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# How to Use Natural Experiments to Estimate Misallocation†

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*We propose a method to estimate the effect of firm policies (e.g., bankruptcy laws) on allocative efficiency using (quasi-)experimental evidence. Our approach takes general equilibrium effects into account and requires neither a structural estimation nor a precise assumption on how the experiment affects firms. Our aggregation formula relies on treatment effects of the policy on the distribution of output-to-capital ratios, which are easily estimated. We show this method is valid for a large class of commonly used models in macrofinance. We apply it to the French banking deregulation episode of the mid-1980s and find an increase in aggregate TFP of 5 percent.* (*JEL* G21, G24, G28, G31, G32, H25)

The misallocation of resources is a central question in economics. Starting with Restuccia and Rogerson (2008) and Hsieh and Klenow (2009), the literature measures equilibrium misallocation by estimating the cross-sectional dispersion of marginal products across firms. This approach suffers from several well-known limitations. For instance, measurement errors in inputs, or adjustment costs, can generate productivity dispersion without resource misallocation (Asker, Collard-Wexler, and Loecker 2014; Bils, Klenow, and Ruane 2020; Gollin and Udry 2021). Also, the *policy relevance* of such misallocation measures is questionable. Misallocation is typically measured relative to a frictionless benchmark, which can be difficult to achieve in practice. Finally, this approach is mostly silent on the particular frictions that generate misallocation and the potential policies that may improve allocative efficiency.

In parallel, a large literature in applied microeconomics exploits (quasi-) experimental settings to estimate the causal effect on firm-level outcomes of economic policies such as financial deregulation, bankruptcy reform, banking regulation, or

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reductions in corporate taxation.[1] These policies are designed to alleviate firm-level frictions, so they should reduce misallocation in the economy. While this empirical literature uses these experiments to measure their effect on firm growth and investment, it does not *quantify* how they affect aggregate allocative efficiency.

Our paper bridges these two approaches. We offer a method to measure allocative efficiency in a (quasi-) experimental settings. This method works as follows. An econometrician observes firm-level data in an economy where a (quasi-) natural experiment has taken place. This experiment changes the set of frictions faced by *treated* firms while leaving control firms unaffected. Under the appropriate identifying assumption, the econometrician can estimate the causal effect of the experiment on firm-level outcomes, using classic difference-in-difference estimators. Standard policy evaluations typically estimate treatment effects on firm size or employment. However, these treatment effects alone cannot speak to allocative efficiency. To do so, we show that the econometrician needs to estimate treatment effects on the distribution of log marginal products of capital (lMRPKs). These estimates can then be injected in a simple aggregation formula to answer two simple questions: (i) how much did the actual policy change contribute to changes in aggregate efficiency (ex post evaluation)? (ii) how would aggregate efficiency have changed if the policy had been extended to all firms in the economy (scale-up)?

Beyond its simplicity, our method has multiple advantages. First, it deals with measurement error and real frictions (by comparing treated and control firms). Second, it is policy-relevant (the experiment has been implemented in practice). Third, our method does not require the potentially strenuous estimation of a structural model of firm behavior, although it is consistent with most models of firm dynamics used in the literature. Finally, our method allows empiricists to avoid specifying exactly how the experiment affects firms, as long as firms' behaviors are within a (large) class of models.[2]

This method, though intuitive and simple, presents conceptual challenges, that we describe in Section I using a simplified version of our baseline model. Conceptually, these challenges arise because we measure treatment effects in the experimental data, but our aggregation formula requires treatment effects in unobserved, counterfactual economies. Consider for instance the case of an econometrician wishing to measure the effect of the experiment on aggregate TFP. She faces two obstacles. First, the experiment affects the average firm in the sample through general equilibrium. This general equilibrium effect is differenced out—and therefore unknown—in a difference-in-difference setting. Second, the aggregation formula relies on estimating treatment effects in a counterfactual world where only the reform takes place. In practice, however, additional shocks may have taken place coincidentally, e.g, a shock to average firm productivity. A priori, nothing guarantees that the treatment effects estimated in the actual data apply to the counterfactual economy, i.e., that they are externally valid.

[1] See the references in the literature review below.

[2] One downside of our framework, relative to a more structural approach, is that it does not quantify the relative importance of various frictions, and, as such, remains silent on the welfare implications of the policy under consideration.

A similar external validity issue arises when the econometrician endeavors to measure how aggregate efficiency would change if the policy was extended to all firms in the economy. All the econometrician can do is to measure the effect of the policy change when the policy is not at scale. However, scaling up the experiment will result in changes in equilibrium conditions. For instance, it may lead to a wage increase, and firms may respond differently to the policy treatment when the labor market is tighter. There again, nothing guarantees that the estimated treatment effects in the real data can be used in a counterfactual exercise where general equilibrium conditions have changed.

While these two obstacles are real, our paper provides a broad set of conditions under which they can be safely ignored. Section II shows that under broad conditions, applicable in most macroeconomic models with heterogeneous firms, the distribution of MRPKs is independent of general equilibrium conditions. This is our main Theorem 1. As we show, this invariance relies crucially on two key assumptions about technology and frictions. First, the sources of distortions (financing frictions and constraints, tax schedules, adjustment costs) are assumed to be homogeneous of degree one. Intuitively, homogeneity guarantees that frictions remain on average constant on a size-adjusted basis. Hence, a change in general equilibrium, which affects firms' size, will not affect the relative distribution of distortions. Second, firm-level production is Cobb-Douglas, with either constant or decreasing returns to scale. While these assumptions may appear restrictive, they are almost always satisfied in the structural macrofinance literature (see our extensive review of the literature in Table 1). As such, our sufficient statistics approach provides a valid alternative to structural estimation in the context of these models.

To close the analysis and provide aggregation formulas, all we need is an aggregation model that details how industries interact in equilibrium through product and labor markets. In the main text below, we provide aggregation formulas using the aggregation model of Hsieh and Klenow (2009), which has become a benchmark in the literature. These formulas show how to combine aggregation parameters with the difference-in-difference estimates of three key moments of the distribution of log-MRPK. The formulas are intuitive as we explain in Section IID. Importantly, our methodology easily extends to more complex market structures and additional sources of heterogeneity in technology. In online Appendix A.A7, we extend the baseline formulas to roundabout production and input-output linkages. In online Appendix A.A8, we explore an extension of Hsieh and Klenow (2009) with variable industry output shares.

The paper concludes with an application of our method to a specific large-scale experiment: the French banking deregulation episode of the 1980s. Prior to this reform, the French banking sector was heavily controlled by the government, which fixed prices and quantities in the loan market, while channeling loans to priority industries. Even in the private sector, the profit motive was largely absent and competition was limited. The reform, implemented in the mid-1980s, organized the rapid transition of the industry into a more classically decentralized, competitive sector. Using a difference-in-difference analysis, Bertrand, Schoar, and Thesmar (2007) qualitatively show that the reform led to a significant increase in capital reallocation across firms. We complement their analysis by providing a precise quantification of the resulting aggregate TFP gains. Using a similar identification strategy, our

Table 1—Select Literature Review

| Paper | Production function: Cobb-Douglas (1) | Adjustment costs: homogeneous (2) | Borrowing constraint: homogeneous (3) | Equity issuance: homogeneous (4) | Tax schedule: homogeneous (5) |
|---|---|---|---|---|---|
| *Panel A. Adjustment cost papers* | | | | | |
| Asker, Collard-Wexler, and Loecker (2014) | Y | Y | – | – | – |
| Bartelsman, Haltiwanger, and Scarpetta (2013) | Y* | Y | – | – | – |
| Bloom (2009) | Y | Y | – | – | – |
| Bloom, Bond, and Van Reenen (2007) | Y | Y | – | – | – |
| Bloom et al. (2018) | Y | Y | – | – | – |
| Cooper and Haltiwanger (2006) | Y | Y | – | – | – |
| Gourio and Rudanko (2014) | Y | Y | – | – | – |
| Hall (2004) | Y | Y | – | – | – |
| Peters and Taylor (2017) | Y | Y | – | – | – |
| *Panel B. Structural and dynamic corporate finance* | | | | | |
| Bolton, Chen, and Wang (2011) | Y | Y | – | Y | – |
| Bolton, Chen, and Wang (2013) | Y | Y | – | Y | – |
| Cummins, Hassett, and Oliner (2006) | Y | Y | – | – | – |
| DeAngelo, DeAngelo, and Whited (2011) | Y | Y | N | N | Y |
| Gamba and Triantis (2008) | Y* | – | Y | Y | N |
| Gomes and Schmid (2010) | Y* | N | Y | N | Y |
| Hackbarth and Mauer (2011) | – | – | Y | N | Y |
| Hennessy and Whited (2005) | Y | Y | Y | Y | N |
| Hennessy and Whited (2007) | Y | – | Y | N | Y |
| Hennessy, Levy, and Whited (2007) | Y | Y | Y | Y | – |
| Li, Livdan, and Zhang (2009) | Y* | Y | – | N | – |
| Liu, Whited, and Zhang (2009) | Y | Y | – | – | – |
| Livdan, Sapriza, and Zhang (2009) | Y* | Y | Y | N | – |
| Miao (2005) | Y | – | Y | – | Y |
| Michaels, Beau Page, and Whited (2018) | Y | Y | Y | Y | – |
| Nikolov and Whited (2014) | Y | Y | Y | Y | Y |
| Riddick and Whited (2009) | Y | Y | – | N | Y |
| Whited and Wu (2006) | Y | Y | N | N | – |

(*Continued*)

aggregation formula implies that the French banking deregulation episode led to a 5.0 percent increase in TFP over the sample period, which corresponds to about one-half of the total aggregate TFP gains in France over this period.[3]

Our paper offers a novel way to quantify the extent of misallocation in the data. Our approach departs from the misallocation literature in three ways. First, we build on the applied microeconomic literature that produces well-identified evidence on the effect of economic policies on firm-level outcomes. A first strand of papers evaluates the effect of financial reforms (see for instance Aghion, Fally, and Scarpetta 2007; Bertrand, Schoar, and Thesmar 2007; Ponticelli and Alencar 2016; Larrain and Stumpner 2017). Others analyze firms' response to the availability of subsidized credit (e.g., Lelarge, Sraer, and Thesmar 2010; Banerjee and Duflo 2014; Brown and Earle 2017), or changes in monetary policy and prudential regulation (Fraisse, Lê, and Thesmar 2020; Blattner, Farinha, and Rebelo 2017). The effect of capital taxes or subsidies on firm investment and hiring is the focus of Yagan (2015); Zwick

[3] Average TFP in France from 1985 to 1992 (or post-experiment sample) is about 8.8 percent higher than between 1976 to 1983 (our pre-experiment sample). See https://fred.stlouisfed.org/series/RTFPNAFRA632NRUG.

