

## Appendix N°4

### **Nota sui punti di dissenso** **A Note on points of dissent**

*by Luigi Pasinetti*

#### **1. Introduction: the issue at stake**

I have come to the decision to add this Note on points of dissent because I feel the issue at stake is an important one. I shall try to express it as simply as I possibly can.

We were asked to evaluate economic research in the Italian university system. To evaluate anything, one needs (consciously or unconsciously) a benchmark with which to be confronted – a yardstick in terms of which to measure whatever is being evaluated.

For some of my economist-colleagues this benchmark exists already. It is well identified with the one that is dominant in the U.S. and is in fact becoming increasingly dominant also in most other countries. This is due to a series of historical circumstances that have given strength to the US educational system (most of all, at the post-graduate level), and have made it attractive for economists coming to be trained from all over the world. But in this way the US system has come to set the standard of professional economists almost everywhere. It tells them what is good research and what is not.

What is wrong with this model? Nothing really, if it did not pretend to be taken as if it were providing *the* benchmark for everything, even for those areas of economic research that, in the US, are not present, or are considered as “marginal” (I shall come back to this term below). In this way, however, we are falling into two serious errors: one in the short, the other in the middle-long, run. First, we are asking the US benchmark to measure something for which it is not appropriate. It is as if we were pretending (to take a sport image) to evaluate light athletic performances simply by concentrating on 100 meter runs (where, by the way, the US are very strong!). Secondly, on a longer time span, we induce a structural transformation of economic research elsewhere, towards becoming exactly like the one now present in the US. (To continue the athletic image, no 500 meter runs, no 10.000 meters, no marathon runs, etc.; only 100 meters run, and little else).

But we have been asked to assess the quality of research in Italy - not to adhere to an externally given model of economic research. One may argue that, if the US model is far superior to any other conceivable model, then the two objectives would in fact coincide. But this is hardly so. I think that much of the success of mainstream economic theory today (in the US) is due – not to intrinsic superiority, but – to the present strength of the US university establishment. If, by absurdity, historical circumstances had brought one single other world power to have become the dominant one, we would have been handed with quite a different (not necessarily better; probably worse!)

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mainstream economics and surely with a very different benchmark to be confronted with. This counterfactual story suggests to me at least three reflections.

First, I am induced not to rely too much on established theory, especially when social sciences and political issues are involved. (During our meetings, I inserted, in the CIVR Archives, an article, published in *Nature* by Jayant Narlikar, a distinguished Indian astrophysicist, a pupil and co-author of Fred Hoyle, also a professor at the College de France, who, precisely with reference to the allocation of research funds, has been arguing the case for extra-protection of non-mainstream theories: in physics! How much more relevant the problem must be in economics!). Let me recall that history of economic thought has taught us how quickly, after Ricardo's publication of his *theory of comparative advantages*, the whole mercantilist ideas, that had dominated political economy for a few centuries, were swept away in a matter of a few years. In the last century, when the capitalist economies fell into a dramatic slump in the 1930's, it was sufficient for a group of bright economists around Keynes to decidedly propose a few commonsense economic policy measures, which the whole economic establishment of the time had considered absurd, to produce the greatest "revolution in economics" of the XX century. Today, we may well be near a similar turning point. Neoclassical (*i.e.* dominant) economics is at a critical stage, even by admission of some major economists (think of Samuelson and Arrow) who contributed to its affirmation. New world-wide events, such as "globalisation" and the acceleration of technical knowledge, are generating serious doubts on the appropriateness of the economic theory we are inheriting. This seems to be the wrong moment for unduly strengthening the dominant paradigm we have. The case for stimulating less conventional, innovative ideas is increasing.

Secondly, in economics, each benchmark is affected by the institutional, geographical and historical conditions in which it is born. What may serve well a specific country that finds itself in specific conditions at a given time, may not serve as well other countries with different traditions, or the same countries in different, evolving new conditions.

Thirdly, an extensive adoption of a benchmark, that is expression of a dominant (yet inevitably reductive) theoretical system, would constrain current research not only to be confronted with the dominant standard (which is o.k.), but also – and this would be its most pernicious effect – to *conform* to it, by forcing a structural change in the composition of the different disciplines that constitutes economics. In our specific case, in Italy, economists have indeed been using mathematics and econometrics, as in the US, but they have also been relying on history *and* on institutional analysis, much more extensively than in the US. Why should Italian economists give them up? I do not really believe that great improvements can be made by simply conforming uncritically to the US research model. We may well risk, instead, to destroy the great variety of

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Italian (and European) economic research. This does not mean, of course, to abstain from taking advantage of efficient US university teaching and research studies, but it cannot mean giving up our best traditions. The aim of improving the quality of economic research in Italy cannot entail destroying its traditional variety and wide scope.

