

The economic model of man and the representative economic researcher

Martin Paldam, Aarhus University ¹

Public choice is defined as the extension of the economic model of man to include non-market behavior. Thus, it explains the behavior of politicians and bureaucrats (public employees) including researchers. Economic man acts in her own well-understood interests. The model has a first simple version where the interest is the maximization of income, but then it has surely expanded to great complexity. People have many interests, but still the model contains the key notion of *rationality* that permits us to predict behavior. It is difficult to predict individual behavior, but once the model is applied to the representative agent, where the interests become simpler due to averaging, the economic model has substantive predictive power.

The *standard model* of public regulation calculate how the decision maker regulates in order to maximize social welfare. It is known as the benevolent dictator model, as it describes the choices of such a hypothetical being. Social welfare is only a well-defined concept in certain highly stylized cases, but anyhow.² It is tempting to see this as a theory of actual behavior. Public choice warns against giving in to this temptation. The standard model is purely *normative* describing an ideal, but it is a poor description of actual behavior.

Regulations are implemented by politicians and bureaucrats. Public choice claims that they act in their own well-understood interests, as do everybody else. The wedge between the normative ideal and the actual outcome is often substantial. Public choice is a theory allowing us to analyze the wedge.

¹ Department of Economics and Business, Fuglesangs Allé 4, 8210, Aarhus V, Denmark.
E-mail mpaldam@econ.au.dk. Home page <http://www.martin.paldam.dk>.

² A discussion of the concept of ‘maximizing social welfare’ would derail the paper. Thus, I just assume that the concept makes sense.

1. Bureaucrats and cost maximization

Think of an individual who works in a bureau (organization) where he is a cost. The “head” of the bureau wants to make sure that he produces more than he costs, but the interest of each of the employees is to maximize his costs, i.e., to increase wages and perks. Many people show great ingenuity in this endeavor. In a capitalist market economy, two strong mechanisms work to turn cost maximization into its opposite: *Competition*, where inefficiency is punished, and private *ownership* that makes the owner the decision maker and residual claimant.³

The term “head” is used for the ones “owning” the bureau, which in the public sector is the public. However, the decision chain between the public and the production of the bureau is long. The public elects the parliament, who enacts the regulation. They also choose the government, who implements the regulation. The government appoints the chief of the bureau, who appoints his staff, the bureaucrats. They are the ones doing the actual work. Many channels allow feedback, and often the chain works in the reverse, so that some bureaucrats start the process to enact the regulation by an idea working its way up to the minister, who after approval by the government sends a law to the parliament for approval.

The regulation produces welfare and rents. The welfare is the ideal purpose, and the rents typically accrue to somebody else. Maybe the welfare recipients have formed an NGO, and it is likely that the rent recipients form a pressure group. Everybody in this chain has interests. In addition, the inertia of old decisions influences decisions, and some people have obtained historical rights to certain rents; maybe they have bought the rights to the rents, etc.⁴ Public choice has a lot to say on all of this. The present just looks at one mechanism, assuming that the chief of the bureau is *captured* by the bureau,⁵ while the interest of the public may be weak in the day-to-day decisions.

In public bureaus, neither competition nor property rights work. Salaries may be on a fixed scale, but if the bureau expands, more chiefs are required. Consider what stops a public bureau from pursuing cost maximization, which is the joint goal of its staff, and you quickly

³ Neither of these mechanisms are nice, and they are often criticized on moral grounds, but a non-market allocation of goods requires a system of strong controls to avoid drowning in cost maximization. Strong controls are unpopular. Thus, it leads to dictatorship.

⁴ The story of the protection of sugar beets in Europe illustrates all these problems. The regulations were introduced to prevent a repetition of the lack of sugar under World War I, and it has continued to this very day, though it makes no sense, except for the producers, who harvest substantial rents.

⁵ Capture means that the chief entrusted to regulate and to control the budget of the bureau for the public actually ends up protecting the regulated and the bureau from the public; see Stigler (1971). Capture is due to two mechanisms: (1) the regulated may be better organized than the beneficiaries are. (2) The chief becomes a bigger chief when the bureau grows. Consequently, his interests have a lot in common with the interests of his underlings.

reach the Niskanen-Kornai model, where a bureau expands until something stops the process. The theory was independently developed in two versions in the early 1970s.⁶

Kornai's model predicts that *soft* budget constraints make a bureau expand until the constraint becomes *hard*. Kornai gives some examples, but he is a theorist deriving his model from economic fundamentals. In addition, his model was developed east of what used to be the Iron Curtain, where it was safer to concentrate on theory.

