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ABSTRACT

Are Rushed Privatizations Substandard? Analyzing Firm-Level Privatization under Fiscal Pressure*

In this paper we provide the first analysis of whether rushed privatizations, usually carried out under fiscal duress, increase or decrease firms' efficiency, scale of operation (size) and employment. Using a large panel of firm-level data from Poland over 1995-2015, we show that rushed privatization has negative efficiency, scale and employment effects relative to non-rush privatization. The negative effect of rushed privatization on the scale of operations and employment is even stronger than its negative effect on efficiency. Our results suggest that when policy makers resort to rushed privatization, they ought to weigh these negative effects against other expected effects (e.g. on fiscal revenue).

JEL Classification: P45, P52, C14, O16

Keywords: privatization, rushed privatization, efficiency, firm size, employment, performance

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1 Introduction

While governments usually set the speed and volume of privatization of state owned firms (SOEs) so that it can be reasonably handled by existing institutions, in many instances governments rapidly privatize a large number of firms. This usually occurs in situations when the government is under fiscal pressure.

These "rushed" privatizations may be undertaken as an autonomous decision of the government or as part of a structural adjustment program carried out by the country in collaboration with external institutions. For example, the privatization program proposed for Greece in 2013 was prepared with the assistance of the International Monetary Fund (IMF), European Central Bank (ECB) and the European Commission, the so-called Troika. Indeed, intensifying privatization was part of the Greek debt restructuring plan and in terms of importance it was on par with fiscal consolidation and labor market reform. The policy objectives for Greece were formulated in terms of funds raised through privatization (€50 billion during 2015-2030 and €22 billion by 2022). Fulfilling the privatization policy objective was a key conditionality in disbursing subsequent installments of financial facilities aimed at Greece's debt buyouts.[1] Similar expectations were in place for Portugal (with a goal of €5.3 billion privatization proceeds between 2011 and 2013). In the Latin American context, while analyzing data from eighteen countries between 1984 and 1998, Doyle (2012) stresses that governments under IMF programs intensified privatizations, *ceteris paribus*.

While easing the fiscal pressure is the usual motivation for rushed privatizations, the question that arises is whether these privatizations tend to produce inferior or superior outcomes in terms of economic performance of firms and countries. An inferior outcome could for instance be brought about by "overwhelming the capacity" of the institutions that prepare and carry out the rushed privatizations (see e.g., Gupta et al. 2008), while a superior outcome could for instance be generated by "focusing the mind and institutional wherewithal" of the given country on the priority task at hand or because privatizations have been carefully prepared for several years beforehand and hence are not really rushed. Alternatively, rushed privatizations could also create less room for asset stripping and cherry picking, thus leading to superior outcomes relative to non-rushed privatizations.

In this paper, we examine this issue by using a large and rich panel of firm-level data from Poland over the 1995-2015 period during which Poland experienced two periods of rushed privatizations in addition to long periods of non-rushed privatization. We analyze the causal effects on total factor

[1]For example, in a joint December 2013 statement with ECB and IMF, the European Commission formulated that fulfilling the privatization objectives is imperative for disbursement of the second tranche of the support to Greece. See http://ec.europa.eu/economy_finance/assistance_eu_ms/greek_loan_facility/pdf/reportcompliancedisbursement-122013_en.pdf. With semi-annual reviews, the IMF provided judgments on stepwise reductions in the €50 billion objective and their feasibility, but some of the EU Member States continued to pressure for the original target, see http://www.imf.org/external/pubs/ft/scr/2016/cr16130.pdf

productivity (TFP), scale of operations and employment level of firms. We focus on TFP because it is a widely used measure of efficiency, scale of operations because it reflects the effect on output, and employment because it is of major policy concern and policy makers often fear that privatization will result in employment decline. We compare the privatized companies to non-privatized SOEs. To identify the causal effects we rely on instrumenting and difference-in-difference approach.

The paper is structured as follows. In Section 2 we provide key insights from the literature on privatization. In Section 3 we present an overview of our data, while in Section 4 we discuss the empirical methodology. In Section 5 we present the empirical results and in Section 6 we summarize the findings and draw conclusions.

2 Insights from literature

The empirical literature about the effects of privatization on firm performance is divided in its findings. One set of (primarily earlier) studies suggests that performance indicators are higher after privatization than before[2] and some studies also find that privatized firms tend to outperform SOEs.[3] On the other hand, a number of studies from the transition economies, many of which are surveyed in Estrin et al. (2009), suggest that privatization to domestic investors reduces efficiency, while privatization to foreign owners improves it. Indeed, as Sabirianova-Peter et al. (2012) show, firms privatized to foreign owners tend to catch up with the global production frontier while those privatized to domestic owners often do not.

The recent literature makes it clear that the incidence of privatization is usually not a random event (see e.g., the meta-analysis of Djankov and Murrell, 2002, and the review by Estrin et al., 2009). Indeed, there appear to be two important sources of selection bias: (a) that of the state, deciding which companies to sell and whether to give up majority shareholding, (b) that of an investor, deciding which companies to buy and whether to accept minority shareholding.[4] Interestingly, both Djankov and Murrell (2002) and Estrin et al. (2009) show that attempts to address the problem of selection bias are found in only a minority of studies. Overall, the various studies suggest that economic efficiency is enhanced by some, but not all, types of privatization.

Looking at a broader concept of performance spillovers, Hanousek et al. (2011) argue that the relatively larger effects of FDI on domestic firms in the context of large scale privatization stem from a likely publication bias as well as an unaddressed selection bias. Indeed, Hanousek et al. (2011) argue

[2] See for instance Megginson et al. (1994) for the UK; Lopez-de Silanes et al. (1997) for the US; Smith et al. (1997) for Slovenia; Barberis et al. (1996) for Russia; Harper (2002) for Czech Republic; D'Souza et al. (2005) for 23 OECD countries.
[3] See e.g., Vining and Boardman (1992), Anderson et al. (1997) and Konings et al. (2005).
[4] Note that unsuccessful negotiations (attempted but not realized privatizations) are usually absent from the data used in empirical analyses.

that fixed effects panel estimators that are by their nature less prone to suffer from this problem, yield lower estimates. Unfortunately, panel data are typically unavailable for the studies of early transition.

Examining studies of non-transition countries, Goerg and Greenaway (2004) and Crespo and Fontoura (2007) find that the literature on FDI spillovers is still weak in identifying factors that have positive effects. Gorodnichenko et al. (2014) argue that the only consistent determinant is the strength of the backward (upstream) linkages between foreign-owned and domestic firms. Girma et al. (2014) emphasize the role of majority shareholding for observing any effects of FDI on firm performance. Higher private ownership concentration post privatization tends to be associated with higher efficiency (see Cabeza-Garcia and Gomez-Anson 2011).

The timing and the mode of privatization seem to matter as well. Indeed, performance has been shown to improve already *before* privatization (e.g., Megginson and Netter, 2001, and Gupta et al., 2008). The mechanism explaining this pattern was suggested already by DeWenter and Malatesta (2001), who point to the role of internal decision-making processes in the companies anticipating privatization. Conceptually, improved pre-privatization performance may result from either window dressing (SOE managers want to attract investors and remain in the managerial position) or from bargaining with the decision makers (supplying the state budget with a higher dividend may be a convincing argument to prevent sale by the state, thus helping the managers to maintain *status quo*). Similar anticipation effects have been found in the context of mergers and acquisitions (see Becher et al. 2012).

Another observed phenomenon is that investors buy better firms -- the so-called “cherry picking”. Nguyen and Ollinger (2006) for example show that in the US meat market it was the better performing plants that were purchased, both up- and downstream. Kim and Lu (2013) estimate impact of changes in corporate governance regulations on the tendency that foreign investors pick better performing firms in emerging markets and confirm that higher gap in investor protection between the country of the acquiring company and the country of the acquired one is conducive to cherry picking.[5] However, this need not be the only or the even the main channel of relationship between pre-privatization change in performance and eventual privatization: In analyzing the insurance industry, Cummins and Xie (2008) show that domestic mergers and acquisitions (M&As) may be motivated predominantly to increase the buyer business diversification, not to increase shareholder value.[6]

[5] In this paper we study the case of a transition country. Cherry picking appears to be prevalent in the transition countries because investor protection there is weaker.

