

Court Jesters, House Gadflies and Economic Critics

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Thou should'st not have been old till thou hadst been wise
(The Fool to King Lear)

In the medieval court of *King Lear*, the fool seems to have been granted an extraordinary liberty to speak truthfully, without resorting to guile or subterfuge. Why was there a tradition that permitted a seemingly incendiary figure licence to speak his mind (assuming the role of professional as opposed to amateur fool was limited to those of the male persuasion)? We might reasonably assume that, from a fool, home truths served only to increase the innocent merriment of the established court players, providing a reasonable release from the more serious strivings of daily life. Truth amuses since court life (the life of a successful courtier) is very much about keeping true thoughts and observations safely out of sight. Fools, or at least authorised fools, are allowed such privileges because they deliberately put themselves outside the intrigues and political competition of the Court.

The profession of economics is not without its own versions of unofficially sanctioned fools, gadflies and critics. They are allowed to speak the truth (sometimes even in the very journals dedicated to perpetuating orthodoxy) because in some sense, to use the Hollywood terminology, they are not considered to be serious players. Although such critics enter into some of the inner sanctums, are allowed to sup and dine with the elect, as a class they are perceived as having no impact on the profession itself. Economists may listen to what they have to say, read what they write, even in principle agree with many of their arguments. But for all intents and purposes, they are largely ignored. Professional gadflies of this sort create only the gentlest of ripples on the placid surface of mainstream economics. These domesticated critics generate no noticeable upheavals and perhaps not even the most modest of changes. (To be honest, this is in many ways preferable to the treatment almost inevitably doled out to those carpers who resist the necessary potty training that would make their presence tolerable. Such deliberate outcasts are simply ignored.) Among those allowed to loiter in the anteroom of prestige and power, David Colander represents one of the best known and accomplished of this species: widely respected, widely read, largely without impact. In one of his many disarming moments, he himself admits his own limitations.

For these students the graduate program in economics is a hoop they must jump through to reach what they want to do. This group is the group I hang around with; it has, however, very little influence in the profession; you won't see its members on the executive committee of the AEA or in one of the AEA offices. (Colander 2001, p. 5)

The fact of being able to observe and publish the profession's shortcomings without having a reasonable hope of influencing that development creates a core tension that provides Colander's overall thought-provoking and often entertaining volume with a rather schizophrenic foundation. The problem itself is

very accurately detailed by a blurb on the inside dust jacket of the book. (Being well acquainted with the publishing practices of Edward Elgar, I feel confident that the actual words are those of David Colander.) ‘The essays are written in a highly accessible style, and can be enjoyed by most non-economists, as well as by those economists who don’t take themselves too seriously. It can be usefully read by all economists, even those who do take themselves too seriously.’ If only those economists who don’t take themselves seriously can enjoy the book (those whom Plato once described as the only truly serious individuals), then not all that many accredited economists are going to be capable of enjoying this collection. Nor is it likely that those economists who do take themselves seriously will benefit from reading this book, even though they are exactly the audience that can best be served by a serious consideration of the ideas contained in Colander’s work. They will be too busy going about what they consider to be the true business of an academic economist and, by doing so, advancing their careers and their own importance. Why should they choose to read or take seriously any analysis of economics and economists that cuts against their own self-interest? Given the human capital already invested (and the subsequent sunk costs), are they likely voluntarily to wave good-bye to their currently appropriated quasi-rents?

Even those economists who find themselves with the time and the proclivity to think about what Colander has said will find that they are still residing in that same criticised academic discipline with the same existing incentives in place. The cost of changing the status quo is simply prohibitive. Before a newly-minted economist becomes established, outward rebellion is at best suicidal. Afterwards, there is noticeably less of an incentive to attack a system that now seems capable of providing adequate nourishment and sustenance. As Frank Knight pointed out in his minimalist defense of conservatism, while the faults of an existing system are readily known, the faults of a new one are not¹ (see Patinkin 1972, p. 801).

It is possible to pursue this line of thought further, but the heart of the problem is clear. The paradox is not unlike the one that Camus presents to Sisyphus. Why bother rolling the boulder up the hill if it must only inevitably trundle back down? Ostensibly nothing is accomplished by such exertions and struggles. Like all such sanctioned critics, Colander never manages to resolve this core contradiction. This does not say that the sermon isn’t worth preaching; instead one rather wonders how many deaf ears make up the relevant target audience (those who might be most assisted by such wisdom). Only the already converted will readily lend their full attention. True, Colander’s collected articles are not simply a sermon depicting the evils of vegetarianism, forcefully delivered to an enthusiastically cheering congregation of cannibals. That would be a largely unfair characterisation. However, while recognising that the incentives within the profession make the changes he advocates unlikely, Colander at times displays the same sort of optimism that tends to infect many similar critics (whether authorised or officially beyond the pale). The necessary trick seems to be an ability to simultaneously know, yet deny one’s own knowledge.²

Self-appointed reformers of the profession seem to tether their optimism to an unarticulated belief that all that is ever required for such change to take place is a sufficiently clear explanation. (Notoriously, the archetypal American tourist holds a firm belief that foreign inhabitants will understand English if it is spoken in a very loud and slow fashion.) This is the sort of ‘reasonable chaps will agree’ prejudice that is just below the surface of Keynes’s own work and underlies the stirring end

of the *General Theory*.³ But in that given case, the profession promptly excised the most disruptive aspects of his work and went about the requisite domestication of Keynes's theory. Much the same spirit imbues the work of Deirdre McCloskey. Over the last decade and a half she has repeatedly explained what seems obvious to her about the way in which economists argue and so go about doing their work. Despite the apparent impenetrability of economic opinion to such analysis, McCloskey has persisted in her criticism. When they are at their worst, such reformers resemble those evangelists who always see indications that the tide is turning and that good is about to triumph over evil. True, this slightly Pollyannaish position is preferable to the sort of pessimism that leads to a combination of defeatism and cynicism. But I find a consistently pragmatic approach more appealing. As Colander himself admits, change can occur only by appealing (at least in some sense) to the self-interest of the practitioners. Serious attempts to produce change will go astray if the underlying imperative is to change economists' preferences. It is perhaps more strategic to demonstrate that an alternative path will better fulfill their existing preferences, than to try to imbue the flinty bosoms of hard-headed economists with a crusading spirit. By this I mean that you can't expect academic economists, whether at their incubation, fledgling, or full flight stage to cease to be careerist, at least to a certain extent. After reducing human motivation largely to a narrow level of self-interest, economists, more so than any other conceivable group, cannot conveniently dodge their own self-interested motives. That of course is what makes change from within so difficult. Old economists die but they don't fade away.⁴ They train others to take their place.