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 the budget become less enthusiastic. Niskanen was an empirical economist, and he gives many examples. In the illustrative, stylized version of the model, costs end up being twice the necessary ones. However, many of his examples are from military procurement, where consumers' surplus is a woolly concept, as it often is in public bureaus that produce a regulation that is not sold in a market.

Cost maximization is not the only goal of a bureaucrat. She typically works in an administration that has an altruistic purpose, i.e., she may want to maximize social welfare as she sees it. She may have joined the bureau precisely because she thinks that the purpose is good, or she may have acquired the purpose from working in the bureau, and she may want to expand the bureau so that it can do more of the good thing. Thus, her own interests may have aligned with her altruistic interests, so that she wants an increase in the staff and the budget anyhow.

Kornai stresses that the upward cost slide is a gradual process. It is something that will inevitably happen, but it takes time. The self-interest in cost expansion is never explicitly mentioned by any bureaucrat, while expansion is always claimed to be necessary for the bureau to serve its noble purpose. However, I trust that we have all seen cases where a bureaucracy is larger than necessary, and we have certainly heard bureaucrats argue that their bureau needs a larger budget for some reason or another.⁷

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. It leads to a competition for the rents that may cause cost increases expending the monopoly rent.

It is difficult to study the Niskanen-Kornai mechanism empirically, as it uses variables that are difficult to measure, such as the consumers' surplus from a regulation as mentioned.

⁶ Niskanen's papers since the early 1970s are reissued in Niskanen (1994), and Kornai has given a fine survey of his theory in Kornai (1986). I heard him present his theory at a UN-ECE conference in Bulgaria in 1971.

⁷ One of the most popular arguments is that a small expenditure now will cause big savings in the future. It is never mentioned that a small expenditure now may be seed money for big expenditures in the future.

One way to acquire an empirical angle is to study the share of bureaucratic costs in a public institution where a reform reduces the constraints on the top management. A well-documented example where the bureaucratic share of the staff increased substantially after the wave of university reforms at the turn of the century; see Paldam (2015), Stage and Aagaard (2020) and Anderson *et al.* (2021), for Nordic examples. The reforms were centralizations, making the top management stronger. It was announced as a mean 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 staff.

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 standard model?

Some evidence that it is substantial 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 30-40%.

Many readers who meet the public choice view on politicians and bureaucrats argue that this is an overly cynical view of the world. People are often appalled when first presented with concepts such as rent seeking and capture. Thus, economists may be nasty to others. Thomsen and Vedel (2017) even present some evidence that this is the case. 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

I consider the goal of academic research to be to produce papers in academic journals, papers that ideally tell the truth. The field of economics is used as an illustration,⁸ but the same story applies to political science and other (social) sciences. It will be demonstrated that economic theory (i.e., public choice) predicts that the typical research paper exaggerates its results, so they report more than the truth.

To predict the behavior of the representative academic economist, it is necessary to disregard the diversity of interests of individual researchers, and look at one general interest that is common to most researchers - namely to publish papers that are cited by others in the profession. I disregard the trade-off between journal quality (ranking) and the probability of acceptance (rejection rate) and just assume that the researcher tries to publish as well as possible considering his/her career possibilities.⁹

Papers are rewarded in three ways: (i) by an increase in the likelihood for career advancement; (ii) by an increase in the chance of obtaining grants incl. travel grants; (iii) by the academic prestige it generates, in a world where everybody can easily ‘google’ each other. Typically, (i) to (iii) are proportional, and can be expressed as a money-equivalent gain. I assess that the present value of all future gains from a published paper in a decent journal is substantial, such as € 20,000, where half is due to the empirical research. The way the benefits are organized is increasingly market like, so that 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 in search engines made for this very purpose. About 150 journals count in this system, and 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 prefers papers with a clear message. This also applies to referees and editors. Thus, your chances of

⁸ This section builds on Paldam (2013, 2018 and 2021).

⁹ I consider that economic research is crudely classified in three levels: A, B and C. The A level consists of about 10 top institutions and the corresponding top journals. The B-level is another 450 research universities and about 150 journals trying to do serious refereeing. The C level is about 20,000 universities that mainly teach and many less prestigious journals. If you want a job in an A-department, you have to publish in A-journals, but it is a risky strategy if you are in the career at a B-department, as it will take a long time and may eventually fail, leaving you without publications. Thus, there are various strategies. These choices are disregarded at present.

publication and citation increase when the paper reaches fine clear results.