Table 1—Select Literature Review (Continued)

| Paper | Production function: Cobb-Douglas (1) | Adjustment costs: homogeneous (2) | Borrowing constraint: homogeneous (3) | Equity issuance: homogeneous (4) | Tax schedule: homogeneous (5) |
|---|---|---|---|---|---|
| *Panel C. Macrofinance with heterogeneous firms* | | | | | |
| Arellano, Bai, and Zhang (2012) | Y | Y | Y | Y | – |
| Buera and Shin (2013) | Y | – | Y | – | Y |
| Buera and Moll (2015) | Y | – | Y | – | – |
| Cooley and Quadrini (2001) | Y | – | Y | Y | – |
| Fernández-Villaverde et al. (2011) | Y | Y | – | – | – |
| Gilchrist, Sim, and Zakrajšek (2014b) | Y | Y | Y | Y | – |
| Gomes (2001) | Y | Y | Y | N | – |
| Gopinath et al. (2017) | Y | Y | N | – | – |
| Gourio (2012) | Y | Y | – | – | – |
| Gourio and Miao (2010) | Y | Y | – | Y | Y |
| Itskhoki and Moll (2019) | Y | – | Y | – | Y |
| Khan and Thomas (2008) | Y | Y | – | – | – |
| Khan and Thomas (2013) | Y | Y | Y | Y | – |
| Liu, Wang, and Zha (2013) | Y | Y | Y | – | – |
| Midrigan and Xu (2014) | Y | Y | Y | Y | – |
| Moll (2014) | Y | – | Y | – | – |
| Percent in line with our assumptions in all 44 papers | 98 | 98 | 93 | 79 | 95 |
| Percent in line with our assumptions in papers that have the economic force | 82 | 97 | 88 | 59 | 85 |

*Notes:* This table checks the validity of our assumption in a select review of 44 recent papers from the literature on firm dynamics. We restrict ourselves to all papers citing Hennessy and Whited (2007); Midrigan and Xu (2014); and Moll (2014), published either in the *American Economic Review, Econometrica, Journal of Political Economy, Review of Economic Studies, Quarterly Journal of Economics, Journal of Finance, Journal of Financial Economics, Review of Financial Studies*, one of the three *American Economic Journal*s, or *Journal of Monetary Economics*. To further restrict the scope of the review, we asked that the papers have at least 50 Google scholar citations. We end up with 44 papers, which we classify in three broad strands of literature: "adjustment cost papers," in which adjustment costs are the only friction, papers using dynamic corporate finance models (some of them corresponding to structural estimate, some of them being pure theoretical contributions) and macrofinance papers with financing frictions as well as competitive equilibrium modeling. For each of these papers, we then report if our core assumptions are satisfied: Cobb-Douglas production, and homogeneity of taxes, financing and real frictions. We report the results in columns 1–5. *Y* means that the assumption is satisfied, *N* that it is not. $-$ means that there is no such force in the model (so that our assumption is by default satisfied). $Y^*$ means that the production function is indeed Cobb-Douglas, but the technology also includes *nonscalable fixed costs* of production. In the bottom two lines of the Table, we report the percent of papers for which the assumption is satisfied. In the penultimate line, the percent is computed among all papers. In the last line, it is computed only *among papers that have the force being in the model*. Hence, in column 3, 88 percent of the papers that have borrowing constraints satisfy the assumptions of Theorem 1, but this corresponds to 93 percent of the papers.

and Mahon (2017); Giroud and Rauh (2019; or Rotemberg (2019). These papers provide important causal evidence on the role of frictions for firm-level outcomes. Our methodology allows evaluating whether the policies investigated in these papers have a significant effect on allocative efficiency. In this sense, our paper offers a way to measure the importance of misallocation in the data by measuring potential reallocation gains from policies actually implemented in the real world.

Our second departure relative to the misallocation literature is that we allow capital wedges to arise from a large class of firm dynamics models. In a seminal paper building on Restuccia and Rogerson (2008), Hsieh and Klenow (2009) show how to compute TFP losses due to misallocation of inputs using a simple sufficient statistics approach. Hsieh and Klenow (2009), however, abstract from the origin of distortions and treat distortions—wedges between the marginal revenue product of factors and their cost—as primitives of the model. Similarly, Baqaee and Farhi (2020) derive a nonparametric formula for aggregating microeconomic shocks in general equilibrium economies with distortions, but, in the spirit of Hsieh and Klenow (2009), they assume distortions can be represented by exogenous wedges. In contrast, our approach allows for endogenous capital wedges but shows that, for a large class of firm dynamics models, the distribution of these endogenous wedges is invariant to macroeconomic conditions. Put differently, we show that it is correct, in a large class of models, to consider the wedge distribution as exogenous to equilibrium conditions.

We are, of course, not the first to offer a quantification of the role of specific frictions or policies on allocative efficiency. Most contributions in the macroeconomic literature, however, rely on structural estimation: researchers start from a particular general equilibrium; firms in the model face frictions when optimizing inputs; the model is usually calibrated using generic moments from macroeconomic and firm-level data; the role of policies or frictions is then analyzed through simulations of counterfactual economies. In this spirit, Asker, Collard-Wexler, and Loecker (2014) look at the contribution of capital adjustment costs on standard misallocation measures. Buera, Kaboski, and Shin (2011); Midrigan and Xu (2014); Buera, Kaboski, and Shin (2012) and Catherine et al. (2022) study the effect of financial frictions on allocative efficiency. Edmond, Midrigan, and Xu (2018) analyze the welfare losses from heterogeneous markups. David, Hopenhayn, and Venkateswaran (2016) and David and Venkateswaran (2019) focus on the effect of information frictions on aggregate TFP. A limitation of this approach, besides its technical complexity, is that it forces the researcher to specify the nature of frictions faced by firms (cash or collateral constraints, quadratic or linear adjustment costs, etc.) and to model explicitly the policy being studied. In contrast, our approach does not require the estimation of a structural model.

The rest of the paper proceeds as follows. Section I describes, in a simplified model, the method to estimate allocative effects from (quasi-) experimental evidence and presents, in the context of this simple model, the key challenge to this methodology. Section II shows the validity of this methodology in common models used in macrofinance; we then provide a simple formula mapping reduced-form estimates from (quasi-) experimental data to aggregate TFP using a simple aggregation model. The online Appendix contains alternative aggregation formulas that entertain a more complex market structure and additional heterogeneity in technology and demand. Section III applies the method in the context of the French banking deregulation episode. Section IV concludes.

## I. Estimating Misallocation in Experimental Data: Challenges

This section uses a simplified framework to illustrate the difficulty in drawing inference about misallocation from (quasi-) experimental data. Details of the proofs are in online Appendix A.A1.

### A. *Setup*

We use a simplified version of the aggregation framework in Hsieh and Klenow (2009). Consider an initial steady-state economy with a continuum of monopolies indexed by $i \in [0;1]$. Each monopoly supplies an intermediate good to a competitive final goods sector that combines them with constant elasticity of substitution (CES) technology with an elasticity of substitution (ES) $\frac{1}{1-\theta}$. Production takes place with a Cobb-Douglas function with constant returns to scale and a capital share $\alpha$. $z_i$ is firm i's log TFP. These assumptions imply that firm's i revenue is $p_i y_i = Y^{1-\theta} e^{\theta z_i}\left(k_i^{\alpha} l_i^{1-\alpha}\right)^{\theta}$, where $Y$ is aggregate output, whose price is normalized to 1. We call $\mathcal{Z}_0 = \left(\int_i e^{\frac{\theta}{1-\theta} z_i} di\right)^{\frac{1-\theta}{\theta}}$ the aggregate TFP in the absence of distortions. Firms face frictions when optimizing their capital stock. We denote $\Theta_0$ the structural parameters governing these frictions. Because of these frictions, firms' marginal revenue product of capital (MRPK), $\alpha\theta . \frac{p_i y_i}{k_i}$, differs from the user cost of capital. We assume that MRPKs are log-normally distributed. There is no distortion in the labor market. Aggregate TFP is then

$$\log TFP_0 = \underbrace{\log(\mathcal{Z}_0)}_{\text{technology}} - \underbrace{\frac{\alpha}{2}\left(1 + \frac{\alpha\theta}{1-\theta}\right)\operatorname{var}\left(\log\frac{p_i y_i}{k_i} | \Theta_0, \mathcal{Z}_0\right)}_{\text{misallocation}}. \tag{1}$$

This equation conditions the distribution of $\frac{p_i y_i}{k_i}$ to the structural parameters governing frictions in optimizing inputs, $\Theta_0$ and aggregate productivity, $\mathcal{Z}_0$.[4] In this economy, these parameters pin down equilibrium prices, which may, in turn, affect $\frac{p_i y_i}{k_i}$. Note also that this equation does not impose structure on the capital frictions driving the distribution of MRPKs. In particular, we do *not* assume here that MRPKs are exogenous; in this paper, they are not.

## B. *Introducing an Experiment*

A policy experiment takes place in this economy. To simplify exposition, assume that a random half of the firms (the treatment group, T) receives a treatment that modifies the set of frictions they face: $\Theta^T$ goes from $\Theta_0$ to $\Theta_1$. Frictions for the remaining firms (the control group, C) remain unchanged at $\Theta^C = \Theta_0$. The new steady state is also possibly affected by an aggregate TFP shock that may coincide with the policy experiment (e.g., a shift to $z_i$ so that $\mathcal{Z}_0 \rightarrow \mathcal{Z}_1$). As we show in our main analysis, our results also hold in the presence of additional confounding shocks (e.g., aggregate labor supply shocks or aggregate shocks to $\Theta_0$).

Given equation (1), aggregate TFP in this new steady state is

$$\log TFP_1 = \log(\mathcal{Z}_1) - \frac{\alpha}{2}\left(1 + \frac{\alpha\theta}{1-\theta}\right)\operatorname{var}\left(\log\frac{p_i y_i}{k_i} | \begin{pmatrix} \Theta^{\mathrm{C}} = \Theta_0 \\ \Theta^{\mathrm{T}} = \Theta_1 \end{pmatrix}, \mathcal{Z}_1\right)$$

[4] We focus on shocks to aggregate TFP to simplify exposition in this section only. Our results carry out for more general shocks on the distribution of $z$, provided these shocks are orthogonal to the policy treatment.

We are interested in two policy-relevant measures of the effect of this policy change on misallocation:

(i) **Ex post evaluation:** How much did the actual policy change affect aggregate efficiency?

(ii) **Scale-up:** How would aggregate efficiency change if the policy was extended to all firms in the economy?

### C. *Ex Post Evaluation: Effect of the Policy on Misallocation*

How much did the policy change contribute to change in aggregate efficiency between the two economies? Formally, the answer to this question is

$$\Delta \log TFP = -\frac{\alpha}{2}\left(1+\frac{\alpha\theta}{1-\theta}\right)\left[\mathrm{var}\left(\log\frac{p_i y_i}{k_i}\Big|\begin{pmatrix}\Theta_0\\ \Theta_1\end{pmatrix}, \mathcal{Z}_0\right) - \mathrm{var}\left(\log\frac{p_i y_i}{k_i}\Big|\begin{pmatrix}\Theta_0\\ \Theta_0\end{pmatrix}, \mathcal{Z}_0\right)\right].$$

The change in aggregate TFP is proportional to the difference in the overall variance of log-MRPKs between the initial economy and a counterfactual economy similar in all dimensions to the initial economy except for the new structural parameters for treated firms ($\Theta_0 \rightarrow \Theta_1$). In particular, in this counterfactual economy, $\mathcal{Z}$ remains at $\mathcal{Z}_0$. We are looking to estimate this counterfactual using changes in the *observed* variance of log-MRPKs for firms in the treatment and control group.

