortcoming of such semantic analyses is that they either present us with a kaleidoscope of opposite positions without explaining why the opponents persisted in holding them in spite of each other's criticisms, or else, in seeking to present the coherence of each view, they overlook the fact that such a coherence is not only the unfolding of an "inner" logic but is, to a large extent, the result of having to cope with the opponent's criticism. Consider, for example, the omnipresent appeal to methodological considerations. It seems that it is the compelling need to defend one's positions and to criticize those of the opponent -- rather than any 'substantive' or 'inner' requirement of the thesis defended -- that is responsible, sometimes, for shifting the discussion to a meta-level of methodological principles. Thus, appeals to methodology often function as just one *argumentative strategy* among others -- an observation that may well account for their somewhat puzzling uses. And these are not the only 'strategical' factors that influence the development of a controversy.

Seen in this broader perspective, a controversy appears not just as an arena where one can observe how 'truth' finds its way against 'error'. Otherwise, only the stubbornness or utter irrationality of one of the participants could be invoked to explain why he doesn't simply acknowledge his mistake, at some point. The study of controversies reveals that the construction of scientific truth involves modes of interaction and argumentation that cannot be reduced, in a simplistic way, to their logico-semantic aspects -- modes that shape not only its 'form' but also its 'contents'.

It is the role and importance of some of these other, so far unattended to, aspects of the Malthus-Ricardo controversy, rather than its well-known semantic features, that we will highlight in what follows. For the sake of concreteness, we will first

single out one of the *cycles* of the controversy and describe its *sequential structure*. We will then apply to the analysis of this cycle some of the *stratagems* we have identified as typically employed by the contenders throughout the controversy, thereby showing their strategic use. After this piece of analysis of *intentional* factors, we will muse about the presumably *non-intentional* factors that manifest themselves in the peculiarities of the *dynamic* unfolding of this and (in all likelihood) other cycles of the controversy. Finally, we will venture into some generalizations concerning the different *styles* of the contenders and the role of these styles both in their modes of argumentation and in the contents of their doctrines. We will, thus, move from a micro- to a macro-level of analysis. No need to say that our analysis of what we have called the 'non-semantic' aspects of the controversy does not purport to be exhaustive.

6. Sequential structure of a cycle in the controversy

We propose to define a cycle in the controversy as a period of comparatively intense and continuous correspondence, usually triggered by some external event, such as a relevant publication by one of the contenders, or some relevant public event. We will illustrate our claims on the way cycles develop and come to an end by reference to the second cycle in our controversy: the discussion that took place between 1813 and 1814 on the foreseeable consequences of a proposed modification of the Corn Laws⁴.

Let us give some general information on the background of the set of letters we are going to examine. The occasion for the beginning of a discussion on foreign commerce in Summer 1813 was given by an external event, the proposal of modifying the Corn Laws. This topic is the second one taken over in the course of the controversy: the first one had been the influence of the currency upon the foreign trade, and it had fuelled the discussion from June 1811 to at least February 1812. Significantly, here as in other phases of the controversy, an external event creates an occasion for a renewal of the discussion: the choice of the new topic depends, no less than the previous topic of currency, by current public discussion. The Corn Laws were the occasion for a bitter polemic between supporters of agricultural protectionism and upholders of free trade. During the Napoleonic wars, the resumption in Britain of a policy of heavy corn duties (that had been abandoned in the period between 1774 and 1791) combined with the impossibility of trade with the Continent plus dependence on uncertain British climate, resulted in price fluctuations ruinous both to the poor and to the manufacturers. With the coming of peace, a fall in the corn prices was inevitable, with the side-effects of either a great reduction of agricultural rents or the ruin of tenant farmers. The Parliament of 1814, dominated by the landed interest, prohibited the import of corn unless the price exceed a certain (rather high) level.

To this debate Malthus himself contributed his *Observations on the Effects of the Corn Laws* of 1814. From direct references in Ricardo's letter of 26 June 1814 we understand that this pamphlet was the starting-point for a phase of close confrontation

between Malthus and Ricardo. In the *Observations*, Malthus declares he is convinced of the soundness of the case for free trade in general, and indeed he thinks that Adam Smith was wrong on the particular point of a bounty on the exportation of corn, in so far as he had left entirely in the background "the broad, grand, and almost unanswerable arguments which the general principles of political economy furnish in abundance against all system of bounties" (Malthus, 1814, p. 87). On principle, corn is not such a special case as to allow for an exception to the laws which regulate the markets for all other commodities; yet, he adds, a number of considerations might be produced, in favour or against some kind of protectionist measure, verging not on matters of pure economic efficiency but on mixed issues such national security, national "quiet and happiness", and the welfare of labouring classes. His final advice was to "delay any final regulation" (Malthus, 1814, p. 109).

Two remarks are in order here. First, Malthus does not believe in 'purely' economic arguments, or better, he believes that arguments perfectly convincing in theory in favour of, say, free trade, can never be applied as such to practice, since you never face problems of economic efficiency in a void; political economy is a part of a wider political and moral science, since "wealth, population and power are, after all, only valuable, as they tend to improve, increase, and secure the mass of human virtue and happiness" (Malthus, 1814, p. 102). Second, Malthus typically resorts to the device of piling up reasons that incline one to believe, showing that no absolutely compelling argument may be produced, and consequences may be drawn which may even be opposed to the most firmly established principles (provided that these are principles of theory, not of practice). He puts forth in this connection one of his most peculiar methodological principles:

Many of the questions, both in morals and politics, seem to be of the nature of the problems *de maximis et minimis* in Fluxions; in which there is always a point where a certain effect is the greatest, while on either side of this point it gradually diminishes (Malthus, 1814, p. 102).

It is worth noting that this, besides being a methodological principle, establishing limits to knowledge, is also a meta-principle for the controversy. It amounts to saying: please, do not attempt to provide a conclusive proof that some factor is good or evil, effective or ineffective, since any factor may be such to a point; rather, try to provide reasons to believe that this or that is the point where its effect is the greatest.