[6] Cherry picking and the opposite “fire sale” phenomena are also reported in the merger and acquisition literature. Erel et al. (2012) demonstrate that valuation plays a role in motivating mergers. Analyzing nearly 20 years of firm-level evidence for nearly 60 000 M&A transactions among the publicly traded firms they show that spikes in stock market value (and relatively high market-to-book value) tend increase the probability of acquiring, whereas the opposite holds for the probability of being

Finally, a number of studies suggest that political economy factors may be important. Three political economy factors stand out. First and most relevant finding from our perspective is that these various privatization effects interact with the fiscal position of the government. While liquid stock markets and GDP per capita are positively correlated with privatization intensity, there is also a close link between public debt and governments' willingness to privatize, as demonstrated by Bortolotti et al. (2004). Indeed, budgetary constraints tend to be conducive to state divesting, which introduces a useful exogenous variation in the privatization decision. Importantly, this effect is universal and not dependent on the political orientation of the government and extent of coalition fragmentation. Moreover, Bortolotti and Faccio (2009) argue that in a majority of privatization events in the 1990s governments retained control over privatized firms, which suggests that the need to raise financing is a dominant motivation, whereas investors become accustomed to having only minority shareholding.

Second, privatizations are more intensive in winner-takes-all democracies, while coalition democracies seem to be characterized by delayed privatizations (see Bortolotti and Pinotti 2003). Moreover, more fragmented coalitions delay privatizations (see Bortolotti and Pinotti 2008). Both of these results are obtained with cross-country datasets developed by the World Bank between mid-1970s and 2000s and as such they encompass a number of business cycles and a large selection of countries. This is an implicit and fairly universal suggestion that a decision to divest indeed is the matter of choosing both the timing and companies to be sold. Policy-makers with greater concerns about re-election also tend to choose domestic investors. Roberts and Saeed (2012) go as far as to state that

acquired. All these findings point to strong 'cherry picking' as well as the so called 'fire sales'. According to the fire-sale FDI hypothesis, countries affected by a crisis experience divestment by foreign owners, who sell assets at a discount. Aguiar and Gopinath (2005) find analyzing the East Asian crisis of late 1990s that firm liquidity plays a significant and sizable role in explaining both the increase in foreign acquisitions and the decline in the price of acquisitions during the crisis. Weitzel et al. (2014) find for the EU that a crisis has a dampening effect on cross-border transactions. More importantly, countries with higher sovereign default risk and lower economic demand attracted more foreign buyers in the crisis Fuchs and Uebelmesser (2014) construct a general equilibrium model where regardless of government benevolence there always is scope for privatizing too much relative to the social optimum.

Moreover, hand in hand with the pre-privatization over-performance may go a post-privatization underperformance. The merger and acquisition literature for instance suggests that shareholders tend to react to news about acquisitions. Some studies show that gains to shareholders emerge among acquired companies, not the buyers (see Datta et al. 1992). Also, evidence from the event studies shows positive abnormal returns between merger announcement and actual merger, but zero or small negative abnormal returns is the subsequent period, which seems to suggest that gains were expected but did not materialize (see Haleblian et al. 2009). A survey of this literature is also provided by Haleblian et al. (2009). Due to data availability the studies have been most extensive in analyzing bank mergers. Given the specificity of this sector, we abstract from financial services in the reminder of this paper. According to Fuller et al. (2002) the reaction is positive only to news about acquiring a private target - there is a negative abnormal return for episodes of acquiring a SOE. Antoniou et al. (2007) shows that this difference is only short-run. However, in the long run, buyers experience significant wealth losses regardless of the target type acquired. Indeed, the ''Ashenfelter dip" may stem from the initially high costs of adaptation (production processes, change of technology, supply chains, sales channels, marketing policies, etc.). Since the anticipation effects and the adjustment effects are likely to work in the opposite directions, the time span over which the causal effects of privatization are observed plays an important role. For example, Brown and Earle (2007) analyzing the case of Ukraine, argue that the difference between the productivity gain between the domestic and the foreign investors in their sample may be partly explained by different entry and sale models operated by the Ukrainian government vis-a-vis the two types of investors.

economic factors explain an unsatisfactorily small fraction of variation in the intensity of privatization and privatization revenues between and within countries, showing a great role for the political factors.

Third, analyzing instances of privatization from 27 developing and 14 developed countries between 1980 and 2002, Boubakri et al. (2008) show that approximately 30% of newly privatized firms have a politician or an ex-politician on the board of directors. Moreover, government residual ownership reduces foreign ownership and increases the probability of politicians directly being involved in management. Boubakri et al. (2011) demonstrate that these findings are universal to both right- and left-oriented governments. Importantly, politically-connected firms exhibit a poor accounting performance compared to their not politically-connected counterparts. Ben-Nasr et al. (2012) show that the cost of equity is higher for politically-connected newly privatized firms and for companies with higher chief executive turnover, whereas the two effects tend to reinforce each other. Also Dinc and Gupta (2011) point to the relevance of political connections.

Overall, the political economy literature indicates that fiscal pressures and other political factors often lead to rushed privatizations. Yet, the effects of such privatizations on performance of firms have not yet been investigated. Moreover, the literature suggests that using high quality data and carefully addressing the selection issues are of particular relevance. We turn to data and methodological issues next.

3 Data

Our annual firm-level data covering the period 1995-2013 come from a census report collected by Poland's Central Statistical Office from all firms employing over fifty (full time equivalent) employees. Using the census data is important for two reasons. First, each year we have on average complete and consistent information on nearly 46,000 firms.[7] Second, this being census rather than a sample, we analyze the developments in the entire enterprise sector.[8] This is also the only complete firm-level data set that covers manufacturing, services, mining, and utilities. The dataset includes information on the two-digit industry of the firm, employment, location, and complete profit-loss statements and balance sheets. We construct a panel data set using a unique firm registration number that does not change over time for a given economic unit.[9]

[7] In comparison, the widely used BEEPs and Amadeus data are much more limited. BEEPS for instance contains only about 1200 firms from Poland, while Amadeus comprises less than 5000 firms for Poland in total prior to 2002.
[8] Firms covered by our sample constitute a significant part of the economy. They employ roughly 90% of the enterprise sector employment, and 42% of all persons employed on a contract basis.
[9] For details on sample atrophy, see Appendix C.

From the perspective of this study the additional advantage of using the census data is that it contains detailed information on firm ownership. In particular, the data show whether a firms is majority state owned, majority private or has a majority or minority share of foreign ownership (data are categorical and do not report actual ownership share).[10] We identify 1,461 cases of privatization – situations in which state was a majority owner and stopped being so in a given year. Using this definition of privatization enables us to capture a majority divestment by the state, one that involves giving up control.

Our data are anonymized and we hence cannot compare it at the firm level to privatization reports by Poland's Treasury. Moreover, the Treasury data reports as privatization every incidence when the state divested, even just partially. Hence, the same firm may appear several times in the Treasury records of privatization, an approach that makes the two samples hard to compare. However, when we integrate out the multiple transactions on the same business entity, the Treasury report for the years analogous to our sample covers 1,601 instances of privatizations, relative to 1,461 in our census data, which implies that our data identifies roughly 91% of all privatizations. Some cases of privatizations missing in the census data concern firms employing less than 50 FTE employees.

The 1,461 cases of privatization that we analyze constitute a relatively large number by the standards of the privatization experiences and analyses. Our data start in 1995 with 4,326 SOEs and 12,537 private firms. By 2015, only 1,098 SOEs remain, while many new private firms have been created. In comparison, Frydman et al. (1999) for instance analyze 506 firms from three countries with 128 instances of privatization, D'Souza et al. (2005) have 129 instances for 23 OECD developed economies and Boubakri et al. (2008) analyze 245 instances of privatization from 27 developed and 14 developing countries. By these standards, the data set analyzed in this study is uniquely large and is (comparable to the relatively few studies that also examine large data sets, e.g., Brown et al., 2006; Sabirianova et al., 2012).

In terms of key statistics, our data indicate that the former SOEs were larger than private firms, but on average they were much less profitable. In addition, they more frequently operated in industries in which foreign investors were present. The before-after change in output has been larger among the private firms, though, with larger increase in capital and smaller reduction in employment.