I may now go on to present my disagreements on specific points.

### 2. On the “relazione finale: Economics”.

During the drafting of this “relazione”, I presented to our co-ordinator (Tabellini) a rough version of mine. I am grateful to Tabellini for accepting, to a good extent, a compromise with my suggestions, by inserting some of them in the first part of the report, and by allocating explicitly to me some paragraphs in the second part of the report.

Despite this compromise spirit, I did not agree in the end with two points (in the first part of the economics report)

- The first point is a sentence introduced at a late stage of the drafting. If maintained (which I would have advised not to do), I had asked to add a footnote containing a different interpretation of the figures given. My proposal was rejected by a majority vote of the panelists of the economics group.

The sentence (page 1 of the relazione finale, Economics) reads as follows

“Per la stragrande maggioranza del restante terzo (i prodotti su cui il consenso non è stato raggiunto all’unanimità neanche nella seconda fase del processo descritto sotto), il consenso è stato negato da un panelist soltanto. Alla fine il consenso è stato negato da più di un panelist in solo circa il 5 % dei casi (25 prodotti, su circa 500 prodotti valutati dal gruppo di economia), sebbene concentrati su un’area in particolare.”

My (rejected) footnote was the following:

*“Luigi Pasinetti si è opposto all’aggiunta di questo paragrafo ... e dissente dal messaggio conciliante che esso vuol lasciar trasparire. La sua versione è la seguente: non c’è stato alcun esito plebiscitario nel giudizio finale. Si sono creati, come si evince dalla seconda parte del rapporto, tre gruppi. Debez e Fitoussi hanno concesso il consenso a tutti i prodotti. Pasinetti ha giudicato caso per caso, distribuendo consensi e non-consensi. Gros, Petretto e Tabellini [formando maggioranza] hanno agito spesso in sintonia nella loro distribuzione dei consensi. Quindi il 95% dei consensi significa solo che Gros Petretto e Tabellini alla fine hanno agito nella quasi totalità dei casi all’unisono.”*

- The second point of disagreement concerns some facts on which I have been involved.

The disputed excerpt of the report (see p. 5, point *e* of the relazione finale: Economics) is the following

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“Nella riunione tenutasi a Roma il 17 ottobre si decise di delegare al presidente del panel (Peracchi) la proposta di un terzo referee, tenendo conto dei suggerimenti avanzati da parte dei panelist che volessero farlo. In quell’occasione, alcuni panelist (Dehez e Pasinetti) manifestarono perplessità e successivamente (insieme a Fitoussi) contestarono questa decisione perchè, a loro avviso, essa metteva in discussione il ruolo del panelist a cui era stato originariamente assegnato il prodotto. In seguito a conversazioni con il Presidente del CIVR e d’accordo con il Presidente del Panel, il panelist Pasinetti ha pertanto proceduto a scegliere il terzo referee, motivando la sua scelta e discutendola con il presidente del Panel, ma in alcuni casi discostandosi dalle proposte avanzate dal presidente del Panel. Una volta raccolto il parere del terzo referee, il consenso è stato raggiunto all’interno del gruppo di economia per quasi tutti i prodotti; per i pochi prodotti su cui restava il dissenso anche dopo il parere del terzo referee, il consenso è stato raggiunto dall’intero panel.”

I had proposed to insert a footnote (rejected), giving my version of the events, namely:

*“Pasinetti dissente da questa lettura. Dal suo punto di vista, i fatti, si sono svolti nel modo seguente. Nella riunione del 17 ottobre si è fatta una proposta, non votata una decisione. Di fronte alla concretizzazione della proposta nei giorni successivi di lasciare al Presidente Peracchi la scelta dei referee di “appello” si sono opposti (nell’ordine) Dehez, Fitoussi e Pasinetti, nel senso che doveva essere il “panelist in charge” ad avanzare una lista e gli altri panelist eventualmente a contestarla. Pasinetti ha esposto la sua lista dei referee nel message-box del CIVR. Il Presidente ha ricevuto altri suggerimenti via Email sui referee e ha stilato una sua lista iniziale (letteralmente) ignorando – a sua discolpa in modo non intenzionale – quella di Pasinetti. Quest’ultimo ha protestato e, dopo colloqui con i Presidenti del CIVR (Cuccurullo), e del Panel (Peracchi), si è giunti alla decisione che le designazioni fossero fatte dal “panelist in charge” (Pasinetti), dopo aver considerato i suggerimenti del Presidente Peracchi, lasciando comunque a quest’ultimo (e anche agli altri panelist) la possibilità di contestare le sue designazioni finali. Questa procedura è stata seguita alla lettera.”*

