PHILOSOPHY

HOW TO PREDICT GOOD RESEARCH

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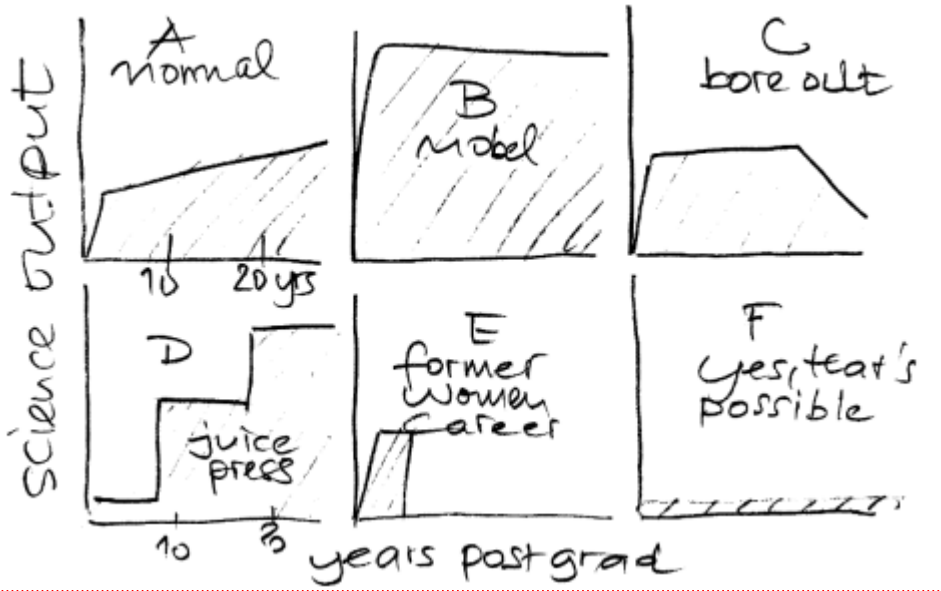
It is difficult if not impossible to foresee future research results. I am sharing the belief with many other colleagues that grant applications are largely a waste of time (in particular if most applications fail). A recent correspondence letter in Nature applauds

> One obvious solution is to put more money into the system… Another suggestion is to change the system for submitting and assessing applications, placing more of the onus on the universities, perhaps via a quota system for applications. A third is to weight the system more in favour of applicants' published track record and less in favour of the proposed science (with a special track for first-time applicants).

Taking the last argument seriously, I wonder if there has been ever meta-socio-science studies looking at the individual output?



Just counting papers and impact will not be sufficient. Type "B" research (>10 papers/year) is a highly dangerous habit if not caused by a truly ingenious spirit as well as type "D" research (who just know how to let others work for them). Yea, yea.

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