2.1 *The increasing popularity of the classical method*

A handful of studies show that economic research is increasingly empirical, and that the classical format given in Table 1 is used more and more in empirical papers; see Hamermesh (2013), Angrist *et al.* (2017) and Paldam (2021).

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

1	A survey of the literature shows why the paper increases our knowledge
2	A theoretical model is developed
3	The model is operationalized as a (much) simpler model that can be estimated
4	The data are chosen and the model estimated, including some robustness tests
5	It is concluded that the theory is confirmed, i.e. the model is not rejected

The last point on the list is that the theory is accepted. Fanelli (2010) studies the rate of acceptance for the theory proposed in articles in 20 sciences. In economics, it is 86%. Students are taught that a negative result is as good as a positive. In principle, journals claim the same thing. Thus, a high number, such as 86%, is an indication that something is amiss. Economics is number 14 of the 20 sciences, while space science is the last, with an approval rate of ‘only’ 70%. The other social sciences are even higher than economics on the list, and thus (even) more problematic.

I think that we all know that the typical path of a research process does not proceed as steps 1 to 5 in Table 1, but it is a convenient convention for presenting research.

A large literature, perhaps initiated by Leamer (1983), shows that this method is *too good*, in the sense that it makes it too easy to produce clear results confirming the theory. It is surely wide open to data mining, where the researcher estimates many model variants and selects the best ones. In many cases, millions of such variants are possible, using different sets of control variables or first stage instruments. Each one gives a different estimate, and then the rational researcher will surely choose the one most likely to lead to publication.

Empirical research has seen large productivity gains: data are increasingly easier to download; computers are constantly getting faster and cheaper; econometric packages keep getting better and more user-friendly. Once everything is in the pc, the time necessary to run one more regression is about 5 minutes, which includes post estimation tests, so the marginal costs of a regression are just a couple of Euros. Above it was assessed that the gain from

producing fine empirical results is € 10,000. Obviously the marginal gain falls after some time, but still it is clear that it requires a great many regressions before they have fallen to equal the marginal costs. Let us assume it is 500 regressions. They may take 2-3 days of work. If you want to use a stepwise regressor or a two-stage instrument-variable regression or to add a few co-integration tests, it may even take a full week, but even this is surely worth spending.

If the true value of the coefficient analyzed is zero, the 5% level of significance means that 500 estimates of model variants should result in 25 significant results of which half are positive. If a significant positive coefficient enhances the chance of publication, it is doable. Moreover, you can also find a handful of nice results showing robustness. If the true value is positive but small, you may find a substantial result. Thus, it is clear that the rational researcher exaggerates. This leads to publication bias, which is defined as a systematic difference between the true value and the published result.

In all sciences, it is well known that results can be believed only after repeated independent replications. That is, replications done by other researchers on new data. Thanks to the flexibility of empirical research in economics, replication is very important.¹⁰ However, replications are rare – Mueller-Langer *et al.* (2019) find that only 0.1% of the studies in 50 economic journals are replicated. Replications often give embarrassing results; see Dewald *et al.* (1986), McCullough *et al.* (2008) and Duvendack *et al.* (2015). In addition, studies of t-ratios find that they cluster just over the 5% level of significance, see Brodeur *et al.* (2016).

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 etc. Methods that are made to increase the solidity of results. However, these methods are bulky 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 progress 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 public choice.

This is all part of *the replication crisis in science*, which is particularly bad in the social sciences and medicine. As of August 2021, Google scholar has 4.2 million hits to *publication*

¹⁰ Sometimes articles reporting fine empirical results, were much discussed when they were published and for a few years. Then more data appeared showing that they did not replicate; see e.g., Burnside and Dollar (2000) and Hansen and Tarp (2000). In the latter case, the failure of replication led the authors to publish a different model with the same policy implications; see Dalgaard, Hansen and Tarp (2004) that nicely illustrate the flexibility of the classical method.

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.

2.2 Can meta-studies replace replications?

While strict replication studies are rare in economics, most important parameters are estimated in many studies that use variants of the same estimation model. Maybe it is as good as replication.