4 Empirical methodology

As may be seen from the data on the relative frequency of privatization given in the left-hand side panel of Figure 1, one wave of privatizations occurred in Poland in 1997. Moreover, data on financial proceeds from privatization (not reported in Figure 1) reveal that in 2001 the number of firms privatized was

[10] For details on ownership and privatization definitions, see Appendix B.

associated with an unprecedented spike in the proceeds from privatization. Years 1997 and 2001 hence constitute candidates for rushed privatizations. There is another spike in 2005, but since the number of remaining SOEs was by then small, the number of companies privatized in 2005 was much smaller than that in 2001.

[Insert Figure 1 about here]

As may be seen from the right-hand side panel of Figure 1, there is a positive relationship between the share of firms to be privatized and fiscal pressure defined as the percentage of annual budget deficit that the government registers as spent by June 30 of each year. This variable takes on values between 13% and 98%, with a mean of 58%. It is strongly correlated with the intensity of privatizations -- the correlation coefficient is 0.63 with a p-value of 0.027 on less than 20 observations. These data hence suggest that rushed privatization may indeed be related to the fiscal pressure faced by the government.

In order to estimate the effect of rushed privatizations, we pursue three complementary identification strategies. First, we estimate the additional effect of rushed privatization on top of the average effect of non-rushed privatization. This amounts to estimating the difference-in-difference-in-difference (triple difference): a difference between before and after privatization; between a treatment group and a control group; and between rushed privatizations and non-rushed privatizations.

Second, in the rush periods the state may privatize SOEs that are different from those in non-rush years. For example, larger companies, with higher value of assets permit the government to meet the demanding privatization revenue plans faster. In order to control for the plausible selection effects, we also generate estimates using propensity score matching prior to privatization. These scores are then utilized to reweight the populations in the spirit of Blundell and Costa Dias (2000).

Third, the very decision to privatize may be driven by firm characteristics, which in turn may drive the subsequent output independently of whether the firm is privatized or not. To address this issue we utilize a novel instrument for the privatization decision. We discuss the control group definition and the three methodological approaches next.

4.1 Control group

In analyzing the impact of rushed privatization, we adopt the usual approach of using the non-privatized SOEs as a control group. The use of any control group presents an estimation issue. The timing of privatization creates a natural anchor in time for the privatized SOEs in that analyses typically demonstrate a change in performance after privatization (e.g. Harper, 2002). However, as discussed earlier, standard estimation approaches are likely to be flawed by anticipation effects, as well as the ‘Ashenfelter dip’. To avoid this problem, one needs an analogous anchor in time for the control group of firms. Hence, we step up from a before-after (difference) approach into a difference-in-difference (DID) approach.[11] In particular, we randomly allocate a *placebo* privatization year among the non-privatized SOEs. In allocating the placebo privatization, we mimic the time intensity of actual privatizations among the SOEs (recall Figure 1). In other words, in each year of our data, the non-privatized SOEs are characterized by the same distribution of the placebo probability to be privatized, as is actually observed for the privatized SOEs.

The anchor created by the privatization event (true in the case of SOEs and placebo in the case of non-privatized SOEs) allows one to identify the effects of privatization comparing the *ex ante* to *ex post* performance of the firm. The interpretation of the placebo privatization is crucial for the control group firms: since nothing really happened to these firms, we expect no systematic changes in their performance measures around the year in question. However, the majority of privatizations in our sample happened in 1997 and over the 2001-2005 period, which were specific years. The former was a period of relatively robust GDP growth, historically low unemployment and high job creation in general. The latter period encompasses a major economic slowdown, with the unemployment rate mounting to 20%, low job creation and high job destruction. To address this point, all of our specifications contain controls for both absolute time (i.e. the calendar year) and the relative time (i.e. the time before/after the event of privatization, true or placebo).

4.2. Propensity score matching and instrumental variables

Reweighing the DID regression through the propensity score matching weights

Our first identification strategy, the DID approach, is reliable if one can satisfy the commonality of trends prior to treatment. If the conditional independence assumption (CIA) implied in the DID approach were questionable and if the two samples (treated and control firms) were to be different in relevant ways, one would need to worry about ‘comparing the comparable’. Developing weights based on propensity score matching and applying them in the DID framework addresses this problem by utilizing to the full extent both the observable and unobservable heterogeneity in estimation (Blundell and Costa Dias, 2000; Hirano et al, 2003, Imbens and Wooldridge, 2006). In order to satisfy the balancing properties, we rely on moments estimation of the propensity score, as proposed by Imai and Ratković (2013). This algorithm ensures that all covariates are automatically balanced. The matching variables include everything that could be known about the firm to an outsider prior to privatization: assets, employment, profits, return on assets, capital-labor ratio, debt, value added, share of export sales in

[11] Harper (2002) explores a natural anchor of the so-called ‘wave’ privatizations, as followed by the Czech Republic. Such policy, however, was relatively rare among the European transition countries.

revenues, and industry dummies. The matching variables reflect firm-specific characteristics whose values can be known by the government and investors before they make their decisions on privatization. We thus use as matching variables a one year lag of assets, structure of costs, capital, employment, and profits. Following Huber et al. (2013) we recode all continuous variables as categorical variables (decimal groups) and interact them to improve the balancing properties of the matching procedure. All variables are interacted.

Once we obtain a propensity score, given the relative sample size of privatized SOEs to control groups and large heterogeneity of firms, we employ kernel matching with Mahalanobis metric on industry and within caliper (D'Agostino and Rubin 2000). We only match within the same year and within the same industry (taken at 2-digit NACE). Weights obtained in this way will then subsequently be introduced in the DID regressions.

There are two specifications for all three stages: propensity score, matching and eventually weights. In the first one, privatized SOEs are the treated group and the non-privatized SOEs constitute the control group. In the second one, SOEs are the control group and we reweight the distribution of firms privatized in rush years to the distribution of firms privatized in other years. Hence, in the second approach, the treatment variable is the rush year.

Instrumenting

The second identification strategy employed in this paper is based on instrumental variables (IVs). We build on an innovative idea of Bloom et al. (2016) who analyze the effects of closing hospitals on a variety of health and economic outcomes in the United Kingdom. They posit and find empirical support for the hypothesis that in marginal constituencies, where the government party is at risk of losing a seat, hospitals are less likely to be closed.[12] We use this insight and construct an instrument that measures whether a governing coalition is likely to lose a seat in a given voting district. In particular, we measure the number of seats in parliament to be gained/lost by a current governing coalition in a given voting district in a given year to instrument for the probability that firms from that region are privatized.

The measure of seats to be lost is obtained in a way that differs from Bloom et al (2016), since Poland is a majoritarian voting political system. In this system, the number of votes in every voting district is allocated based on the total number of votes in the country, using the d'Hondt rule. Knowing general election polls, each politician may estimate his instantaneous probability of keeping the seat, losing it or obtaining one more seat in every voting district. Our instrument thus has both a regional and time variation.

[12] See also Mitra and Mitra (2015) for a similar use of marginal constituency as instrument for analysis of inequality and redistribution in the context of India.

As in Djankov and Murrell (2002) and Estrin et al. (2009), we also include FDI intensity in a sector as a regressor in the first stage regression with the rationale that this may capture the 'demand' from the foreign investors to establish production facility in Poland. This indicator is measured by the share of foreign affiliates in all firms active in a given sector - it takes an average value of 4% and ranges between 0 and 50% over sectors and years. We also include a variable that measures how many SOEs are still left to be privatized in each sector. This reflects the potential supply of firms to be privatized. As we show in Appendix D, all variables in the first stage have the expected signs and the political instrument is very significant with a relatively high explanatory power.

4.3 Estimation

As mentioned earlier, we adopt a triple DID strategy and estimate the differences in trend performance between firms privatized in rush times and firms privatized in non-rush times on top of carrying out a before-after estimation comparing differences between privatized and non-privatized firms. In doing so, we instrument for the privatization decision.

We estimate the effect of rushed and non-rushed privatization on total factor productivity (TFP)[13] by estimating a production function[14] with firm fixed effects:

$$\ln(VA)_{i,t} = \beta_0 + \beta_{1j}\ln(K)_{i,t} + \beta_{2j}\ln(L)_{i,t} + \gamma priv_i + \theta rush_i + \delta priv_i * rush_t * TE + \epsilon_i + \varepsilon_t, (1)$$

where VA = value added (deflated by industry-specific producer price deflators),[15] K = capital (fixed assets + intangible assets at current market value, also deflated), L = labor (full-time equivalent employees) and TE = annual time effects measuring years before and after privatization. The coefficient of interest is δ, denoting the effect of rushed privatizations in time after privatization (accounting for the calendar year, privatization and rush), for each of the true privatizations. Given our model specification, ϵ_i and ε_t are uncorrelated.[16] The production function is estimated as Cobb-Douglas with sector-specific slope coefficients and we verify that our results are robust to alternative assumptions about production technology by estimating also a translog production function.