Colander's Second Voyage

Well then, it seemed to me next, since I'd wearied of studying the things that are, that I must take care not to incur what happens to people who observe and examine the sun during an eclipse; some of them, you know, ruin their eyes, unless they examine its image in water or something of that sort. I had a similar thought: I was afraid I might be completely blinded in my soul, by looking at objects with my eyes and trying to lay hold of them with each of my senses. So I thought I should take refuge in theories, and study in them the truth of the things that are. (Plato 1975, p. 51)

David Colander has made a very comfortable career for himself as an outsider, an economist who examines other economists as a profession in much the same way that anthropologists put under observation the customs and practices of isolated (and what was previously characterised as primitive) tribes. Am I envious of Colander's success? You bet. Do I think it is well deserved? Absolutely. So perhaps what I harbour isn't true envy. I wouldn't mind being in the same position, but I don't think for a moment that I am in any sense more deserving.

As Colander himself explains in his latest collection of essays, he stumbled on to his current path after having failed to engage the profession on issues relating to his own particular macroeconomic ideas. Like a disappointed debutant, he was puzzled by the almost complete indifference with which much of his work was met. What was it about economists that led to such strongly enforced professional norms?

What is not understandable to me, and what led me to start looking at the economics profession rather than economics, was that I couldn't even get most economists to understand my arguments informally. Somehow thinking informally about very abstract issues was not something that most economists could do. Their minds seemed compartmentalized. They thought about economic theory in a highly formal way, but when they thought about economic policy, they often left their abstract analysis aside and relied on pedestrian analysis. (Colander 1991, p. 11)

In this sense Colander has embarked on a second career parallel to the second voyage Socrates declares he took when faced with the failure to find truth directly through the observation of natural phenomena. Colander decided to analyse the actual practices of economists (what they do and don't do) rather than tackling economics in a more direct fashion. Lest such an enterprise be sneered at by successful or wannabe doers (those who can, do; those who can't, write about doing), observers of the economic tribe must be as steeped in their lore as ever an anthropologist was or is about the relevant kinship rites in their own preferred *clan du jour*.

His career as an economics watcher really began with a study done in association with Arjo Klamer (1987) detailing the way in which graduate education was conducted (or misconducted) at top-ranked departments in the US (being at that time, and still, the relevant economic universe for the profession). He stumbled on to the obvious. Much of what Colander found to be annoying about economics could be recognised clearly, as in the sharp depictional lines of a miniature, when examining the way in which economists were not only trained for, but also socialised into, the profession. The incentives driving the activities of its members were inculcated into its apprentices. It was here that the MIT categorical imperative was honoured; thou shalt employ only simple formal models:

The MIT approach is ... sterile and highly limiting for most economists ... it eliminates the passion in doing economics and instead directs economists' goals toward financial gain and institutional success. Economics becomes a job, not a vocation. (Colander 2001, pp. 95-96)

This core insight arose largely from the responses provided by surveyed graduate students. The puzzle it presented (how did this come about? what purpose does it serve?) provided the impetus for Colander's further investigations. The initial article was in fact widely read (and largely ignored since the same set of incentives within the profession continue to dominate). Though individually and as a profession we may have at best a very limited attraction to outsiders, those who toil within its confines tend to be more than a bit self-absorbed (as are all those of any profession). Generalised, this only demonstrates what the tabloids have known for years: gossip sells. Yes, we do want to pretend that we are reading the material for high-minded reasons, but at some basic level idle curiosity does rule. However, like most gossip, although it is largely read, it tends to have little impact on the lives or habits of those who read it. Gossip about the profession doesn't change the inherent institutional incentive structure that dominates that same profession.

This current collection of articles follows the success of a previous collection published a decade ago. A casual reader might be tempted to dismiss Colander's current effort by saying that he has said all this and more in his previous books and articles (including books consisting of past articles). Even worse, the current effort just consists of recycled (republished) articles cobbled together with

the aid of an introduction. In effect we are asked to purchase (theoretically, since few individuals can afford Elgar's prices), the product of academic double-dipping, squeezing new out of old publications. Such charges would be entirely unfair. By collecting his work of the last decade, consistent themes in all of Colander's anthropological studies of economists become clear. These links are illuminated when the separate pieces are presented as a whole. Not only are connections clarified, but the current volume also takes forward issues and themes previously explored in a more tentative fashion in earlier work. As it should, Colander's thinking on the issues is evolving, though not largely changing course. Even people already familiar with much of Colander's work should gain from reading this volume. Those coming to his work for the first time will find that they are able to profit greatly from a careful consideration of the ideas in this volume, despite lacking any previous familiarity.

The book itself might be better titled 'Keeping Economists (and Other Drunks) Away From Lamp Posts' in honour of the joke often attributed to James Tobin (who needless to say, did not win a Nobel Prize for humour).⁵ What Colander constructs is an overarching argument based on his observation and analysis of the profession itself. In a very deft way, his own approach to the economist's trade illustrates his proposed model for analysing economic activity in general. An understanding of Colander's one over-reaching theme provides insights into both the strengths and the weaknesses of all his work in this vein.

