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Essays in Empirical Macroeconomics

by

Sebastian Stumpner

A dissertation submitted in partial satisfaction of the
requirements for the degree of
Doctor of Philosophy

in

Economics

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Yuriy Gorodnichenko, Co-Chair
Professor Atif Mian, Co-Chair
Professor Ross Levine
Professor Edward Miguel
Professor Andres Rodriguez-Clare

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Abstract

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Doctor of Philosophy in Economics

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Professor Yuriy Gorodnichenko, Co-Chair
Professor Atif Mian, Co-Chair

This dissertation consists of two chapters which study questions at the intersection of macroeconomics, trade, and finance. The first chapter investigates the role of trade for the geographic spread of the 2007-09 recession within the U.S.. The second chapter, co-authored with Mauricio Larrain, studies the role of financial market reforms for changes in aggregate productivity, using the example of Eastern European countries in the late 1990s and early 2000s.

In the first chapter, I use the large spatial variation in consumer demand shocks at the onset of the Great Recession to study the mechanisms behind the ensuing geographic spread of the crisis. While the initial increase in unemployment was concentrated in areas with housing busts, subsequently unemployment slowly spread across space. By 2009, it was above pre-crisis levels in almost all U.S. counties. I show that trade was an important driver of this geographic spread of the crisis. To identify the trade channel empirically, I make use of heterogeneity in the direction of trade flows across industries in the same state: Industries that sold relatively more to states with housing boom-bust cycles grew by more before the crisis and declined faster from 2007-09. These results cannot be explained by a collapse in credit supply. I then link the reduced form empirical evidence to a formal model of contagion through trade. In a quantitative exercise, the model delivers a cross-sectional effect of similar magnitude as the one found empirically and reveals that the trade channel can explain roughly a third of the overall spread.

The second chapter analyzes the microeconomic channels by which financial sector reforms affect aggregate productivity. We use a large firm-level dataset to study the episode of financial market liberalization in 10 Eastern European countries starting in the late 1990s. We exploit cross-sectoral differences in external financial dependence and find that financial reform increases productivity disproportionately in industries heavily dependent on external finance. We show that this productivity increase is driven entirely by improvements in the within-industry allocation of resources across firms, as opposed to within-firm productivity improvements. According to our results, reform allows financially-constrained firms to take on new debt, increase market share, and produce closer to optimal level. A back-of-the-envelope calculation suggests that financial reform increases aggregate manufacturing productivity by 17%. Our results highlight financial markets' key role in improving the within-industry allocation of capital.

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Chapter 1

Trade and the Geographic Spread of the Great Recession

1.1 Introduction

While the initial increase in unemployment during the Great Recession was concentrated in areas with housing busts, subsequently unemployment slowly spread across space. By 2009, it was above pre-crisis levels in almost all U.S. counties. Figure 1.1 maps this "geographic spread" of the crisis. How did local shocks diffuse through the economy, causing business cycle co-movement across U.S. states?

I argue that trade across U.S. states can explain a substantial fraction of the spread of the crisis across space. To the extent that producers of tradable goods across the U.S. depend on markets experiencing a housing bust and consumption collapse, they face a shock to their market size. I empirically trace the effect of these demand shocks through the trade network that connects U.S. states at the industry level. I then link the reduced form empirical evidence to a formal model of contagion through trade. In a simulation exercise, the model delivers a cross-sectional effect of similar magnitude as the one found empirically and reveals that the trade channel can explain roughly a third of the overall spread.

I exploit differences in trading patterns across industries that are located in the same state to separate the trade channel from other potential contagion mechanisms. Within a state, industries differ in their shocks to market size to the extent that they depend on markets experiencing a consumption collapse. The empirical approach relies on the identification assumption that industries that sell *relatively* more to states experiencing a housing bust are not *relatively* more affected by other shocks. Thus, other potentially confounding shocks (such as credit supply shocks or expectation shocks) are controlled for to the extent that they do not affect industries differentially in a way that is correlated with the direction of trade flows.