The discussion between Malthus and Ricardo, starting with the Corn Laws, soon included more and more side-issues, reaching finally the overarching topic of the causes of the growth of wealth. Discussion of causality became soon the distinctive methodological topic in this cycle. The distinction between effects and causes, as well as that between "necessary" and "probable" connections or between "natural and necessary" vs "accidental" causes, as well as the opposition of mono- and pluri-causality come to the fore more than once. Other key-notions appear, as the ground of the confrontation keep expanding: the idea of "tendency", the notion of "final cause", the issue of the limits to effectual demand vs the unlimited character of the "wants and tastes". The discussion between Malthus and Ricardo, starting with the Corn Laws, soon included more and more side-issues, reaching finally the overarching topic of the causes of the growth of wealth. Discussion of causality became soon the distinctive

methodological topic in this cycle. The distinction between effects and causes, as well as that between "necessary" and "probable" connections or between "natural and necessary" vs "accidental" causes, as well as the of mankind. At one point, the controversy reaches the ground of meta-principles, namely, the distinction between the "truth" and the "utility" of a "principle", a new version of Ricardo's distinction between "questions of fact" and "questions of science" (to Malthus 18 December 1814. In Ricardo 1951-73, VI: 163).

After the cluster of issues raised keeps expanding for a while, with a snow-ball effect, towards the end of 1814 side-issues start to be gradually dropped and both contenders seem to lose interest in the discussion, limiting themselves to formulating proposals for a re-definition of the point of real dissent, till (in February 1815) the discussion has a fresh start on a new topic, rent. The new start is occasioned this time (not unlike the start of the cycle analysed) by a publication by Malthus, *On the Nature and Progress of Rent*.

7. Strategies

In this section we will talk of *stratagems* in the sense of devious moves involving some kind of deception. Stratagems are often ascribed to rhetoric, in the derogatory sense of the word, but as we use the term, they are understood as having a cognitive dimension. We will talk also of *strategies*, both in the everyday-language sense of *global* lines of conduct directed at reaching some goal and in the sense of game theory, according to which a strategy is a sequence of choices made under conditions of uncertainty, given unpredictable choices by other players in the game. In fact, in any controversy, since the discussion is carried out *in the course of time* and any choice of reasons has to be made *before* all the possible pro and con reasons have been explored, and since few (if any) proofs are of the 'necessitating' kind -- most of them being of the 'inclining' kind --, each contender has to choose from a number of available 'legitimate' moves which he considers effective at a given point of the debate, which he presumably would not use at some other point.

We have described elsewhere several stratagems at work in the controversy between Malthus and Ricardo, including the *tu quoque* argument, the *argumentum ad verecundiam*, complaint of misunderstanding, retreating to safer ground, insulating moves, resorting to methodology, escaping either to idealization/abstraction or to realism/complexity (Dascal and Cremaschi forthcoming).

The fact that both contenders constantly use such stratagems, instead of restricting themselves to the more sober tools of logical entailment or empirical verification/falsification need not shock the reader: after all, in the course of the controversy Malthus and Ricardo had to provide pertinent answers to objections in real time (often they corresponded daily) *before* all possible arguments had been duly examined, and their motivations for engaging in the controversy went beyond pure 'love

of truth' as described in scientific hagiography without lapsing into fraud and manipulation; among these there was a desire to 'win', a desire to improve one's arguments through the exercise of criticism and counter-criticism, a desire of establishing and/or maintaining a reputation. Nevertheless, however devious, the stratagems chosen by a contender must at least bear the appearance of conformity with the implicit norms of the argumentative game.

Each contender resorts at least on some occasion to all the gamut of stratagems available. Yet, he has recourse to each of them more or less frequently according to preferences he apparently has. For example, each resorts to methodological considerations from time to time (even if Malthus does so more often than Ricardo), and tends to introduce his preferred methodological arguments. However, the very fact of shifting to methodological considerations and the use which is made of these considerations are a contingent and unpredictable matter. Nevertheless, we may say that the contenders' choice of moves and stratagems corresponds, to some extent, to 'strategic action' in the sense of a concatenated set of moves designed to achieve a number of major objectives. A strategy in this sense is different from a wholly explicitly formulated set of objectives and rules to be applied in the production of theories (a theorizing machine). Hence it does not determine whether, when, and how the contender will adopt a stratagem. But a strategy is also different from a style in the artistic or literary sense (although we are going to talk below of 'scientific styles') in that the contender is much less free than the artist or the poet to do whatever he likes. A strategy is a set of objectives and heuristic principles, possibly not entirely explicit, which guides the choices to be made in the face of objections. This attitude has links of elective affinity with a constellation of factors of which the contender's specific scientific style consists: his methodology, the kind of positive doctrines he adopts, his philosophical, theological, and political background. Yet, no deductive link exists between the contender's scientific style and the strategies he adopts.

8. The dynamics of the controversy

Whereas the choice of strategies and stratagems is clearly an intentional matter, there are a number of regularities in the cycle that seem to be independent from the participants' intentions. If one examines a cycle in the controversy, say the cycle on the Corn Laws in the second half of 1813 (See Dascal and Cremaschi forthcoming), a few peculiar features may be easily detected: one is that, if we consider the cycle from the viewpoint of the frequency of the correspondence and of the number of the issues raised, we see it as endowed with a peculiar shape. This shape results from the facts that: (i) correspondence intensifies in terms of time span on the middle of the cycle; (ii) it also intensifies in terms of the number of new topics (the dispute "spreads" since, apparently, it is perceived as useful); (iii) it also intensifies in terms of the participants coming closer to what they see as the main issue; (iv) at the beginning, the topic is the one prompted by the external event, but this retains its interest only up to the middle.