Positive, Normative, or Artistically Bent – The Madness in Method

As the terms are here used, a *positive science* may be defined as a body of systematized knowledge concerning what is; a *normative or regulative science* as a body of systematized knowledge relating to criteria of what ought to be, and concerned therefore with the ideal as distinguished from the actual; an *art* as a system of rules for attainment of a given end. The object of a positive science is the establishment of *uniformities*, of a normative science the determination of *ideals*, of an art the formulation of *precepts*. (Keynes 1890, pp. 34-35)

The pity is that Colander uses John Neville Keynes's slick-sounding tripartite prescription to judge modern economics and find it wanting. The fallen nature of the profession then is the result of not only tasting, but also swallowing whole, Milton Friedman's update of this Keynesian recipe. Though quoting Keynes correctly, Friedman's methodological cake contains only the positive and normative elements. Accepting this unstable version of the original led postwar economics astray. Colander thinks that the inherent contradiction between what Friedman preached, and what he actually did, destroyed his ability to influence, let alone maintain, a benchmark for method. However, the illegitimate division between positive and normative continued to rule the thinking of practitioners as well as the textbooks training the successive generations of economists. Such an analysis provides Colander with a powerful tool for dissecting economics and economists, but it is ultimately a tool that indiscriminately damages critic and object alike:

In reality, Friedman and other post-1950s Chicago economists were enormously interested in the art of economics, but because they did not distinguish between the methodology appropriate to the art of economics and methodology appropriate to the science of economics,

post-1950s Chicago economists were heavy-handed in drawing policy conclusions from scientific laws. They certainly did not follow Neville Keynes's conclusion that the art of economics would 'have vaguely defined limits' and be 'largely non-economic in character'. Their art of economics was photographically sharp and used only an economic lens. (Colander 1992b, p. 114)

Methodology is a subject that makes the eyes of most economists cross. As Colander would admit, economists don't have any incentive to be introspective. There are no clear, tangible rewards for trying to understand what one is doing and why. Too much introspection and we become like so many millipedes worrying about which foot to put down first. Economists want to get on with their work. They don't want to analyse either what they are doing, or what they might be doing in a far better world. Acting upon this very pragmatic stance, questions of methodology and epistemology are best left to take care of themselves. Unfortunately, since Friedman's one great and influential effort at methodology forms the launching pad for Colander's criticism and recommendation, we need to understand, at least at some level, what Friedman (1953) was both reacting against and promoting in those dim days, some fifty years ago.

Though awarded sole credit for articulating this now canonical approach of the profession, the self-named positive methodology of Milton Friedman is perhaps equally the responsibility of Friedman's close colleague George Stigler.⁶ While graduate students at Chicago, both (along with Allen Wallis) had come heavily under the influence of Frank Knight. Knight's determining characteristic was his all-embracing scepticism. This is clear in his reaction to those who wished to turn economics into a science with a capital S:

The author (T.W. Hutchinson) is a positivist, i.e., one of those who always think of 'science' with a capital S... and use it in a context which conveys instructions pronounced in the awe-inspired tone chiefly familiar in public prayer ... The attempt to build a social science on these foundations suggests that the human race ... having at long last ... found out that the objects of nature are not like human beings – are not actuated by love and hate and caprice and contrariness, and subject to persuasion, cajolery, and threats – have logically inferred that human beings must be like natural objects. (Knight 1956a, p. 151)

The staking out of a strident positivistic methodology reflects a *long struggle of escape* on the part of Knight's most admiring students. Friedman's manifesto included an undeniable imperative, which insisted on deciding issues of fact by means of empirical testing. This clearly rejected Knight's use of deductive logic aided and abetted by the institutional structure in which the economy operated.⁷ 'Knight taught economic theory in a loose, 'literary', philosophical fashion-and was antipathetic to mathematical economics' (Patinkin 1973, p. 789).⁸ Such an approach, however, became fundamentally unsatisfactory to Friedman (and Stigler) for at least two reasons. Much of economics, when judged by prewar published articles, seemed decidedly ad hoc.⁹ In reaction, both were pushing for more empirical testing of what might otherwise seem to be pure assertion. Had this not been the case, they might have been quite happy to bury all and any methodological issues. As previously pointed out, practising economists, like any other group of practising researchers, want to get on with their work. They tend not to be particularly introspective about what they are doing. Stigler would have dismissed any such fixation as so much navel gazing.

We can see here a yearning for the precision and decisiveness of a true science (with a capital *S*). Further, the breakthroughs in statistical testing and a burgeoning increase in available data sets seemed to provide an empirical grounding to what had been largely a discipline with scatty bits of data presented according to the preferences of the individual author. More hidden is what I believe to be a second reason for this particular methodological approach. Knight's critical stance did not provide even an approximate defence of market competition.¹⁰ Meaning that a defence of such a system could not be provided by Knight's version of a priori reasoning. Friedman's goal was to eliminate such an obvious Achilles heel, while putting to rest further methodological debate. David Colander should not find it ironic, then, that Milton Friedman's essay created a lack of interest in taking the methodological debate any further. The purpose of the paper was in part an attempt to leave such issues behind (Get behind me Satan!) so that theories could be tested against available data.

Friedman's methodological approach seems to have met a prevailing demand within the profession because it closely agreed with the pre-existing preferences held by a good many economists. It was also officially or quasi-officially adopted in a wide range of textbooks. An encapsulated approach of this sort could be brought up as an officially sanctioned stance, result in a multiple-choice question on an exam, but then be entirely left behind and forgotten for the rest of any course. Even today, it remains in many textbooks because it is easy to teach and to test if anyone feels obliged to do so. In any case, most academic economists find such discussions not to be a particularly congenial chore, if a chore performed at all. Given the low payoff to teaching, most academic economists are more than willing to deal with the issue by avoidance. As a result, there isn't really a strong need by textbook writers to present any sort of digression on methodology. On this point, Colander goes off track a bit. Dethroning positivist methodology would have little impact. If methodologists could agree on an alternative (in essence seizing the high ground), it is likely that methodology questions in textbooks would simply vanish, as they already have in some recent texts. It isn't therefore any degree of accuracy that has made Friedman's encapsulation such a textbook fixture, but rather the way in which it dismisses such issues while leaving an impression of scientific precision.

