Measuring the effect of competition policy on productivity: evidence from competition authority annual reports

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Abstract

I perform a partial replication of Buccirossi et al.'s 2013 research on the effect of competition policy on productivity growth. I focus on more quantitative indicators, taking advantage of improved reporting of competition authority activity. I use country-industry data, employing government programmatic position as an instrument and use lagged independent variables and two-way fixed effects to further alleviate endogeneity concerns. Only one of my results successfully replicates the findings of Buccirossi et al., although it is not robust to alternative specifications.

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1. Introduction

While there is a broad consensus among economists about the (generally) positive effects of competition, no such agreement exists about the effects of competition policy. Studies that try to empirically assess it typically use either very broad and crude indicators (Gutmann and Vogit 2014; Petersen 2013), or subjective indicators assigning scores to qualitative elements of the legal and policy environment (Buccirossi et al. 2013; Dutz and Vagliasindi 2000; Hylton and Deng 2007; Voigt 2006). This paper tries to recreate Buccirossi et al. (2013), a paper from this latter category, but instead uses more quantitative proxies for competition authority activity.

I estimate the impact on industry-level productivity of several indicators of intensity of competition policy: the fraction of mergers not cleared in phase 1; and the number of merger cases handled by and the budget of the competition authority, both deflated by GDP. Using lagged independent variables, country-industry and time fixed effects, and instruments in the form of government programmatic position, I regress industry-level Total Factor Productivity- and Labor Productivity growth on my indicators. While the fraction of mergers not cleared in phase 1 has a significant on TFP growth in line with the results obtained by Buccirossi et al. (2013), none of the other results are significant.

2. Literature Review

When analyzing the impact of competition policy, there are generally three definitional questions that need to be addressed. The first of these is how to measure the level of competition in the subject market, sector or country. The second concerns the matter of quantifying characteristics of competition policy. Finally, there is the measurement of the outcome variable: usually some indicator that needs to proxy for general welfare or prosperity. Since many studies can be classified according to their approach to these three questions, I will discuss previous papers grouped by each of these facets rather than a more ad-hoc grouping that would result from discussing the studies sequentially.

2.1 Measuring competition

High degrees of market concentration are so closely linked to anticompetitive outcomes that it they become almost synonymous when one is not careful with definitions. Because of this, measures describing market shares (or asymmetry of market shares in the case of the Herfindahl-Hirschman Index) seem to be suited for the task of measuring competitive pressure in a market (Motta, 2004). However, this approach runs into practical problems. Defining what exactly constitutes a market and which firms are competitors is difficult enough in single merger- or cartel cases, and gathering such data on sufficient scale is often infeasible. Furthermore, complementary data is often also not available in a way that easily matches. The only paper that I have found which relies on such an approach is Dutz & Vagliasindi (2000). They use the average market share of a firm's main product, across all industries, as a measure of structural concentration in transitioning economies in Eastern Europe and the Caucasus as an outcome variable, alongside other measures. This approach may not generalize well to multi-product firms.

The other "classic" measure of competition is the Price-Cost Margin, or Lerner Index, which is sometimes inverted to make empirical results more intuitive. This entirely sidesteps the market definition issue and is also easier to reconcile with industry-level aggregations. Because of this it is used more frequently, such as in Nickell (1996), Aghion et al (2005), Hashimi (2013), and Polder and Veldhuizen (2012), where it is used as an independent variable.

However, both these measures have cases in which increased competition in theoretical parametrizations does not correspond to the desired change in measurements (Boone, 2008). He also proposed a new measure based on the ratio between profits made by more efficient firms and those made by less efficient ones. However, I have not found many studies employing this method to find the impact of competition policy, possibly because a large amount of research in this area relies on aggregate data. In the papers included, only Polder and Veldhuizen (2012) use the Profit Elasticity measure as an independent variable, as one of their specifications to measure the effect on innovative activity.

While they do not exactly correspond to competition, entry and exit may also constitute channels through which competition policy has an effect (Aghion & Howitt, 2006). According to Aghion et al. (2004; 2009), more entry increases innovative activity, which also translates to higher productivity.