Other interesting features concern the closure of the cycle. From its middle, Malthus and Ricardo first manifest awareness that they are not settling the issue that way, and another way is needed; it is at this point that the proposal to settle the issue by talking it over is made by both (to Malthus 30 Aug. 1814, In Ricardo, 1951-1973. VI: p. 128; to Ricardo, 11 Sept. 1814, *Ibid.*, p.131); and at the same time they manifest the belief that they do not differ too much on the main issue; curiously enough, after short, they realize they differ a lot; they begin to slow down the correspondence (and Malthus explicitly suggests to do so); they begin to silence topics; they finally come closer to agreement on the definition of the ground of disagreement, namely the permanent vs temporary issue, and seem to be content with having grasped fairly well what the other means and where the difference between them lies.

Keeping these curious features in mind, one might muse that what is apparently achieved through a cycle of the controversy is not persuasion nor victory, nor solution of any problem; indeed, the end of the cycle is marked by loss of interest by both; what is achieved is instead *understanding*. That is to say: (i) understanding of the other's positions; (ii) understanding of one's own positions in the light of objections; (iii) understanding of the range of possibilities of interpretation of the concepts used.

The pattern followed in this (half-aware) path to understanding is:

A. they begin discussing a positive issue introduced by an external event.

B. Each states his view; they understand that they cannot agree; they realize that they don't agree because of some underlying presupposition, for ex. a methodological view which they don't share (the permanent vs temporary issue).

C. Then they begin again to discuss positive issues (what precisely is permanent and what is not); they are looking for an inductive way (by means of examples) of solving this underlying issue.

D. But this proves not to be sufficient, since concepts remain obscure; and they finally drop the cluster of topics under discussion while realizing that they would need further clarification, but they have clarified at least something.

Considered as a whole, the cycle displays some revealing characteristics of each of the disputers: (a) the recurrence of "Yes-But" clauses in Ricardo's letters; this is the mark that he is taking the position of the 'defendant'; (b) the way Ricardo deals with factual refutations, namely by addition of facts in order to deflate Malthus's objections (were such additions, in Lakatos's words, "progressive" or were they merely ad hoc?); (c) use of repetition (of previously made admissions, of disclaimers); (d) recurrent attempts to define "what is our present question"; (e) reinterpretation moves; (f) shifts in the argument direction by use of such clauses as "quite" and "almost" that play the role of suggesting that a given factor holds in almost 100% cases, and therefore may be assumed to hold generally, or that it does not hold in a certain percentage of cases, and

accordingly does not hold generally; (g) silences, either on aspects that are not useful, or even dangerous, from one's point of view, or on issues one wishes to drop, since they do not appear any more useful in order to clarify what is perceived as the "present" question; (h) use of clauses such as "we agree on" in order to obtain some concession from the other party, or to draw limits to the ground of the define "what is our present question"; (e) reinterpretation moves; (f) shifts in the argument direction by use of such clauses as "quite" and "almost" that play the role of suggesting that a given factor holds in almost 100% cases, and therefore may be assumed to hold generally, or that it does not hold in a certain percentage of cases, and accordingly does not hold generally; (g) silences, either on aspects that are not useful, or even dangerous, dispute.

One is entitled to ask what is, after all, the real difference between them, as expressed in this cycle. This is itself an important point in the discussion since, after Malthus's *Observations*, some time is spent in trying precisely to clarify what position each of them is defending. Ricardo's proposal for an assessment is that the (real) issue between them is whether there is any causal relationship between restrictions on importation of corn and reduction of capital (to Malthus 25 July 1814. In Ricardo 1951-73, VI: 113-14). At the end of the cycle Ricardo proposes a re-definition of the issue, namely whether "the rate of profits can... permanently rise unless capital be withdrawn from the land" (to Malthus 18 Dec. 1814. In Ricardo 1951-73, VI: 163). To this Malthus has little to object; in fact, he restricts his objection to a terminological issue: we "should explain -- he suggests -- what we mean by permanently", and he adds that all that he contends for is "that a period of some duration may occur (20 years for instance) when the profits of commerce will take the lead, and regulate the profits of agriculture" (to Ricardo 29 Dec 1814. In Ricardo 1951-73, VI: 167). Ricardo has the final word, and curiously enough he further restricts the topic. He says: "I thought you maintained... I now understand you to say" that the profits of commerce will regulate the profits of agriculture for a period, possibly of 20 years; and the only thing he has to object concerns the length of such a period, that he would limit to "about 4 or 5" years (to Malthus 13 Jan. 1815. In Ricardo 1951-73, VI: 170).

What in the beginning appeared to be a grand theoretical question, with a sequel of contingent predictions, and occasioning charges of inconsistency or of having shifted position, turns out at the end to be just a difference in degree, concerning magnitudes in a model that both now accept. Both seem to have lost any interest in discussing the remaining difference and in convincing the other; they seem to rest content with having better understood the difference between themselves, or with having better understood the issue under discussion. Nobody has won, nobody has persuaded the other, nobody has solved any problem, and yet everybody is happy.

We noted that the key-word in the concluding phase of the cycle is the word "understanding": we have already quoted Ricardo saying "I thought you maintained... I now understand". So, does the cycle end with a *rapprochement* of their positions? Our answer is "No". The difference is there all the time, and it underlies the final tiny difference on which magnitudes should be inserted into the shared model. The difference in fact is the one which persists along the whole course of the controversy, and it may come to the fore precisely because a quite remarkable ground is shared (the classical model of rent and profit). A difference on what is permanent and what is temporary, and on how long the intervals between permanent states of equilibrium may

be, is both a marginal difference and something that reflects overarching deep differences in the comprehension of economic theory's goals, method, and style.

The interesting question we are trying to answer here arises from an examination of both this individual cycle and the controversy as a whole. The question is: why did two political economists who were outstanding minds, honest persons, gentlemen, and friends at once differ so much and so little, and why did they keep an interest in their controversy for so long time, even if the its 'results' were apparently so disappointing? Or, what was the difference that made a difference between them?

Our answer is: it was a difference in *scientific style*; this was the source of endless disagreement but also the controversy's unmoved motor.

9. Intermediate considerations on scientific styles

The notion of style has now been popular for some time with historians and philosophers of science -- let us mention Alastair Crombie, and Ian Hacking. We believe too that this notion may be useful for historians of science no less than for historians of art, but also that, in order to be of real use, it should be made more precise.