[13] In the remainder of this paper we take TFP as our measure of efficiency. However, it is worth noting that the literature is expanding to comprise also other approaches. For example, Siegel and Simons (2010) analyze the effects of mergers and acquisitions on firm productivity in the framework of the human capital approach, showing empirical evidence that such transactions constitute a mechanism for improving the sorting and matching of plants and workers.

[14] Literature on firm-level heterogeneity in productivity is massive. Bloom and Van Reenen (2010) provide an excellent review of the empirical findings. In the context of multinational enterprises the literature receives tribute in Wagner (2011). Equally numerous are the ways to adequately estimate the production function itself – relatively recent developments have been reviewed in de Loecker (2011) and Van Beveren (2012)

[15] Value added = Gross profit + (Wages+ Non-wage employment costs) + Interest + Income tax + Taxes + Depreciation.

[16] A potential source of bias in (1) remains the response of firms to productivity shocks (Olley and Pakes, 1996, Levinsohn and Petrin, 2003). However, given the before-after framing of our model, this problem is not likely to affect the coefficient of interest.

In addition, we estimate the unconditional effect of non-rush and rushed privatization on the scale of firms' operation by regressing value added on the privatization variables without controlling for labor and capital. These estimated effects of privatization indicate whether firms tend to become smaller or larger after privatization, an issue that is important from a policy standpoint, given that there are frequently concerns that privatization will reduce the size of firms:

$$ln(VA)_{i,t} = \beta_0 + \gamma priv_i + \theta rush_i + \delta priv_i * rush_t * TE + \epsilon_i + \varepsilon_t \quad (2)$$

A closely related policy concern is that privatization will result in a reduction in employment in the privatized firms. We therefore also estimate the unconditional effect of non-rush and rushed privatization on firms' employment by regressing employment on the privatization variables:

$$ln(L)_{i,t} = \beta_0 + \gamma priv_i + \theta rush_i + \delta priv_i * rush_t * TE + \epsilon_i + \varepsilon_t \quad (3)$$

5 Summary statistics and estimation results

The summary statistics of the main variables used in the analysis are provided in the appendix Table A1. The variables display reasonable mean and extreme values, as well as considerable variance. Given that we assign placebo privatizations to non-privatized SOEs, we also replicate the approach of Harper (2002) and run Wilcoxon test of equality of medians of privatized firms and SOEs, comparing them *before* and *after* the event of privatization (Table A2). The results demonstrate that the difference between the two sets of firms in inputs were negligible or insignificant before the privatization. In fact, there are only small differences in K/L ratio that stem mostly from the sectoral selection into privatization. However, post-privatization inputs diverge between the privatized firms and those that remain SOEs, with the difference being most marked with respect to employment policies.[17]

In Table 1 we report selected estimated coefficients of equations (1), (2) and (3), with the privatization and rushed privatization dummy variables being interacted with a time dummy variable taking on value zero before privatization and value one after privatization. The estimated coefficients hence provide the average effect over time of non-rushed privatizations and rushed privatizations, with the latter effect being measured as an additional effect relative to that of non-rushed privatization. As may be seen from the first two columns of the table, which correspond to equation (1), non-rushed privatizations are estimated to be associated with a 20.6 percent increase in efficiency when the estimate reflects DID with rebalancing weights coming from a (propensity score) matching of privatized to non-

[17] These results cannot be interpreted in causal terms, because which company gets privatized and the timing of this process may be endogenous. In the case of Czech Republic, as analyzed by Harper (2002) this choice was partially exogenous to the firm performance due to the *ex ante* split between the "waves" of privatization. However, the reasons for a firm to be placed in the first or in the second wave could be largely dependent upon firm performance and thus particular political interests as well. To address this point in analyzing Czech Republic, Sabirianova et al. (2012) apply alternative identification strategies to analyze the role of ownership in firm performance.

privatized SOE firms (DID STATE in Table 1), and 14.9 percent when the estimate is based on DID with rebalancing weights coming from matching of rushed privatized to all other (non-rush privatized and SOE) firms (DID RUSH in Table 1).[18]

[Table 1 about here]

The estimated coefficients on rushed privatization in turn indicate that rushed privatization reduces efficiency by 17.2 percent relative to non-rushed privatization when the estimate reflects DID with rebalancing weights coming from a matching of privatized to non-privatized SOE firms, and 13.8 percent when the estimate is based on DID with rebalancing weights coming from matching of rush privatized to all other firms. The results hence suggest that non-rush privatization has a positive effect on TFP, while rushed privatization is found to have a negative effect and in both cases this negative effect virtually offsets the positive effect of non-rush privatization and more than offsets it when private firms are taken as the comparison group.

In columns 3 and 4 of Table 1 we report estimated coefficients of equation (2) with privatization and rushed privatization dummy variables being interacted with a single time dummy variable taking on value zero before privatization and value one after privatization. In these columns we provide estimates of the basic relationship between privatization and the firm's scale of operations. As may be seen from the table, non-rush privatizations are associated with a 27.5 percent increase in the scale when the estimate reflects DID with rebalancing weights coming from a matching of privatized to non-privatized SOE firms (column 3), and 19.0 percent when the estimate is based on DID with rebalancing weights coming from matching of rush privatized to all other firms (column 4).

The estimated coefficients on rushed privatization in turn indicate that rushed privatization reduces scale by 33.1 percent relative to non-rush privatization when the estimate reflects DID with rebalancing weights coming from a matching of privatized to non-privatized SOE firms, and 31.6 percent when the estimate is based on DID with rebalancing weights coming from matching of rush privatized to all other firms. These findings suggest that the negative effect of rushed privatization on the scale of operations is even stronger than its negative effect on TFP.

In columns 5 and 6 of Table 1, we report the estimated effects on employment (equation (3)), with privatization and rushed privatization dummy variables being interacted with a single time dummy variable taking on value zero before privatization and value one after privatization. In these columns we provide estimates of the basic relationship between privatization and the firm's employment setting. As may be seen from the table, non-rush privatizations are associated with a 12.7 percent increase in

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[18] Strictly speaking, all the estimated coefficients refer to log point effects. Since these effects are not large, in presenting the results we use the term percentage effects as an acceptable approximation.

employment when the rebalancing weights come from a matching of privatized to non-privatized SOE firms (column 5), and 4.4 percent when the rebalancing weights come from matching of rush privatized to all other firms (column 6).The estimated coefficients on rushed privatization in turn indicate that rushed privatization reduces employment by 28.6 percent relative to non-rush privatization when the rebalancing weights come from a matching of privatized to non-privatized SOE firms, and 24.5 percent when the rebalancing weights come from matching of rush privatized to all other firms. These findings first of all suggest that on average non-rushed privatizations have a significant positive, rather than the frequently expected negative, effect on employment. Second, they indicate that the effect of rushed privatization on employment is negative and considerably larger that the positive effect of non-rushed privatization. Moreover, the negative effect is much larger than the corresponding effect on TFP.

In Table 2 we report estimated coefficients from equations (1), (2) and (3) in which we run the same specifications as before but use the IVs described in Section 4 above. Columns (1) and (2) again report the estimated effects of non-rush and rushed privatization on TFP, columns (3) and (4) the effects on scale of operations and columns (5) and (6) the effects on employment.[19] These IV estimates are very similar to the estimates reported in Table 1. In particular, as may be seen from the first two columns, non-rushed privatizations are estimated to be associated with a 18.7 percent increase in efficiency when the estimate reflects DID with rebalancing weights coming from the matching of privatized to non-privatized SOE firms, and 14.6 percent when the estimate is based on DID with weights coming from matching of rushed privatized to all other (non-rush privatized and SOE) firms.

The estimated IV coefficients on rushed privatization in turn indicate that rushed privatization reduces efficiency by 14.7 percent relative to non-rushed privatization when the estimate reflects DID with rebalancing weights coming from a matching of privatized to non-privatized SOE firms, and 11.3 percent when the estimate is based on DID with rebalancing weights coming from matching of rush privatized to all other firms. The results are virtually identical with those reported in Table 1 and they suggest that non-rush privatization has a positive effect on TFP, while rushed privatization is found to have a negative effect and in both cases this negative effect virtually offsets the positive effect of non-rush privatization. The results are virtually identical with those reported in Table 1 and they suggest that non-rush privatization has a positive effect on TFP, while rushed privatization is found to have a negative effect and in both cases this negative effect virtually offsets the positive effect of non-rush privatization.