I shall simply add that this (rejected) version was drafted by the only panelist in the Economics group who had personally lived through the event. The other 5 panelists (who approved the version without this footnote) had all learned the facts second-hand. An interesting example of paradox of majority voting.

### 3. On the “Area 13 Final Report”

All in all, I do not find my point of view – in those places in which it appears in this Report – as fairly presented. During the drafting stage, I sent our President many remarks, which did not make their way into the final Report. Here is a sample of such cases.

- First, during the first stage of the CIVR exercise, there were cases of non-consensus to other panelists’ syntheses of referees’ evaluations. I had initially 20 products blocked in this phase – very substantially more than any other panelist in the whole Area. The Report presents this fact in the following way:

“For these [blocked] products, the evaluations given by the experts and the consensus proposal by the Panelists in charge were questioned by other Panelists as not entirely consistent with the CIVR criteria.”

I am sorry to stress that they were indeed questioned “as not entirely consistent with *their*

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*own personal interpretation of the CIVR criteria*". The italics is inserted because of its high significance. What was their interpretation? I shall put it at the extreme, for simplicity and clarity. It was as follows: if the paper is published in a "top field journal" (according to the US standards) it is "excellent"; if it is not published there, it cannot generally be excellent, and not even "good". It could be "acceptable" or most often "limited". This turned out to be an almost inevitable case, when the product under scrutiny was from "marginal areas" (see below for this term). An emblematic case came up at the end. A product in economic history, published as an Italian book, was blocked by some panelists up to the very end. Two well respected external referees, after reading the book, had both evaluated it as "good", and the panelist in charge had confirmed it as "good". But some panelists objected (*both* to the two referees, *and* to the synthesis drawn by of the panelist in charge) contending that the product should be downgraded. Note that the product was not available on line. The three opposing panelists thus had based their assessment on a pure face value of 10 lines abstract or on some shades in the referees' reports – for a book 500-page long, 4-years work! The case had to be solved by the President at the very end, "according to wisdom and common sense", as I wrote to him. But the doubt remains: was the opposition in accordance to the CIVR criteria or to a very *personal interpretation* of such criteria?

- Language is a symptom of one's beliefs. The use made in the Report of the term "marginal" areas is revealing. The Report reads<sup>1</sup>:

"Two other panelists (Dehez, Fitoussi) also expressed concern with the possible penalization of some marginal areas."

In fact the two panelists named these areas as "specific areas". The term "specific" was thus turned into "marginal". It is true that it is unfair to hang a person with a word. But the doubt remains: what are these marginal areas, and why are they "marginal"? I have given my timid suggested interpretation in the "Introduction: the issue at stake" above,.

- There is then a problem of a "chicken and the egg" nature: Impact Factor vs quality of the product. The Report reads:

"One Panelist (Pasinetti) argues that this statistical evidence reveals a clear bias towards products published in journals with an IF."

To be fair it is not "statistical evidence". It is first hand documentary evidence from the referees' reports I had read, before offering (or denying) my cross-panelist consensus. The referees (two for each product) had to fill in answers to 4 different questions concerning:

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<sup>1</sup> [See *Post Scriptum*, at the end of this Appendix.]

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quality, relevance, originality, internationalization. Cases like the following was the first cause of my denying consensus (without any effect, being always in a minority). Quality of the product<sup>2</sup>: “This paper is published in a top field journal, the IF of the journal is high, hence the paper is excellent.” or conversely (always on quality of the paper) “this paper is published in my opinion in a non serious journal [in the specific case of this quotation it was the *Journal of Post Keynesian Economics*], hence the quality is “limited.” Notice that the evaluation I am referring to is on *quality*, not on *internationalization* of the product! Our President seems, in the end, proud to write:

“40% of the products published in journals without an IF received a grade of “good” or “excellent”, and so did about 20% of the book chapters and about 25% of the books.”

Had he enquired a little further into who were the panelists who had saved him from the embarrassment of finding more evidently “which was the egg and which was the chicken”, he would perhaps have found more sympathy for the panelists who paid more attention to the “marginal” areas.