Note $\sigma_C^2\left(\begin{pmatrix}\Theta_0\\ \Theta_1\end{pmatrix}, \mathcal{Z}\right)$ (resp. $\sigma_T^2\left(\begin{pmatrix}\Theta_0\\ \Theta_1\end{pmatrix}, \mathcal{Z}\right)$) the variance of $\log\left(\frac{p_i y_i}{k_i}\right)$ for firms in the control group (resp. treatment group) when firms in the control group face frictions $\Theta_0$, firms in the treatment group face $\Theta_1$ and aggregate productivity is $\mathcal{Z}$. Assuming that the policy treatment is small ($|\Theta_1 - \Theta_0| \ll 1$), we obtain the following approximation:

$$\Delta \log TFP \approx -\frac{\alpha}{2}\left(1+\frac{\alpha\theta}{1-\theta}\right)\left(\underbrace{\Delta\sigma^2(C)}_{\text{GE effect}} + \underbrace{\frac{1}{2}\Delta\Delta\sigma^2}_{\text{Treatment effect}}\right), \tag{2}$$

where

- $\Delta\Delta\sigma^2 = \sigma_T^2\left(\begin{pmatrix}\Theta_0\\ \Theta_1\end{pmatrix}, \mathcal{Z}_0\right) - \sigma_T^2\left(\begin{pmatrix}\Theta_0\\ \Theta_0\end{pmatrix}, \mathcal{Z}_0\right) - \left(\sigma_C^2\left(\begin{pmatrix}\Theta_1\\ \Theta_0\end{pmatrix}, \mathcal{Z}_0\right) - \sigma_C^2\left(\begin{pmatrix}\Theta_0\\ \Theta_0\end{pmatrix}, \mathcal{Z}_0\right)\right)$ is the difference-in-difference effect of the policy change on the dispersion of log-MRPK in the counterfactual where $\mathcal{Z}$ remains at $\mathcal{Z}_0$. It measures how the variance of log-MRPKs of treated firms evolves *relative to firms in the control group*. $\Delta\Delta\sigma^2$ thus captures the causal effect of the policy change on the variance of treated firms in the counterfactual economy.
- $\Delta\sigma^2(C) = \sigma_C^2\left(\begin{pmatrix}\Theta_0\\ \Theta_1\end{pmatrix}, \mathcal{Z}_0\right) - \sigma_C^2\left(\begin{pmatrix}\Theta_0\\ \Theta_0\end{pmatrix}, \mathcal{Z}_0\right)$ is the change in the variance of log-MRPKs for firms in the control group in the counterfactual economy (i.e.,

the economy where $\mathcal{Z}$ remains at $\mathcal{Z}_0$). This change *only arises through general equilibrium* (*GE*) *effects*: the structural parameters for control firms are not affected (they remain equal to $\Theta_0$), but the change affecting treated firms may modify equilibrium prices, which, in turn, may affect control firms' choices and therefore the variance of their log MRPKs. $\Delta\sigma^2(C)$ thus captures the change in the variance of log-MRPKs that would purely arise through equilibrium effects if only the experiment takes place.

Estimating equation (2) is challenging. Consider first the GE term, $\Delta\sigma^2(C)$. In the post-experiment data, equilibrium prices are changing not only because of the policy experiment but also because of the change in $\mathcal{Z}$. Therefore, the observed change in the variance of log-MRPKs for control firms may not be a valid estimate for $\Delta\sigma^2(C)$. Our main result, Theorem 1, helps us overcome this challenge. This theorem shows that, in the class of models we consider, the distribution of log-MRPK for a group of firms facing similar frictions $\Theta$ solely depends on the frictions faced by these firms and not on the frictions faced by other firms in the economy, so that

$$\sigma_C^2\left(\begin{pmatrix}\Theta_0\\ \Theta_1\end{pmatrix}, \mathcal{Z}_0\right) = \sigma_C^2\left(\begin{pmatrix}\Theta_0\\ \Theta_0\end{pmatrix}, \mathcal{Z}_0\right) \Rightarrow \Delta\sigma^2(C) = 0.$$

In the counterfactual we consider, and for the class of models described below, the GE effect is, therefore, equal to zero.

Consider now the treatment effect, $\Delta\Delta\sigma^2$. In the data, we observe

$$\begin{aligned}\widehat{\Delta\Delta\sigma^2} = {} & \sigma_T^2\left(\begin{pmatrix}\Theta_0\\ \Theta_1\end{pmatrix}, \boldsymbol{\mathcal{Z}}_1\right) - \sigma_T^2\left(\begin{pmatrix}\Theta_0\\ \Theta_0\end{pmatrix}, \mathcal{Z}_0\right)\\ & - \left(\sigma_C^2\left(\begin{pmatrix}\Theta_1\\ \Theta_0\end{pmatrix}, \boldsymbol{\mathcal{Z}}_1\right) - \sigma_C^2\left(\begin{pmatrix}\Theta_0\\ \Theta_0\end{pmatrix}, \mathcal{Z}_0\right)\right),\end{aligned}$$

while $\Delta\Delta\sigma^2$ only depends on the dispersion of log-MRPK when $\mathcal{Z} = \mathcal{Z}_0$. Again, Theorem 1 helps us solve this issue. The theorem ensures that, within the class of models we consider, the distribution of log-MRPK for a group of firms facing similar frictions $\Theta$ is independent of equilibrium prices, and therefore independent of $\mathcal{Z}$:

$$\sigma_T^2\left(\begin{pmatrix}\Theta_0\\ \Theta_1\end{pmatrix}, \mathcal{Z}_0\right) = \sigma_T^2\left(\begin{pmatrix}\Theta_0\\ \Theta_1\end{pmatrix}, \mathcal{Z}_1\right) \text{ and: } \sigma_C^2\left(\begin{pmatrix}\Theta_0\\ \Theta_1\end{pmatrix}, \mathcal{Z}_0\right) = \sigma_C^2\left(\begin{pmatrix}\Theta_0\\ \Theta_1\end{pmatrix}, \boldsymbol{\mathcal{Z}}_1\right).$$

This result implies that the empirical difference-in-difference, $\widehat{\Delta\Delta\sigma}^2$ is a valid estimate for the counterfactual difference-in-difference: $\widehat{\Delta\Delta\sigma}^2 = \Delta\Delta\sigma^2$. Together, these results imply that the counterfactual change in aggregate efficiency is proportional to the empirical difference-in-difference estimate:

$$\Delta\log TFP = -\frac{\alpha}{2}\left(1 + \frac{\alpha\theta}{1-\theta}\right)\frac{\widehat{\Delta\Delta\sigma^2}}{2}.$$

### D. *Scaling Up the Experiment*

Our second exercise asks: How would aggregate efficiency change if the policy was extended to all firms? Formally, the answer to this question is

$$\Delta \log TFP = -\frac{\alpha}{2}\left(1+\frac{\alpha\theta}{1-\theta}\right)\left[\operatorname{var}\left(\log\frac{p_i y_i}{k_i} \middle| \begin{pmatrix}\Theta_1\\ \Theta_1\end{pmatrix}, \mathcal{Z}_0\right) - \operatorname{var}\left(\log\frac{p_i y_i}{k_i} \middle| \begin{pmatrix}\Theta_0\\ \Theta_0\end{pmatrix}, \mathcal{Z}_0\right)\right],$$

Again, estimating this equation from the experimental data is a priori challenging. The experimental data allows us to estimate $\widehat{\Delta\Delta\sigma^2}$: the causal effect of the policy change on the variance of treated firms when, in the post-experiment data, $\mathcal{Z} = \mathcal{Z}_1$ and *only the treated firms receive the policy treatment*. In contrast, the counterfactual requires an estimate of this treatment effect in an economy with different equilibrium prices: $\mathcal{Z}$ remains unchanged at $\mathcal{Z}_0$ and *all firms receive the policy treatment*. However, thanks again to Theorem 1, we know that, for a group of firms facing similar structural parameters, the distribution of log-MRPKs solely depends on these parameters, i.e., it is independent of $\mathcal{Z}$ or the frictions that other firms in the economy face. Theorem 1 therefore implies that the causal effect of the policy treatment on treated firms observed in the experimental data, $\widehat{\Delta\Delta\sigma^2}$, would be similar even if all firms were treated and $\mathcal{Z}$ had remained at $\mathcal{Z}_0$. This implies the counterfactual change in aggregate efficiency is again proportional to the empirical difference-in-difference $\widehat{\Delta\Delta\sigma^2}$:

$$\Delta \log TFP = -\frac{\alpha}{2}\left(1+\frac{\alpha\theta}{1-\theta}\right)\widehat{\Delta\Delta\sigma^2}.$$

The difference between this formula and the "ex post evaluation formula" is the $1/2$ factor. In ex post evaluation, only half of the firms (the randomly chosen treatment group) are affected, while in the scale-up, all of them are.

## II. Setup and Main Result

This section introduces a class of models of firms dynamics with frictions for which the distribution of MRPKs does *not* depend on the general equilibrium of the economy, i.e., Theorem 1 holds.

### A. *Setup*

Time $t$ is discrete and there is no aggregate uncertainty. There is a continuum of firms indexed by $i \in [0;1]$. Each firm belongs to an industry $s \in [1,S]$. Firm i revenue is given by $p_{it}y_{it} = A_{s,t}^{1-\theta_s} e^{\theta_s z_{it}} k_{it}^{\alpha_s\theta_s} l_{it}^{(1-\alpha_s)\theta_s}$. $k_{it}$ is the capital stock and $l_{it}$ employment. Log productivity $z_{it} \in Z$ follows a (potentially firm-specific, i.e., including fixed effects) stationary Markovian process and is i.i.d across firms. $A_{s,t}$ is an industry-specific revenue shifter. We discuss below several market structures that are consistent with such a revenue function. We consider the steady state of this economy. At the steady state, households' consumption Euler equation ties the equilibrium interest rate $r_t$ to their discount rate so that the safe interest rate $r$ is pinned down throughout our analysis.[5] We note $A_s = A_{s,t}$ the steady-state industry shifter, and $w_s$ the, potentially industry-specific, steady-state wage. The aggregation models we will explore below assume a single labor market and therefore a single wage, so that we will assume here that $w_s = w$. However, this assumption is not required for Theorem 1 below to hold.

To clarify exposition (ie, it accomodates regional labor markets), we assume no frictions in adjusting labor, so that firm $i$'s profit writes

$$\pi(z_{it}, k_{it}; w, A_s) = \max_l \left\{ A_s^{1-\theta_s} e^{\theta_s z_{it}} k_{it}^{\theta_s \alpha_s} l^{\theta_s(1-\alpha_s)} - wl \right\} = \Omega_s \left( \frac{A_s^{1-\Phi_s}}{w^{\Phi_s \frac{1-\alpha_s}{\alpha_s}}} \right) e^{\frac{\Phi_s}{\alpha_s} z_{it}} k_{it}^{\Phi_s},$$

where $\Phi_s = \dfrac{\alpha_s \theta_s}{1-(1-\alpha_s)\theta_s} < 1$, and $\Omega_s$ is an industry-specific constant. As it turns out, the assumption of frictionless labor choice is not necessary: our main results are robust to the presence of some forms of labor adjustment costs.

Firms face several frictions in optimizing capital input. $\boldsymbol{\Theta}_s$ is a vector of structural parameters characterizing these frictions for firms in industry $s$.[6] $\delta_s$ is the depreciation rate of capital in industry $s$. Investment is subject to a one period time-to-build. Firms can finance investment with short-term debt or equity. $b_{it+1}$ is the debt payment due to creditors in period $t+1$. Debt and capital are the endogenous state variables of the firm's problem which we note: $\mathbf{x}_{it} = (k_{it}, k_{it+1}, b_{it}, b_{it+1})$. $r_{it}$ is the interest rate charged by lenders, so that $\frac{b_{it+1}}{1+r_{it}}$ is the proceed from debt issued at date $t$. Investment and debt financing at date $t$ can be subject to adjustment costs $\Gamma(z_{it}, \mathbf{x}_{it}; w, A_s; \boldsymbol{\Theta}_s)$. We also assume that firms pay taxes and receive subsidies: $\mathcal{T}(z_{it}, \mathbf{x}_{it}; w, A_s; \boldsymbol{\Theta}_s)$ corresponds to the net tax paid by the firm.

Finally, we also allow for generic forms of financing frictions. First, equity issuance (negative net cash flows) may be costly, and we note such costs $\mathcal{C}(z_{it}, \mathbf{x}_{it}; w, A_s; \boldsymbol{\Theta}_s)$. These costs are zero when the firm does not issue equity. Second, the amount of outside financing may be constrained, which we capture through a vector of constraint: $\mathbf{M}(z_{it}, \mathbf{x}_{it}; w, A_s; \boldsymbol{\Theta}_s) \leq 0$. Third, the interest rate on debt is described by a function $r()$ such that $r_{it} = r(z_{it}, \mathbf{x}_{it}; w, A_s; \boldsymbol{\Theta}_s)$. As we see, this function allows for risky debt and may embed costs of financial distress, such as liquidation costs.

