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# The economic model of man

## and the representative economic researcher

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Chapter ## refers to other chapters in the Handbook

***Public choice*** is the extension of the economic model of man to include non-market behavior. Typically, it is used to explain the behavior of politicians and bureaucrats. This essay extends the model to explain the behavior of academic researchers.

The ***economic man*** acts in her own well-understood interests. The model contains the key notion of ***rationality*** that permits us to predict behavior. People have many interests, so it is difficult to predict individual behavior. However, the model has much predictive power when it is applied to the representative agent, as only the joint interest of most people counts.

The ***ideal model*** of public regulation assumes that decision-makers regulate to maximize social welfare. It is known as the benevolent dictator model, as it describes the choices of such a hypothetical being. It is tempting to see this as a theory of actual behavior. Public choice warns against giving in to this temptation. The ideal model is *normative*, and it is a poor description of actual behavior.

The ***rational model*** notes that regulations are implemented by politicians and bureaucrats. They act in their own well-understood interests, as does everybody else. The gap between the ideal and the rational outcome is often substantial.[2] It is important to analyze the nature and magnitude of the gap, even when it is difficult to find systematic evidence.

This survey extends the two models to the representative researcher and shows that a gap often occurs between empirical findings of the ideal and the rational researcher. In the common case where many studies report estimates of the same parameter, it is often possible to assess the gap. As discussed in section 2, it is often substantial.

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[2] I think that we all believe that real behavior is somewhere inside the gap.

## 1. Bureaucrats and cost maximization

Think of an individual who is employed in an organization where he is a cost. The head of the organization wants to make sure that he produces more than he costs, but the interest of the employee is to maximize his costs. That is, to increase his own wages and benefits. Many people show great ingenuity in this endeavor. In a capitalist market economy, two strong mechanisms work to control cost maximization: *competition*, which punishes inefficiency, and private *ownership*, which makes the owner the decision-maker and residual claimant. In public bureaus, neither mechanism work, so cost maximization has a wider scope.

Public bureaus administrate regulations (broadly defined). They produce both social ***welfare*** and ***rents***; see Chapter ##. While the welfare is the ideal purpose that benefits many, the rents typically accrue to a much smaller group that typically are the ones regulated. They often organize to put pressure on the bureau to increase the rents. Many cases have been reported where the bureau is ***captured*** by such organizations. In addition, the inertia of old decisions influences decisions. Public choice has a lot to say about these relationships.

Public salaries may be on a fixed scale, but if the bureau expands, more staff and chiefs are required. Thus, budget maximization is the joint interest of the bureau. The self-interest in cost expansion is never explicitly mentioned by any bureaucrat, while the expansion is always claimed to be necessary for the bureau to serve its noble purpose. This takes us to the Niskanen-Kornai model, where a bureau expands until something stops the process. The two versions of this theory were independently developed in the early 1970s.[3]

Kornai's model predicts that ***soft*** budget constraints make a bureau expand until the constraint becomes ***hard***. The upward cost slide is a gradual process that inevitably happens. Kornai is a theoretician deriving his model from economic fundamentals.

Niskanen's model is much the same, but he predicts that the costs of the bureau expand until it has consumed the full consumers' surplus it produces, whereupon the politicians providing its budget become less enthusiastic. As an empirical economist, Niskanen gave many examples. In a stylized version of the model, costs end up being ***twice*** the necessary ones.

It all tallies with the description of the bureau as a protected monopoly organization where the expansion of the budget is a special case of rent seeking, see Chapter ##. It leads to a competition for the rents that may cause cost increases expending the monopoly rent.

[3] Niskanen (1994) contains his main papers. Kornai (1986) is a fine survey of his theory. It was developed east of what used to be the Iron Curtain, where it was safer to concentrate on theory.

It is difficult to study the Niskanen-Kornai mechanism empirically, as it uses variables that are hard to measure, such as consumers' surplus from a regulation not sold in a market. In addition, most of Niskanen's examples are from military procurement, where it is hard to get an empirical handle on the very concept of consumers' surplus. We would like to have solid macro estimates of the average share of bureaucratic rents in the typical budgets. Is it really 50% as predicted by a stylized version of Niskanen's model? Or is it just a minor qualification to the ideal model?