It can be seen, therefore, that there can be substantial difficulties in measuring competition. Furthermore, the policy relevance of research in this area is about making recommendations for competition policy, which has its own difficulties but which will ultimately be the independent variable of choice for this paper. Therefore, I will examine established ways to measure competition policy in the next subsection.

2.2 Measuring competition policy

Comparing competition policies transnationally in a quantifiable way poses a significant challenge to researching its effects. One of the most transparent measures is to use a binary indicator to check whether a given country has any competition laws on the books at a given time. This is the approach taken by Gutmann and Voigt (2014) and by Petersen (2013). While this approach has the advantage of leaving little room for disagreement about classification, it makes no distinctions at all concerning the existence or quality of enforcement between countries.

To add more granularity, several authors have tried to systematically assign scores to important aspects of countries' competition laws. Buccirossi et al. (2013) use competition authority independence, plaintiff/adjudicator separation, scope, maximum sanctions and "closeness" of rules to their effect on social welfare as inputs to a composite score, alongside some characteristics of enforcement (see below). They apply this to 12 OECD countries. Dutz and Vagliasindi (2000) score enforcement activity, advocacy activity and the quality of the institutional setup of antitrust laws and authorities of transitioning Eastern European countries. Hylton and Deng (2007) create an index of competition law scope and separate stringency indices for 102 countries worldwide. Ma (2011) uses Hylton and Deng's scope index, and pairs it with data on administrative efficiency from Kaufmann, Kraay and Mastruzzi (2009). Finally, Voigt (2006) creates four scores: how deeply competition laws and objectives are engrained in the legal system; to what extent competition laws are constructed based on economic rather than legal theory; the *de jure* independence of competition authorities; and their *de facto* independence. They apply these classifications to 57 countries. While such measures are more fine-grained, they rely on subjective judgments. Furthermore, they are generally harder to interpret: it is not always clear, for example, what a unit increase in an index means.

As pointed out by Voigt (2006), there can be a large difference between the law on the books and the enforcement regime. Some studies focus on this instead of looking at the properties of the competition laws across countries. Buccirossi et al. (2013) also use the antitrust authority's budget, as well as information on "the quality and the amount of human resources a competition authority can rely on", as inputs into their Competition Policy Index. Hylton and Deng (2007) use the CA's budgets as a fraction of the country's GDP as an alternative specification. Clougherty (2010) uses CA budget as an independent variable, and also looks at how its composition has changed over time. He finds that the both the shares of personnel with a legal background and with an economic background have remained roughly constant over time, and contrasts that with the idea that "economism" has triumphed over "legalism" in antitrust doctrine since the 1990's. He also shows that, on average, CA activity (measured by the number of cases handled) has increased, and that this has gone roughly hand in hand with increasing budgets.

Finally, there are some studies that use perceived effectiveness of competition authorities. Borrell and Tolosa (2008), Dutz and Hayri (1999), and Krakowski (2005) used survey responses taken from business executives. While these subjective measures may capture the deterrence effect of competition policy, they likely are very noisy measures because perceptions among industry executives may vary wildly for a given level of true unobserved strictness. Furthermore, they depend on expectations formed about a country's competition policy. For example, a poorly performing CA doing some good, well-publicized work will get overrated (since the bar is very low) whereas a consistently high-performing CA will likely get underrated. This is supported by the fact that Hylton and Deng (2007) find no effect of CA budget as a fraction of GDP on perceived competition, while Krakowski (2005) finds that perceived CA effectiveness and perceived competition are strongly correlated: it indicates that related perceptions correlate well with each other, but not necessarily with more objective indicators.

I will partially lean on Clougherty (2010) by using the number of cases handled by the Competition Authority as one of my enforcement proxies. I also follow Hylton and Deng (2007) and Buccirossi et al. (2013) by also including their budgets as one of my independent variables. Finally, I use the proportion of mergers not cleared in phase 1 as a proxy. These three indicators each conceptually correspond to a slightly different quality of the competition authority, giving me a well-rounded picture. Budget shows what resources the CA has at its disposal, the number of cases shows the volume of work done, and the proportion of mergers not cleared in phase 1 gives an idea of the CA's stringency within that volume.