We note $e_{it}$ the cash flows to equity holders, net of equity issuance costs:

$$\begin{aligned} e_{it} &= \pi(z_{it}, k_{it}; w, A_s) - \left(k_{it+1} - (1-\delta_s) k_{it}\right) + \left( \frac{b_{it+1}}{1 + r(z_{it}, \mathbf{x}_{it}; w, A_s; \boldsymbol{\Theta}_s)} - b_{it} \right) \\ &\quad - \Gamma(z_{it}, \mathbf{x}_{it}; w, A_s; \boldsymbol{\Theta}_s) - \mathcal{T}(z_{it}, \mathbf{x}_{it}; w, A_s; \boldsymbol{\Theta}_s) - \mathcal{C}(z_{it}, \mathbf{x}_{it}; w, A_s; \boldsymbol{\Theta}_s) \\ &= e(z_{it}, \mathbf{x}_{it}; w, A_s; \boldsymbol{\Theta}_s), \end{aligned}$$

[5] This "exogeneity" of $r$ results from our steady-state assumption, and holds for any additively separable utility function.

[6] Our main setup only allows for heterogeneity in structural parameters $\Theta_s$ *across industries*. This is to simplify exposition. Our framework can accommodate heterogeneity in structural parameters within an industry. For instance, some firms within an industry could be more constrained, and/or have larger adjustment costs. Our main results and aggregation formulas would still go through.

i.e., cash flows depend on state variables $(z_{it}, \mathbf{x}_{it})$, the equilibrium wage $w$ and industry-specific demand shifter $A_s$, and structural parameters governing industry-s frictions, $\boldsymbol{\Theta}_s$.

The timing is standard. At the beginning of period $t$, productivity $z_{it}$ is realized. Then the firm produces, it selects the next period stock of capital $k_{it+1}$, pays the corresponding adjustment costs, reimburses its existing debt $b_{it}$ and receives the proceeds from new debt issuance $\frac{b_{it+1}}{1+r_{it}}$. Then, the period ends. We allow for, potentially strategic, default: the firm will operate in period $t+1$ if and only if its productivity belongs to a "survival set": $z_{it+1} \in \mathcal{Z}(k_{it+1}, b_{it+1}; w, A_s; \boldsymbol{\Theta}_s)$. When $z_{it+1} \notin \mathcal{Z}(k_{it+1}, b_{it+1}; w, A_s; \boldsymbol{\Theta}_s)$, default occurs and the continuation value to the firm's owner is assumed to be zero.[7] So that the number of firms remain constant at steady state, we assume that each exiting firm is replaced by a new firm endowed with no debt and the average capital stock of exiting firms.

Firms maximize the expected present value of cash flows. To save on notations, let us temporarily omit the *it* index and denote with primes next-period variables. We make additional technical assumptions listed in online Appendix A.A2, and briefly explain why they are satisfied in most models. These assumptions ensure that the Bellman equation has a unique solution.[8] Under these assumptions, the present value of expected cash flows $V(z, k, b; w, A_s; \boldsymbol{\Theta}_s)$ is uniquely defined as

$$(3)\quad V(z,k,b;w,A_s;\boldsymbol{\Theta}_s) = \left| \begin{array}{l} \max\limits_{k',b'} \left[ e(z,k,k',b,b';w,A_s;\boldsymbol{\Theta}_s) + \beta \mathbb{E}_{z' \in \mathcal{Z}(k',b';w,A_s;\boldsymbol{\Theta}_s)} \big( V(z,k,b;w,A_s;\boldsymbol{\Theta}_s) \,|\, z \big) \right], \\ \mathbf{M}(z,k,k',b,b';w,A_s;\Theta_s) \ \leq \ 0 \end{array} \right.$$

where $\beta < 1$ is the firm's discount rate, which is constant in this set-up with no aggregate uncertainty. This equation also uniquely defines investment and financing policy functions, which therefore depend on structural parameters $\boldsymbol{\Theta}_s$ and $(w, Y_s)$.

Last, note that our framework accommodates flexible heterogeneity across firms. For instance, some firms may be financially constrained, others may face large adjustment costs. We do not index these parameters with an $i$ to lighten notations, but our results are robust to heterogeneous frictions.

## B. *Main Result*

This section provides sufficient conditions under which the joint distribution of MRPKs $\left(\frac{py}{k}\right)$ is independent of $(w, A_s)$. As we explain in the following section, these conditions hold in a large class of models of firm dynamics, commonly used in macrofinance.

[7] This stark assumption is for the sake of exposition. Our results would carry through for a larger class of default continuation values, as long as they satisfy a homogeneity property similar to the assumptions in Theorem 1.

[8] These results are broadly used in the literature. They arise from the extension of the results in Stokey and Lucas (1989) from strictly continuous to piecewise continuous cash flow functions. See Caballero and Leahy (1996) for a version of the proof with fixed adjustment costs and Hennessy and Whited (2007) for a version of the proof with fixed equity issuance costs. In both cases, the logic is the same and applies whenever the cash flow is piecewise continuous.

THEOREM 1 (Distribution of Wedges): *Define* $S_s = \frac{A_s}{w^{\frac{(1-\alpha_s)\Phi_s}{(1-\Phi_s)\alpha_s}}}$, *the "scale" of industry s. Assume that*

(*i*) *adjustment costs* $\Gamma()$, *taxes* $\mathcal{T}()$, *funding constraints* $\mathbf{M}()$, *and the equity issuance cost function* $\mathcal{C}()$ all *satisfy the following property:*

$$\forall \ (z, \mathbf{x}; w, A_s; \mathbf{\Theta}_s), Q(z, \mathbf{x}; w, A_s; \mathbf{\Theta}_s) \ = \ S_s \times Q\Big(z, \frac{\mathbf{x}}{S_s}; 1, 1; \mathbf{\Theta}_s\Big); \tag{4}$$

(*ii*) *the interest rate* $r()$ *satisfies the following property:*

$$\forall \ (z, \mathbf{x}; w, A_s; \mathbf{\Theta}_s), r(z, \mathbf{x}; w, A_s; \mathbf{\Theta}_s) \ = \ r\Big(z, \frac{\mathbf{x}}{S_s}; 1, 1; \mathbf{\Theta}_s\Big); \text{ and} \tag{5}$$

(*iii*) *the survival set* $Z()$ *does not depend on aggregate conditions:*

$$Z(k', b'; w, A_s; \mathbf{\Theta}_s) \ = \ Z(k', b'; 1, 1; \mathbf{\Theta}_s). \tag{6}$$

*Then, for firms in industry s, the steady-state distribution of MRPKs,* $\frac{py}{k}$, *does not depend on* $(w, A_s)$.

PROOF:

See online Appendix A.A3.

This theorem shows that, given structural parameters $\Theta_s$, the steady-state distribution of MRPKs does not depend on the wage $w$ or revenue-shifter $A_s$. This is the key result of the paper. It has important implications for inference in (quasi-) experimental settings. Consider an experiment that changes $\Theta_s$ for a particular industry. Because this experiment changes the frictions faced by firms, it will affect the distribution of MRPKs in this industry. Theorem 1 says that a similar change in $\Theta_s$ would similarly affect the distribution of MRPKs even if it occurred in an economy with different $A_s$ and $w$. In other words, estimates of the effect of this change in policy on the distribution of MRPK in industry $s$ are externally valid: they solely depend on the actual policy changes and not on the equilibrium where these effects are estimated. As we explained in Section I, this theorem, combined with an aggregation model, allows us to measure the effect of a policy change on equilibrium misallocation using (quasi-) experimental data.

Theorem 1 rests on two key assumptions. The first one is that the elasticity of revenue to factors is constant. This property ensures that firm-level operating profits scale proportionally to $S_s = \frac{A_s}{w^{\frac{(1-\alpha_s)\Phi_s}{(1-\Phi_s)\alpha_s}}}$:

$$\pi(z, k; w, A_s) \ = \ \max_l \big\{ A_s^{1-\theta_s} e^{\theta_s z} k^{\theta_s \alpha_s} l^{\theta_s(1-\alpha_s)} - wl \big\} \ = \ S_s \pi\Big(z, \frac{k}{S_s}; 1, 1\Big).$$

There is indeed evidence that capital and labor are more complementary than suggested by Cobb-Douglas (Oberfield and Raval 2014), but the overwhelming majority of macrofinance models use revenue functions with constant elasticity in labor and capital. The second key set of assumptions is the homogeneity of frictions in equations (4)–(6). We show in the following section that these homogeneity assumptions are satisfied in most models of firm dynamics.

### C. *Validity of Theorem 1 in Standard Models of Firm Dynamics*

We now relate the assumptions in Theorem 1 to standard models of firm dynamics. We then conduct an extensive review of the recent literature on firm dynamics and find that in the overwhelming majority of models we review, these assumptions hold. Our sufficient statistics approach therefore provides a valid alternative to structural estimation when computing aggregate counterfactuals in the context of these models.

*Adjustment Costs*.—Standard linear and quadratic adjustment costs to capital satisfy the assumptions in Theorem 1 since they are homogeneous of degree 1 in $k$. Similarly, fixed costs or asymmetric costs that scale with the capital stock, such as $k\mathbf{1}_{\{k'-(1-\delta)k<0\}}$, $\mathbf{1}_{\{k'-(1-\delta)k\neq 0\}}k$ also satisfy these assumptions. Finally, fixed costs that scale with output (e.g., $\mathbf{1}_{\{k'-(1-\delta)k\neq 0\}}py$) also work since $p_i y_i \propto S_s^{1-\Phi_s} e^{\frac{\Phi_s}{\alpha_s}z_i} k_s^{\Phi} = S_s \times e^{\frac{\Phi_s}{\alpha_s}z_i}\left(\frac{k}{S_s}\right)^{\Phi_s}$. Instead, if fixed costs are expressed in absolute terms (i.e., they do not scale with firms' output or capital stock), then the assumptions in Theorem 1 no longer hold.

*Financing Frictions*.—Standard models of financing frictions also satisfy the assumptions of Theorem 1. Consider first the interest rate function. For instance, in Michaels, Beau Page, and Whited (2018) or Gilchrist, Sim, and Zakrajšek (2014a), debt is risky and in the event that the firm is unable to repay, the lender can seize a fraction $1-\zeta_s$ of the firm's fixed assets k. The firm's future market value cannot be used as collateral, so that a firm's access to credit is mediated by a net worth covenant, which restrains the firm's ability to sell new debt based on its current physical assets and liabilities. Concretely, default is triggered when net worth reaches 0, which defines a threshold value for productivity $z_s^*$ such that

$$0 = \kappa_0 \times S_s^{1-\Phi_s} e^{\frac{\Phi_s}{\alpha_s}z_s^*} k^{\Phi_s} - b + p_k(1-\delta_s)k, \tag{7}$$

with $\kappa_0$ a constant and $p_k < 1$ is the second-hand price of capital, which we treat as a technological parameter. As in Michaels, Beau Page, and Whited (2018), the right side of the previous equation represents the resources that the firm could raise in order to repay its debt just prior to bankruptcy. The zero-profit condition for lenders then pins down the risky interest rate,

$$\beta\left[\int_0^{z_s^*}\left(\kappa_0 S^{1-\Phi_s} e^{\frac{\Phi_s}{\alpha_s}z'} k_s'^{\Phi} + (1-\zeta_s)(1-\delta_s)k'\right) dH(z'|z) + \left(1 - H(z^*|z)\right)b'\right] = \frac{b'}{1 + r(z,\mathbf{x};w,Y_s;\Theta_s)}. \tag{8}$$

Equations (7) and (8) define the survival set $Z()$ and interest rate function $r()$. It is clear from equation (7) that $z_s^*(k,b;w,A_s;\Theta_s) = z_s^*\left(\frac{k}{S},\frac{b}{S};1,1;\Theta_s\right)$. As a result, it is direct to see that equation (8) implies that $r()$ is scale independent $r(z, \mathbf{x}; w, A_s; \Theta_s) = r\left(z, \frac{\mathbf{x}}{S}; 1, 1; \Theta_s\right)$, and satisfies the assumption in Theorem 1.