Some evidence is found in studies that compare the costs of similar projects when some are made directly by the bureau, and some are outsourced after competitive bidding. The typical finding is that the competitive bidding reduces the costs by 30%, see Chapter ##.

Another set of evidence is to study the share of bureaucratic costs in a public institution when a reform reduces the constraints on the top management. A well-documented example is the wave of university reforms at the turn of the century. The reforms were centralizations, making the top management stronger. They were announced as devices to increase efficiency, but the bloated bureaucracies it created were a much larger inefficiency than the efficiency gain reached by a tighter control over the academic staff.[4]

Many readers who encounter the public choice view of politicians and bureaucrats argue that it presents an overly cynical view of the world. Vedel and Thomsen (2017) even present some evidence that economists are relatively cynical. Consequently, it is only fair to turn our tools towards ourselves. This provides a case study that nicely illustrates the arguments sketched above.

## 2. The public choice of academic research

The ***ideal*** goal of academic research is to produce papers in academic journals that take us closer to the truth. However, economic theory/public choice predicts that the ***rational*** researcher exaggerates results. This is a push away from the truth. [5] The following sees the exaggeration as a bias caused by rational researchers. We know a great deal about this bias.

The interest common to researchers is to publish papers in good journals and to obtain citations. Papers are rewarded by an increasing chance of: (i) a better career, (ii) more grants, and (iii) more prestige. The rewards are correlated and can be expressed as a money-equivalent

[4] Paldam (2015), Stage and Aagaard (2020), and Anderson *et al.* (2021) provide Nordic examples.
[5] This section builds on Paldam (2018 and 2021).

gain. The present value of all future gains from a published paper in a decent journal is substantial, such as € 20,000.[6]

The reward can be predicted from a set of market-like mechanisms that have been established for this purpose. Here researchers, journals, and universities are rated in pseudo-price systems. They 'price' papers by the ranking of the journals in which they are published and by the number of citations they obtain. About 150 journals count in this system – they publish about 15,000 papers per year.

Nobody can read 15,000 papers a year, and most will look at the top journals first, so even if the number of journals increases almost as fast as the number of researchers, it is difficult to 'burn through' in a crowded market. It is a simple fact that everybody, including referees and editors, prefer papers with a clear message. Thus, the chances of publication and citation increase when the paper reaches clear results.

Table 1. The classical method/format of an empirical paper in economics

| | |
|---|---|
| Step 1 | A survey of the literature shows why the paper increases our knowledge |
| Step 2 | A theoretical model is developed and operationalized for estimation |
| Step 3 | The data are presented |
| Step 4 | The model is estimated, and it is concluded that the theory is confirmed |
| Temptation 1 | Loops: The empirical work causes revision of model, so that it is confirmed |
| Temptation 2 | Data mining: Many regressions are run and only few of the best are reported |

### *2.1 The increasing popularity of the classical method*

A handful of studies show that economic research is increasingly empirical.[7] By far the largest set of empirical papers use the classical method surveyed in Table 1. The technique of meta-analysis is developed to study such research. About 1,000 meta-studies have been made in economics, so we can assess the size of the biases generated by the rational behavior of researchers. It is more difficult to assess the effect of rationality on papers in pure theory and in experimental economics, but anecdotal evidence suggests that the story is similar.

The last step (4) in the classical method is that the theory is accepted. It might also be rejected, but Fanelli (2010) studies the rate of acceptance for the theory proposed in articles in 20 sciences, which in economics was 86%. Students are taught that a negative result is as good as a positive and, in principle, journals claim the same thing. Thus, a number as high as 86%

[6] If the paper is empirical, the estimates reported account for a substantial part of the reward.
[7] See Angrist *et al.* (2017) and Paldam (2021). One reason for the big rise of empiric research is the shift in relative prices, making empirics cheaper: The costs of computers have dropped, more data are available on the net, and econometric programs become better and more user-friendly.

is an indication that something is amiss. In addition, studies of t-ratios find that they cluster just over the 5% level of significance, see Brodeur *et al.* (2016).

