Comment on David Colander's
"Economists, Incentives, Judgment, and Empirical Work"

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David Colander has provided an important paper that deserves publication (although considerable revision might be necessary). It illustrates a more general problem in methodology (and "epistemology", as it might be said in Europe) that has plagued North American economics since the 1960s. The fundamental issue, as Colander explains, is whether a model builder should be concerned with the realism of the assumptions employed or instead be concerned just with the logical adequacy of the model's and its assumptions. This methodological choice represents a problem that has implications for the business of academic departments – specifically, the business of career advancement through promotions and tenure. Let me explain.

Most North American economic model builders see their research methodology as a matter of a developing *sequence of models* (see Weintraub 1979, p. 15; Boland 2003, p. 228). As a result, raising a question of the realism of assumptions at an early stage is considered counter-productive or, at minimum, premature.

It is this methodological stance that allows for the situation Colander describes to exist in North American universities. Since economists in these universities see no need to worry about the realism of their assumptions, they can easily turn out all sorts of theoretical models. At best, their objective is to use their models to explain empirical evidence that have collected (usually without regard for the quality of the evidence – see Boland 1989, pp. 158–64); at worst, to explain only what they call "stylized facts" (see Boland 2008). Usually the latter are not verified reports of observations, but instead, simplified or abstracted (and thus assumed) states of an economy. It is in this sense that I understand what Colander is calling the "theory comes first" approach to macroeconomic research. And the excuse used to postpone any questioning of the realism of the assumptions is the promise offered by the sequence of models.

At a deeper level, all model builders make a choice regarding the realism of their assumptions. One the one hand, many take the view that models (and their assumptions) should not be judged as either true or false but only as better or worse according to the currently accepted conventional criteria. Sometimes it is even claimed that all theories or models are false (e.g., Solow 1956, p. 65) – even though this theory of theories itself is thus self-contradictory! On the other hand, other model builders seemingly follow Milton Friedman's famous1953 methodology essay and claim that the truth status of the assumptions, as instruments or tools, does not matter so long as they work. Sometimes econometricians are unaware that they are following Friedman (see Boland 1997, pp. 283–4). But, in either case (i.e., as conventional truths or as mere tools), the question of the realism of assumptions is either postponed or ignored.

It could be claimed that since econometrics is used to test economic theories or models we should not be so quick to criticize. After all, such tests are intended to determine the truth status of the theories or models in question. But can econometric tests actually do this?

Before I consider this question, it should be remembered that Colander's main point is that by following what he says has been the standard practice in Europe – whereby one would be expected to do the extensive research needed to determine whether one's assumptions fit the facts before developing models to explain those facts – not much publishable research could ever be produced. As a result, the quantity of published research would not be a useful criterion for promotion or tenure and instead a more subjective criterion will be used. In North America subjective criteria have been suspect since the 1960s. One reason might be the fast growth of universities both in size and number during the 1960s. Graduate programs grew in response with the obvious consequence that the size of the community of economists in North America became too large for one member to know much about all but a very few other members of the community. Even economics departments are too large to expect any one member of their tenure and promotion committees to know much about the work of any candidate. While weight might be given to solicited reference letters, it is even difficult to be sure that the letters are honest if one does not know the referee personally. As a result, most departments have resorted to counting publications. But worse, this means that the essential judgment is being made by journal editors – but that is another topic (see Grubel and Boland 1986).

While avoiding the use of subjective criteria may solve one problem of unreliable promotion and tenure assessments, it seems to result in another problem, the one Colander identifies. Obviously, in the North American system, the aim of someone who wishes to advance their careers in the economics profession should be to maximize the number of publications. Since the mid-1970s we have seen an explosion in the supply of specialized publications in economics. This has facilitated the widespread counting of publications and thereby the ubiquitous careerism by providing the needed opportunities for publication (for more on careerism in economics, see the Preface and Epilogue of Boland 1997). Add to this the now common tactic of allowing PhD theses composed of several essays and the tactic of multiple authors – thereby increasing the number of publications for the same effort – and you have what Colander calls “gaming” the system and thereby maximizing the number of publications regardless of any assessment of quality beyond conforming to the currently accepted modeling techniques – without any concern for the thorny question of the truth status of the assumptions used in the models.

But, surely it will be said, econometrics is often used to assess various proffered economic models and theories. Maybe this is so but even this suffers from the problem of the urgency of producing publications. Judging by the many published tests of theories or models, the profession seems to fail to understand what it would take logically to actually perform a test of any theory or model and its assumptions. Even if we allow the use of arbitrary conventions for acceptable truth status, as I explained many years ago (Boland 1989, chapter 8), if one just shows that one’s test of a theory or model fits the available observable data, one has not done enough. That is, to test a theory or model as a whole (rather than all of its assumptions individually), one would have to not only test the theory’s assumptions but test at least one possible “counter-example” (viz., a state whose truth status is denied if the theory is true). Of course, “truth status” is difficult to determine. Instead, we would use either confirming or disconfirming statistical conventions (note well that “disconfirmed” is not logically equivalent to “non-confirmed”). As I explained in my 1989 book, the only convincing test is a complete test of the theory or model. For a complete test we would have to show that not only that the theory passes (fails) and the counter-example fails (passes) using the currently accepted conventions for either confirmation or disconfirmation. Needless to say, if economic researchers did all this before publishing their tests, few tests would be published.

All of this is to support Colander’s argument that there are good reasons for why some commonly used econometric techniques will not be in use for macroeconomic research in North America even though they may still be used in some places in Europe.

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