Similarly, the specification of debt renegotiation in Hennessy and Whited (2007) would also satisfy our assumptions. More generally, these models make the probability of default independent of the scale of the economy $S$, and the loss given default proportional to $S$. These properties ensure our assumption about $r()$ in Theorem 1 is satisfied. Models of risk-free debt, such as Midrigan and Xu (2014), also satisfy our assumption.

Our assumption on the cost of equity is also verified in Michaels, Beau Page, and Whited (2018) and Gilchrist, Sim and Zakrajšek (2014a), who posit that equity issuances are subject to underwriting fees such that there is a positive marginal cost to issue equity:

$$\mathcal{C}(z, \mathbf{x}; w, A_s; \boldsymbol{\Theta}_s) \propto -\min\left\{0, e(z, \mathbf{x}; w, A_s; \boldsymbol{\Theta}_s)\right\}.$$

Given that $e(z, \mathbf{x}; w, A_s; \boldsymbol{\Theta}_s) = S_s e\left(z, \frac{\mathbf{x}}{S_s}; 1, 1; \boldsymbol{\Theta}_s\right)$, it is direct that $\mathcal{C}(z, \mathbf{x}; w, A_s; \boldsymbol{\Theta}_s) = S_s e\left(z, \frac{\mathbf{x}}{S_s}; 1, 1; \boldsymbol{\Theta}_s\right)$. Thus, financing frictions specified in Gilchrist, Sim, and Zakrajšek (2014a) and Michaels, Beau Page, and Whited (2018) satisfy the assumptions of Theorem 1. Additionally, fixed or quadratic equity issuance costs would also satisfy these assumptions as long as they scale with the size of the firm. For instance, issuance costs proportional to $\frac{\min\{0, e\}^2}{k}$ or $k\mathbf{1}_{e<0}$ belong to that category.

Finally, our formulation of financing frictions also encompasses debt constraints as in Midrigan and Xu (2014) or Catherine et al. (2018). In both models, debt is assumed to be risk-free through full collateralization $b' \leq \xi k'$, and producers can only issue claims to a fraction $\chi$ of their future profits, $-e \leq \chi \times V$. In this case, the vector $\mathbf{M}()$ of funding constraints consists of the last two inequalities, which are both homogeneous of degree 0 in $S$ and thus satisfy the assumptions of Theorem 1. Of course, any combination of the constraints in Midrigan and Xu (2014) and Hennessy and Whited (2007) would also satisfy these assumptions. Note that our model also encompasses debt constraints (instead of equity constraints) where debt financing is limited by existing or future cash flows.

*Taxes*.—Standard specifications for the corporate income tax, $\mathcal{T}(\cdot) = \tau \max\left(0, \pi(\cdot) - \delta k - b\right)$, satisfy the assumption of Theorem 1 since $\pi(z, \mathbf{x}; w, A_s; \boldsymbol{\Theta}_s) = S_s \pi\left(z, \frac{\mathbf{x}}{S_s}; 1, 1; \boldsymbol{\Theta}_s\right)$. A progressive tax system could be consistent with these assumptions provided that the tax brackets are defined in terms of percentile of the firm profit distribution. Similarly, size-based regulations typically will not generically satisfy the assumption in Theorem 1. However, a specification where regulation is specified as a piecewise linear labor tax and the thresholds are defined in terms of percentile of the output distribution would satisfy the assumptions in Theorem 1.

*Recent Literature Review*.—The previous section discussed standard features of firm dynamics models that fall within the assumption of Theorem 1. How common are these features in the literature? We answer this question by conducting a systematic literature review. We search for all papers citing Hennessy and Whited (2007); Midrigan and Xu (2014); and Moll (2014), published within a list of twelve journals[9] and with at least 50 Google Scholar citations. This search delivers a list of 44 recent papers. We group the set of assumptions used in these models into five categories: production function, adjustment costs, borrowing constraint, equity issuance costs, and taxes. For each of the 44 papers and each of these 5 categories, we check whether the paper's assumptions satisfy the conditions of Theorem 1 for this category. Table 1 shows that modeling choices made in these papers are almost always consistent with Theorem 1's assumptions. *All* papers but one assume Cobb-Douglas production. In five of these papers, operating leverage is modeled through a nonscalable fixed cost. Nonscalable fixed costs do not fit Theorem 1's assumptions. These types of fixed costs, however, have the unpleasant feature that they become irrelevant as firms grow and are therefore not desirable features for models of firm dynamics. In almost all papers, the specification for physical adjustment costs scales linearly with $S_s$. Even when there are fixed costs of investment, these costs are assumed to be proportional to total sales and therefore also scale with $S_s$. In all but three papers, the borrowing constraint is scale-free. Equity issuance costs constitute the most frequent deviation from the assumptions of Theorem 1: nine papers introduce fixed equity issuance costs that do not scale with the size of the firm. Finally, all but two papers introduce a standard corporate tax, which naturally scales with $S_s$. Overall, the assumptions of Theorem 1 hold in most existing models in the recent literature.

### D. *Aggregation Model*

Equipped with Theorem 1, we now proceed to discuss how to use difference-in-difference estimates to perform aggregate counterfactual analysis. What is needed at this stage is an aggregation model.

To fix ideas, we use here the aggregation model of Hsieh and Klenow (2009). Our approach works with more complex aggregation models as we describe below. There is a continuum of firms indexed by $i \in [0;1]$ partitioned into industries indexed by $s \in [1,S]$. Each firm produces intermediary inputs $y_i$. Industry output $Y_s^0$ is produced by combining intermediate inputs $y_i$ with a CES technology. A perfectly competitive final good market aggregates industry output using a Cobb-Douglas technology:

$$\log\left(Y^0\right) = \sum_{i=1}^{S}\phi_s \log\left(\left(Y_s^0\right)^{\phi_s}\right) \text{ and } Y_s^0 = \left(\int_{i\in s} y_i^{\theta_s}\right)^{\frac{1}{\theta_s}}.$$

Production follows a Cobb-Douglas technology $\left(y_i = e^{z_i} k_i^{\alpha_s} l_i^{1-\alpha_s}\right)$, so that firm i's revenue is similar to the one introduced in Section IIA with $A_s = Y_s\left(\frac{Y_s}{Y}\right)^{-\frac{1}{1-\theta_s}}$. Labor is supplied elastically by households as described in Section IID below. There is a perfectly elastic supply of the capital good, which is produced from the final good by a perfectly competitive market using a constant return-to-scale technology. Note that this assumption, however, is not necessary for our results. In online

[9] *American Economic Review, Econometrica, Journal of Political Economy, Review of Economic Studies, Quarterly Journal of Economics, Journal of Finance, Journal of Financial Economics, Review of Financial Studies*, one of the three *American Economic Journal*s, and the *Journal of Monetary Economics*.

Appendix A.A9, we show, in a simplified version of our model, that even when the supply of capital is imperfectly elastic, Theorem 1 still holds, and our aggregation formula for TFP is unchanged.

Firms face no frictions when optimizing labor inputs. In contrast, firms select their capital stock under a set of frictions described in Section II and that satisfy Theorem 1's assumptions. $\Theta_s^0$ describes the vector of structural parameters that govern frictions for firms in industry $s$ in this initial economy. These frictions generate a wedge between MRPKs, $\frac{p_i y_i}{k_i}$ and firms' user cost of capital. Following Hsieh and Klenow (2009), we assume that within each industry, the joint distribution of log-productivity $z_i$ and log-MRPKs is normal.

With this aggregation model, aggregate efficiency, *TFP*, is naturally defined, as

$$\log(TFP) = \log(Y) - \alpha^* \log(K) - (1-\alpha^*)\log(L),$$

with $K$ the aggregate capital stock, $L$ is aggregate employment, and $\alpha^* = \sum_{s=1}^{S} \phi_s \alpha_s$ is the weighted-average capital share, weighted by the industry share in total output.

*Ex Post Evaluation of Policy Experiments*.—We introduce a policy experiment in this economy. In industry s, firms receive a policy treatment that changes their structural parameters by $d\Theta_s$. At the same time as this policy treatment, two potentially confounding changes take place:

(i) Average productivity changes so that the undistorted aggregate TFP, $\mathcal{Z} = \prod_{s=1}^{S} \left( \int_{i \in s} e^{\frac{\theta_s}{1-\theta_s} z_{is}} di \right)^{\frac{\phi_s(1-\theta_s)}{\theta_s}}$, goes from $\mathcal{Z}_0$ to $\mathcal{Z}_1$.[10]

(ii) All firms experience a common change $d\Theta$ to their structural parameters.

Post-experiment, the structural parameters in industry $s$ are therefore

$$\Theta_s^1 = \Theta_s^0 + d\Theta + d\Theta_s.$$

We assume that the policy experiment and the aggregate shocks are small: $d\Theta_s \ll 1$, $d\Theta \ll 1$. We also assume that there is limited ex ante heterogeneity in frictions: for all s and s', $\left|\Theta_s^0 - \Theta_{s'}^0\right| \ll 1$.

We are looking to estimate the contribution of the policy change to change in aggregate TFP: how much would have aggregate TFP increased *if the only change taking place in the economy was the change in policy* $d\Theta_s$? We show in Proposition 2 the answer depends on three sufficient statistics that can be directly estimated in the (quasi-)experimental data:

- $\widehat{\Delta\Delta\sigma^2}(s)$ is the difference-in-difference estimate of the effect of the policy change on the variance of log-MRPK in industry $s$. It corresponds to the change in the variance of log-MRPK for firms in industry $s$, relative to a set of industries

[10] The same result would hold in the presence of other aggregate shocks such as a shock to the elasticity of labor supply or a shock to aggregate labor supply.

that are not affected by the policy change. As we explain below, this statistic can also be estimated using a heterogeneous treatment intensity approach, which is commonly used in the empirical literature.

- $\widehat{\Delta\Delta\mu}(s)$ is, similarly, the difference-in-difference estimate of the effect of the policy change on the mean log-MRPK in industry $s$.
- $\widehat{\Delta\Delta\,\sigma_{lMRPK,lpy}}(s)$ corresponds to the difference-in-difference estimate of the effect of the policy change on the covariance between log output and log sales in industry $s$.

Empirically, our assumption that the treatment is "small" ensures that these difference-in-difference estimates are an order of magnitude smaller than one. This is needed in order to obtain the simple aggregation formulas below in the presence of heterogeneous industries. This is also directly testable in the data—as we will see in our example.

PROPOSITION 2 (Ex Post Evaluation of Policy Experiments): *Assume that the firm-level model satisfies the assumptions in Theorem 1. Then, the counterfactual change in aggregate TFP purely due to the change in policy* $(d\Theta_s)_{s\in[1,S]}$ *is approximated by the following expression:*

$$\begin{aligned}(9)\qquad \Delta\log(TFP) \approx\ & -\frac{\alpha^*}{2}\sum_{s=1}^{S}\kappa_s\left(1+\frac{\alpha_s\theta_s}{1-\theta_s}\right)\widehat{\Delta\Delta\,\sigma^2}(s)\\ & -\sum_{s=1}^{S}(\phi_s\alpha_s-\kappa_s\alpha^*)\Bigg[\widehat{\Delta\Delta\mu}(s)+\widehat{\Delta\Delta\,\sigma_{lMRPK,lpy}}(s)\\ & \qquad\qquad +\frac{1}{2}\frac{\alpha_s\theta_s}{1-\theta_s}\widehat{\Delta\Delta\,\sigma^2}(s)\Bigg],\end{aligned}$$

*where* $\kappa_s$ *is the share of industry s in the total capital stock of the pre-reform economy.*

PROOF:

See online Appendix A.A4.