At the same time, Colander is perfectly correct in claiming that Milton Friedman never employed his own prescribed methodology. Coase (1994a) comes even closer to the truth in saying that no one, including Milton Friedman, ever used the method he propounded, because the approach was essentially impossible to apply even if someone had desired to do so¹¹. Instead, the Friedman article served the purpose of dismissing the need for further methodological discussion. By fusing the prescriptive with the descriptive it shut off any meaningful (anthropological?) examination of how economists actually go about their work. In his article, Friedman indicates how he believes economists should act and then proceeds as if this is in fact how they could or even did act:

When I was a graduate student we were taught a paradigm of how you do research. I've got to tell you, it's all wrong. It's not the way we operate. We don't sit up here and develop hypotheses and go out and test them. That's just not what we do. George taught me that. Milton taught me that. They're wrong! And I understand that. I'm older enough now to figure out that's not the way we do work. (Sam Peltzman, conversation with the author, October 1997)

Colander, though, claims that the wheels came off Milton Friedman's methodology when (faced with the impossibility of using his own stated methods) he opted to follow something like a Marshallian straddle. This desire to claim the status of positive economics for what was essentially applied policy objectives eventually brought him down:

I argue that Friedman, like Alfred Marshall before him, tried to straddle a fence between policy and logical-deductive theory, combining the artistic science of the historical and institutional school with the logical-deductive science of economics under a single category – which Friedman called positive economics. This combination worked for Marshall, in part because in Marshall's day formal logical-deductive economic analysis was still in its infancy, and didn't need to be separated from applied policy economics, and in part because of Marshall's proclivity to avoid taking strong policy stands, hence avoiding normative issues in the art of economics. (Colander 2001, p. 28)

Colander is clear that, had Friedman resorted to John Neville Keynes' tripartite categorisation instead of getting bogged down in the morass of a Marshallian muddle, Friedman could have found a way out of his dilemma. It is here that I feel compelled to disagree with Colander's analysis. (Although I suspect that Friedman didn't perceive any such muddle, that is not the issue here.) The pristine tripartite approach that so attracts Colander's interest has the appeal of abstract clarity but the reality of concrete confusion. Though I could wholeheartedly support returning art to economics in the sense Colander uses the term, I don't see Colander's particular categorisation as furthering this aim.

How the approach works is actually clearer in the original Keynesian version than in Colander's adaptation. Keynes presents the example of tax incidence. Determining the incidence of a tax belongs to the positive branch of economics. It is concerned with finding out what is, meaning ferreting out the facts of what the case actually is. We then move from fact in this scheme to what ought to be, namely the normative issue. The debate over what ought to be (normative science) is every bit as important as the fact-finding stage. In the case of tax incidence, what should be the ideal system? Keynes would not accept the implicit (and sometimes quite explicit) assumption held by some Chicago-type economists that what is, is what ought to be.¹² Moreover, positive science (in the case of economics) makes no sense to Keynes if pursued as an end in itself.¹³ The art, or applied aspect, of this tax issue requires us to determine the appropriate rules to achieve our desired tax incidence objective. Keynes labels this endeavour as an art in distinction to a science, because more than a simple mechanical application of economic theory is required:

But more usually – when we pass, for instance, to problems of taxation ... – it is far from being the case that economic considerations hold the field exclusively. Account must also be taken of ethical, social, and political considerations that lie outside the sphere of political economy regarded as a science. (Keynes 1890, pp. 56-57)

As presented on paper, the division sounds intuitively obvious. Colander then can make his plea for an appropriate division of labour based on the tasks at hand.¹⁴ Specialisation of separate tasks always appeals to economists. But specialisation won't do, if we are essentially talking about output that is to a significant degree a joint product, divisible in theory but not in practice. This would make Marshall's muddle more sensible than Colander would care to admit.¹⁵

Implicit in any discussion of a single aspect of economics are the other two. It is to Keynes's credit that he at least wrestles with this problem, though ultimately arguing that such a division is eminently practicable despite some inherent problems. In fact, in Keynes's discussion, the problem isn't quite as acute since he doesn't argue for different types of economists based on his categorisation. Part of Colander's problem is that he sincerely wants to present his prescription as an entirely doable approach. Given the current composition of the profession, along with its incentives and prejudices, Colander realises that he will be unable to interest economists unless he retains a positive branch that does science with a big *S*. Colander would put off too many of his potential readers, if the idea of economics as positive science (big *S*) were to be given a much-deserved funeral.

Certainly the idea that the truth is out there to be discovered and tested was never part of Adam Smith's approach. For Smith, the order we find in nature is not an existing order but rather an order that we impose to relieve the mental discomfort caused by what, otherwise, would be chaotic observations. For example, using this perspective, Newton did not discover the laws of Nature but rather invented them. They arose out of Newton's imagination rather than being results dictated to him by his own observations:

Philosophy is the science of the connecting principles of nature ... [It] endeavours to introduce order into this chaos of jarring and discordant appearances, to allay this tumult of the imagination, and to restore it ... to that tone of tranquility and composure, which is both most agreeable to itself, and the most suitable to its nature. Philosophy, therefore, may be regarded as one of those arts which address themselves to the imagination. (Smith, 1980, pp. 45-46)

The use of the science term (especially science with a big *S*), like a highly starched lab coat, makes me want to slip into something more comfortable. However, I doubt that this insistence on science can be reduced entirely to an underlying core of insecurity, like that which provokes a teenager into attempting to grow a wispy moustache to assert his masculinity. It is more (again consistent with Friedman's objectives) a desire to emphasise the precision and certainty of economics, even if implicitly sacrificing something in terms of accuracy. Chicago could not make space for anything resembling economics as an art, since that could have undermined a basic belief in the assured effectiveness of the price system and the efficiency of the market. Truly understanding what is (in this view), allows you to understand that it is what should, or at least what can, be. Who then needs an artistic sensibility to accomplish policy objectives? The development and subsequent promotion of an approved methodological approach complemented the marketing of a particular Chicago type view and its associated set of policy recommendations. Effective promotion necessitated claiming a certain scientific vision, in this case involving verifiable predictions:

There's a lot of salesmanship, there's a lot of taking positions, defending them. The facts will win out. I'm not saying that we're not in that sense correct. The facts do win out. But the process by which that happens is not the clean one of scientific method rigorously applied all the time. I've come to understand that, and George [Stigler] had a big part in that process. He staked out a position. People attacked it. The empirical work goes back and forth and after a while it does sort itself out. (Sam Peltzman, conversation with the author, October 1997)

