

White Paper for NSF Grand Challenges

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Abstract

I discuss some developments in economics and what I think are “Grand Challenges” for the social sciences over the next 10-20 years. One recurrent theme is the importance of heterogeneity in performance between firms and how this links to management practices. Splitting economics from other NSF funding would also be desirable.

I. Introduction

I am writing this in response to a letter by Myron Guttman requesting “Grand Challenges” for the social sciences over the next two decades. This is a formidable task, akin to writing the music of the future. After all, if one could indeed write such lyrics, they would already have been penned. Nevertheless, I will take this opportunity to reveal my prejudices regarding key areas that would in benefit from increased resources.

Two of the most important developments in economics the last decade have been (i) the massive growth of micro-economic data and (ii) the methodological move towards credible identification. The growth of huge databases of firm-level information in the public and private sector has been driven by the phenomenal fall in the quality adjusted price of information technology. This has made the storage, manipulation and analysis of data much easier and has led to a “Golden Age” of micro-econometric work. Liberalization of access to Census Bureau information has also helped as has greater regulatory requirements of the disclosure of company accounts.

Alongside the flourishing of large-scale datasets has been a move towards more transparent methods of understanding the causal relations between variables. Researchers are now much more careful to seek to identify exogenous changes in the variable of interest (either from nature or policy-makers) or, if this is not possible, to design and implement their own (often randomized controlled) experiments.

Although writing down a structural model and using an “off the shelf” secondary dataset even (if highly unsuited) still goes on, it is not in the dominant position that it once was. This is not so say that structural modelling has no place – it definitely does (see sub-section on “methodology” below), but theory is no substitute for good empirical design.

II. Grand Challenges – Some main themes

Organizational Heterogeneity

In my view, one of the most profound facts uncovered about modern economies is the huge variation in performance between plants and firms in narrowly defined industries. For example, within a typical four digit sector in US manufacturing output per worker is four times as high for the plant at the 90th percentile as the plant at the 10th percentile. And for total factor productivity the difference is still about double. Even wider distributions are evident in other nations.

Most economists' initial reaction to these performance differences was denial. First, it was said that these differences were purely transitory – they were not, they are relatively persistent. Second, the view was that inputs and outputs were badly mismeasured. This is true, but better measurement actually tended to make the differences larger. For example, plant-level price information has recently become available for some industries and when this is used to correct the measure of output (which typically used industry deflators) productivity differences were even wider (as the more efficient firms tended to charge lower prices). Thirdly, it was argued that the estimation of production function parameters was flawed. There has been significant methodological advance in this area (and still more needed) but the bottom line is that the differences persist under a wide variety of estimation procedures.

Many papers suggest that the evolution of productivity differences – through the creative destruction process of allocating more output to the most efficient and driving the less productive from the market – is a key factor in the time series aggregate growth of nations and aggregate TFP differences between nations (about half of the US-India difference for example).

The key challenge then is what is the cause of these between plant productivity differences?

Management Practices

One answer to the question on the causes of productivity heterogeneity is that the differences lie in technology. This is, of course, only a proximate answer because the deeper responses need to rest to structural features of societies – product, financial and labor markets, culture, etc. Nevertheless, unravelling the first part of the puzzle would be a start. There has been a large and substantial literature looking at the various “hard” technological variables that influence productivity – R&D, patents, observable innovation measures, diffusions measures (especially information and communication technologies, ICT). This is valuable but (1) a large residual remains after accounting for these observable indicators of technology; (2) the impact of technologies on productivity is very heterogeneous and seems to depend in a substantive way on the management of firms. Bloom, Sadun and Van Reenen (2007) for example, find that the impact of ICT is much stronger for firms with better “people management” (i.e. careful hiring, pay and promotion based on effort/ability rather than just tenure, rigorous procedures for dealing with underperformers, etc.).

This suggests that management is a key factor in understanding productivity. There are two big challenges here. First, how to quantify management practices across different

organizations in a comparable way. Second, is the correlation of management on productivity causal? And third, what are the theories that can account for the relationship?

We discuss these three questions of measurement, identification and theory in turn.