This formula decomposes misallocation into two terms. The first one captures how the policy change affects within-sector misallocation. The second one measures how the policy change induces cross-industry reallocation of production, which, in turn, affects aggregate efficiency. This last term corresponds to the sum of changes in industry output (the terms in squared brackets), weighted by the difference, for each industry, between output share and capital share $(\phi_s\alpha_s-\kappa_s\alpha^*)$. This difference is larger for more distorted industries, those with a higher sales-to-capital ratio than average. Hence, through this last term, TFP is lower if output increases more in distorted sectors.

*Practical Estimation.*—There are two standard ways in the empirical literature to estimate the statistics in equation (9). A first approach assumes that a control group, made of a subset of industries, does not receive any policy treatment. In this case, for each industry $s$ receiving a treatment $d\Theta_s$, the statistics are estimated using the standard difference-in-difference estimate relative to this control group. For example, $\widehat{\Delta\Delta\,\sigma^2}(s)$ corresponds to the pre-post change in the observed variance of log-MRPKs for firms in industry $s$ minus the same pre-post change for firms in control industries.

A second approach, more common in the literature, and which we use in our empirical application, imposes a linear structure on the effect of the reform. Industries are heterogeneous in their *exposure* to an aggregate policy change: $d\Theta^{\text{reform}}$. $\lambda_s$ is the exposure of industry $s$ to $d\Theta^{\text{reform}}$. This structure allows us to also allow for a confounding idiosyncratic shock, $\eta_s$ as long as it is orthogonal to $\lambda_s$ (a classic identifying assumption in this literature):

$$\Theta_s^1 \;=\; \Theta_s^0 + \lambda_s d\Theta^{\text{reform}} + d\Theta + \eta_s, \quad \text{with} \quad \eta_s \perp \lambda_s.$$

Note in particular that this approach assumes that industries with $\lambda_s = 0$ are not directly exposed to the aggregate policy change $d\Theta_s$. In this setting, we can estimate $\widehat{\Delta\Delta\,\sigma^2(s)}$ by regressing the observed variance of log-MRPK across industries, $\widehat{\sigma^2}(s)$, against the exposure measure $\lambda_s$ and a post-experiment dummy, and then multiplying the resulting coefficient estimate by $\lambda_s$. $\widehat{\Delta\Delta\mu(s)}$ and $\widehat{\Delta\Delta\,\sigma_{lMRPK,lpy}(s)}$ can be estimated in a similar way.

*Scaling Up Policy Experiments*.—We now consider the case of an experiment that is not at scale within each industry. The model is similar to Section IID except that, in each industry $s$, only a random share $\nu_s$ of firms receives the policy treatment $d\Theta_s \ll 1$. We are looking to estimate the counterfactual change in aggregate TFP that would result from extending the policy $d\Theta_s$ to all firms in each industry $s$. The answer can be simply expressed as a function of three new sufficient statistics, estimated *within* industry:

PROPOSITION 3 (Scaling-Up Policy Experiments): *Assume that the data-generating process belongs to a model that satisfies the assumptions in Theorem 1. Then, relative to pre-experiment economy, the counterfactual change in aggregate TFP resulting from extending the policy experiment to all firms $(d\Theta_s)_{s\in[1,S]}$ is approximated by the following expression:*

$$(10)\quad \Delta\log(TFP) \;\approx\; -\frac{\alpha^*}{2}\sum_{s=1}^{S}\kappa_s\left(1+\frac{\alpha_s\theta_s}{1-\theta_s}\right)\overline{\widehat{\Delta\Delta\,\sigma^2(s)}}$$

$$-\sum_{s=1}^{S}(\phi_s\alpha_s - \kappa_s\alpha^*)\left(\overline{\widehat{\Delta\Delta\mu(s)}} + \overline{\widehat{\Delta\Delta\,\sigma_{lMRPK,lpy}(s)}}\right.$$

$$\left. + \frac{1}{2}\frac{\alpha_s\theta_s}{1-\theta_s}\overline{\widehat{\Delta\Delta\,\sigma^2(s)}}\right),$$

*where $\kappa_s$ is the share of industry s in the total capital stock of the pre-reform economy and $\widehat{\widehat{\Delta\Delta\sigma^2(s)}}$, $\widehat{\widehat{\Delta\Delta\mu(s)}}$ and $\widehat{\widehat{\Delta\Delta\sigma_{lMRPK,lpy}(s)}}$ can be estimated through a difference-in-difference within industry s.*

PROOF:
See online Appendix A.A5.

In this context, the statistics $\widehat{\widehat{\Delta\Delta\sigma^2(s)}}$, $\widehat{\widehat{\Delta\Delta\mu(s)}}$, and $\widehat{\widehat{\Delta\Delta\sigma_{lMRPK,lpy}(s)}}$ can be directly estimated through a difference-in-difference within industry $s$. For instance, $\widehat{\widehat{\Delta\Delta\sigma^2(s)}}$ corresponds to the pre-post change in the variance of log-MRPKs for firms in the treatment group in industry $s$ relative to firms in the control group in industry $s$. To estimate these statistics, it is critical to estimate treatment in a subsample of firms representative of the population. As such, exposure designs—like the one in our empirical application—are typically not well suited to analyzing scale-ups, since they do not distinguish, within group, which firms are treated and which ones are not.

*Effect on Output*.—Our approach is not restricted to analyzing the effect of policy change on misallocation. We can also estimate, in similar (quasi)-experimental settings, the contribution of a policy change to change in aggregate output or employment. In order to do this, we need to introduce the household side of the economy to obtain an aggregate labor supply curve. Assume a representative household has GHH preferences (Greenwood, Hercowitz, and Huffman 1988) over consumption and leisure, $u(c_t, l_t) = \frac{1}{1-\gamma}\left(c_t - \frac{l_t^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}}\frac{\bar{w}}{\bar{L}^{\frac{1}{\epsilon}}}\right)^{1-\gamma}$, where $c_t$ is period $t$ consumption, $l_t$ is period t labor supply, $\epsilon$ is the Frisch elasticity and $(\bar{w}, \bar{L})$ are normalizing constants. The representative household owns all the firms in the economy, as well as a safe asset that offers real return $r$ in unlimited supply. In the absence of aggregate uncertainty, at the steady state, optimal consumption and labor supply decisions imply that $L_t^s = \bar{L}\left(\frac{w}{\bar{w}}\right)^{\epsilon}$, and that the firm's discount factor β is pinned down by the household's psychological discount factor.

Consider now a policy experiment similar to the one described in Section IID. As in Section IID, we are interested in estimating how aggregate output would have changed *if the only change taking place in the economy was the policy change $d\Theta_s$*. We show, in online Appendix A.A6, that this counterfactual change in aggregate output can be easily estimated using the same sufficient statistics,

$$(11) \qquad \Delta \log Y = -(1+\epsilon)\sum_{s=1}^{S}\frac{\phi_s\alpha_s}{1-\alpha^*}\left(\widehat{\Delta\Delta\mu(s)} + \frac{1}{2}\frac{\alpha_s\theta_s}{1-\theta_s}\widehat{\Delta\Delta\sigma^2}(s) + \widehat{\Delta\Delta\sigma_{MRPK,py}}(s)\right),$$

where the sufficient statistics $\Delta\Delta\mu(s)$, $\Delta\Delta\sigma^2(s)$, and $\Delta\Delta\sigma_{MRPK,py}(s)$ are defined in Section IID. Equation (11) is intuitive. First, if the experiment results in an increase in the average log-MRPK, aggregate output will decrease as the experiment depletes the capital stock. Second, if the policy change leads to an increase in the within-industry dispersion of log-MRPKs, total output decreases since aggregate production has become less efficient. Finally, if the experiment results in large firms being more distorted, it will lead to aggregate output losses. As shown in Section IID, these statistics can be estimated in this setting using a standard difference-in-differences approach in the presence of control industries, or through linear regression in the context of heterogeneous treatment intensity.

*Alternative Aggregation Models*.—Our approach is compatible with more complex aggregation models. In online Appendix A.A7, we present similar aggregation formulas in a model with roundabout production and an input-output network. The formulas described above are only marginally affected. In online Appendix A.A8, we explore an extension where the revenue shares of industries are allowed to vary (the Cobb-Douglas aggregator in Hsieh and Klenow (2009) forces industry output shares to be constant across the steady-states we consider). One restriction with this model, however, is that we can only define aggregate TFP when the capital share is constant across industries: $\alpha_s = \alpha$. Online Appendix A.A8 details the aggregation formula for this model.

In all these models, we assume that workers are perfectly mobile across industries so that a single wage clears the labor market. This assumption is standard but not necessary. Our framework can be extended to account for the imperfect mobility of workers across industries (or regions). Note also that, in all these models, we assume that production has constant return to scale and imperfect competition generates decreasing returns to scale in revenues. Our approach, however, also applies when there are decreasing returns to scale in production, with appropriate, but marginal, modifications to the aggregation formula.

*Limitations of Our Approach*.—Like all aggregation exercises, our approach relies on several key assumptions: Cobb-Douglas production technology, homogeneous frictions, and frictionless labor optimization. These assumptions are sufficient to obtain the result in Theorem 1, which ensures that the observed changes in moments of the MRPK distributions in the experimental data are externally valid. On the production side, the result does not hold with CES technology. On the labor side, the result carries through with linear labor taxes, but not with labor adjustment costs.

Our approach does not apply to any aggregation model. It relies on CES aggregation across firms and industries so that firm-level output is proportional to industry-level and aggregate output. Finally, the steady-state assumption is essential. Our results do not hold in the presence of business cycle fluctuations. This has important implications for empirical applications: our approach requires that we observe a sufficient number of periods around the experiment to captures the steady-state changes in the distribution of log-MRPK.

## III. Application: French Banking Reform of 1985

We now use our framework to evaluate the macro-economic impact of the French banking deregulation episode of 1984–1985. Bertrand, Schoar, and Thesmar (2007) analyze this reform using cross-industry heterogeneity as a source of identification. We combine their identification strategy and the methodology described above to estimate how much the reform contributed to aggregate TFP and output growth in this period.

### A. *Brief Description of the Reform*

In the early 1980s, the French banking system was under the firm grip of the Treasury, who controlled, directly or indirectly, nearly all of the banking system. Interest rates were kept low to encourage investment. Corporate loans were subsidized under hundreds of different programs corresponding to different priorities of the government (preserve jobs, modernize industry, support agriculture, etc.). In order to prevent banks from overlending and keep inflation under the lid, banks were subject to close monitoring from the Bank of France and to monthly lending ceilings. In the mid-1980s, the finance ministry embarked on a series of reforms of the banking system. On January 24, 1984, the French Banking Act went into effect, increasing competition between banks, allowing deposit-taking banks to have investment banking activities, encouraging investment in new branches. Subsidized loans were eliminated. Bank of France regulation was removed while interest rates were raised. The bond and money markets were revived, allowing banks to raise wholesale funding. Overall, the main effect of the reform was to move to a more decentralized decision-making process on loan amounts and interest rates and to introduce a stronger for-profit motive among banks. We defer the reader to Bertrand, Schoar, and Thesmar (2007) for a more detailed description.

### B. *Data and Empirical Strategy*

We are looking to estimate the contribution of banking deregulation to the change in aggregate TFP around the reform. As explained above, one cannot do this by simply looking at aggregate TFP changes, as the reform may have coincided with a persistent change in technology or another policy reform. In other words, we are interested in how much aggregate TFP would have increased *if the only change taking place in the economy was banking deregulation*. The answer to this question is provided by equation (9). To estimate the three sufficient statistics required in this formula, we follow the identification strategy in Bertrand, Schoar, and Thesmar (2007), which relies on varying treatment exposure at the industry-level.