Coase (1940a) points out that understanding rather than predictive accuracy is what is required. Making sense of what we observe is the prescriptive requirement, rather than simply correctly guessing some next-period scenario. In this regard, assumptions do matter. Though, in his famous essay, Milton Friedman dismisses any consideration of the underlying reality of theoretical assumptions (a most un-Marshallian stance), it is doubtful that he ever regarded any of his own assumptions in that same dismissive light. Certainly his close colleague George Stigler did not. But, in quite an essential sense, Friedman did not want a discussion that focussed on the relative accuracy of assumptions when it came to competing theories. This would have had the unfortunate consequence of shifting the terms of debate to what Friedman would see as sterile territory. Meaning, he would place himself on rather treacherous ground, forced to defend an inherently weaker position. Certainly, on such terrain, the assumptions forming the foundations of perfect competition would face an uphill battle against those comprising monopolistic competition.¹⁶

The core problem is that the sort of positive science that Colander accepts demands an outside objective observer. Here, in a strange fashion, Colander is guilty of the same category of transgression that he cavalierly assigns to Milton Friedman. By blurring the supposed division between positive and normative, conservative scholars (like Friedman) were able to identify what is (in terms of market competition) with what ought to be and to recommend policies accordingly. But, in order to counter the worst excesses of such an approach, Colander insists on a strict categorical separation. Unfortunately, he doesn't feel any need (unlike Keynes) to provide a reasonable defence for his stance. In both cases, expediency seems to be driving the logic of the debate. A more effective way to counter ideological-based presentations might be simply to admit the impossibility of moving with any convincing rigour beyond one's underlying preferences.¹⁷

What is worse is that Colander is never clear-cut about the exact nature or extent of the positive end of economics. At times, it seems to consist purely of deductive logic, like much of the work involving general equilibrium theory. However, Colander strongly indicates that positive economics involves empirical testing. Two problems quickly arise (both of which he acknowledges to a certain extent). Much of the theoretical work can't be tested and much of the empirical work fails to convince.¹⁸ Nor can a purely positive basis exist for choosing the appropriate evidence with which to test these general theories of fact. Data to start with are never generic or abstract but rather specific by definition.¹⁹ But the type of data-mining that Colander champions as a substitute for more traditional statistical methods does not seem an overly productive route to explore. Too often, the heavy use of computer analysis and simulation acts as a substitute rather than as a complement to economic intuition and thought. An even more fundamental issue is that Colander divorces future specialists in positive economics from the type of economic intuition demanded of those practising the art of economics or framing the normative objectives of the discipline. Equally, applied workers would lack much of the rigorous foundations needed to handle the fruitful output of the positive division (if any should exist). Thus, models would be handled mechanically and perhaps incorrectly (as econometric packages often are today).²⁰ You could caricature Colander's approach as involving billions of monkeys grinding out billions of theories like so many multi-flavoured sausages, while billions of orang-utans throw billions of these sausage theories against brick walls to see if any of them stick:

... my approach calls for two increasingly polar branches of economics; a positive branch – a highly abstract, deductive, branch in which work is subject to rigorous empirical tests before it allows an insight to become a law, and an art branch: a loose, non-formal, application of the theoretical insights to the real world ... In the art of economics one accepts the general laws and models that have been determined by the profession and one tries to apply the insights of economic models to real-world problems. Applied policy economics has nothing to do with testing a theory; it has to do with applying the insight of that theory to a specific case. (Colander 2001, pp. 52, 48)

Colander is simply trying too hard. The cool, clear divisions he adopts from John Neville Keynes undercut rather than advance what I would concede to be an excellent aim, reviving economic intuition (the art of economics). A more congenial environment in which to advance such goals might be just that Marshallian muddle which, at times, Colander seems to scorn. By prescribing what remains an impracticable and too complex set of rules for reforming economics, he is in danger of generalising his own preferences. A more productive option would be for him to observe how economists actually operate, an ability he possesses and repeatedly demonstrates in the highest degree. No wonder that Marshall shied away from all and any such detailed methodological discussions!

Instead of John Neville Keynes, Colander should keep in mind the working methods of his son John Maynard Keynes, who ultimately aligned himself more with his teacher Alfred Marshall than with his father:

He [Keynes] was a student of Marshall, and he said, 'The theory of economics is a method rather than a doctrine, an apparatus of the mind, a technique of thinking which helps its possessor to draw correct conclusions.' ... This quotation is the epitome of the Marshallian method. It tells us to use economics as an engine of analysis, not as a set of principles. If we keep that Marshallian method in mind, we will be giving our students a good foundation in understanding macroeconomics and we will be treating Keynes the way he should be treated-as an economist who carried on an important tradition in Classical economics. (Colander 2001, p. 90)

McCloskey has made the same point about Keynes, though from the perspective of rhetoric:

I would claim that Keynes is one of a long if thin line of economic sophists as against the massed phalanx of economic Platonists. Most economists have been Platonists. The Platonists believe that Truth is out there on the blackboard somewhere, or less commonly out there in the econometrics or in the experiment. That their program has failed repeatedly does not discourage them any more than it has discouraged Platonists in philosophy these two-and-a-half millennia past. They carry on seeking the one immutable Truth for the ages, and scorn the practical sophist like Maynard Keynes making arguments for the day. (McCloskey 1996, p. 233)

This represents a more pragmatic approach that identifies economics as an art employing a number of tools and methods. Its inherent messiness explains why John Maynard Keynes thought that, despite the commonplace nature of the subject matter, a good economist (like a *good* man) was hard to find. Rather than employing any rigid categorisation, it can be more helpful to adopt Robert Solow's

simple division of economists into system builders and puzzle solvers. By doing so, much of the debate then centres on the way in which economists solve puzzles. For instance, Chicago economists largely felt no need to build systems since, to Friedman or Stigler, Adam Smith had already accomplished all that needed to be done in that respect. Their challenge was to show how any observation, no matter how seemingly aberrant, could be explained by means of the self-interested choice of rational economic agents. For Solow, then, economics is not Science with a capital S:

...economics should try very hard to be scientific with a small *s*. By that I mean only that we should think logically and respect fact ... My hunch is that we can make progress only by enlarging the class of eligible facts to include, say, the opinions and casual generalizations of experts and market participants, attitudinal surveys, institutional regularities, even our own judgments of plausibility. My preferred image is the vacuum cleaner, not the microscope. (Solow 1994, pp. 202-203)

Such an approach lacks the glamour of an elegant categorisation, but captures much of what Colander wants to accomplish without introducing a counterproductive definitional straightjacket.