A literature, perhaps initiated by Leamer (1983), shows that the classical method is ***too good***, in the sense that it makes it too easy to produce results confirming the theory proposed. The fact that it happens so often suggests that many researchers succumb to the two temptations in Table 1. They invalidate the tests of the model and make the need for replications urgent.

Models are flexible, and it is easy to make model versions and to run regressions, so many are run. If the true value of the coefficient analyzed is positive but small, and 500 regressions are run, more than half are positive, and the 25 largest are surely (far) too high. Large estimates are often significant too, so it is easy to choose a fine result and a dozen additional ones demonstrating robustness. This choice will make the paper clear and relatively easy to publish. Thus, the rational researcher has an incentive to exaggerate. This leads to publication bias, which is a systematic difference between true values and published results.

In all sciences, it is well known that results can be believed only after repeated independent replications by other researchers using new data. Thanks to the flexibility of empirical research in economics, replication is very important, but replications are rare. Mueller-Langer *et al.* (2019) find that only 0.1% of the studies in 50 economic journals are replicated.[8]

Thus, the classical method is problematic, and the problems have led to great efforts by econometricians to develop post-classical methods such as co-integration techniques, Bayesian estimations, VAR-techniques, and other methods that increase the solidity of results. These methods will not be discussed at present. However, they are burdensome to report, and they often produce unclear results. The studies of the methods used in economic research show that they have not managed to make a dent in the increasing use of the classical method.

Thus, the classical method produces results that are often too good. Newer methods produce results that are often too dull. The choice of the profession is that too good is better than too dull – exactly as predicted by the rational reaction of researchers to incentives.

This is all part of the replication crisis in science, which is particularly severe in the social sciences and medicine. As of August 2021, Google scholar has 4.2 million hits on "*publication bias*". The various versions of "*sponsor effects"* and "*replications*" give large numbers of hits as well. The specific term "*replication crisis*" produces 300,000 hits.

[8] Replications often give embarrassing results; see Dewald *et al.* (1986), McCullough *et al.* (2008) and Duvendack *et al.* (2015).

### *2.2 Can meta-studies replace replications?*

While strict replication studies are rare in economics, important parameters, $\beta$, are often estimated in many studies that use variants of the same estimation model, different datasets, estimators etc. Consider for example the effect on unemployment of a change in the value added tax rate. Economic theory (and common sense) predicts that the effect is positive.

A careful search may produce 50 studies of a certain $\beta$, each presenting its best estimate and a dozen additional ones showing robustness. Thus, instead of one study and a handful of independent replications of the same study, we have a ***swarm*** of 500 partial replications that claim to be estimates of $\beta$. This means that the universe of relevant data has been searched in hundreds of ways using all the models a large group of researchers have thought of in order to catch $\beta$. This is surely great, but it is still a swarm of related estimates.

It is often illuminating to consider the distribution of the swarm using the funnel representation, where the estimates are depicted over their precision (the inverse standard error). Funnels should be symmetrical, but they often show asymmetries, such as truncations of results with the "wrong" sign according to theory. In addition, funnels are often amazingly wide; it is common to see estimates that differ by both three and four times.

The first way to sum up the findings in the $\beta$-literature is to calculate the ***mean*** of the estimates. However, given the previous argument, the average researcher is likely to exaggerate his findings, giving the literature a publication bias. Meta-analysis has developed a set of tools which detects and corrects for publication bias identified as asymmetries in the distribution and produces a ***meta-average*** that is normally smaller than the mean.

The difference between the meta-average and the mean is a measure of the gap between ideal and rational researchers. It has been estimated in a great many cases, see Ioannidis et al. (2017). It differs somewhat, but a good rule-of-thumb is to expect an exaggeration by a factor two. Thus, the gap between the ideal and the rational man is substantial also in research.

## 3. Conclusion

If we accept that academic researchers are humans who behave much as we predict that everybody else behave, we can say a great deal. The evidence shows that the gap between the ideal research result and the actual one as produced by rational researchers is substantial, perhaps as much as by a factor of two. This neatly extends the prediction of Niskanen.

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