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Evaluation of the PhD thesis of Jason Allan Collins

The thesis of Jason Collins is definitely an ambitious endeavour. It models changes in population-level traits such as innovativeness, technological progress and population growth based on the idea that humans differ genetically in traits that contribute to these phenomena, allowing either slow or (sometimes) rapid evolutionary change to occur. In addition to a brief introduction outlining the contents of the thesis (chpt 1), the thesis comprises a chapter (chpt 2) addressing the links between economic theory and evolutionary and genetic work on humans; a model on the quantity-quality trade-offs leading to an interplay of population and economic growth (chpt 3); a model of evolving innovativeness (chpt 4); a model of male conspicuous consumption in the presence of female preferences (chpt 5); and a model that shows that genotypes that are high fecundity after an environmental change will predominate over low-fecundity ones, when before the change these alleles were neutral (chpt 6).

It is always a great challenge to review interdisciplinary work. I feel it is therefore appropriate for me to describe my own background, as I have training both in mathematical methodology and in evolutionary biology, but considerably less so in economics. Some of my observations surely will stem from this background: a thesis that is strictly in my field would usually be directed to satisfy certain goals, which may or may not be the norm in current economic writing.

Thus, I am writing from the viewpoint of a mathematical modeller in the evolutionary sciences. The candidate shows undoubtedly excellence with respect to original thinking and mathematical competence; he is able to advance previous thought and provide new insights on a path that few have taken before. Some models are really very simple (e.g. chpt 6 when described the way I did above) but simplicity can also be advantageous if the context in which competition among alleles is new.

I also find weaknesses in mainly two areas, which are interrelated.

First, there is the challenge that every student encounters: how to distil the relevant literature out of a potentially vast number of papers and books that might be relevant. Here I found that the candidate has perhaps not been at the receiving end of much training that would have stressed the need to evaluate existing literature critically; the symptom of this is that papers are cited if they have been inspirational for constructing his argument, but in places the message is extrapolated far beyond what the original paper intended to show, while contrary evidence or work emphasizing other aspects (e.g. phenotypic plasticity as opposed to genetic change) is not paid much attention to. Now, being inspired is of course fine as such, but in my own field examiners tends to raise eyebrows if students do not display sufficiently thorough appreciation of limitations, caveats and alternative explanations that are present in every study.

The other area that I would consider a weakness (at least for a thesis that would be more firmly rooted in the traditions of biological modelling) is an approach where a model is clearly built to support a particular argument: on several occasions, the candidate states that parameter values investigated are intentionally restricted rather tightly around the range that can best reproduce certain observations about humans. While I appreciate the honesty with which this approach is adopted and also explicitly stated, this also narrows our view towards a particular narrative, i.e. invites us away from critical examination of the robustness of the results obtained.

Given that we in reality know very little about whether, say, it was genes causally linked to thrift and hard work that caused economies to shift in recent times (a definitely interesting idea [attributed to another scholar cited in the thesis] — but one could also quote more objectionable traits that can documentably lead to getting ahead at the expense of others), or the extent to which demoraphic transition involves genetic change or, instead, simple behavioural plasticity responding to quality-quantity tradeoffs using old rules but with new outcomes when the environment changes, it would have been refreshing to see what each model produces "as a whole", together with a discussion of the likelihood of each particular outcome (including but not limited to the best-fitting case). A thorough examination of how robustly the outcomes follow from the assumptions is part of the current standard of eco-evolutionary modelling.

It is, of course, very challenging to examine what precisely happened during the recent evolutionary past of precisely one species. Then, the importance of thinking about model assumptions is particularly pressing. For example, in a demographic transition context, how should we defend the assumption that the

initial fertility decline is an unavoidable shock while the subsequent potential recovery is an evolutionary response? Or, in a mate choice context, for example, humans are a species in which direct benefits (material goods being made available by a mate for its partner) are known to play a huge role, so one might have to spend time defending the assumption that there is an unspecified 'quality' that impacts offspring numbers that a female can produce. Given that male quality exists irrespective of how much the male has conspicuously consumed might be considered a less than ideal modelling choice.

That said, there is never a model with no such quibbles! The main reason I feel compelled to write a lot of detail in my report above is that the Australian system of finishing a PhD does not give the examiners the chance to engage in on-site debate with the candidate, which would have been the ideal setting in which to ask the candidate to defend his approach and test the depth of understanding of such issues. I am sure such an event would have been a great scientific discussion with a lot of intellectually stimulating exchanges.

Therefore, as a whole, I found this thesis inspirational; it shows competence and it is also a brave piece of work, given that interdisciplinarity can attract criticism from both sides when some established rules of a mature field (even if they usually exist for a good reason) have not been fully obeyed. The thesis is sometimes provokative, as it paints quite a specific picture of what has happened, with a rather broad paintbrush approach when it comes to suggesting how these ideas might be tested in the future. I would not penalize a thesis for provokativeness, but the above weaknesses (how robustly known literature supports a view; how robust are the model assumptions and outcomes) also prevent me from rating the academic work as highly as I'd do if thought-provokingness alone was the criterion.

Yours sincerely,