The word style is taken from classical rhetoric, where it appeared occasionally with Cicero and Quintilian. An author's *stylus*, i.e. his pencil, was his own manner of writing, and was thought to belong to *elocutio*, one of the four parts of discourse. Not earlier than in the late sixteenth century the term started being applied also to the visual arts (Gombrich 1968). An eighteenth-century innovation was that of ascribing style to another part of discourse, *inventio*, and a related innovation was that made by Georges Buffon by describing style in terms of a harmony between ideas: "le style n'est que l'ordre et le mouvement qu'on met dans ses pensées" (*Discours sur le style*, 1753), whose side effect is the harmony of words. An even more decisive innovation was introduced by Johann Joachim Winckelmann and consecrated by Hegel's aesthetics, by talking of style in terms of a constellation of traits that is the mark of an epoch in the history of art (See Le Guern 1991).

Among nineteenth- and twentieth-century aestheticians and historians of art the notion of style became commonplace, understood in a sense somewhat wider than Hegel's, as that feature or set of features which mark the production of an individual author, school or epoch, and defined as the relationship of *form* and *content* (Moulin 1991); the implied suggestion was that a style results from an artist's conscious choice among alternative forms with which a given content may be brought into relationship (as still contended in Gombrich 1968).

The first notion of a 'style of thinking' in the history of science dates back probably to Ludwick Fleck [1935], even if the idea of a style of thinking, understood in a loosely Hegelian sense, could be found in authors as different as Husserl and Collingwood. Fleck introduced the notion of a *Denkstyl* as a means of accounting for the circularity between theories and facts combining the insights of the Vienna Circle with those of the sociology of knowledge. According to Fleck, a system of belief, normally shared by a "thought collective", has its own kind of inner unity, which is not of the same

character as the unity of a logical system, depending on a coherence of "style": style is what grants harmony to the system, what makes for admissible and inadmissible beliefs. And science is different from myth precisely because of its style: science endeavours to accommodate into its own system as many "passive" elements of thought as possible, or as many empirical constraints as possible, without caring too much for the degree of clarity of the overall framework; myth encapsulates very few "passive" elements, but does so "in an artistic manner".

Crombie and Hacking have recently revived a Hegelian notion of style as a proxy for Kuhn's 'paradigms'. A style is a set of shared commitments concerning existence, knowledge, and their meaning: commitments to "conceptions of nature within the general scheme of existence and of its knowability to man", in turn conditioned by language (Crombie 1994, I: 56-57), "to a conception of science and of the organization of scientific inquiry, argument and explanation" (*Ibid.*, p. 59), to "conceptions of what is desirable and possible in view of evaluations of the nature, purpose and circumstances of human life" (*Ibid.*, pp. 61-62) are what conditions the scientific thinking to be found in a particular period or society. For Hacking, a "style of reasoning" is a holistic set of procedures and ways of arguing that marks an epoch of the history of science and establishes boundary conditions for sentences to become viable truth-candidates; styles may evolve or be abandoned, but are "curiously immune to anything akin to refutation... it is the content of what we find out, not how we find out, that is refuted" (Hacking 1982, p.57; 1992, p. 20).

We believe that Hacking is right when he says that "the very word 'style' is suspect" and that "art critics and historians have not evolved a uniform connotation for the word" (1982, p. 51). In fact, what even Crombie's and his own use of the word does is pointing at a problem more than presenting a solution. What we need as a solution is a theory of the way in which the given is re-described (or sentences are transformed into candidates for truth). The most ambitious attempt to work out a comprehensive theory of style solving these questions has been made by Gilles-Gaston Granger. In his view, style is a feature of every human artefact, not a feature of systems of symbols as such, but of "the *use* of symbolism" or of the process through which form and content are brought into mutual relationship in every kind of practice, be it technical, artistic, or scientific (Granger 1968, p. 10). This is why a study of the logical syntax of scientific theories, on which traditional philosophy of science had concentrated, should be integrated with a complementary approach, namely a study of their style, similar to the one traditionally carried out for artistic works.

Style, in Granger's definition, is "the way of incorporating an individual element into a wider process", and such a process is a kind of 'work', "that is an aspect of every kind of practice" (Granger 1968, p. 8). Style has to do with the fact of the polysemicity of untutored experience: the given, as opposed to those "structures" that are established through scientific conceptualization (See Granger [1967], ch. 7), bears redundancy or overdetermination; in so far as science *qua* practice comes to bear on the individual, and "in so far as certain regularities may be detected in its treatment of experience", also in science there is a *style*" (Granger 1968, p. 8). In other words, style governs the relationship between the given and the model into which it is 'translated' by scientific conceptualization; style is a key for sorting out of the given certain traits that will be combined into a picture of one domain. And also -- as Goodman later repeats -- the

received view that understands style in terms of opposition of form and content is too narrow: in so far as an analysis of style makes any sense "the redundant elements of the given interpreted by science are not an absolute content or an irreducible and formless matter" (*Ibid.*)

Furthermore, style constitutes a kind of 'apriori' for practice: an author takes a number of rules, out of the gamut his civilization puts at her/his disposal, as an apriori for the activity into which s/he is engaging; these function as the rules of a game which are constitutive of the game itself (with the proviso that scientific practice undergoes a number of constraints unknown to games understood as playful activities).

Thus, a discipline, an epoch, and an individual scientist all of them have a peculiar style. Individual scientists may even be supposed to have a "character", analogous to the characters studied by psychologists. But scientific 'characters' (think of Poincaré's distinction between "intuitive" and "calculating" mathematicians), should not be understood in terms of individual psychologies *determining* scientific behaviour: each trait of a 'character' should not be understood as a *force* among other forces, and the character as a whole as an *equilibrium* of forces; instead a character should be conceived in terms of the structure of one language, whose existence is not tantamount to the physical production of fragments of the spoken language (*Ibid.*, p. 14).