On the measurement side, there have been some advances in recent years (see Bloom and Van Reenen, 2010, for a discussion), but the challenge is how to develop such methods further and how to integrate them into standard statistical series such as the Economic Census. This needs to be done internationally to obtain cross-country comparisons. Can the (high skilled) labor intensive methods of Bloom and Van Reenen (2007) be simplified so that they can be mainstreamed in statistical agencies routine data collection?

On the identification side, how can we get at causal effects? The gold standard approach here is, in my view, randomised control trials. Although these are expensive, it is difficult to see how the evidence can be made secure without this type of approach.

On the theory side there are now a wide range of models that seek to account for the heterogeneity of management. Although some management styles are fads and fashions, mainstream modern economics correctly deems them as part of the chosen organizational design of the firm. This “design” approach applies much of standard optimization and equilibrium concepts to the theory of the firm. Although powerful (e.g. Personnel economics), the empirical basis for organizational economics is based too much on case studies and anecdote than solid data. Further, there is an element of management that is linked to productivity that makes it more akin to a technology. This may be static and non-transferable (embodied in people as in Lucas, 1978 or in firms as in Melitz, 2003) or dynamic and transferred between firms like any other technology. This is still poorly understood and needs theoretical development.

Intangible Capital

Management is one part of the intangible capital of the firm. Increasingly, the core assets of firms are not easily on the company balance sheet and the assets of a nation are barely tracked (human capital, intellectual property, brands and marketing, for example). There is a challenge to better measure and understand the accumulation of these intangible assets.

Much more so than conventional forms of capital, intangible capital is beset by uncertainties, externalities and potential failures of financial markets.

Unlocking Business data

Large amounts of data are collected by private sector firms and kept secret. This also used to be the case for governments, but increasingly these are being opened. Firms tended to underutilize their data, but with the new abundance of information, firms are starting to use their data more systematically.

Just as with government-academic cooperation, there is a huge opportunity to make more business data available to tackle the questions of heterogeneity and the causal impact of business practices

Macro-economics and Finance

The trends towards credible identification and deeper use of micro-data have penetrated some fields more than others. Macro-economics at some point seemed to turn its back on data and retreat into a focus on tightly specified models with empirical data used loosely to calibrate parameters of these models.

Most macro-models share the unfortunate assumptions of frictionless financial markets, an assumption that has fared extra-ordinarily badly over the financial crisis.

Macro-economics needs a Perestroika moment where the imperfections of financial markets take a pride of place. It also needs to re-discover respect for data and causal identification. Some of its problems are inescapable – a paucity of data of severe downturns, for example, and difficulty in running experiments. But a grand challenge for macro is to reflect economic reality of frictions much more seriously.

III. Other Themes and Challenges

These are mainly obvious so I will list them in a rather staccato way

- **A richer conception of human capital.** On the other end of the scale to macro, we need a richer concept of human capital. People's facilities rely not just on their physical and cognitive endowments, but also on their non-cognitive resiliency. Studies of the human brain and behavior have shown how important these non-cognitive aspects are in economic behavior. How can we model the accumulation of mental health? What policies best influence the development of human capital in this respect?
- **Methodology.** The best methods combine credible identification with good theory. Encouragement for work which combines experimental and quasi-experimental evidence with theory (so structural estimation is possible) is the ideal.
- **Climate change** and its economic effects. What are the adaption policies? How can policy be used to influence innovation to tackle climate change
- **The growth of emerging powers**, above all China. What effect will this have on the political economy of the world?
- **Demographics** The impact of aging and changing demographics
- **Africa.** Why has Africa stayed so poor? Is this going to change

IV. Recommendations

Funding should focus on the areas identified above, especially for III.

Since many of my themes cross disciplines, there is ample scope for inter-disciplinary work. Yet my experience is that the best research is done in the discipline one knows and setting up explicit inter-disciplinary funding leads only to tokenism. Economists have the best tools to tackle the questions I have identified and I think it would be better to split NSF funding so there was a distinct stream solely for economics, rather than mixing the funding stream with other disciplines.

References

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