In columns 3 and 4 of Table 2 we report estimated IV coefficients of equation (2), relating privatization to the scale of operations. As may be seen from the table, non-rush privatizations are associated with a 25.9 percent increase in the scale when the estimate reflects rebalancing weights that come from a matching of privatized to non-privatized SOE firms (column 3), and 19.3 percent when the

[19] The first stage specification and estimates are reported in Appendix.

estimate is based on DID with rebalancing weights coming from matching of rush privatized to all other firms (column 4). The corresponding estimated coefficients on rushed privatization in turn indicate that rushed privatization reduces scale by 31.5 percent relative to non-rush privatization when the estimate reflects rebalancing weights coming from a matching of privatized to non-privatized SOE firms, and 30.6 percent when the estimate is based on DID with rebalancing weights coming from matching of rush privatized to all other firms. As in Table 1, these findings again suggest that the negative effect of rushed privatization on the scale of operations is even stronger than its negative effect on TFP.

In columns 5 and 6 of Table 2, we report the IV estimates of the effects of privatization on employment (equation (3)). As may be seen from the table, non-rush privatizations are associated with a 7.8 percent increase in employment when the rebalancing weights come from a matching of privatized to non-privatized SOE firms, and 13.2 percent when the rebalancing weights come from matching of rush privatized to all other firms. The estimated coefficients on rushed privatization in turn indicate that rushed privatization reduces employment by 16 percent relative to non-rush privatization when the rebalancing weights come from a matching of privatized to non-privatized SOE firms, and 16.6 percent when the rebalancing weights come from matching of rush privatized to all other firms. These findings suggest even more strongly than the estimates in Table 1 that non-rushed privatizations have a significant positive, rather than negative, effect on employment. Moreover, they indicate that the effect of rushed privatization on employment is negative and probably larger that the positive effect of non-rushed privatization. Finally, the negative effect is similar or possibly even larger than the corresponding effect on TFP.

6 Summary and Conclusions

A typical policy recommendation for a country with relatively large public sector and fiscal imbalances includes privatization of state owned enterprises (SOEs). Such policy is expected to relieve budgetary constraints and possibly also yield productivity improvements among privatized firms. While governments usually privatize SOEs with a speed and volume that can be reasonably handled by the existing institutions, in the instances of strong fiscal pressure they are forced rapidly to privatize a large number of firms. These "rushed" privatizations may be undertaken as an autonomous decision of the country or as part of a program carried out by the country in collaboration with an external institution such as the International Monetary Fund, the European Central Bank and the European Commission (as in e.g., Greece).

In this paper we provide the first analysis of whether rushed privatizations tend to produce superior or inferior outcomes in terms of economic performance of firms and countries. A superior outcome could for instance be generated by "focusing the mind and institutional wherewithal" of the given country on the priority task at hand. In contrast, an inferior outcome could be brought about by “overwhelming the capacity” of the institutions preparing and carrying out the privatizations.

Using a large and rich panel of firm-level data from Poland over the 1995-2015 period during which Poland experienced two periods of rushed privatizations in addition to long periods of non-rushed privatization we find that non-rushed privatizations are on average associated with higher efficiency (TFP), while rushed privatization has a negative effect relative to non-rush privation and this negative effect virtually offsets the positive effect of non-rush privatization.

Our results with respect to the scale of operation of firms indicate that non-rush privatization are associated with an increase in the scale of operations. The estimated coefficients on rushed privatization in turn indicate that rushed privatization reduces the scale of operations. These findings suggest that the negative effect of rushed privatization on the scale of operations is even stronger than its negative effect on TFP.

Our findings with respect to employment suggest that non-rushed privatizations have a significant positive, rather than the frequently expected negative, effect on employment. Second, they indicate that the effect of rushed privatization on employment is negative and considerably larger that the positive effect of non-rushed privatization. Moreover, the negative effect is much larger than the corresponding effect on TFP.

Overall, our results suggest that when policy makers resort to rushed privatization, they should expect the effect on efficiency and size of privatized firms to be inferior relative to non-rush privatizations. This effect obviously needs to be compared to other expected effects (e.g., on government revenue) of rushed privatization.

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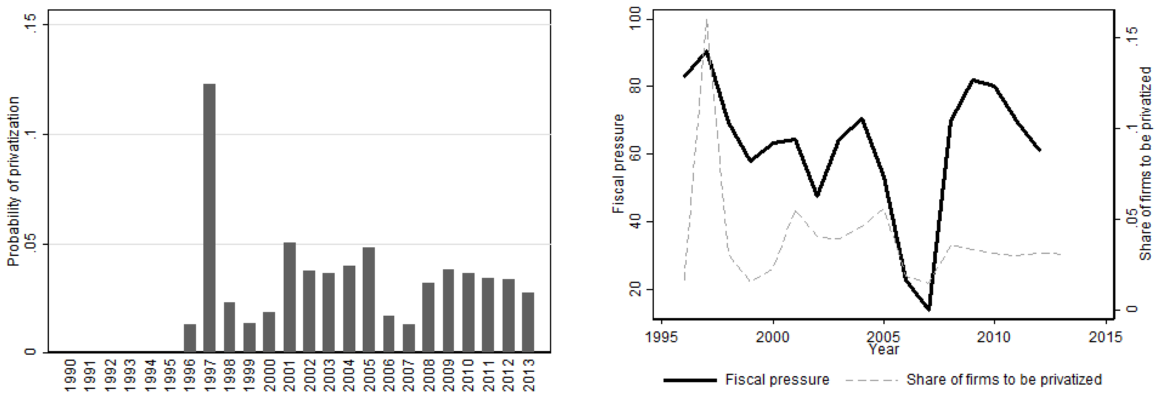
Figures and Tables

Figure 1: The time intensity of privatizations in the sample and relationship with fiscal pressure



Source: firm census data for privatizations and Ministry of Finance for fiscal data. *Notes:* census data reflect firms with 50+ FTEs

Table 1. Estimates of the average effect of rushed and non-rushed privatization

	log VA - Cobb-Douglas with sector specific slopes		log VA - No inputs		Employment	
	(1)	(2)	(3)	(4)	(5)	(6)
	DID STATE	DID RUSH	DID STATE	DID RUSH	DID STATE	DID RUSH
Non-rushed privatization before	-0.028	-0.035	-0.105*	-0.061*	0.054*	-0.018
	(0.049)	(0.029)	(0.056)	(0.031)	(0.032)	(0.017)
Non-rushed privatization after	0.202***	0.149***	0.275***	0.190***	0.127***	0.044***
	(0.050)	(0.029)	(0.056)	(0.032)	(0.031)	(0.017)
Rush before	-0.056	-0.029	-0.124*	-0.089***	0.062*	0.036**
	(0.056)	(0.027)	(0.066)	(0.031)	(0.036)	(0.017)
Rush after	-0.031	0.002	-0.065	-0.013	0.073**	0.056***
	(0.052)	(0.025)	(0.059)	(0.028)	(0.035)	(0.017)
Rushed privatization before	-0.074	-0.007	0.008	0.024	-0.067	0.008
	(0.071)	(0.042)	(0.081)	(0.047)	(0.045)	(0.026)
Rushed privatization after	-0.172***	-0.138***	-0.331***	-0.316***	-0.286***	-0.245***
	(0.067)	(0.039)	(0.074)	(0.043)	(0.043)	(0.025)
Constant	4.637***	4.323***	9.823***	9.388***	6.206***	5.699***
	(0.245)	(0.166)	(0.015)	(0.010)	(0.009)	(0.005)
No of observations	13,488	13,488	13,488	13,488	20,015	20,017
R-squared	0.882	0.912	0.840	0.879	0.904	0.935
Number of firms	1927	1927	1927	1927	3338	3339
Number of privatized firms	1029	1029	1029	1029	1227	1227

Notes: fixed effects models, standard errors in parentheses,*** p<0.01, ** p<0.05, * p<0.1. -DID STATE denotes DID-PSM rebalancing for privatization, DID RUSH denotes DID-PSM with rebalancing for rushed privatizations, see Appendix D. Included controls for calendar year, before/after privatization dummy and the rush privatization dummy and their interactions. Constant included, not reported.