In a word, stylistic analysis focuses the interrelation between the *form* and what is organized by the form itself; it highlights the contingent character of "structures" described by theories, as well as the plurality of possible alternative structures; it highlights "style effects" (*Ibid.*, p. 299) carried by each particular relationship between *model* and *phenomenon*. And particularly in the social sciences the wealth of meanings conveyed by ordinary language carries a number of unreflective interpretations of the given that call in an even more decisive way for stylistic analysis (*Ibid.*, p. 298). Granger adds that in the social sciences, across the different styles of various phases and schools, a permanent tension persists between the opposed polarities of "rigor" and "exactness"; "rigor" tends to reduce the terms which appear in theories to "empty symbols"; exactness tends to preserve some "semantic reference" for these terms, even if, as theories become less abstract, "the rigor of their logical structure seems to be obfuscated". Thus, in economics, for example, "what is won in terms of exactness, goes lost in terms of rigor, and what is won in terms of rigor goes lost in terms of exactness" (Granger 1959, p. 103).

Nelson Goodman (1978), without apparently being aware of Granger's contribution, has added a few comments that point roughly at the same direction travelled by Granger. He notes too that the distinction form vs content cuts across the distinction style vs non-style; that a style does not depend on an author's intentions; that style is not only a question of *how* but also of *what*; that it refers to what and how a work symbolizes; and accordingly that a style is a feature only of "artefacts", or, in Granger's words, of products of some "work" (Goodman 1978, p. 36).

Let us come back now to McCloskey's discovery of style in economics. The first remark to make is that not only this discovery, as the discovery of rhetoric in general, has been somewhat delayed in comparison with analogous rediscoveries by philosophers of science, but also that it is based on a restricted notion of style, that of classical rhetoric. What McCloskey has to say is that also economic writing has a 'style', that is, an unreflective theory of writing, even if this particular style is hidden behind the scientific

tale of Reality speaking for herself. McCloskey attacks the scientific style in economic literature by comparing the latter with the modern novel, suggesting that the suppression of the authorial I in the latter responds to the same outlook as the suppression of the I in science, yielding "represented reality" or "unheralded assertion". Also the present dominating style in economic literature does depend on a particular theory of writing, and indeed on a theory based on strange commonplaces, like that style may be separated from content, or that *inventio*, that is, "the framing of arguments worth listening to", is all there is (McCloskey 1994, pp. 124-5).

The same point as McCloskey's had been done in more convincing way for biological prose by Gross (1990, pp. 69-84). In particular, Gross shows how current stylistic choices in biology conspire in smuggling dubious assumptions as obvious. The main point addressed by Gross in the naive realism shared by the scientific community based on an equivocation between social and linguistic practices and its hypostatization as a set of physical objects (pp. 81-82). In the case of Gross's reconstruction, it is more clear how the link between style in writing and overall style of thinking may be established: the effects of style (as denounced by, but not confined to, choices in the manner of writing) in biology consist in emphasis, deletion, supplementation or redescription (what Granger would call 'interpretation') of the given, in order to organize it in terms of one among several ontologies⁵.

10. Two scientific styles within one paradigm

10.1. *Both 'classical'*

When Marx introduced the term "classical", it was in order to draw a distinction between those economic writers who, like Petty, Adam Smith, and Ricardo, had contributed to the growth of knowledge and those who, starting precisely with Malthus, had degenerated into "vulgar" economists, being unable to draw any distinction between the subjective perception of economic phenomena by social actors and the objective structure of economic laws hidden behind those appearances. Curiously enough, the term "classical" was later adopted by historians of economic thought with a modified meaning, referring to all the economists between Adam Smith and Marshall. Once classicism is so redefined, the confrontation between Malthus and Ricardo loses its rationale of a holy war between science and ideology, and the question becomes: why did two authors sharing the same paradigm disagree in such a sustained way?

More recently, Würgler and Paglin have argued for a partly unclassical character of Malthus's approach on the basis of the emphasis it lays on processes and disequilibrium instead of static states of equilibrium, an emphasis that makes it resemble the German Historical School⁶. And yet, the basic model of the economy both start with is the same, namely Adam Smith's model with the addition of the Malthusian

population principle and of the theory of rent⁷. This does not mean that they don't differ; there are indeed differences between Malthus's and Ricardo's main works. Malthus, no less than Ricardo, accepts *The Wealth of Nations* as the paradigmatic work; he even insists that this work "is still of the very highest value" (Malthus [1820], I: 5) and that it would be pointless to oppose any new "consistent whole" to Adam Smith's doctrine in this phase, since a new theoretical synthesis might come, at most, as the final output of a phase of discussion in which what is true may be separated from what is false (*Ibid.*). In other words, Malthus believes that his contribution fits in a "paradigmatic" phase whose paradigm-constituting exemplar is *The Wealth of Nations*. Ricardo believes instead that there was one big puzzle to be solved, value theory, and that his own solution to the puzzle will give the paradigm more stability. Malthus, though a more fervent admirer of Adam Smith, believes that there is more than one puzzle to solve, and that the discussion should be kept open on all controversial points, while avoiding any rash attempt to synthesize a new "consistent whole".

Thus, some ambivalence may be detected in Malthus's attitude to *The Wealth of Nations*: the warmest admirer is also the one who believes it contains a greater number of dubious points. This attitude explains why Malthus's *Principles* had a more complex and 'open-ended' structure than Ricardo's. Malthus had been pondering over publishing a collection of essays (to Ricardo 18 Oct. 1818. In Ricardo 1951-1970, VII: 312) as the time looked "unpropitious to the publication of a new systematic treatise" (Malthus [1820], I: 5). The choice of discussing extensively methodological issues in the preface of this work has something to do with these doubts: differences on positive issues are intertwined with different attitudes in *method* and, in order to highlight the reasons supporting his viewpoints, he believes he needs to resort to *methodology*. For example, Malthus differs from Ricardo on such matters as rent and effective demand precisely because he thinks that more factors are at work, those which for Ricardo are given or 'frozen'.