2.3 Outcomes

While almost all outcome variables used by different papers are supposed to be proxies for broader prosperity or welfare, there are significant differences between them. In basic theories, stronger competition reduces deadweight loss, thereby increasing welfare through improved output and allocative efficiency. In this vein, there have been several studies using GDP per capita as their dependent variable. Clougherty (2010) finds that increasing a one standard deviation increase in CA budget relative to GDP increases GDP growth by 0.84 percentage points, although he does so using fixed-effects Ordinary Least Squares with some controls. Using a difference-in-differences approach, Gutmann and Voigt (2014) find that the introduction of competition laws increases real GDP per capita levels. In developing countries, it seems to be attributable to increased investment and goes hand in hand with reducing corruption. Petersen (2013) however, finds no immediate effect on nominal GDP per capita using a pooled OLS estimator: according to their research, the increased GDP takes ten years to show up.

Alternatively, one could expect increased allocative efficiency to show itself more clearly in productivity data. This can either be measured with Labor Productivity (GDP per work-hour) or by Total Factor Productivity, also known as the Solow Residual. Indeed, many papers that use TFP as a dependent variable run their same specification on Labor Productivity (LP) as a robustness check. Borrell and Tolosa (2008) find a positive effect of perceived competition on TFP levels. They caution against identification issues, as their OLS estimate is 18% higher than their Instrumental Variable estimate. Buccirossi et al. (2013) use an IV design and find significant positive effects of their CP index on both TFP and LP growth in industries. However, Gutmann and Voigt (2014) find no significant effect on either TFP level or growth at a country level, despite their positive findings on real GDP per capita. Ma (2011) finds an association between Hylton and Deng's (2007) scope indicator and LP levels, but only in developed countries. Voigt (2006) only finds non-robust effects on TFP.

Overall, the evidence seems to be relatively weak, although all significant findings point towards a positive effect of stricter competition policy. Out of the two productivity measures, my preference goes out to using Total Factor Productivity as my proxy, since it is not confounded by changes in the capital stock and is better suited to picking up the market deadweight loss reductions that would theoretically be the main static channel through which competition policy affects welfare.

An overview covering solely the research on static effects would be incomplete: after all, innovation is the main determinant of welfare growth in the long run, and the effect of competition on innovation is less clearly understood and might even run counter to the static effects. Thus, a narrow focus on the static effects is only justified under a very high societal discount rate, where the future welfare increases from innovation do not matter.

This specific debate is often opened with a juxtaposition of Schumpeter (1942) and Arrow (1962). The former argued that innovation necessitated some amount of monopoly rents to pay for (and motivate) research investment, while the latter highlighted the risk of complacency induced by the lack of competitive pressure leading to low innovation efforts. These two viewpoints were synthesized in the influential paper by Aghion et al. (2005) into a hypothesis of an "inverted-U" relationship concerning the level of competitiveness in an industry: they showed that neither too much, nor too little competition leads to high innovation (measured by patenting activity, but that a moderate amount stimulates it most. They found evidence for this by using an IV strategy, with PCM as their independent variable and patenting activity as their outcome. These findings have subsequently been cautiously strengthened by Hashimi (2013), also with patenting as an outcome, and by Polder and Veldhuizen (2012), using TFP. While one could argue against the suitability of patents as a metric for innovation, Aghion et al. (2009) find that patenting measures and productivity measures are similar in their effects. On the whole, this speaks for using productivity as an outcome, especially a rich measure like TFP, as it seems to encapsulate both allocative efficiency effects and the results of innovation.

Finally, there should be some discussion of institutional and policy complementarities. Buccirossi et al. (2013) find that their treatment effect increases with higher scores on the Fraser Rule of Law Index and on the WGI's legal system index. Ma (2011) finds that, once administrative efficiency is included, the scope of competition law only has an effect on Labor Productivity among developed countries. This is to be contrasted with Gutmann and Voigt (2014), who find an especially strong effect in developing countries, but mostly driven by increased capital accumulation. Finally, Voigt's (2006) findings of the effect of *de facto* independence of the competition authority are not robust to inclusion of indicators of general institutional quality.

