EVOLUTIONARY BIOLOGY

Postgenomic Musings

Massimo Pigliucci

Everyone in biology keeps predicting that the next few years will bring answers to some of the major open questions in evolutionary biology, but there seems to be disagreement on what, exactly, those questions are. Enthusiasts of the various "-omics" (genomics, proteomics, transcriptomics, metabolomics, and even phenomics) believe, as Michael Lynch puts it in the final chapter of *The Origins of Genome Architecture*, that "we can be confident of two things: the basic theoretical machinery for understanding the evolutionary process is well established, and we will soon be effectively unlimited by the availability of information at the DNA level." Others (*1*–*4*), among whom Lynch for some reason singles out Sean Carroll (*5*) for special criticism, are a bit more skeptical. They maintain that we are still missing some explanatory principles accounting for the complexity of living organisms and that the tsunami of "-omics" information, although valuable, is actually hitting a field that is unprepared for it, both conceptually and in terms of analytical tools.

The Origins of Genome Architecture

by Michael Lynch

Sinauer Associates, Sunderland, MA, 2007. 510 pp. $59.95. ISBN 9780878934843.

But before we get to the controversy, let me say that the book's first 12 chapters are a must-read for anyone interested in the evolution of genomes. This *Origins* represents a serious, valiant, and highly scholarly attempt at making sense of the new data provided by the genomic revolution. To that aim, Lynch deploys the full array of conceptual tools that make up the modern synthesis paradigm in evolutionary biology.

Lynch (an evolutionary biologist at Indiana University) guides us through a host of fascinating phenomena, from the evolution of sex chromosomes to the disappearance of operons in eukaryotes, from the population biology of transposons to the mechanisms of origin and loss of introns. Throughout, he reminds evolutionary biologists (and perhaps lets some molecular biologists know for the first time) that the "population thinking" so central to the modern synthesis, and in particular the solidly developed theory of population genetics, ought to be part of any postgenomic understanding of molecular evolution.

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One of the central theses of the book is that natural selection is not necessarily the central evolutionary mechanism, as quite a bit of the details of genomic structures and evolution can be accounted for by invoking the neutral mechanisms of mutation, recombination, and drift. Lynch is certainly correct on this point, and he backs his argument with much empirical and theoretical detail. Yet, we must be hanging around with different crowds, because I hardly know anybody who would seriously contend that evolution is just a matter of natural selection. Lynch himself cites the now-classic paper by Gould and Lewontin (*6*) railing against "panselectionism," and most evolutionary biologists have already gotten the message.

But the really interesting, and certainly debatable, part of the book is its last chapter, "Genomfart" (from Swedish for "the way forward," we are told). There Lynch honestly states at the onset that he is going to shift gear and engage in an advocacy piece, something that I found refreshing: scientists have opinions, and they are most interesting when they are controversial. I have little patience for the pretense of a "fair and balanced view," when we all know that balance comes out of discussions and disagreements among peers, not from the point of view of a single individual (*7*). Lynch's thesis, as mentioned above, is that the theoretical apparatus of evolutionary theory is complete and that people should stop whining about missing pieces and the need for a new synthesis: just study your population genetics and everything will be all right.

This is, of course, a perfectly respectable opinion—although the repeated, if oblique, parallels Lynch draws between legitimate scientific opponents of his view and creationists who advocate intelligent design become increasingly irritating by the end of the chapter. Lynch, however, seems convinced that all that evolutionary theory has to explain is changes in allelic frequencies within populations. If that were indeed the case, the job is done, and we are now left with simply systematizing the huge amounts of information coming forth from genomic studies. As Carroll complains [in (*8*), quoted by Lynch], this is a rather uninspiring theme.

Lynch's comment that science isn't about inspiration (I guess it truly must be about perspiration), however, misses Carroll's point: what the modern synthesis has not given us is a theory of form, and applying population genetics to genomics—as valuable an exercise as that is in its own right—isn't going to give us one either. As much as genes are fundamental to the evolutionary process, there is much more to biology than genes and their dynamics. The very fact that molecular biologists are now talking (albeit often naïvely) about higher-level "-omics," all the way to phenomics, means that they appreciate that genomes are only a part of the story, arguably the simplest part to figure out.

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Lynch correctly identifies complexity, modularity, robustness, and evolvability as some of the key concepts of the recently emerged field of evo-devo (evolution of development), but he dismisses them as "buzzwords," glossing over mounting empirical and theoretical efforts aimed at articulating these notions and exploring their relations with the standard modern synthesis. Lynch makes a big deal out of the claim that the burden of proof is on people who think these and other ideas will be useful during the shaping

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of an extended synthesis in evolutionary biology. Fair enough, although one has only to read some of the several books in this field that have come out during the last decade to see that people aren't simply shooting the breeze.