Table 2. Estimates of the effect of rushed and non-rushed privatization with privatization instrumented

	log VA - Cobb Douglass with sector specific slopes		log VA - No inputs		Employment	
	(1)	(2)	(3)	(4)	(5)	(6)
	DID	DID PSM	DID	DID PSM	DID	DID PSM
	STATE	RUSH	STATE	RUSH	STATE	RUSH
Non-rushed privatization before	-0.078	-0.095***	-0.160***	-0.146***	0.000	0.000
	(0.050)	(0.032)	(0.058)	(0.037)	(0.000)	(0.000)
Non-rushed privatization after	0.187***	0.146***	0.259***	0.193***	0.078***	0.132***
	(0.048)	(0.030)	(0.053)	(0.034)	(0.023)	(0.033)
Rush before	0.401	0.012	0.399	-0.168*	0.000	0.000
	(0.284)	(0.066)	(0.349)	(0.088)	(0.000)	(0.000)
Rush after	-0.020	0.005	-0.040	0.007	0.123***	0.070
	(0.051)	(0.025)	(0.057)	(0.029)	(0.044)	(0.052)
Rushed privatization before	-0.406	0.046	-0.497	0.071	-0.064	-0.044
	(0.290)	(0.078)	(0.356)	(0.100)	(0.054)	(0.072)
Rushed privatization after	-0.147**	-0.113***	-0.315***	-0.305***	-0.160***	-0.166**
	(0.064)	(0.038)	(0.071)	(0.044)	(0.052)	(0.068)
Constant	4.098***	4.209***	9.548***	9.519***	5.776***	5.805***
	(0.236)	(0.180)	(0.055)	(0.044)	(0.081)	(0.072)
Observations	16,691	16,691	16,691	16,691	20,015	20,017
Number of firms	3464	3464	3464	3464	3338	3339
Number of privatized firms	1007	1007	1007	1007	1227	1227

	Fist stage estimates					
	(1)	(2)	(3)	(4)	(5)	(6)
	DID	DID PSM	DID	DID PSM	DID	DID PSM
	STATE	RUSH	STATE	RUSH	STATE	RUSH
Political instrument	-0.004	-0.001	-0.004	-0.004	-0.019*	-0.013
	(0.009)	(0.013)	(0.013)	(0.009)	(0.011)	(0.014)
square	0.029***	0.001	0.001	0.029***	0.026***	0.000
	(0.003)	(0.005)	(0.005)	(0.003)	(0.005)	(0.005)
^3	0.000	0.000	0.000	0.000	0.001***	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
^4	-0.000***	0.000**	0.000*	-0.000***	-0.000***	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
FDI intensity	1.831***	1.081***	1.093***	1.831***	1.881***	1.196***
	(0.128)	(0.144)	(0.145)	(0.128)	(0.184)	(0.154)
SOE intensity	-0.621***	-0.625***	-0.625***	-0.621***	-0.579***	-0.620***
	(0.015)	(0.019)	(0.019)	(0.015)	(0.052)	(0.020)
square	0.066***	0.076***	0.076***	0.066***	0.060***	0.075***
	(0.002)	(0.003)	(0.003)	(0.002)	(0.007)	(0.003)
^3	-0.002***	-0.003***	-0.003***	-0.002***	-0.002***	-0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
^4	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.782***	1.288***	1.287***	0.782***	0.828***	1.294***
	(0.021)	(0.027)	(0.027)	(0.021)	(0.029)	(0.027)
art rho	0.243***	0.060	0.164***	0.243***	0.748*	0.367***
	(0.087)	(0.069)	(0.055)	(0.087)	(0.382)	(0.105)
ln sigma	-0.960***	-1.014***	-0.856***	-0.960***	-1.356***	-1.307***
	(0.018)	(0.023)	(0.022)	(0.018)	(0.103)	(0.029)
Observations	16,691	16,691	16,691	16,691	20,015	20,017
Number of firms	3464	3464	3464	3464	3338	3339
Number of privatized firms	1007	1007	1007	1007	1227	1227

Notes: instrument as in Bloom et al (2015), see Appendix D, fixed effects models, standard errors in parentheses,*** p<0.01, ** p<0.05, * p<0.1. -DID STATE denotes DID-PSM rebalancing for privatization, DID RUSH denotes DID-PSM with rebalancing for rushed privatizations, see Appendix D. Included controls for calendar year, before/after privatization dummy and the rush privatization dummy and their interactions. Constant included, not reported.

Table 3. Estimates of the effect of rushed and non-rushed privatization with privatization instrumented, over time, log value added as dependent variable

	No controls		Cobb-Douglass		Cobb-Douglas with sector specific slopes		Translog	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DID STATE	DID RUSH	DID STATE	DID RUSH	DID STATE	DID RUSH	DID STATE	DID RUSH
log K			0.125***	0.113***			0.396***	0.440***
			(0.011)	(0.019)			(0.054)	(0.097)
log L			0.706***	0.652***			1.121***	0.938***
			(0.029)	(0.043)			(0.119)	(0.171)
log L # log L							0.097***	0.125***
							(0.016)	(0.023)
log K # log L							-0.156***	-0.176***
							(0.016)	(0.025)
log K # log K							0.031***	0.035***
							(0.004)	(0.006)
Privaitzation	-0.722***	-0.782***	-0.362***	-0.469***	-0.315*	-0.177	-0.321***	-0.450***
	(0.105)	(0.132)	(0.085)	(0.104)	(0.164)	(0.201)	(0.090)	(0.113)
Rush years	1.637***	1.681***	0.837***	0.953***	0.627***	0.682***	0.602***	0.731***
	(0.119)	(0.127)	(0.084)	(0.096)	(0.134)	(0.180)	(0.085)	(0.098)
Privatization # rush # all years before	0.037	-0.027	0.026	-0.092	0.026	-0.088	0.024	-0.097
	(0.043)	(0.071)	(0.038)	(0.063)	(0.037)	(0.062)	(0.037)	(0.063)
Privatization # rush # 2 years after	-0.088**	-0.081	-0.007	-0.001	-0.002	0.001	-0.010	-0.001
	(0.043)	(0.068)	(0.040)	(0.066)	(0.039)	(0.065)	(0.039)	(0.065)
Privatization # rush # 3 years after	-0.282***	-0.296***	-0.128***	-0.166**	-0.121***	-0.170**	-0.130***	-0.159**
	(0.046)	(0.081)	(0.042)	(0.076)	(0.041)	(0.074)	(0.042)	(0.076)
Privatization # rush # 4 years after	-0.367***	-0.512***	-0.141***	-0.347***	-0.127***	-0.348***	-0.131***	-0.328***
	(0.053)	(0.079)	(0.049)	(0.077)	(0.049)	(0.076)	(0.049)	(0.077)
Privatization # rush # 4 years after	-0.511***	-0.514***	-0.197***	-0.283***	-0.193***	-0.261***	-0.194***	-0.260**
	(0.055)	(0.107)	(0.051)	(0.105)	(0.049)	(0.100)	(0.051)	(0.105)
Privatization # rush # 5 years after	-0.580***	-0.652***	-0.204***	-0.331***	-0.224***	-0.349***	-0.194***	-0.304***
	(0.067)	(0.119)	(0.059)	(0.106)	(0.056)	(0.098)	(0.057)	(0.106)
Constant	9.473***	9.525***	4.447***	4.889***	4.400***	4.704***	1.957***	2.417***
	(0.050)	(0.085)	(0.158)	(0.270)	(0.178)	(0.235)	(0.407)	(0.696)
No of observations	18,523	18,521	18,523	18,521	18,523	18,521	18,523	18,521
Number of firms	3092	3091	3092	3091	3092	3091	3092	3091
Number of privatized firms	1152	1152	1152	1152	1152	1152	1152	1152

Notes: fixed effects two-stage models with non-linear first-stage, standard errors in parentheses,*** p<0.01, ** p<0.05, * p<0.1. -DID STATE denotes DID-PSM rebalancing for privatization, DID RUSH denotes DID-PSM with rebalancing for rushed privatizations, see Appendix C. Included controls for calendar year, before/after privatization dummy and the rush privatization dummy and their interactions. Constant included, not reported. instrument as in Bloom et al (2015), see Appendix E for detailed statistics of the first stage, see Table D1.