3. Methodology

For my research, I take advantage of the fact that, in the past few years, many competition authorities have made access to systematic information on their activities easier. Buccirossi et al. (2013) employed an elaborate weighted index considering a variety of aspects of the competition policy, law and enforcement. While the procedural and legal environment can neither be freely ignored nor easily quantified, I think it is valuable to juxtapose an index containing subjective ratings of these factors with a similar research question focusing solely on more quantitative measures of competition authority activity. I therefore use a very similar design, looking for the effect of competition authority activity proxies on industry-level productivity as defined by the NACE rev. 2 classification, and using the government's programmatic position as an instrument.

Following Buccirossi et al. (2013), I use industry-level data on productivity changes as my dependent variable. This data, as well as human capital data used as a control, are retrieved from the EU KLEMS database. It includes a wide array of industry-level growth and analytical accounts. Ostensibly all EU countries plus its main competitor economies (the United States, Canada, and Japan) are included, but data quality varies significantly between countries, with some Eastern European countries being entirely unusable for my productivity calculations. The variables that I use as inputs for productivity calculations are industry value added, labor and capital services compensation, and hours worked by employees.

For robustness, I test all my variables on both Total Factor Productivity (TFP) and Labor Productivity (LP). Both variables are only available as country-industry level index series "out of the box", so I have computed these myself. For LP, this entails a relatively straightforward division of value added by the number of hours worked by employed persons.

The calculation is more problematic for TFP: in order to compute it, it needs to be assumed that capital and labor get paid their marginal products. Buccirossi et al. allow for market power by multiplying the flow of capital and labor services with the average markup in the industry based on Griffith, Harrison and Macartney (2006), but this does not work for my data since the only numbers on capital services are constructed as the complement of labor services. Therefore, I have computed TFP as the residual of the natural log of value added after accounting for the logs of labor and capital services, fitting a new model for each country-industry so as to not "impose a straitjacket" on specific industries or countries. Despite these problems, and despite the fact that TFP is harder to interpret than LP, it will still be valuable to include in my analysis because of the richness of the measure. Due to its residual nature, it can pick up more sources of efficiency improvements, and is also not confounded by capital stock adjustments.

The main difference between my research and that of Buccirossi et al. is the choice of independent variable. While the objective in both cases is to find a proxy for the activity level and stringency of the competition authority, my goal is to use more objective and quantitative proxies. The first of these is the fraction of merger cases that were moved to phase 2 or not cleared at all. While the number of mergers notified is not exogenous (since pre-notification talks exist), the “demand” for mergers can be viewed as such. Therefore, the fraction of mergers sent to phase 2 can be used as a good approximation of the activity level of the competition authority: a less well-supplied CA will subject a smaller fraction of merger cases to a full investigation. These data have been collected by reading through the annual reports of the relevant competition authority: my hand-collected datasets have columns containing URLs to the used documents as well as page numbers, and are available upon request.

Furthermore, I will also perform tests using the total number of merger cases closed and using the CA’s budget, both deflated by GDP. Merger cases closed is a relatively straightforward indicator of activity. I have tried to use the total number of cases handled, but the non-merger cases tend to drag over multiple years. I have found them to be too irregularly reported in the annual reports to develop a consistent rule for assigning them to years, and make up a very small part of cases handled anyway. Budgets are a good indication of the resources the CA has at its disposal, and Clougherty (2010) has shown that it generally tracks other measures of activity. However, they suffer from the fact that I have not been able to find them for a number of countries, including such major jurisdictions as Spain and Italy.

My functional form will also follow Buccirossi et al, using the lagged value of the independent variable, as well as fixed effects for both country-industry and year. The purpose of this is to alleviate endogeneity concerns: prosperous countries can afford to maintain more rigorous competition authorities, adverse short term economic shocks are likely to both increase the two explanatory variables deflated by GDP as well as lowering productivity, and if productivity is indeed related to anticompetitive practices, productivity stagnation in a given country might induce more stringent competition policy.