But the burden-of-proof argument cuts both ways. Lynch boldly claims that "many (and probably most) aspects of genomic biology that superficially appear to have adaptive roots … are almost certainly also products of nonadaptive processes," but all we get in support of this position is a plausibility argument. Even though I agree with his contention that neutral processes have contributed to the evolution of genomes (but not, I am willing to bet, of phenomes) to a much higher degree than usually acknowledged, the evidence Lynch adduces is far from overwhelming. Throughout the book, we are treated to a series of plausible scenarios about the evolution of introns, transposons, spliceosomes, and the like. These scenarios are backed by clever applications of population genetics theory (most of which has been developed for simple one- or two-loci systems, not for genomics), but they hardly meet the high standard of historical proof (if there can be any such thing).

Lynch claims that nonadaptive processes should be considered as null hypotheses, but this gives him the unfair advantage of shifting the burden of proof against selective scenarios. What justifies this move is not at all clear, because Lynch thinks of selection as only one of the four fundamental mechanisms of evolution: if it is one of four, why treat it as a special category? To see how easily the table can be turned, just consider Dennett's diametrically opposite position that natural selection should be treated as the default explanation for complex phenotypes, unless one can show that it didn't play a role (*9*). A truly fair and balanced approach is to simply treat any hypothesis as an equal contender in the set of plausible explanations, and see how it fares against its opponents without the advantage of playing on a home field.

Ultimately, the main reason we need an expansion of the modern synthesis was pointed out by Popper several years ago: "[the Darwinian theory] is strictly a theory of genes, yet the phenomenon that has to be explained is that of the transmutation of form" (*10*). Lynch's contribution in *The Origins of Genome Architecture* goes a long way toward completing our explanation of how genes (and genomes) change over time. Nonetheless, although indeed necessary, population genetics is not even close to sufficient for understanding how phenotypes evolve. There is much more to do, and a large undiscovered country lies out there. Let's take a look.

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HISTORY OF SCIENCE

The U.S. in the Rebuilding of European Science

Jean-Paul Gaudillière

One day in 1946, the French biochemist Jacques Monod visited the laboratories at the marine biological station in Woods Hole. The visit made a strong impression on him, as he noted in a letter to his wife:

> Very big laboratories, huge library, three seminars a week, impressive organization, etc. The idea that 350 biologists are working here, that they accumulate observations; that they complete experiments, measurements, weightings; that they operate Warburg apparatus, centrifuges, and microtomes while piling up articles. All this has a somehow depressing effect on me. I am used to thinking that my work is something rare, highly personal, something I have almost invented. In my understanding, this is what makes it valuable. Here it is no longer possible to cherish such illusions. I feel the same way I felt on [Jones] Beach, when facing 50,000 cars and 500,000 bathers.

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This reaction was not rare. European scientists traveling in the United States during the first decade after World War II experienced mixed feelings. They perceived the U.S. research system simultaneously as a model, a challenge, and a threat.

Such ambiguous relationships are at the center of John Krige's *American Hegemony and the Postwar Reconstruction of Science in Europe*. The issue of the role the sciences played in transatlantic affairs after 1945 is important. We all suspect that U.S. aid was as crucial to the reconstruction of European science as it was to the economic reconstruction of the old continent. However, this conclusion is unsubstantiated, because historians of science have rarely addressed the question. When discussing science and the Cold War, they have explored the intellectual achievements of the period, the advent of big science as a system of funding, or the material culture of the physics laboratory. Krige's novel and timely perspective has been to investigate the mobilization of science for general political goals and more precisely to explore the uses of science policy as an instrument in the construction of U.S. postwar hegemony.

American Hegemony and the Postwar Reconstruction of Science in Europe

by John Krige

MIT Press, Cambridge, MA, 2006. 388 pp. $40, £25.95. ISBN 9780262112970. Transformations: Studies in the History of Science and Technology.

Hegemony is evidently a question of power, but it does not simply mean order, control, and command. U.S. elites of the postwar era placed a strong emphasis on the intimate and quasi-natural alliance of market economy, freedom, and democracy as the essence of American specificity. As a consequence, Krige (a historian at the Georgia Institute of Technology) suggests, hegemony was not only to be manifested and reproduced but also to be accepted by those who had to live with it—and to some extent co-constructed with them, at least with those living on the old continent. The scientific relations between the United States and (Western) European countries constitute a privileged terrain for evaluating this thesis because the engagement of the United States, both governmental and private, was massive and, the book demonstrates, had