We use accounting information from corporate tax files that cover the 1975–1999 period (INSEE and DGFiP 1975–1999). As in Bertrand, Schoar, and Thesmar (2007), we restrict the sample to firms that have either more than 100 employees or more than 20 million euros of sales for at least four years. We exclude firms in the financial sector (banking and insurance industries). We run our regression analyses on a window centered around the reform, 1976–1994.[11] Our regression sample contains 323,840 firm-year observations, corresponding to 27,461 unique firms. For each industry in our sample, we define our measure of exposure to banking deregulation,

[11] Our window starts in 1976 since our measure of firms' capital stock use information on previous year's net book value of assets and the data start in 1975.

$\lambda_s$, as the average ratio of total bank debt over total liabilities in the pre-reform period, i.e., between 1976 to 1983. This heterogeneous treatment intensity approach follows Bertrand, Schoar, and Thesmar (2007) and is in the tradition of the literature on financial development (e.g., Rajan and Zingales 1998). Intuitively, industries that are more financially dependent on banks prior to the reform are more affected by the deregulation. We defer the reader to Bertrand, Schoar, and Thesmar (2007) for an in-depth discussion of the underlying identifying assumption.

We measure a firm's capital stock as the average of the net book value of assets at the beginning and end of the fiscal year. We measure log-MRPK at the firm level as the log of the ratio of value-added to the firm's capital stock. This ratio is winsorized at the 1 percent level every year to limit the influence of outliers.[12] To compute the moments of the log-MRPK distribution at the industry-year level, we require that there are at least five firms in each industry-year cell. This results in a sample of 493 unique industries $s$ for which we can compute moments of the log-MRPK distribution: the mean and variance of log-MRPK, as well as the covariance of log-MRPK and log value-added. Table 2 provides summary statistics for the main variables we use in our empirical analysis. Our aggregation formulas rest on the assumption that log MRPKs are normally distributed before the treatment. We show this holds inthe data in Figure 1.

For each of these three moments $M_{st}$, we evaluate the effect of the financial reform by adopting a standard difference-in-difference estimation strategy with heterogeneous treatment exposure:

$$M_{st} = \delta_t + \eta_s + b_M . \lambda_s \times POST_t + \mu_s \times t + \eta_{st}, \tag{12}$$

where $\delta_t$ is a year fixed effect and $\eta_s$ is an industry fixed effect. $\lambda_s$ is the industry-level measure of exposure to banking deregulation, and $\mu_s \times t$ are industry-specific trends. Finally, $POST_t$ is a dummy variable for the post-reform period. Since it is difficult to unambiguously assign 1984 to the pre- or post-reform period, we exclude 1984 from our regression sample. Standard errors are clustered at the industry level.

To check robustness we also estimate a "quartile of exposure" specification, where we split the exposure variable, $\lambda_s$, into quartiles of treatment intensity,

$$M_{st} = \delta_t + \eta_s + \sum_{j=2}^{4} c_{jM} . 1_{\lambda_s \in Q_j} \times POST_t + \mu_s \times t + \eta_{st}, \tag{13}$$

where $Q_j$ is the jth quartile of treatment intensity (i.e., industry leverage pre-reform).

As explained in Section IID above, $\widehat{\Delta\Delta\,\sigma^2(s)} = \sum_{j=2}^{4} \widehat{c_{j\sigma^2}} . 1_{\lambda_s \in Q_j}$: the estimated coefficient when the dependent variable is the variance of log-MRPK is the empirical estimate of $\Delta\Delta\,\sigma^2(s)$, one of the sufficient statistics we use in our counterfactual analysis. The same inference applies for the two other sufficient statistics—$\widehat{\Delta\Delta\mu}$

[12] In Table O.A.1 of the online Appendix, we show our results are similar if we trim this ratio at the 1 percent level instead, or if we use the gross value of asset to measure firms' capital stock.

Table 2—Summary Statistics

| | Mean (1) | SD (2) | p10 (3) | p90 (4) | Observations (5) |
|---|---|---|---|---|---|
| Exposure ($\lambda_s$) | 0.20 | 0.08 | 0.11 | 0.28 | 7,917 |
| $\sigma^2$ | 0.50 | 0.56 | 0.09 | 1.17 | 7,917 |
| $\mu$ | −0.24 | 0.51 | −0.83 | 0.34 | 7,917 |
| $\sigma_{lMRPK,\log(py)}$ | 0.23 | 0.56 | −0.17 | 0.78 | 7,917 |

*Notes:* The sample period is 1976 to 1994. The sample corresponds to 493 unique industries $s$. Exposure $\lambda_s$ corresponds to the industry-average ratio of total bank debt over total liabilities in the pre-reform period (1976–1983). $\sigma^2$ is the variance of log-MRPK computed at the industry-year level. $\mu$ is the industry-year mean of log-MRPK. $\sigma_{lMRPK,\log(py)}$ is the covariance of log-MRPK and log-value added computed at the industry-year level.

and $\widehat{\Delta\Delta\,\sigma_{lpy,lMRPK}}$. Importantly, this inference relies on the assumption that industries in the first quartile of treatment intensity are not directly affected by the banking deregulation.

### C. *Empirical Results*

We start with graphical evidence. For each industry $s$ and year $t$, we first compute the within-industry variance of log-MRPK. We then split the sample into high exposure industries (industries with above median bank dependence $\lambda_s$) and low exposure industries ($\lambda_s$ in the bottom half). For these two groups of industries, we compute the average variance of log-MRPK relative to its pre-reform (1976–1983) average. Figure 2 reports the yearly difference between the two groups over time. This difference mimics the difference-in-difference estimator described in equation (12). Figure 2 shows no clear evidence of differential trends between high and low-exposure industries in the period leading up to the reform. We confirm this result through regression analyses.

Table 3 estimates equation (12) using the cross-sectional variance in log-MRPK at the industry-year level, $\sigma_t^2(s)$, as the dependent variable. Columns 1 and 2 use bank exposure $\lambda_s$ linearly (equation 12), while column 3 uses instead quartiles of $\lambda_s$ (equation 13). Columns 2 and 3 control for industry-specific trends, while column 1 does not. Across all three specifications, we find a significant reduction in $\sigma_t^2(s)$ in the post-reform period for the most exposed industries. Columns 3 confirms that the treatment effect is monotonic with industry exposure $\lambda_s$. For industries in the top quartile of exposure, we measure a statistically significant decrease in $\sigma_t^2(s)$ of 0.24 relative to industries in the bottom quartile. These results confirm the analysis in Bertrand, Schoar, and Thesmar (2007). We find evidence of efficient capital reallocation between firms.

Columns 1–3 of Table 4 investigate the effect of the reform on average log-MRPK. A priori, the reform has an ambiguous effect on this moment. On the one hand, increased banking competition should lead to an expansion in credit supply for previously constrained firms. This effect should lower the mean log-MRPK. On the other hand, the reform severely reduced subsidized loans, which restricted credit supply. This effect would lead to an increase in mean log-MRPK. Bertrand, Schoar,

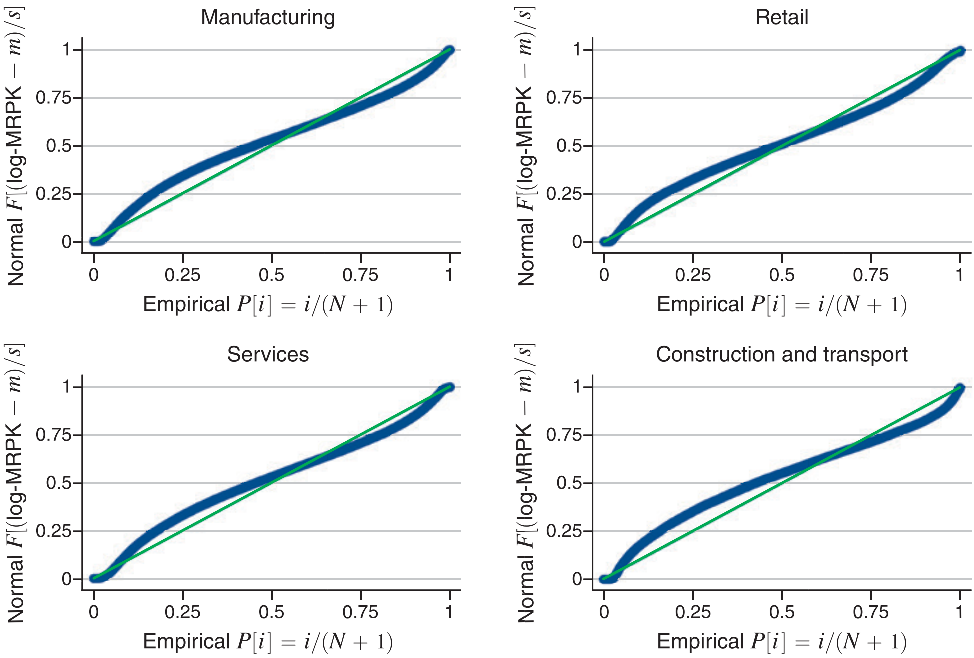


FIGURE 1. LOG-NORMALITY OF MRPKS IN THE DATA

*Notes:* We divide our sample into four broad industries: manufacturing, retail, services, and construction and transport. The figure shows normal probability plots for these four industries for the distribution of log-MRPK. MRPK is computed as the ratio of value added to the net book value of total assets (average between the beginning and end of fiscal year values).

and Thesmar (2007) shows that, on average, the reform did lead to a reduction in corporate leverage. We find a small, marginally significant effect of the reform on the average log-MRPK, which is consistent with Bertrand, Schoar, and Thesmar (2007)'s finding of a reduction in leverage. Column 4–6 report a significant and negative effect of the reform on $\sigma_{MRPK,py}$, the covariance between firm output and lMRPKs. While the average capital wedge increases following the reform, the covariance between output and these wedges becomes smaller. In other words, banking deregulation leads to a reduction in financial constraint for more productive firms.

Our aggregation formula requires that the effect of the experiment is "small," which means that the difference-in-difference estimates are negligible compared to 1. As Table 3 and Table 4 show, this is broadly satisfied in our setting, although the Q4 estimate for the dispersion in log-MRPK ($0.24^{***}$) is in the upper part of the admissible range for a first-order approximation.

Taken together, these results confirm the interpretation in Bertrand, Schoar, and Thesmar (2007): the French banking deregulation of the mid-80s is mostly an experiment in improved capital reallocation. In the next section, we combine these estimates with our aggregation formula to quantify the effect of banking deregulation on aggregate TFP.

TABLE 3—VARIANCE OF LOG-MRPK AND BANKING DEREGULATION

| | Var(log-MRPK) | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Exposure × post | −0.8 | −1 | |
| | (0.2) | (0.32) | |
| Q2 exposure × post | | | −0.08 |
| | | | (0.063) |
| Q3 exposure × post | | | −0.15 |
| | | | (0.061) |
| Q4 exposure × post | | | −0.24 |
| | | | (0.073) |
| Year fixed effects | Yes | Yes | Yes |
| Firm fixed effects | Yes | Yes | Yes |
| Industry-trends | No | Yes | Yes |
| Observations | 7,917 | 7,917 | 7,917 |
| Adj. $R^2$ | 0.55 | 0.59 | 0.59 |

*Notes:* We estimate the following model:

$$X_{st} = \delta_t + \eta_s + b_X . \lambda_s \times POST_t + \mu_s \times t + \epsilon_{st},$$

where $X_{st}$ is the variance of the log-MRPK distribution in industry $s$ and year t. $\delta_t$ is a year fixed effect and $\eta_s$ is an industry fixed effect. $\lambda_s$ is the industry-level measure of exposure to banking deregulation, and $\mu_s \times t$ are industry-specific trends. Finally, $POST_t$ is a dummy variable for the post-reform period (1985–1992). All columns include year and industry fixed effects. Columns 2 and 3 include industry-specific trends. Standard errors are clustered at the industry level.