Consider the parameter $\beta = \partial y / \partial x$, which is the effect on y of a change in x . It may be the effect on unemployment of a change in the value added tax rate, or any other important parameter used in policy-making. If the literature is carefully searched, maybe 100 studies of β are found.¹¹ This is the β -literature, where each study presents its best estimate of β . No study is an exact replication of any other study. Often, the theory differs, and so do the control variables, the data set, etc. However, the estimation models are more alike. They have the general form:

(1) $y = bx + \dots$, where b is the estimate of β . The rest of the model “...” differs.

The 100 studies of the same effect say that the universe of relevant data has been searched in a hundred ways for the best estimate of β . It is often claimed that the estimates should be weighted with the quality of each study. This can be done by using the impact factors of the journals where the studies are published as weights. The typical finding is that such weights do not change the mean. This will be taken for granted in the rest of the paper.

The first way to sum up the findings in the β -literature is to calculate the *mean* of the 100 estimates. However, given that economists are rational, the average researcher may exaggerate his findings. This gives the literature a publication bias. The tool developed to summarize such literatures is meta-analysis. It is made to detect publication bias and to correct for such bias. When it is corrected for, one reaches a *meta-average* that is normally smaller than the mean. My rule-of-thumb is to expect an exaggeration by a factor two. Section 2.3 briefly explains the logic of meta-analysis.

In addition to the mean and the meta-average, one may also look at the standard errors,

¹¹ If a few studies are not found, it does not matter, but it obviously matters if the omissions are systematic, so it is important that the search for the full β -literature is thorough, and that the list of primary studies covered is published so that anyone knowing the literature can see if the list is biased. For ease of presentation, it is assumed that β has one true value which is positive. It is also assumed that each of the papers have a different author, and he/she points to one estimate as the right one. Methods exist to control for deviations from these assumptions.

s , and consider the distribution of the estimates in the form of a *funnel plot*. It is the (p, b) -scatter, where $p = 1/s$ is the precision. Funnel plots are always amazingly wide considering the fine t-ratios of the estimates. One of the reasons researchers should make meta-studies is that it is an eye-opener to see how wide funnels are! Thus, it is highly risky to trust a few studies.

Already about 700 meta-studies have been made in economics covering at least 10,000 primary studies. This is a major effort, which allows us to say a great deal about the estimates, and hence about the public choice of economic research.

2.3 *The logic of meta-studies* ¹²

Statistical theory predicts that a funnel plot due to random variation should look as a symmetrical X-mas tree, with a broad base for low precision, and a top for high precision. Statistical theory further predicts that the true value is found at the limit, where precision goes to infinity:

$$(2) \quad b_i \rightarrow \beta, \text{ for } p_i \rightarrow \infty$$

Symmetry means that the size of the estimates is independent of their precision, and thus their standard error. If some parts of the funnel are missing, it deviates from symmetry. This will be the case if researchers systematically choose the best results. This has led to the FAT, Funnel Asymmetry Test, which is the regression:

$$(3) \quad b_i = a s_i = a / p_i \quad \text{if } a \neq 0, \text{ the funnel is asymmetric.}$$

However, the b s will still converge to β . T.D. Stanley was the first to see that by combining (2) and (3), one gets a joint estimate of b_M , the meta-average and the FAT.

$$(4) \quad b_i = b_M + a s_i = b_M + a / p_i \quad \text{this is the FAT-PET MRA.}^{13}$$

Equation (4) started a quest for the best estimator of b_M . It has been discussed how (4) should be estimated, and perhaps the second term (the FAT) may be squared, etc. In addition, a number of additional tests have been proposed. In my experience, the many improvements to (4) only matter marginally.¹⁴

¹² This section refers to Stanley and Doucouliagos (2012), which is the textbook on meta-analysis in economics.

¹³ PET means Precision-Effect Test and MRA means Meta Regression Analysis.

¹⁴ Equation (4) has been studied by making many simulations of sets of variants of a known model, i.e., where the true value is known, but where researchers choose the best in the set. This biases the mean, but the meta-average is (much) closer to the true value. Such simulations studies require the estimates of millions of regressions, but computers do not tire even when they run through the night.

Chris Doucouliagos has built a data bank of the coded papers behind about 400 meta-studies covering about 8,000 primary studies. Two meta-meta-studies have been made on these data: Ioannidis *et al.* (2017) that covers 6,700 studies and Doucouliagos *et al.* (2018) that covers 4,300 studies. Both meta-meta studies document that exaggeration is quite common. There is considerable variation in the amount of exaggeration, but one may take an exaggeration of two times as an average.