David Colander's Brave New World

We shall once more value ends above means and prefer the good to the useful. We shall honour those who can teach us how to pluck the hour and the day virtuously and well, the delightful people who are capable of taking direct enjoyment in things, the lilies of the field who toil not, neither do they spin. (Keynes 1973b, p. 331)

True, Keynes allowed some 100 years for this to occur and placed caveats concerning war and population growth as hedges against his predictions. But even with some 30 years still to run, very few people are looking forward to such a millennium. The economic problem is not close to being solved (though progress, whatever that may mean, has been made).

The quote from Keynes, though, should be a warning to all economists concerning the dangers of peering too confidently into the future. As Galbraith is said to have remarked, 'Economists forecast, not because they know, but because they are asked.' No matter how enticing, no matter what inducement, the black hole of pontification should be avoided at all costs. Strength of character comes from resisting all and any opportunities to offer accurate glimpses of the future, as well as avoiding these obvious occasions on which to pronounce dubious judgments. Colander fails to let the fateful cup pass by. Because he does in fact give in to the allures of prognostication, of being wise before his time, the last two chapters of his current volume inevitably remind me of one of Keynes's essays in persuasion (*Economic Possibilities for Our Grandchildren*). These are all essentially economic bedtime stories for well-behaved infants. Needless to say, I think such attempts weaken, rather than strengthen, what until this final section has been an excellent collection.

In particular, the last chapter is fey where it means to be fun. This may reflect simply my own distaste for science fiction as my choice of pulp reading material (favouring detective novels instead). Nonetheless, when economists wax lyrical about some achievable golden age for the economics profession, I feel

queasier and more embarrassed than if the same person were to wax lyrical about his or her sex life. The major difference is that the latter might be of more interest and certainly of more practical applicability (though this is possibly a stretch given some members of the profession).

The problem as stated in the first five parts of the volume certainly exists. The profession tends to take abstract models and jump from the implications of those general (but at the same time highly restricted models) to policy conclusions. This makes no sense, displaying instead finely attuned symptoms of what Colander labels 'artiphobia', essentially a hard-core contempt for serious policy analysis. The remaining challenge, then, is the way in which the status quo might be changed. What is interesting in much of this attempt at constructing a future vision is that it is clearly at odds with Colander's well-honed skill of looking for and locating prevailing economic incentives. Instead, he seemingly abandons his concrete practicality and opts for unwarranted flights of fancy.

Usually, the all too sensible Colander takes the only consistent approach to understanding what economists do and why economics is as it is today. This also provides the only reasonable basis for trying to peer into the future, if we cannot be persuaded to refrain from all such activity. Colander is at his best when he searches for and locates those dominant incentives that motivate individual economists. Certainly, we can wish that those incentives were different. But, in reality, the self-selection that goes on in graduate school, and in the job market thereafter, almost ensures that those who do succeed will be comfortable (for the most part) with what they are doing. This would make change from within unlikely:

The below-age-45 professors are now almost all highly technical economists who are comfortable with the existing situation. Given current selection procedures, in 20 years almost all criticism of the profession will come from without rather than within since potential critics will never enter the profession. (Colander 2001, p. 145)

This inconsistency in his approach (between wish and reality) comes when Colander seems to think that preaching to the existing congregation of largely self-satisfied economists will initiate tangible, discernable change. At this point, he comes close to agreeing with Stigler that the internal dynamics of the profession must dominate. 'On my reckoning, the immediate stimulation to advances in economics has overwhelmingly been the attempt to improve on other economists' work' (Stigler 1997, p. 106). Colander is certainly leaning this way when he somehow assumes that grabbing the methodological high ground and imposing the fruits of victory on the leading squadron of textbook writers will shape a new generation of economists. This, as I have previously pointed out, seems more of a flight of fancy than a calculated likelihood.

Where Colander is on stronger ground is when he ironically resorts to standard Chicago reasoning. At the moment, the economics profession has a monopoly on granting accreditation to practising economists. This unofficial licensing procedure is done via the awarding of PhDs. As Colander carefully argues, the profession selects out all but those who fit the prevailing mode of theorising. If, though, the market for economists (teachers, policy makers, business analysts) expresses sufficient dissatisfaction with the existing model, alternative sources of producing economists more suitable to the appropriate market will appear. In simple terms, clear potential for gain will produce academic competition.

It is also true, as Colander points out, that one clear venue of change lies in the type of teaching we do. As it now stands, undergraduate teaching is often

regarded as a necessary evil. According to Colander, it is an awkward compromise; a series of badly related models with enough flesh slapped on to make it palatable in a very loose sense. In its place, Colander would prefer to substitute teaching the art of economics. The drastic nature of such a reform should not be underestimated. This is just the sort of economic thinking that John Maynard Keynes found to be in such scarce supply.

I have tried to teach something like the art of economics to my final-year undergraduates. I persist due to what must be an ingrained and improperly acknowledged streak of pure masochism. First, you have to try to disabuse students of what economics is. (No, it isn't simply memorising pointless models.) Next, you have to supplement whatever text you use with relevant case study material. Then, you have to be firm against the whining of students who simply don't want to put in the requisite time and effort. They have become acclimatised to a mindless level of committing to memory meaningless material the week before an exam. (Colander (1992d) himself points this out in some of his earlier writings.) I suspect that students may walk away with something from my efforts. This is not difficult, since they often walk away from standard courses with absolutely nothing. But McCloskey ultimately may be right: perhaps you can't teach undergraduates the art of economics. At best, you can only hope to provide a glimpse of what that process is:

Our undergraduates, and most of our graduate students, can learn a lot *about* economics that they can use in life. But unless they join us in that slog up the spiral they are not going to think like economists. Perhaps we should redirect our energies, teaching them economic facts and stories, say, or inspiring them for later study. (McCloskey 1992, p. 239)