This difference in strategies carries different ways of reading *The Wealth of Nations*: Ricardo, as we have noticed, concentrates on one "puzzle" in the paradigmatic work; Malthus, even if he believes he is the truest follower of Adam Smith, believes that no point of the science should be taken for granted, and that even what Adam Smith had left in his unpublished papers, namely discussion of anthropological and methodological assumptions, should be brought to the forefront in a treatise of political economy.

10.2. *But with two different "casts of mind"*

Keynes, describing his own impressions when faced with the newly found correspondence between Malthus and Ricardo, spoke of a clash between two very different "casts of mind" or two temperaments, unable to communicate with each other. He wrote:

The contrast between the intellectual gifts of the two were obvious and delightful. In economic discussions Ricardo was the abstract and a priori theorist, Malthus the inductive and intuitive investigator who hated to stray too far from what he could test by reference to the facts and his own intuitions. Time

after time in these letters Malthus is talking plain sense, the force of which Ricardo with his head in the clouds wholly fails to comprehend. Time after time a crushing refutation by Malthus is met by a mind so completely closed that Ricardo does not even see what Malthus is saying (Keynes 1931, p. 90).

Keynes here describes the opposition between Malthus and Ricardo in the broadest possible terms, that is, in terms of global psychologies. Such a holistic characterization of a global attitude is akin to what linguists and literary theorists call "styles" -- albeit when applied to the construction of scientific theories.

The contrast Keynes draws between Malthus and Ricardo focuses on psychologies, or on "intellectual gifts". This conceals a rather naive view of the role of individual psychology in the context of discovery, a view that during the Sixties was at the root of endless and rather inconclusive debates within the Popperian school. We believe that what was the explanans for Keynes as well as for Popper should be viewed instead as the explanandum, or that 'psychologies' should be conceived in non-psychologist terms. In other words, what matters are scientific styles, and these result from a constellation of positive doctrines, policies, philosophical assumptions, explicitly formulated methodological theses, theological underpinnings, political outlooks, and, last but not least, a cluster of basic metaphors. These are similar to the factors that make a literary style but recognizing the presence of styles in scientific works is not tantamount to a shallow identification of science with literature. No doubt the writing of literary works is a serious affair, more than philosophers of science used to believe until recently, but what matters here -- in our contrast of literary and scientific styles -- is that a style in the creative activity that produces scientific works is under different constraints than those of literary works, namely empirical and argumentative constraints.

The factors that define a scientific style -- we contend -- may be best discerned through a careful analysis of the actual unfolding of a controversy. What makes styles not so easy to discern is rather the circumstance that these factors fit in a whole which may not be very easily overviewed since it is partly below the waterline; they are related with each other by a relationship that is not one of logical deduction but one of "elective affinity"; they explain individual choices of positive doctrines, of methodological claims, of tactical moves not in terms of a Platonic relationship between what is behind the veil and what is manifest, but in terms of a whole-part relationship.

The style of each author does not imply exclusive use of this or that kind of move, methodological consideration or strategy; in fact, both resort to practically all the repertoire of such moves. The mark of a style is instead a kind of 'preference' for, or an 'elective affinity' with, some sub-set of the above factors.

The notion of scientific style may help in accounting for the difference between Malthus and Ricardo in terms that make sense without denying their shared adhesion to the classical paradigm. Within that paradigm, they two poles of the essential tension which Granger has detected in the social sciences between rigor and exactness.

Something close enough to what we are trying to capture here with the help of the notion of style has been expressed by Khalil (1992) by means of the notion of "temperament". Khalil draws a typology of three temperaments (fox, hedgehog, and wise owl): the hedgehog likes the formal method or "pen and paper gymnastics", whereas the fox likes the informal method or "pictorial images of non-metric

potentiality"; the hedgehog has rigor but lacks exactitude, whereas the fox has exactitude but lacks rigor, and the owl has both. In comparison with Khalil's typology, our typology includes (at least for the case-study for which it has been worked out) only two cases: the methodological apriorist (who happens to be in our case, among other things, also an epistemological sceptical) and the methodological inductivist (among other things, an epistemological 'weak' rationalist). Besides, our typology makes no room for Khalil's transcendentalizing concept of "wise owl": we believe that occasionally both were able to be wise, but for a short time and almost by accident⁸. In terms of Christian theologies, Khalil is a bit of a Pelagian, who believes that we may reach wisdom by our own merits; we are more Augustinian and believe that it is the controversy's 'enlightening grace' what may dispense sparks of wisdom.

10.3. *And with two different attitudes to controversy*

By a careful analysis of a controversy we may also reconstruct the implicit 'metatheory' of scientific interaction held by each contender, which in a sense expresses his most central views about what the scientific enterprise is all about. This is revealed by their attitudes regarding what is the role of criticism and debate in science and hence about the role of controversy in the pursuit of knowledge.

Ricardo clearly believed that his correspondence with Malthus was a way to clarify his own thoughts; in a later phase, when he was considering the possibility to publish his *Notes on Malthus*, he seemed to be inclined for a while to believe in public controversy as a means of conquering the enlightened part of the public to his ideas; later on, he seems to become pessimistic on the value of the controversy (and this happens at a time when he seems to slow down the tempo of his correspondence with Malthus; but recall that we may have evidence only about the flow of *written* communication between them); finally, at the time of the discussion about the measure of value, he seems to become more and more pessimistic about the controversy's value for the search of truth.

Malthus is apparently more constant in his attitude and probably more optimistic on the controversy's value; he tries to take Ricardo's *Notes* seriously into account while revising his *Principles*, at least as far as we can judge provided that the revision was not completed by the author.