The second study just mentioned finds that the meta-average in many fields becomes stable after the publication of 30-50 studies, but the results are typically rather far from that value in the beginning, when there are just a few studies. It is possible that the studies subjected to meta-analysis are studies that are relatively controversial, so that the exaggeration is relatively high, but this is by no means certain.

2.4 *Meta-analysis does replace replication, but it can be misused*

This all leads to the conclusion that when a β -literature has run for some time, meta-analysis can actually provide a fine estimate of the true value, and thus it does replace replications. They are rare in economics, but at least for studies using the classical method we have a cookbook describing how a meta-study should be made; see Stanley and Doucouliagos (2012).

Other methods are used in economics, where meta-analysis is more difficult to apply. For the growing method of lab-experiments, a few meta-studies have already been made, though the meta-technique still seems to need some development. The big replication study Camerer *et al.* (2018) demonstrates that meta-studies are needed, as it finds that the average lab-experiment reports a result that is twice too good!

In addition, there are few field experiments where a strict protocol sets out a gold standard of double blind and a pre-announced sampling plan; see Christensen and Miguel (2018). In economics such studies are rare, but they are the standard in medicine, and here they are routinely submitted to meta-studies, which suggests that even when it seems that everything is done by the golden book, the results still deviate a bit too much.

Finally, it should be mentioned that meta-analysis can be misused. Meta-studies often supplement (4) with augmented versions such as:

$$(5) \quad b_i = b_M + a s_i + d z_i \quad \text{where } z \text{ is a binary } (0,1)\text{-variable saying if control } z \text{ is included in estimate } i$$

Equation (5) treats z as an omitted variable when it is not included in the original article, and it gives the meta-average assuming that z is *randomly* included. Thus, it should have had the same effect on b when it is not included as it has when it is included. If you have found a publication bias using (4), you know that the control variables have been *systematically* selected for their effect on b . Hence, equation (5) increases the bias.

Most β -literatures have a pool of 2-3 dozen of possible controls, where a handful are included in the typical paper. Then (5) can be augmented with a selection of many variables. This gives an equation that can increase or decrease the meta-average within a broad range. Thus, equation (5) allows meta-analysis to be misused for meta-mining; see Paldam (2021b).

Fortunately, the pure estimate (4) allows the meta-analyst to see if (5) makes sense. It is a strong sign of meta-mining if (4) shows an exaggeration, and (5) produces a meta-average that is larger than the mean.

3. Conclusion

Thus, if we academic researchers submit ourselves to public choice analysis, it is clear that we are humans that behave much as we predict that everybody else behave. Some evidence shows that the wedge between the ideal research result and the actual one as produced by rational researchers is substantial. Perhaps as much as by a factor 2.

The paper started by considering the wedge between the ideal outcome and the actual outcome as regards the production of a bureau. The wedge is the field of public choice. The predictions are case-specific, and often involve variables that are difficult to measure.

However, many cases have been found to allow assessments of the wedge, and it is often found to be substantial. Thus, there is a trade-off between the welfare and the rents a regulation creates. It is quite easy to find cases where the rents are larger, though it is also possible to find cases where the welfare gains are larger. This is especially the case for new regulations addressing a new (or newly discovered) externality. However, over time the regulatory bureaus grow and capture its board.

A typical case is the regulation of financial markets. When the international banking crisis broke out in 2007, it led to many proposals for regulations. It was rarely mentioned that all countries for long have had agencies regulating financial institutions. However, they had regulated very little when they were needed, exactly as predicted by Stigler (1975, notably Cpt. 6). Also, as predicted by the Kornai-Niskanen model, the bureaucrats working in the agencies were quick to point out that they were too few and needed more staff. The administration of the EU also took the opportunity to propose a big new agency.

Acknowledgement:

A version of this paper was given as a joint plenary speech with Chris Doucouliagos at the (US) Public Choice Meeting in Louisville, Kentucky before the corona pandemic. Thus, some of the ideas are his.