Table 4. Estimates of the effect of rushed and non-rushed privatization with privatization instrumented, domestic privatizations only, log value added as dependent variable

	Cobb Douglass with sector specific slopes (1)	No inputs (2)	Employment (3)
Non-rushed privatization before	-0.033	-0.107**	-0.010
	(0.042)	(0.048)	(0.016)
Non-rushed privatization after	0.192***	0.281***	0.065***
	(0.036)	(0.039)	(0.013)
Rush before	0.008	0.137***	-0.094***
	(0.030)	(0.036)	(0.011)
Rush after	-0.025	-0.233***	0.036**
	(0.020)	(0.022)	(0.018)
Rushed privatization before	-0.077	-0.005	0.036**
	(0.064)	(0.072)	(0.018)
Rushed privatization after	-0.180***	-0.380***	0.037**
	(0.055)	(0.059)	(0.015)
Constant	4.552***	9.546***	5.653***
	(0.212)	(0.053)	(0.075)
Observations	16,899	16,899	17,086
Number of firms	2868	2868	3002
Number of privatized firms	948	948	960

Notes: fixed effects two-stage models with non-linear first-stage, standard errors in parentheses,*** p<0.01, ** p<0.05, * p<0.1, DID-PSM rebalancing for privatization, see Appendix C. Included controls for calendar year, before/after privatization dummy and the rush privatization dummy and their interactions. Constant included, not reported. instrument as in Bloom et al (2015), see Appendix E for detailed statistics of the first stage, see Table D1.

Appendices

Appendix A. Descriptive Statistics

Table A1: Descriptive statistics

	Privatized SOEs				Non-privatized SOEs				t-test	
	mean	max	min	st. dev.	median	max	min	st. dev.		
Return on assets	-0.9%	145.2%	-277.4%	19.3%	-4.1%	287.4%	-297.4%	23.0%	-10.3	***
ln($assets$)	10.6	17.6	2.6	1.6	9.5	17.4	3.0	1.8	-57.0	***
ln(k)	9.6	17.1	1.1	1.8	8.9	17.3	-2.0	2.0	-31.2	***
ln($employment$)	5.6	11.0	0.0	1.1	5.0	12.5	0.0	1.1	-33.1	***
K/L ratio	183.6	29152.1	0.0	893.2	163.3	130444.9	0.0	1014.4	-2.7	***
ln($value\ added$)	9.7	16.6	2.6	1.4	8.8	16.0	-1.3	1.4	-50.1	***

Note: Welch (1947) mean's equality test between privatized and private incumbents "randomized" for the analysis, *** represent difference significant at 1%, 5% and 10% levels, respectively.

Table A2: Before - after comparison

Variable	Period	Privatized	SOEs	z-statistic
Return on assets	t-2	0.95%	1.71%	(3.13) ***
	t=0	0.82%	1.79%	(5.26) ***
	t+2	0.73%	1.54%	(3.59) ***
Return on sales	t-2	0.88%	1.49%	(2.16) ***
	t=0	0.77%	1.48%	(4.41) ***
	t+2	0.72%	0.99%	(1.94) ***
K/L ratio	t-2	37.09	30.44	3.21 ***
	t=0	40.31	32.70	3.96 ***
	t+2	48.34	36.98	3.99 ***
ln (L)	t-2	5.24	5.62	(6.43)
	t=0	5.14	5.49	(7.35) ***
	t+2	5.09	5.37	(6.99) *
ln(K)	t-2	9.20	9.27	(3.83) *
	t=0	9.12	9.31	(5.57) *
	t+2	9.13	9.21	(3.29) ***
Job creation	t-2	3.47%	4.03%	(1.07)
	t=0	3.41%	4.91%	(1.87)
	t+2	3.25%	4.41%	(1.14) ***
Job destruction	t-2	3.49%	3.70%	(1.20)
	t=0	3.41%	3.73%	(1.51)
	t+2	3.28%	4.38%	(2.45) ***

Note: Wilcoxon (1945) median's equality test between privatized and private incumbents "randomized" for the analysis, *** represent difference significant at 1%, 5% and 10% levels, respectively. Before-after changes correspond to a three year compound change (a year before event to a year after event). The year of t=0 is randomly assigned to SOEs, following the annual intensity of privatizations (e.g. if in a given year a probability to be privatized was 10%, a non-privatized SOE has the commensurate probability to be assigned this year as its t=0 for the purpose of this comparison). Job creation measure averages the positive changes in employment in a given period, relative to the previous year, in percent. Job destruction does the same for the negative changes in employment.

Appendix B. Definition of ownership

Census data, provided by Central Statistical Office (CSO), provide classification of the ownership form, but do not provide detailed information on ownership structure. Hence, we rely on the CSO classification to identify the events of privatization. The categories which we classify as private ownership include majority domestic private ownership, majority private ownership with majority domestic ownership, majority private ownership with minority domestic ownership.

The categories which we classify as state ownership include: majority ownership by treasury, majority ownership by state legal entities and majority ownership by communal authorities. In the case of some publicly traded companies, after an initial public offering in the stock exchange (IPO), the treasury or another state legal entity retained the controlling package of shares until later divestment. These cases are classified as majority state ownership and hence privatization occurs in our data when the state becomes a minority shareholder or stops being shareholder at all. So long as the treasury or another state legal entity had control over the vote (through majority ownership or privileged stocks in voting), the firm remains an SOE in our sample. It would take another public offering or non-public divestment (e.g. through a privatization agreement with a legal entity) for the firm to become majority private. However, if the IPO concerned majority ownership (and/or majority vote), then the IPO is equivalent to privatization. Our ownership identification satisfies this criterion.

In some sense, this definition of privatization may be controversial. After all, once a company becomes publicly traded, all its financial record become public information. However, the decision-making is still based on shareholding majority, which implies that the state is able to control the enterprise despite it being publicly traded. Hence, we consider the timing of majority divestment as the timing of privatization so as to be internally consistent with the research question we analyze.

Our data are anonymized, which means that we cannot attempt to identify the known cases or privatization, e.g. for stock-listed companies.

As in other transition countries, Poland launched mass privatization through a voucher privatization program (VPP)[20]. The implementation of VPP coincides with one of our rushed privatization years (1997). National Investment Funds (NIFs) became tradable and citizens received their vouchers in 1997. However, this coincidence has no bearing on our data – a large number of privatizations in 1997 had no connection to the VPP.

1. NIFs were legally a state public entity. Hence, transfer of ownership from Treasury to NIFs would not signify change of ownership from public to private in our data.
2. NIFs obtained typically between 1% and at the most 3% of assets. Hence, any change in ownership in the aftermath of VPP was minor and unlikely to signify a privatization in our data. Of the 512 companies included in VPP[21], only 35 had 33% of their shares transferred into the investment fund. The remaining 477 companies had a minority status in the funds, amounting to around 1.9% of their shares. The 35 companies with one-third of all their stock in one of the National Investment Funds were stipulated to be sold as a whole (meaning all the shares of the company fund in the investment fund had to be sold to one investor).
3. According to a 2006 report by the Ministry of Treasury, of the original 512 companies, in 2000s 135 firms were still majority state owned and 130 firms were eventually liquidated without privatization due to poor performance. In fact, only 232 firms, i.e. less than half of the firms, was privatized at all by 2005.

[20] The mass privatization schemes in the Czech Republic and Russia were referred to as voucher privatization. The Polish case cannot be defined similarly. In the Russian and Czech case, all eligible citizens could purchase a voucher, which was in essence a coupon for a certain amount of bids. These vouchers could be used to bid for shares of companies involved in the program. The Polish case did not give citizens a choice in investment, nor did it ask them to incur a cost to be allocated a share certificate. In fact, all adult citizens were eligible to collect a certificate free of charge and either hold it or immediately sell it. Subsequently, vouchers were converted into holdings in NFIs, with fixed proportions of each NFI and only afterwards owners could trade according to preference.

[21] The list of firms to be included on VPP was completed by a Prime Minister decree from 1993 and a subsequent one from 1994.

Table B1. The ownership structure in the sample

Year	SOEs	Private
1995	3,755	8,109
1996	3,516	9,138
1997	2,824	10,427
1998	2,602	11,360
1999	2,412	11,403
2000	2,198	11,958
2001	1,936	11,336
2002	1,757	10,992
2003	1,594	11,349
2004	1,449	11,826
2005	1,315	12,329
2006	1,242	12,780
2007	1,192	13,633
2008	1,133	13,948
2009	1,244	15,147
2010	1,182	14,941
2011	1,142	14,667
2012	1,088	14,735
2013	1,080	14,579

Note: data comes from CSO census of all firms employing at least 9 workers in full-time equivalents.