Furthermore, following Schumpeterian endogenous growth models, one should expect TFP growth to be determined by frontier growth and by distance to the frontier (Aghion and Howitt, 2006). Finally, one should assume that the speed of productivity convergence further depends on other observables such as human capital and trade openness (Griffith et al. 2004). For human capital intensity, I used the share of employed persons with a higher education degree as a proxy, which also was available through the EU KLEMS database. I have tried to also include R&D intensity and import openness as controls, but the data was too sparse. The full equation to be estimated then looks like:

1. $$\Delta P_{(i,j,t)} = \alpha + \beta_1 E_{(i,t-1)} + \beta_2 \Delta P_{F,j,t} + \beta_3 \frac{P_{F,j,t}}{P_{i,j,t}} + \gamma X_{i,j,t-1} + \delta_{i,j} + \zeta_t + \varepsilon_{i,j,t}$$

With $P_{i,j,t}$ being the productivity proxy, $P_{F,j,t}$ the value of the productivity proxy at the frontier (the most productive sector *j* out of all countries at time *t*), $E_{i,t-1}$ the proxy for enforcement, and $X_{i,j,t-1}$ the other control variables mentioned above. Both the naïve OLS and the instrumental variables model are estimated using the plm package in R.

I will use the same instrumental variables as Buccirossi et al.: several indicators about the programmatic position of the government from the Manifesto Project database. This is a dataset covering many countries where for each election, the manifestos of all parties that obtained at least one seat in the legislature were assigned a value indicating what percentage of sentences contained a phrase indicating support for a number of specific positions. These indicators are used as instruments because predicting my explanatory variables from them isolates the variation that is caused by pressure on governments from societal preferences. I assume that this changes more slowly over time than economic conditions, and that it is influenced a lot less strongly by market confounders.

The positions Buccirossi et al. and I use are: need for market regulation (per403), need for economic planning (per404) and need for welfare state limitations (per505). Because data on which parties formed a government is extremely sparse, I have aggregated the positions of the parties by taking the average, weighted by the number of seats each party obtained. The year-by-year data on countries is then obtained by simply filling in forward the values associated with the last election.

4. Results

After harmonizing and combining the different data sources, there are 16 countries, 391 country-industries and 22 years for which data is available on at least one variable. However, no variable covers that entire set, and many cover only a small part. This is exacerbated by the fact that while many of my hand-collected data on the national competition authorities have very good coverage over the years 2017-2019, the EU KLEMS data has so far only been released until 2017. When also taking into account the lagging of my CA variables, I cannot use any of the data for years later than 2016.

As can be seen from the table of descriptive statistics below, this leads to a very high number of missing values: the data on mergers has 3052 NA’s while the dataset this table was created from has only 4692 observations. However, that does not mean that any observation has a uniform 65% chance of being missing. Missing observations tend to cluster very strongly within country-industries and within (early) years. The further back in time, the higher the chance that there are no data available. Once a competition authority starts publishing the necessary statistics in their annual reports, it generally does not stop doing so. On a similar note: if a well-curated dataset like EU KLEMS has no data available for a specific country-industry in a given year, there is a very high chance it does not exist for all the other years. The challenge then becomes formulating a consistent and accountable way of removing years and country-industries, and in doing so balancing the length and breadth of the panel.

Table 1: Descriptive statistics of independent, dependent, and instrumental variables.

Descriptive statistics	Mergers not cleared in phase 1	Handled mergers over GDP	CA budgets over GDP	Change in TFP	Change in LP (NAC ths)	per403_lag (%)	per404_lag (%)	per505_lag (%)
Min.	0	0.000000477	0.000000128	-2.86382	-0.03831	0.26217	0	0
1st Qu.	0.01628	0.00000661	0.00000264	-0.00303	0.02331	2.25320	0.09809	0.23946
Median	0.03509	0.0000555	0.00000548	0.00016	0.04246	3.38462	0.22093	0.92143
Mean	0.07982	0.000143	0.0000167	0.000636	0.37475	3.77611	0.55622	1.01997
3rd Qu.	0.09804	0.000205	0.0000141	0.003904	0.11930	4.88511	0.67799	1.38279
Max.	0.53846	0.000650	0.000210	1.39154	27.4778	8.33671	2.62548	3.85597
NA's	3052	3052	2038	391	534	2038	2038	2038