References:

- Anderson, F.W., Jordahl, H., Kärnä, A., 2021. Ballooning bureaucracy: Tracking the growth of high-skilled administration within Swedish higher education. IFN working Paper No 1399, Stockholm
- Angrist, J., Azoulay, P., Ellison, G., Hill, R., Lu, S.F., 2017. Economic research evolves: Fields and styles. *American Economic Review (Papers & Proceedings)* 107, 293–97
- Brodeur, A., Le, M., Sagnier, M., Zylberberg, Y., 2016. Star Wars: the empirics strike back. *American Economic Journal, Applied Economics* 8, 1–32
- Burnside, C., Dollar, D., 2000. Aid, Policies and Growth. *American Economic Review* 90: 847-68
- Camerer C.F., Dreber A., Holzmaster F., Ho T.-H., Huber J., Johannesson M., Kirchler M., Nave G., Nosek B.A., Pfeiffer T., Altmejd A., Buttrick N., Chan T., Chen Y., Forsell E., Gampa A., Heikensten E., Hummer L., Imai T., Isaksson S., Manfredi D., Rose J., Wagenmakers E.-J., Wu H., 2018. Evaluating replicability of laboratory experiments in economics. *Nature Human Behaviour* <https://www.nature.com/articles/M2.11562-018-0399-z>
- Christensen G., Miguel E., 2018. Transparency, reproducibility, and the credibility of economics research. *Journal of Economic Literature* 56, 920-80
- Dalgaard, C.-J., Hansen, H., Tarp, F., 2004. On the Empirics of Foreign Aid and Growth. *The Economic Journal*, Volume 114 (496), F191–F216
- Dewald, W.G., Thursby, J.G., Anderson, R.G., 1986. Replication in empirical economics: the journal of money, credit and banking project. *American Economic Review* 76, 587–603
- Doucouliagos H., Paldam M., Stanley T.D., 2018. Skating on thin evidence: Implications for public policy. *European Journal of Political Economics* 54, 16-25
- Duvendack, M., Palmer-Jones, R.W., Reed, W.R., 2015. Replication in economics: a progress report. *Economic Journal Watch* 12, 164–191
- Fanelli, D., 2010. “Positive” results increase down the hierarchy of the sciences. *PLoS One* 5 (4), e10068
- Hamermesh, D., 2013. Six decades of top economics publishing: Who and how? *Journal of Economic Literature* 51, 162–172
- Hansen, H., Tarp, F., 2000. Aid effectiveness disputed. *Journal of International Development* 12, 375-39
- Ioannidis, J.P.A., Stanley, T.D., Doucouliagos, H., 2017. The Power of Bias in Economics Research. *Economic Journal* 127 (605), 236-65
- Kornai, J., 1986. The soft budget constraint. *Kyklos*, 39(1), 3–30
- Leamer, E.E., 1983. Let's take the con out of econometrics. *American Economic Review* 73, 31–43
- McCullough, B.D., McGeary, K.A., Harrison, T.D., 2008. Do economics journal archives promote replicable

- research? *Canadian Journal of Economics* 41, 1406–1420
- Mueller-Langer, F., Fecher, B., Harhoff, D., Wagner, G.G., 2019. Replication studies in economics - How many and which papers are chosen for replication, and why? *Research Policy* 48(1), 62-83
- Niskanen, W.A. Jr., 1994. *Bureaucracy and Public Economics* (6 older essays). Aldershot: Edward Elgar
- Paldam, M., 2013. Regression costs fall, mining ratios rise, publication bias looms, and techniques get fancier: Reflections on some trends in empirical macroeconomics. *Econ Journal Watch* 10(2), 136-56
- Paldam, M., 2015. The public choice of university organization: A stylized story of a constitutional reform. *Constitutional Political Economics* 26(1), 137–58
- Paldam, M., 2018. A model of the representative economist, as researcher and policy advisor. *European Journal of Political Economy* 54, 6-15
- Paldam, M., 2021a. Methods used in economic research. An empirical study of trends and levels. *Economics* 15(1), 1-13
- Paldam, M., 2021b META-mining. Working paper available from <http://martin.paldam.dk/Papers/Method/12-Meta-mining.pdf>
- Stage, A.K. and Aagaard, K., 2020. Danish universities under transformation: Developments in staff categories as indicator of organizational change. *Higher Education* 78(4), 629-52
- Stanley, T.D., Doucouliagos, H., 2012. *Meta-regression Analysis in Economics and Business*. Routledge, Abingdon
- Stigler, G.E., 1975. *The Citizen and the State. Essays on Regulation*. Chicago UP, Chicago
- Vedel, A., Thomsen, D.K., 2017. The dark triads across academic majors. *Personality and Individual Differences* 116, 86–91