Each contender's attitude to the controversy and his preference for one kind of strategy is revealed particularly in connection with one issue: *the (un)knowability of essences*. This is not only a major topic in the epistemology of Malthus and Ricardo (be it explicitly formulated or inherited from any of his sources) but also in each one's attitude to the controversy. Ricardo's radical scepticism goes hand in hand with an attitude to the controversy that assumes it to be always inconclusive as a way of ascertaining the truth. To Ricardo's mind there is always an equilibrium between opposing arguments, and the only way of having access to truth is progressive retreat into abstraction, where the thesis argued for acquires more and more the character of a tautology. It is indeed interesting that, his meta-attitude to the controversy's value notwithstanding, Ricardo appears to be the one who has changed more in his positions as a direct result of Malthus's criticism. On the opposite, Malthus's 'rationalistic

empiricism' makes him believe that there is always a way to ascertain the truth, and that if controversy. Ricardo's radical scepticism goes hand in hand with an attitude to the controversy that assumes it to be always inconclusive as a way of ascertaining the truth. To Ricardo's mind there is always an equilibrium between opposing arguments, and the only way of having access to truth is progressive retreat into abstraction, where the thesis argued for acquires more and more the character of a tautology. It is indeed interesting that, his meta-attitude to the truth cannot be reached by conclusive argument, there are at least arguments that *incline* to one conclusion even if they cannot *necessitate* adhesion to it.

As a result, the conduct of the controversy by each is different: Malthus, the 'rationalist', seeks to resolve the controversy, i.e. to amass reasons that incline; Ricardo, the 'sceptic', seeks to show that they are in fact unresolvable, and that arguments of equal weight can be piled up on both sides (and *therefore* his apriorism is always right).

10.4. *Styles under constraints*

A controversy is the setting where two styles come into interaction. That is, a controversy is not a juxtaposition of two monologues. If, on the one hand, the choice of individual moves by one opponent depends on his scientific style as a whole, on the other, this choice is always made under a network of syntactical, semantical, and pragmatical constraints imposed by the context of the controversy. Within these constraints, he may reveal a preference for a given kind of positive or methodological claims, but he is first of all under a pressure to face in a pertinent way the controversy's 'demands'. Furthermore, the association of one scientific style with the choices made at a given stage of the controversy is not arbitrary, although it is certainly not compulsory. The controversy thus displays a peculiar mixture of contingent and quasi-logical aspects, which endow it with a 'rationality' of a special kind.

When forced by the course taken by the controversy, one of the opponents may resort to a strategy he dislikes; for example he may reluctantly engage into methodological discussion while he had previously avoided it, or he may charge the opponent with inconsistency, even if he had always preferred the virtues of realism to those of consistency. This is apparent, for instance, in the following two aspects of the present controversy.

The first is *the issue of exceptions*. Given Ricardo's methodology, which supports a view of explanation as explanation of ideal behaviour, counterevidence instantiated by concrete examples tends to be seen as not affecting the theory; on the level of strategies, this leads to discard objections based on such counterevidence as "irrelevant". Malthus, on the contrary, is not encouraged by his methodology in adopting such a strategy, and, in the face of counterevidence, always tries to find a "middle" between theory and fact, adding more and more qualifications to his basic laws, or immunizing them by recourse to the notion of multicausality (the same notion that for Ricardo makes any attempt at explaining non-idealized phenomena hopeless).

The second is *the issue of misunderstanding*. Because of Ricardo's views on

scientific language favouring explicit definitions and rigour, complaint of misunderstanding is more easily available to him than to Malthus. This leaves the way open for him for a defensive strategy which he often employs claiming that he has not been properly understood in the precise meaning he intended to attach to terms.

Economic methodologists are familiar with either a Popperian-Lakatosian trend in the philosophy of science, whose exponents in economic methodology have been Mark Blaug, Terence Hutchison, and Lawrence Boland, or a 'rhetorical' trend (according to a sense of the word rhetoric that we find too reductive) whose exponent has been McCloskey. Our approach intends to contribute to the development of a third way in the philosophy of economics, going beyond objectivism and subjectivism, or, using McCloskey's blissful expression, towards *conjectivism* (even if the golden mean lies for us slightly nearer to objectivism than for MacCloskey)⁹. Our North-West Passage is precisely what has been sought for by post-empiricist philosophy of science since 1962, and our route is different from McCloskey's in that we believe that (even in its recent more reasonable version) his version of rhetoric is still too much a kind of subjectivism in drag. Our case study shows that economists do "make things with words" as McCloskey contends, but also that, with words, they cannot do whatever they like. In other terms: (a) the theoretical networks they build are in constant interaction with practices *in* and *about* the economy (and these practices are no more mere "conversation" than blind mechanisms); (b) these networks emerge out of conjective conversational practices (i.e., controversies) where economists do at once something less and something more than "persuading" each other; indeed they are both players and pawns of a game they don't fully control in so far as they are willy-nilly caught within the controversy's *dynamics*.

The fact that the rationality of the game does not overlap with the participants' rationality does not imply that the game is irrational, since the game's rationality is written in its rules, not in the players' consciousness. Controversies are an example of the "unintended results" principle that Adam Smith contributed so much in detecting. That is, controversies are a kind of purposive behaviour, and such a behaviour depends on the contenders' intentions; but most of the contenders' intentions are opposed (both intend to win, both intend to win the partner's respect if not to convince him, both intend to increase their reputation) and the shared intentions (such as the intention of contributing to a dispassionate examination of issues) are rather weak.

Besides, controversies are no Popperian third-world affairs: they develop *in time*, and each contender has to choose reasons and objections complying both with the controversy's demand and with temporal constraints (that is, *before* all the possible pertinent reasons in favour or against any claim have been duly weighed); thus, as it was suggested by Gadamer, but also clearly perceived by Leibniz before him, also the scientist's rationality is a kind of Aristotelian phronesis.

The conscious ends that motivated Malthus and Ricardo to engage in controversy were not identical with the actual results the controversy yielded. The latter were to a wide extent non-intentional effects. The controversy did by no means afford either victory or persuasion, or solution of problems; what it afforded was understanding of issues and of differences: firstly, the opponent's criticism forced each of them to spell out his own positions and to see more implications of these positions; secondly, in a number of cases it did force them to modify their views, even if denying the importance

of the admissions made, or resorting to claims of misunderstanding of their previous formulations, or trying in some other way to under stress the opponent's reasons and to stress their own reasons.