## D. *The Aggregate Effect of the Reform*

*Implementing the Methodology*.—Our methodology requires to calibrate some parameters. We assume that demand elasticities and capital shares are constant across industries. We use a standard calibration (e.g., David and Venkateswaran 2019): the capital share in production is set to 0.33 and the price elasticity of demand is set so that $\theta \approx 0.85$. This price elasticity of approximately 6 is roughly in the middle of the range of values used in the literature. We compute $\phi_s$ and $\kappa_s$, the pre-reform share of industry $s$ in total sales and capital directly in our firm-level dataset. For our quantification exercise, we use the "quartile of exposure" specification equation (13) (i.e., column 3 in Table 3 and columns 3 and 6 in Table 4). For industries in the fourth quartile of treatment exposure (resp. second and third), we estimate the following treatment effects: $\widehat{\Delta\Delta\sigma^2(s)} = -0.24$ (resp. $-0.08$ and $-0.15$), $\widehat{\Delta\Delta\mu(s)} = 0.1$ (resp. 0.02 and 0.02), and $\widehat{\Delta\Delta\sigma_{MRPK,py}(s)} = -0.22$ (resp. 0.03 and $-0.07$).

Theorem 1 (and our identifying assumption) ensures that these estimates correspond to the counterfactual change in the statistics that would have been observed if banking deregulation was the unique source of change in the economy. As a

Table 4—Mean log-MRPK, Covariance of log-MRPK and log-VA, and Banking Deregulation

| | Mean(log-MRPK) | | | Cov(log-MRPK,log-VA) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Exposure × post | 0.23 | 0.42 | | −0.73 | −0.8 | |
| | (0.19) | (0.2) | | (0.21) | (0.38) | |
| Q2 exposure × post | | | 0.016 | | | −0.036 |
| | | | (0.035) | | | (0.073) |
| Q3 exposure × post | | | 0.02 | | | −0.073 |
| | | | (0.034) | | | (0.076) |
| Q4 exposure × post | | | 0.1 | | | −0.22 |
| | | | (0.038) | | | (0.093) |
| Observations | 7,920 | 7,920 | 7,920 | 7,917 | 7,917 | 7,917 |
| Adj. $R^2$ | 0.86 | 0.89 | 0.89 | 0.32 | 0.41 | 0.41 |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-trends | No | Yes | Yes | No | Yes | Yes |

*Notes:* We estimate the following model:

$$X_{st} = \delta_t + \eta_s + b_X . \lambda_s \times POST_t + \mu_s \times t + \epsilon_{st},$$

where $X_{st}$ is the mean of the log-MRPK distribution in industry $s$ and year t (column 1–3) and the covariance of log-MRPK and log value added (column 4–6). $\delta_t$ is a year fixed effect and $\eta_s$ is an industry fixed effect. $\lambda_s$ is the industry-level measure of exposure to banking deregulation, and $\mu_s \times t$ are industry-specific trends. Finally, $POST_t$ is a dummy variable for the post-reform period (1985–1992). All columns include year and industry fixed effects. Columns 2, 3, 5, and 6 include industry-specific trends. Standard errors are clustered at the industry level.

result, they can be used in formula (9) to obtain the counterfactual change in aggregate TFP:

$$\Delta \log(TFP) \approx \underbrace{-\frac{\alpha}{2}\left(1+\frac{\alpha\theta}{1-\theta}\right)\sum_{s=1}^{S}\kappa_s\widehat{\Delta\Delta\,\sigma^2(s)}}_{5.5\%}$$

$$\underbrace{-\frac{\alpha}{2}\left(1+\frac{\alpha\theta}{1-\theta}\right)\sum_{s=1}^{S}(\phi_s-\kappa_s)\left(\widehat{\Delta\Delta\mu(s)}+\widehat{\Delta\Delta\,\sigma_{MRPK,py}}(s)+\frac{1}{2}\frac{\alpha\theta}{1-\theta}\widehat{\Delta\Delta\,\sigma^2(s)}\right)}_{-0.5\%}.$$

$$\approx 5.0\%$$

Our estimation of reallocation gains from banking deregulation are large: TFP increases by 5.0 percent. This corresponds to slightly more than one-half the aggregate TFP growth over the post-reform period (8.8 percent between 1976–1983 and 1985–1994).[13] The gains from within-sector reallocation contribute to 5.5 percent of increased TFP over this period. Cross-industry production reallocation lowers the overall TFP gain by −0.5 percent since the reform leads to an increase in production in industries that are relatively more distorted in the pre-reform economy.

[13] While large, this increase is inconsistent with other estimates from the literature (e.g., Midrigan and Xu 2014; Catherine et al. 2022; Bau and Matray 2022). For instance. Bau and Matray (2022) find that the Indian capital liberalization episode increased aggregate productivity of the Indian manufacturing sector by at least 6.5 percent.

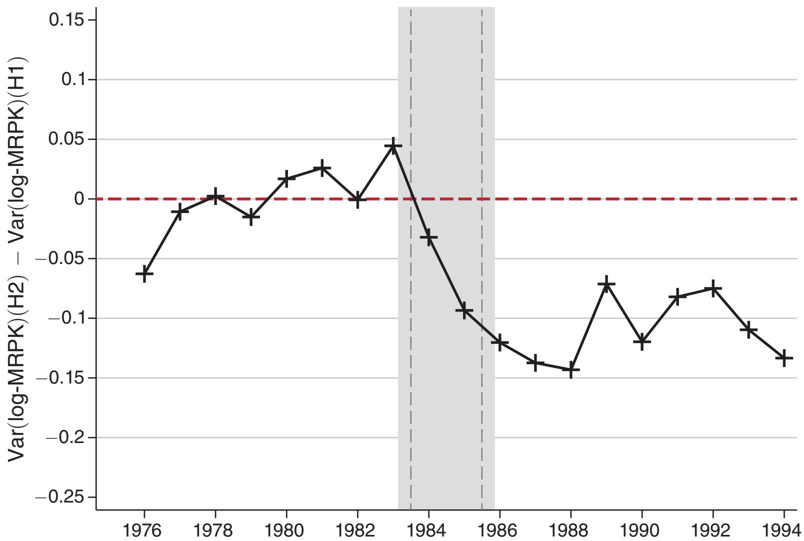


Figure 2. Variance log-MRPK Distribution and Banking Deregulation

*Notes:* For each four-digit industry-year in our sample, we compute the variance of log-MRPK. MRPK is computed as the ratio of value added to the net book value of total assets (average between the beginning and end of fiscal year values). We split the sample into two groups: exposed industries (industries with exposure $\lambda_s$ above the median) and low-exposure industries ($\lambda_s$ in the bottom half). Every year, we compute the average of the variance of log-MRPK for both group and difference out the group's pre-reform (1976–1983) average. The figure shows the yearly difference between the highly exposed group and the low-exposure group.

We can also compute the counterfactual gain in aggregate output, using equation (11):

$$\Delta \log Y \approx -\frac{\alpha(1+\epsilon)}{1-\alpha}\sum_{s=1}^{S}\phi_s\Big(\widehat{\Delta\Delta\mu}(s) + \frac{1}{2}\frac{\alpha\theta}{1-\theta}\widehat{\Delta\Delta\,\sigma^2}(s) + \widehat{\Delta\Delta\,\sigma_{MRPK,py}}(s)\Big) \approx 10.8\%.$$

The gains to aggregate output in this experiment come first from the within-industry reallocation gains documented above. While the average capital wedge in treated industries increases—banking deregulation leads to a higher average cost of capital—this effect is dominated by the significant reduction in the correlation between capital wedges and productivity. This latter effect amplifies the output gains from banking deregulation as, on average, more productive firms tend to be subject to fewer distortions. In terms of aggregate output, these reallocation gains are amplified in general equilibrium: labor supply responds endogenously to the increased wage on the labor market, which further increases aggregate output.

It is interesting to compare our aggregation result with a "naïve" approach that linearly aggregates empirical estimates. A typical approach in the empirical literature would simply estimate the effect of the reform on firm-level log-output using a similar identification strategy. In this naïve approach, the resulting treatment effect on log-output corresponds to the counterfactual aggregate effect of the reform on

Table 5—Banking Deregulation and log Output

| | Average log$(py)$ | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Exposure $\times$ post | $-0.066$<br>(0.28) | $-0.14$<br>(0.27) | |
| Q2 Exposure $\times$ post | | | 0.046<br>(0.039) |
| Q3 Exposure $\times$ post | | | 0.028<br>(0.043) |
| Q4 Exposure $\times$ post | | | 0.028<br>(0.047) |
| Year fixed effects | Yes | Yes | Yes |
| Firm fixed effects | Yes | Yes | Yes |
| Industry-trends | No | Yes | Yes |
| Observations | 349,186 | 349,186 | 349,186 |
| Adj. $R^2$ | 0.29 | 0.30 | 0.30 |

*Notes:* We estimate the following model:

$$\log\left(py\right)_{ist} = \delta_t + \eta_s + b_X . Z_s \times POST_t + \mu_s \times t + \epsilon_{ist},$$

where $\log\left(py\right)_{ist}$ is the log value added of firm i in industry $s$ in year t. $\delta_t$ is a year fixed effect and $\eta_s$ is an industry fixed effect. $Z_s$ is the industry-level measure of exposure to banking deregulation, and $\mu_s \times t$ are industry-specific trends. Finally, $POST_t$ is a dummy variable for the post-reform period (1985–1992). All columns include year and industry fixed effects. Columns 2 and 3 include industry-specific trends. Standard errors are clustered at the industry level.

log value-added. Such an approach is erroneous because it fails to account for both the general equilibrium and allocative effects induced by the reform and described above.

In Table 5, we estimate equation (12) using firm-level log-sales as a dependent variable. We find that the reform does not affect significantly firm-level output. A naïve aggregation approach relying solely on this regression would therefore conclude that banking deregulation did not increase aggregate output. Again, such an approach would be misguided as it fails to account separately for the effect of banking deregulation on allocative efficiency and for general equilibrium effects. To evaluate quantitatively how the reform reallocates resources in the economy, an aggregation model is necessary, in particular some structure on (i) production technology and (ii) competition in product and input markets. This is what our aggregation model does, and why our approach allows us to estimate the allocative efficiency gains that result from the quasi-natural experiment we study.

## IV. Conclusion

Quantifying input misallocation has become an active topic of research in macroeconomics. In their seminal paper, Hsieh and Klenow (2009) compare the dispersion in log-marginal revenue products across countries to estimate the importance of input misallocation in India and China. This approach—inferring misallocation from dispersion in productivities in microeconomic data—has become standard in the literature. There are, however, well-known limitations to this methodology. For instance, measurement errors in inputs or adjustment costs can create cross-sectional variations in observed marginal products even when resources are efficiently allocated. This approach is also silent on the particular frictions that generate misallocation and the potential policies that may improve allocative efficiency. In contrast, a large literature in applied microeconomics exploits (quasi-) experimental settings to estimate the causal effect on firm-level outcomes of policies that have the potential to reduce misallocation. Yet, this literature has mostly evaluated their microeconomic effect without quantifying how they affect allocative efficiency.

Our paper offers a simple methodology to provide such quantification in a standard quasi-experimental setting. Our methodology is consistent with a large class of models of firm dynamics but does not require the structural estimation of a particular model. In particular, we do not have to make specific parametric assumptions on the nature of distortions, nor do we need to precisely map the policy change to the model's structural parameters. Our approach thus provides a simple way to measure gains from reallocation that can be achieved with actual policies, implemented in practice.

We apply our methodology to the French banking deregulation episode of the 1980s, previously analyzed in Bertrand, Schoar, and Thesmar (2007). While Bertrand, Schoar, and Thesmar (2007) show that the banking reform led to significant capital reallocation, their analysis does not quantify the effect of this reform on aggregate efficiency. Applying our methodology, we find that the banking deregulation led to an increase in aggregate TFP of about 5.0 percent in the post-reform period.

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