Despite some purple prose, flights of fancy and the dreaded sin of pontification, David Colander once again provides us with a nourishing and welcome diet. His work is thought-provoking and should (but probably won't) cause both those in the mainstream and those critical of the profession to reassess their thoughts. What more could anyone desire? I await with some anticipation David Colander's next census of the economics trade in 2011. He looks forward to some noticeable changes in the profession with what I would describe as measured optimism. Being less buoyant by nature, I would expect after canvassing whatever changes are wrought over the next decade simply to respond, '*Plus ça change, plus la même chose.*'

If a discipline changes, it is because of informed complaints about what the discipline is doing and because of what critics of the discipline are doing. I see my challenges as part of the process through which the invisible hand of truth operates. (Colander 2001, p. 14)

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Notes

1 Patinkin makes it clear that though Knight was a trenchant critic of the market system, his radicalism was conservative by nature. Rather than trying to insist upon any unalloyed virtue for competitive economic systems, he was content to claim that

whatever grave failings it might have, alternatives offered the risk of even worse outcomes:

Primitive society was wise in its conservatism, for it knew at least that the group had previously lived somehow, both as individuals and as a group. (Knight quoted by Patinkin 1973, p. 801)

2 Colander clearly realises that any endogenous change must reflect a change in graduate education. He frankly doesn't see this happening nor does he expect it to happen in the future. Instead, he sees change in the economics profession coming by means of external competition:

I am a realist; I do not expect graduate economics programs to change. Thus I see applied economists gravitating more and more to public policy schools and interdisciplinary programs. As the rigor and selectivity of these schools and programs increase, more and more of the demand for applied economists will come from them, not from graduate economics programs. (Colander 2001, p. 146)

This is a clear reiteration of his stance ten years earlier:

There is no easy way out of the current state of economics. It is a self-reinforcing state that will require Herculean efforts to change, because change goes against the very interest of individuals who would be required to make that change. Unfortunately, history provides few examples of such changes from within. Instead institutions go through the motions of implementing change, but they do not change. That seems to be the path chosen by the economics profession. (Colander 1991, pp. 79-80)

Yet there are other points in the volume where at least an impression is created that economists will inevitably listen to sweet reason no matter what the relevant issues of self-interest may be.

3 The quote is well known in the profession:

But, soon or late, it is ideas, not vested interests, which are dangerous for good or evil. (Keynes 1972a, p. 384)

This is a real Cambridge call to arms. It is not the sort of clarion call you would expect to hear coming from the environs of Chicago.

4 Life may be brief, but not that of many economists. I admit to having made no rigorous investigation, but consider the age reached by many well-known economists, starting with Friedrich von Hayek.

5 The punch line of the joke, quoted on the book's dust-jacket, has a drunk telling a policeman that he is looking for his house keys under the street lamp, not because they are there, but because the light is better.

6 This can be traced in letters written between the two in the years leading up to the publication of Friedman's article. Certainly, a version of the positivist position is presented in Stigler's 1949 lecture on monopolistic competition. Milton Friedman willingly admits his debt (conversation with the author, August 1997). According to Mark Blaug (conversation with the author, June 1998), one of Stigler's former students, George Stigler always felt a bit miffed about the lack of recognition accorded to him.

7 Empirical testing became an almost religious tenet of the Chicago School, charted from Milton Friedman's laying out the path to righteousness to George Stigler's (1965) AEA Presidential Address whose evangelistic tone reveals the coming promise of salvation through data. Such attitudes were poles apart from that of their teacher, Frank Knight:

Insistence on a concretely quantitative economics means the use of statistics of physical magnitudes, whose economic meaning and significance is uncertain and dubious... In this field, the Kelvin dictum

very largely means in practice, 'if you cannot measure, measure anyhow!' That is, one either performs some other operation and calls it measurement or measures something else instead of what is ostensibly under discussion, and usually not a social phenomenon. (Knight 1956a, p. 166)

- 8 This extended to questioning the rationality proposition in economics:

Another basic element of Knight's teaching was the view that the study of man – including economic man – could not proceed within the same deterministic framework as the study of nature. For the behavior of man must also express his freedom of will – to which there is no counterpart in the physical world. (Patinkin 1973, p. 790)

- 9 Armen Alchian (conversation with the author, October 1997) characterised much of this output as garbage (or words to that effect). Similar sentiments were expressed to the author by a number of other Chicago School types. (Though Alchian never was connected officially with Chicago, Samuelson, for one, has termed him 'more Catholic than the Pope' in terms of Chicago style doctrine (conversation with the author, October 1997). As Colander points out:

In many ways the modern movement to applied modeling is laudable. It is empirical and is an attempt to avoid the pontificating which characterized earlier periods. Modern applied modeling looks to the empirical evidence through models. (Colander 2001, p. 164)

- 10 The defence that Friedman and Stigler eventually would erect ended up equating market outcomes with ethical results. Like Knight, they maintained 'freedom of choice' as an objective in and for itself. Unlike Hayek (as well as Knight), they wouldn't accept that individual freedom could be inconsistent with distributive justice. Such an allowance naturally brings in questions of trade-offs and possible state intervention:

Moreover, neither the income distribution which we observe nor that which would result from the perfect competition of theory is to any great extent in conformity with any acceptable ethical standard. (Knight 1956b, p. 95)

- 11 Coase, like Colander, is bothered (perhaps for different reasons) by a seeming confusion between positive and normative standards. But even as a prescription only, Coase cannot see how it could be a fruitful one:

If choosing theories in accordance with Friedman's criteria is to be treated as a positive theory, economists would need to adopt a procedure somewhat similar to the following. When a new theory is advanced, economists would compare the accuracy of its predictions, preferably about 'phenomena not yet observed,' with that of predictions of the existing theory and would choose that theory which gave the best predictions ... An insistence that the choice of theories be made in accordance with Friedman's criteria would paralyze scientific activity. (Coase 1994, pp. 23-24)

- 12 This implicit position can be seen in much of the later work of George Stigler and Gary Becker. It is best formulated in Armen Alchian's dictum that 'What is, is efficient'. The onus then becomes one of arguing that a seemingly inefficient result should be a desired objective.