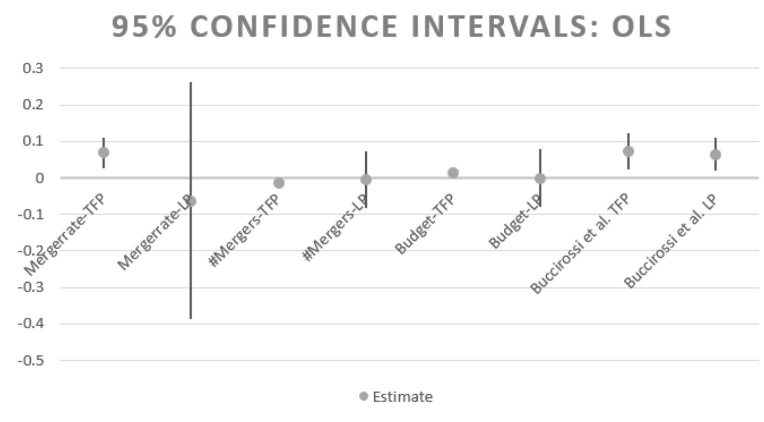
My approach involved a "stepwise" removal of alternating years and country-industries that had too high of a proportion of NAs. I first make an initial sift with a cutoff value of 0.9. I remove all years with more than 90% NAs, and then all country-industries with more than 90% NAs. Then, I repeat the same process but with lower cutoffs. Doing it once more for years with a cutoff of 0.4 and then for country-industries with a cutoff of 0.2 gives the datasets I have used for the results presented below. However, the attached code is structured in such a way that it is very easy to replicate the analysis with differing cutoff values.

This also means that the sample is slightly different for the merger-related indicators and budget indicator, since there are some countries that report the one but not the other. For both the "mergerrate" variable (the fraction of mergers not approved in phase 1) and the total amount of merger cases handled, the panel stretches from 2012 to 201 and includes Austria, the Czech Republic, France, Greece, Hungary, Latvia, and Slovakia for a total of 996 observations. For the "budgets" variable, the panel covers the period 2010-2017 for Austria, Belgium, France, Greece, Hungary, Ireland and Slovakia for a total of 1680 observations.

The naïve OLS regressions already show an interesting divergence with Buccirossi et al.'s results. Whereas the effect of the mergerrate on TFP is significant and roughly in line with the original paper, many others are strongly insignificant. The correlation between budgets and TFP is also significant, but has a point estimate of almost an order of magnitude lower. All other point estimates are negative, and none of the Labor Productivity correlations are distinguishable from noise.

Table 2: Estimates from OLS regressions with standard errors in brackets, significant at 0.05 (*) and at 0.01 (**)

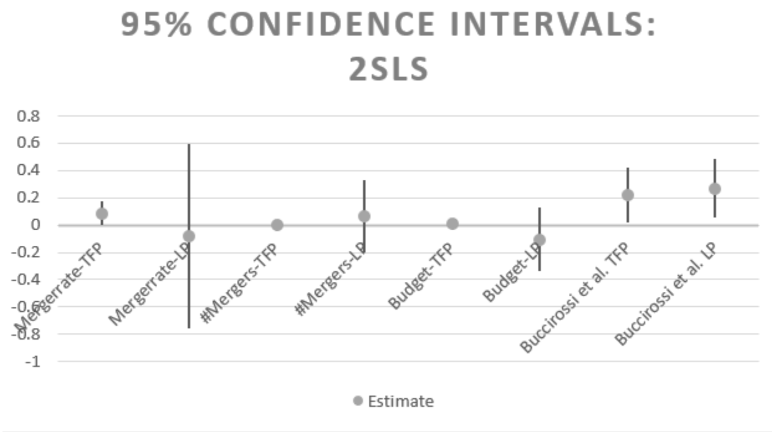
Regressor	Dependent variable: TFP change	Dependent variable: LP change
Lagged mergerrate	0.06906 (0.02133)*	-0.06213 (0.16188)
Natural log of lagged mergers, deflated by GDP	-0.01450 (0.00518)**	-0.00477 (0.03938)
Natural log of lagged CA budgets, deflated by GDP	0.01328 (0.00620)*	-0.000512 (0.03928)