- I can hardly resist at this point a word of dissent on a proposition which just follows the previous one, in the Report:

“Other panelists (Gros, Petretto, Tabellini) instead argued that there is a broad agreement among the international scientific community on basic evaluation criteria, and that moving away from those criteria may lead to self-referencing and to the adoption of criteria that unduly favor some marginal areas.”

Leaving aside the “marginal areas”, on which I have commented already, what raises my eyebrows here is the key-word “self-referencing”. But the Impact Factor itself *is* all based on self-referencing! I should like to ask: what is the different degree of self-reference among the referees, let us say, of the *Cambridge Journal of Economics* and among those of the *American Economic Review*? I may recall that neoclassical economic theory has been recently opposed by a European movement of students (mainly based in France, but not only there) for being an *autistic theory* – *i.e.* a theory closed onto itself.

- A final note. The report writes<sup>3</sup>:

“One panelist (Pasinetti) argues that it is difficult to draw conclusions about the strengths and weaknesses of economic research in Italy, due to the diversity of the criteria and the heterogeneity of the different areas.”

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<sup>2</sup> I am slightly re-phrasing some quotations, being careful not to distort the message.

<sup>3</sup> [See *Post Scriptum*, at the end of this Appendix.]

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Frankly, the presentation of my argument is being reversed. The reader may check for him/herself. I did, indeed, draw specific conclusions. Some of them are reported in the “Rapporto finale: Economics”. Others, more extensive, were given in a draft, and, for space constraints, were not included. But after drawing such conclusions, I always stressed caution about their significance, since such results were not robust, in the sense that removal of the evaluations of one panelist (or even better of two panelists, both concerned with “marginal” areas) would have produced very different outcomes.

### **4. A concluding note and an invitation to reflect**

I should like to conclude on a more positive note. It is clear that we had vivacious discussions and strong disagreements during the CIVR exercise. But sharp disagreements, when they are strongly founded, are never to be feared. When the issues at stake are important, as in our case I think they are, they should better be welcome rather than suppressed. The disagreement that emerged among us – I am convinced – concerned not so much the assessment of this or that particular “product”. It was more fundamental. It concerned the search for the correct benchmark with which to confront economic research. The result of our efforts will now materialise in an apparently scanty ranking list. And that is all that will be paid attention to, as it will indeed be important for distributing research funds. But the by-products of our efforts may in the long run be even more important than the immediate material results.. Evaluating research in our discipline is a serious business, not to be taken lightly. Most importantly, it has to be done with full awareness of the fact that it has the power to shape how economic research is going to develop in the future. Those who are concerned with the future of economics should discuss openly and responsibly the appropriate methods to separate the wheat from the chaff; to consider carefully what their advantages and drawbacks are, so as to avoid to throw away the baby with the dirty water. This risk is higher, the stronger the dominant theory is, and the more organised and the stricter the rules are for the evaluation of the results. This is our present situation. There is little we can do about this in the immediate, but we must be aware of it and try to find full precautions. That is the reason why I have been so determined in examining aims and ideas of the less conventional attempts, in the widest fields. Of course, one would always like to see imagination and originality clearly and immediately, but this is not frequently so. Yet, who can be sure of the absence of seeds of future possibilities, now unseen? I have found it interesting that an eminent scientist, the astrophysicist, whom I mentioned in my introductory section above, should have put this problem in the most challenging terms; and should even have dared to propose – with all cautions–some devices to avoid to kill from the start all potentially fruitful research attempts in unconventional fields. I mention and take it as an invitation, for us, economists, to be a little humbler, and learn to reflect.

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### *Post scriptum* (10<sup>th</sup> January 2006)

President Peracchi, after receiving this *Note on Points of Dissent*, on the 9<sup>th</sup> of January, has changed two words in his Final Report (which had been given as closed and approved by the 31<sup>st</sup> of December). I am therefore compelled to add this *post-scriptum*, to avoid the impression of making inaccurate quotations.

1) The term "marginal", that I had quoted from his Report (p. 5 of this Note) has now been turned into "specific". Thanks for that.

2) An extra adjective ("sharp") has been introduced in the passage from his Report that I have quoted and criticized in my last point of dissent (p. 6); *i.e.*, the quotation: "One panelist (Pasinetti) argues that it is difficult to draw conclusions" has now been turned into "...that it is difficult to draw sharp conclusions". Not sharply, but softly better as compared with the previous version.