- 13 It is important to keep in mind that, under Keynes's tripartite scheme, no one division is privileged. The point of economics is ultimately (for Keynes and Colander as well) to achieve certain objectives (economics being a distinctly applied discipline). The positive branch provides the basis for discussing normative objectives and guiding practical policy applications:

No one desires to stop short at the purely theoretical enquiry. It is universally agreed that in economics the positive investigation of facts is not an end in

itself, but is to be used as the basis of a practical enquiry, in which ethical considerations are allowed their due weight. (Keynes 1890, p. 47)

14 Colander does intend specialisation to be understood in the strictest sense of the word. What I doubt is the possible gains from trade by groups that share no core language or understanding (or perhaps just enough to seriously misunderstand each other). No common medium of exchange (the role that money plays in standard commerce) exists to overcome this basic lack of knowledge. The inherent transaction costs attached to such exchange would then become simply prohibitive:

Such specialization opens up the possibility of trade, and ideally economics would have two types of economic researchers making trades – formal theorists dealing with highly complex and abstract analysis almost devoid of institutions, and intuitive institutionally-based theorists dealing with real-world institutions and informal abstract analysis. The MIT approach of simple formal models would make sense if there were not increasing returns to scale in research, but it seems obvious that there are increasing returns, so not to take advantage of them and not to encourage specialization is, in my view, a highly inefficient approach to understanding. If you are going to be formally abstract, then go all the way and don't let the real-world issues contaminate the purity of your analysis. If you are going to be concerned with the real world, don't formalize more than the least precise real-world element. (Colander 2001, p. 98)

15 Marshall, writing his *Principles* at the same time that John Neville Keynes was writing his volume, disagreed with Keynes sharply, though, given his nature, Marshall tried to minimise those differences. Still, he saw little use for the sharp divisions as set up by Keynes:

I never discuss any line of division or demarcation except to say that Nature has drawn no hard & fast lines, & that any lines Man draws are merely for the convenience of the occasion: & shld never be treated as though they were rigid. (Marshall writing to Keynes, quoted in Coase 1994b, p. 168)

16 Bronfenbrenner (1962), a Chicago contemporary of Friedman and Stigler, points out their tendency to favour the simplest over relatively more realistic hypotheses:

As an indication of the relative downgrading of 'realistic' hypotheses H_2 , one finds at Chicago a correlative underemphasis on what the detailed facts are (as distinguished from the figures), and on the narrative or insightful history of how the facts developed to be what they are. (Bronfenbrenner 1962, p. 75)

17 Demsetz doesn't deny the role that individual preferences play in policy positions. He does claim that such elements can be sifted out, at least to some degree, based upon underlying analysis. But this simply takes the argument back a step. Acceptance of certain forms of argument and evidence has to be predicated once again (at least to a given extent) on deep-rooted preferences:

All policy preferences, of course, ultimately derive from a view of the world combined with one's preferences. The taste component remains a personal matter, but the view of the world can be convincing to others or not. Opposition to a tariff because of its impact on consumers is based on a convincing view of the world ... because of our agreement as to the correctness of the underlying analysis. (Demsetz 1989, p. 38)

18 Clearly Colander agrees that econometrics as it is currently practised has failed to live up to its early postwar promise. But he is not sufficiently clear as to what sort of empirical evidence would be a convincing substitute. Simulations and data mining seem

similarly flawed. The problem is in thinking that there can be a knock-down test or body of evidence. As McCloskey has insisted, a resort to empirical argument is simply another form of rhetoric, and not necessarily the most convincing form (see McCloskey 1994b for just one example). In a complementary fashion Solow indicates that:

The austere view is that ‘facts’ are just time series of prices and quantities. The rest is hypothesis testing. I have seen a lot of those tests. They are almost never convincing, primarily because one senses that they have very low power against lots of alternatives. There are too many ways to explain a bunch of time series. (Solow 1997, p. 202)

Similarly Coase maintains:

... I doubt whether such studies have often led to a change in the views of the authors. My impression is that these quantitative studies are almost invariably guided by a theory and that they may most aptly be described as explorations with the aid of a theory. In almost all cases, the theory exists before the statistical investigation is made and is not derived from the investigation. (Coase 1994a, p. 26)

Taking this one step even further, it is hard to cite even one theory in economics that has been discredited once and for all. Unfashionable theories bide their time, are eventually resurrected, tarted up and described as new breakthroughs. In this way they gain another lease on life. This may be true to a certain extent in all the sciences, natural as well as social, but not to the same extent as in economics.

19 Unfortunately, I don’t find that Colander’s explanations provide sufficient clarity to dispel the problems raised by his strict categorisation of positive economics versus the art of economics. I quote an extended passage to give you some of the flavour of the underlying confusion. At least in my mind, Colander’s arguments and explanations raise more questions than they answer. At a sufficiently deep level, I’m not at all sure what the following statement actually means:

In the art of economics one accepts the general laws and models that have been determined by the profession and one tries to apply the insights of economic models to real-world problems. Applied policy economics has nothing to do with testing a theory; it has to do with applying the insight of that theory to a specific case ... Similarly, once a generalized model is developed, it makes little sense to test specific implications of that generalized model, other than as an educational exercise. If one believes the generalized model, one believes it applies; applied policy economics has nothing to do with empirically testing theories; it has to do with applying theories – theories that one is willing to tentatively accept as true – to the real world. To do that, one must add back into the model all the assumptions that were made as it was being generalized. The question in applied policy economics concerns whether the theory fits the application, not whether the theory is true. The applied policy issue is: How can the insights of positive economic theory be translated into real-world policies which achieve society’s goals. (Colander 2001, pp. 49,51)

20 Colander’s partial solution to the lost art of economics requires quarantining applied mathematicians to a reservation he labels positive economics and shrinking their numbers by cutting down funding for their projects. They could only survive as pure researchers, since they would be incapable of teaching the art of economics (the new standard economics curriculum). But the usefulness of their work would then seem to me extremely limited. In this regard, Solow observes:

What I am saying is that the economist cannot dispense with keeping his or her eyes open, looking around, and forming judgments about what makes

sense and what is simply farfetched. Those cannot be uncritical judgments, but judgments to be defended by appeal to observation and to logic. (Solow 1997, p. 203)

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