The instrumental variable results tell a similar story. The effect of mergerrate on Total Factor Productivity is still there, indicating that an increase in the proportion of phase II of one percentage point cases raises TFP by 0.09 percentage points on average. However, the differences with Buccirossi et al.'s estimates become larger. As can be seen in the table below, this result can also not be called robust, as none of the other regressions show a significant result, even when taking into account the fact that 2SLS estimators produce larger standard errors. Buccirossi et al. used their IV estimates similarly to a robustness check before focusing on the more efficient OLS estimates, but in my case such an argumentation would be undermined by the differences between the OLS and IV estimates.

Table 3: Estimates from 2SLS regressions with standard errors in brackets, significant at 0.05 (*)

Regressor	Dependent variable: TFP change	Dependent variable: LP change
Lagged mergerrate	0.08773 (0.04448) *	-0.08329 (0.33710)
Natural log of lagged mergers, deflated by GDP	-0.0000289 (0.01762)	0.06481 (0.13221)
Natural log of lagged CA budgets, deflated by GDP	0.00663 (0.01897)	-0.10635 (0.11604)



5. Discussion

This analysis has several limitations. Firstly, it is hamstrung by the aforementioned “early stop” of EU KLEMS data availability. This leaves a promising opportunity to repeat the analysis in a few years’ time, when the WIIW has had the time to process and curate the latest data. Not only would this add three extra years to the panel, but it would also allow for a substantial expansion of the countries covered.

Secondly, there is a risk of measurement error due to not accounting for markups when computing the Solow residuals. To incorporate this factor, it is necessary to have access to independently constructed data on capital services at the industry level. Currently the capital services variable is constructed as the complement of labor services in value added, which automatically sets markups as computed by Griffith and Macartney (2006) to zero. Using the total capital stock as the capital input is also problematic, since it is unknown what fraction of the capital stock is generated as “services”. This fraction will likely be different between countries and between industries.

I have also not succeeded in gathering enough data on the merger notification threshold. I originally intended on including the turnover threshold for merger notification divided by average turnover in a country-industry as a proxy for competition authority attention to that industry. However, while it was generally quite easy to gather data on the *current* notification standards, I have not found reporting about the changes in this standard over time that was widespread enough to construct a dataset from. Since the very latest year that I could use was 2016, I have omitted this part of the analysis.

Finally, there is an opportunity to expand the data collection about the competition authorities. While it would be difficult to acquire much wider data on the budgets, almost all competition authorities maintain catalogues of finished and ongoing cases on their websites. While this likely could be a source of much broader information reaching back further than just the information in the annual reports, I have not used these sources of information. At the beginning of the information gathering process, I did not know which of the variables to be collected would yield a useable amount of data. Therefore, I spread my time gathering information on different indicators, making the time constraints for each individual variable more pronounced. Since the mergerrate variable is the only one with results successfully replicating Buccirossi et al., it would be promising to see if the results hold up in an expanded sample.

6. Conclusion

The aim of competition policy is to increase prosperity by increasing market effectiveness. This paper tries to estimate the effects of better funded, of more active, and of more stringent competition authorities using an instrumental variables approach on industry-level panel data. While this paper is modeled after earlier research by Buccirossi et al. (2013), by and large it fails to obtain similar results. The exception is my indicator measuring the share of merger cases that were not cleared in phase 1: it shows an increase in Total Factor Productivity in the same ballpark as Buccirossi et al., but these findings do not generalize to ordinary Labor Productivity.

In conclusion, this research underlines well-known difficulties in competition policy research: ensuring high-quality proxies for the factors one wants to measure. While my main test cautiously backs up the results obtained by Buccirossi et al., they are not robust to different dependent or independent variables. While improvements to the reporting done by competition authorities have been made since Buccirossi et al. published their paper, there is still room for expanding the sample by gathering harder-to-reach information.

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