This is why both Malthus and Ricardo after all realized that joining the controversy game was worth their time and that their effort was not wasted in that apparently useless pursuit. They also realized that precisely the fact of sharing so much made the difference between them both tiny and important. It was the intensive scrutiny and elaboration of this difference that became the motionless engine that propelled the growth of knowledge in the high years of classical political economy.

References

- Auroux, S. (ed.) 1990. *Les notions philosophiques. Dictionnaire*. 2 vols. Paris: PUF.
- Beauchamp, T.L. 1987. "Ethical theory and the problem of closure". In Engelhardt and Caplan 1987, pp. 27-48.
- Cremaschi, S. 1996. Review of: D.N. McCloskey, "Knowledge and Persuasion in Economics". *Pragmatics and Cognition* 4.2: 425-429.
- Cremaschi, S. and Dascal, M. 1996. "Malthus and Ricardo on Economic Methodology". *History of Political Economy* 28.3: 475-511.
- Cremaschi, S. and Dascal, M. 1998. "Persuasion and Argument in the Malthus-Ricardo Correspondence". *Research in the History of Economic Thought and Methodology*. 16.
- Crombie, A.C. 1994. *Styles of Scientific Thinking in the European Tradition*. 3 vols. London: Duckworth.
- Dascal, M. 1990. "The Controversy about Ideas and the Idea of Controversy", in F. Gil. (ed.), *Controversias científicas e filosóficas*. Lisboa: Editora Fragmentos, pp. 61-100.
- . 1995. "Epistemologia, controversias y pragmática". *Isegoría* 12: 8-43.
- Dascal, M. and Cremaschi, S. forthcoming. "The Malthus-Ricardo Correspondence: Sequential structure, argumentative strategies, and rationality".
- Engelhardt, T.H. Jr., and Caplan, A.L. (eds.) 1987. *Scientific Controversy: Case studies in the Resolution and Closure of Disputes in Science and Technology*. Cambridge: Cambridge University Press.
- Fleck, Ludwik, [1935] 1980 *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*, Frankfurt: Suhrkamp.
- Gombrich, E.H. 1968. "Style". In *International Encyclopaedia of the Social Sciences*. 18 vols. New York: Macmillan - Free Press. 15: 352-61.
- Goodman, N. 1978. *Ways of Worldmaking*. Indianapolis: Hackett.
- Granger, G.-G. 1959. "Sur la connaissance philosophique". *Revue Internationale de Philosophie* 13: 96-111.
- , 1968. *Essai d'une philosophie du style*. Paris: A. Collin.
- Gross, A. 1990. *The Rhetoric of Science*, Cambridge, Mass: Harvard U.P.
- Hacking, I. 1982. "Language, Truth and Reason". In Hollis, M. and Lukes, S. (eds.), *Rationality and Relativism*. Cambridge, Mass: MIT Press, pp. 48-66.
- . 1992. "Style for Historians and Philosophers". *Studies in the History and Philosophy of Science* 23: 1-20.
- Khalil, E.L. 1992. "Fox, Hedgehog, and Owl: Three Temperaments in Economic Discourse". *Methodus* 4.1: 101-109.
- Keynes, J.M. [1933] 1972. "Thomas Robert Malthus". In *Essays in Biography*. Cambridge: Macmillan and St. Martin's Press.
- Le Guern, M. 1990. "Style" [ling.]. In Auroux, 1990. II: 2473-75.

- Malthus, Th.R. 1986. *The Works of Thomas Robert Malthus*. 8 vols. Ed by E. A. Wrigley and D. Souden. London: Pickering.
- . [1813]. *Observations on the Effects of Corn Laws*, In Malthus 1986, VII: 87-109.
- . [1820, 1836] 1989. *Principles of Political Economy*. Variorum edition edited by J. Pullen. Cambridge: Cambridge University Press for the Royal Economic Society.
- McCloskey, D.N. 1994. *Knowledge and Persuasion in Economics*, Cambridge University Press.
- McMullin, E. 1987. "Scientific Controversy and its Termination". In Engerhardt and Caplan 1987, pp. 49-91.
- Moulin, A.-M. 1991 "Style" [*épist. gén.*]. In Auroux, 1990. II: 2473.
- Paglin, M. [1961] 1973. *Malthus and Lauderdale. The Anti-Ricardian Tradition*. Clifton, NJ: Kelley.
- Ricardo, D. 1951-73. *The Works and Correspondence of David Ricardo*. 11 vols. Ed. by P. Sraffa with the collaboration of M.H. Dobb, Cambridge: Cambridge University Press.
- . [1928] 1951. *Notes on Mr Malthus*. In Ricardo 1951-1973. II.
- Samuelson, P.A. 1978. "The Canonical Classical Model of Political Economy", *Journal of Economic Literature* 16: 1415-1434.
- Würgler, H. 1957. *Malthus als Kritiker der Klassik. Ein Beitrag zur Geschichte der klassischen Wirtschaftstheorie*. Winterthur: Keller.

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Notes

1. The death of a participant is not specifically listed as one of the ways in which a controversy is terminated (indeed, sometimes controversies go on even after that, as in the case of the Arnauld/Malebranche controversy -- cf. Dascal 1990). But it could be included as one form of 'closure' due to 'non-epistemic factors' (McMullin 1987) or as a particularly tragic way of 'waning interest that leads to the 'natural death' of a controversy (Beauchamp 1987). For a discussion of these typologies of controversies' endings, see Dascal 1995.
2. Cycles 5 and 6 are in and around both authors' main works.
3. This cycle refers to the third edition of Ricardo's *Principles*.
4. For a more detailed analysis see Dascal and Cremaschi forthcoming.
5. See Würigler 1957; and Paglin [1961].
6. See Samuelson 1971.
7. Gross perhaps establishes a more drastic alternative between social-linguistic practices and realism than we would go for; we would rather talk of ontological pluralism than of anti-realism.
8. Khalil (1992) classifies Malthus as a fox; more recently, as a reaction to Cremaschi and Dascal (1996) he suggested that he had been perhaps wrong, and Malthus may be a "wise owl" (private communication)
9. McCloskey 1994, p. 378; for comments see Cremaschi 1996.