Appendix C. Sample atrophy, new firms and identification of privatization

Similar to other census sources, firms may disappear from our sample for a variety of reasons. First, their employment may fall short of the 50 FTE employee threshold. We are able to address this effect, because the full census data at our disposal cover firms with 9 or more FTE employees (reporting on important financial information is more narrow in the 9+ sample, which prevents us from utilizing it in the analysis). Hence, we actually trace all firms balancing on the threshold and thus we are able to identify if they get privatized in the unobserved period(s).

Second, some firms may actually go bankrupt, with another entity purchasing their assets and re-establishing a similar activity under a new registration number. Such new entries could be privatizations in disguise if privatization through bankruptcy was the preferred option by the state and the investor. This issue cannot be addressed, as the former registration number is not reported with the new entity. However, it appears that this phenomenon was relatively rare. First, the legislation prevented bankruptcy, introducing a legal vehicle named "liquidational privatization", which introduces protection from creditors on par with bankruptcy, but allows to preserve all the licenses, permits etc. Second, the actual bankruptcy required approval from a longer chain of command (regional authority, central authority and prime minister, and finally the registry court), whereas the "liquidational privatization" could be concluded with the single authority in charge of a given SOE and the registry court.

As a consequence of this legal situation, exits of SOEs have on average been much rarer than privatizations, see Table C1. Moreover, entries are substantially larger than privatizations and exhibit independent time dynamics in the period of our study (see Table C1). Hence, it does not appear that our results are substantially affected by unobserved privatizations in the guise of bankruptcy.

Table C1. The sample atrophy, exits of SOEs and privatizations

Year	No of SOEs	No of Privatizations	No of Exits of SOEs	No of Entries
1995	4,326	-	377	-
1996	4,098	65	311	1722
1997	3,359	538	227	1568
1998	3,137	96	193	1649
1999	2,554	39	187	1327
2000	2,361	54	169	1801
2001	2,067	114	89	1153
2002	1,875	76	119	943
2003	1,694	66	98	1339
2004	1,500	69	91	1373
2005	1,359	76	55	1282
2006	1,281	24	44	1289
2007	1,219	18	59	1571
2008	1,156	41	61	1465
2009	1,281	43	43	2405
2010	1,214	38	33	969
2011	1,178	35	40	1014
2012	1,112	35	31	1218
2013	1,104	34	-	963

Note: data comes from CSO census of all firms employing at least 9 workers in full-time equivalents.

Appendix D. Instrument

Bloom et al (2016) analyze the effects of closing hospitals on a variety of health and economic outcomes in the UK. They posit that in marginal constituencies, where the government party is at risk of losing a seat, hospitals are less likely to be closed and they find empirical support for this claim. Following Bloom et al (2016) we construct an instrument that identifies if a party in government is likely to lose a seat in a given voting district.

Unlike the UK, Poland has proportional voting system. The constituencies are voting districts, characterized by certain number of seats, proportional to the population. The number of seats is defined in the legislation and does not change, i.e. it does not depend on total number of votes, voter turnout, etc. Similar to many other countries with the proportional voting system, the so-called d'Hondt rule defines the number of seats for every party, conditional on the total number of seats in a given district. The only needed information is the total proportion of votes.[22] We use opinion polls from CBOS, who provides the longest history of the opinion polls (1991 onwards). Monthly polls were averaged for every year.

Over the analyzed periods there were two relevant changes to the voting mechanism. First, until 2001 there was a so-called national list. This list comprised 50 seats (in 460 seats parliament) and allocated seats between parties according to the d'Hondt rule. After 2001 the national list was no longer in place and all seats were allocated to the voting districts. Second, there was one change in the delineation of the voting districts, subsequent the administrative reform of 1997. To address the first issue, we treat the national list as additional voting district, without a regional assignment. To address the second issue we redefine the voting districts prior to 1997 to match the delineation post 1997. When needed, smaller voting districts were

[22] Wikipedia provides an illustrative example, which explains how votes are translated to number of seats for every party. https://en.wikipedia.org/wiki/D%27Hondt_method

collapsed. Note that given the d'Hondt rule, this procedure does not affect the number of votes for each party for a given distribution of votes.

In sum, we obtain a variable, which takes the value of 0 if a party in government is not at risk of losing a seat in a given voting district and has positive values otherwise. The expression "at risk of losing a seat" signifies the following conditionality: if the elections were held in a given year and the actual turnout was the same as in polls, a current coalition party would lose a seat in a given voting district. The voting districts are matched to the region of firms operation. Table D1 reports the performance of the instrument.

Table D1. The first stage

	Fist stage estimates					
	(1)	(2)	(3)	(4)	(5)	(6)
	DID	DID PSM	DID	DID PSM	DID	DID PSM
	STATE	RUSH	STATE	RUSH	STATE	RUSH
Political instrument	-0.004	-0.001	-0.004	-0.004	-0.019*	-0.013
	(0.009)	(0.013)	(0.013)	(0.009)	(0.011)	(0.014)
square	0.029***	0.001	0.001	0.029***	0.026***	0.000
	(0.003)	(0.005)	(0.005)	(0.003)	(0.005)	(0.005)
^3	0.000	0.000	0.000	0.000	0.001***	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
^4	-0.000***	0.000**	0.000*	-0.000***	-0.000***	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
FDI intensity	1.831***	1.081***	1.093***	1.831***	1.881***	1.196***
	(0.128)	(0.144)	(0.145)	(0.128)	(0.184)	(0.154)
SOE intensity	-0.621***	-0.625***	-0.625***	-0.621***	-0.579***	-0.620***
	(0.015)	(0.019)	(0.019)	(0.015)	(0.052)	(0.020)
square	0.066***	0.076***	0.076***	0.066***	0.060***	0.075***
	(0.002)	(0.003)	(0.003)	(0.002)	(0.007)	(0.003)
^3	-0.002***	-0.003***	-0.003***	-0.002***	-0.002***	-0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
^4	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.782***	1.288***	1.287***	0.782***	0.828***	1.294***
	(0.021)	(0.027)	(0.027)	(0.021)	(0.029)	(0.027)
art rho	0.243***	0.060	0.164***	0.243***	0.748*	0.367***
	(0.087)	(0.069)	(0.055)	(0.087)	(0.382)	(0.105)
ln sigma	-0.960***	-1.014***	-0.856***	-0.960***	-1.356***	-1.307***
	(0.018)	(0.023)	(0.022)	(0.018)	(0.103)	(0.029)
Observations	16,691	16,691	16,691	16,691	20,015	20,017
Number of firms	3464	3464	3464	3464	3338	3339
Number of privatized firms	1007	1007	1007	1007	1227	1227

Note. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The explained variable takes the value of 1 if in a given year and all subsequent years firm is privatized and 0 otherwise.

Table D1. The first stage for privatizations to domestic investors only

	Fist stage estimates		
	(1)	(2)	(3)
Political instrument	0.002	0.005	-0.004
	(0.013)	(0.013)	(0.008)
square	-0.000	-0.000	0.034***
	(0.005)	(0.005)	(0.003)
^3	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)
^4	0.000**	0.000**	-0.000***
	(0.000)	(0.000)	(0.000)
FDI intensity	0.022	0.002	1.014***
	(0.190)	(0.189)	(0.141)
SOE intensity	-0.458***	-0.458***	-0.472***
	(0.021)	(0.021)	(0.010)
square	0.047***	0.047***	0.040***
	(0.003)	(0.003)	(0.001)
^3	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)
^4	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)
Constant	1.102***	1.103***	0.497***
	(0.029)	(0.029)	(0.020)
art rho	0.124**	0.013	0.224***
	(0.062)	(0.059)	(0.040)
ln sigma	-0.850***	-1.024***	-1.487***
	(0.022)	(0.024)	(0.007)
Observations	16,899	16,899	17,086
Number of firms	2868	2868	3002
Number of privatized firms	948	948	960

Note. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. DID-PSM rebalancing for privatization, see Appendix C. The explained variable takes the value of 1 if in a given year and all subsequent years firm is privatized and 0 otherwise.