My empirical analysis finds a sizable role for trade in the transmission of the crisis: First, I find that a one standard deviation in the variable measuring exposure to demand shocks causes a 3 percentage point difference in 2007-09 employment growth, which corresponds to 20% of the total dispersion in employment growth among tradable producers. This result is robust to focusing only on variation in trade flows that arises from different transportation costs across industries. Moreover, it is specific to trade flows *to*, but not trade flows *from* highly levered states. Second, to learn about the role of trade in the transmission of business cycles more generally, I study the dynamic evolution of the industries over a longer (10 year) horizon. I find that industries selling particularly to high-leverage states were booming before the crisis, thus benefitting from the housing and consumption boom in these states. This pattern reverses with the beginning of the recession in 2007 and reaches a low in 2009 (when the national unemployment rate peaked). With the recovery starting in 2009, the differential effect across industries again slowly converges back to zero. I thus find evidence that strongly supports the view that trade in goods is important for linking fluctuations across states.

An additional test based on industry heterogeneity in product differentiation further supports the main empirical results. A standard Armington model with heterogeneity in product differentiation across industries predicts that the reduction in employment following a shock to market size is stronger for industries producing more differentiated goods. The reason is that a higher degree of product differentiation results in a lower trade elasticity, i.e. a lower sensitivity of trade flows to costs: Industries producing more homogeneous output can more easily offset a shock to a particular market by increasing their market share at other destinations. I find precisely this pattern of differential adjustment in the data, which further supports the main empirical results on the trade channel.

The results on the trade channel cannot be explained by a credit supply shock. An adverse shock to credit supply may be viewed as the main competing mechanism for the trade channel in spreading the crisis: Due to the collapse in their asset values and troubles in the interbank lending market, commercial banks may have cut their credit supply to businesses. I construct a variable measuring the credit supply shock at the county level, making use of heterogeneity in pre-crisis bank health and in bank market shares across counties. I find that the effect of the trade channel remains unchanged when the credit supply shock is controlled for.

Finally, a model of crisis diffusion through trade allows me to assess general equilibrium effects and to gauge the aggregate contribution of trade to the spread of the crisis. While the reduced-form estimates can econometrically identify the role of the trade channel, they only inform me about the *relative* magnitude of the shock across industries. To assess the overall contribution of trade to the spread of the crisis, I build a model that captures the geographic diffusion of the crisis through trade. The model is built so that it maps directly into the states and industries observed in the empirical part. I compute the model's response to the expenditure shocks at the state level that are observed empirically, and then re-run the same regressions on the model-implied values. The model yields coefficients that are of similar magnitude to the ones found empirically. I then use the model to ask questions that the empirical part cannot answer: I identify general equilibrium channels that determine the model's response, and compute the share of the crisis spread that the trade channel can account for. I define the total spread of the crisis in the model as the deviation in growth at the state level between the data and the growth that would have prevailed if each state was a closed economy. The *spread of the crisis* refers to the redistribution of the crisis across states (as opposed to amplification). Using a within-model comparison, I compute how much closer the trade model gets to the data, compared to the closed economy. This share of trade is roughly a third.

This project relates to several strands of literature. First, it relates to the literature that highlights the role of demand shocks as the main trigger of the recession. On the theory side, Midrigan and Philippon (2011) [44] and Eggertsson and Krugman (2012) [17] provide models of the recession driven by a collapse in aggregate demand. On the empirical side, Mian et al. (2011) [41], using micro consumption data, document the fall in consumer demand as a result of the housing bust. Mian and Sufi (2011) [43] show that this collapse in consumption was the main driver of unemployment in cities that experienced housing boom and bust cycles. I use the demand shocks identified by these authors and trace their effect through the within-U.S. trade network.

Second, this paper is closely connected to the literature on trade, volatility, and business cycle comovement. Frankel and Rose (1998) [19] were the first to highlight that countries that trade more with each other tend to have more correlated economic outcomes. Following this work, several papers have investigated the relationship between trade openness and volatility and trade openness and output comovement. Typically, this literature has focused on the statistical association between trade openness and the variance of output growth or the correlation of output growth across countries.[1] My paper complements this literature by studying the diffusion of a specific shock across space through the trade channel.

Finally, this work is related to the literature on the contagion of crises, such as van Rijckeghem and Weder (2001) [55], Glick and Rose (1999) [24], and Kaminsky and Reinhart (2000) [34]. This line of literature has mostly focused on estimating the channels of contagion *across* countries using aggregate data on cross-country financial and trade linkages. In contrast, I focus entirely on within-country contagion, and use industry heterogeneity in the direction of trade flows to identify the trade channel.

1.2 Empirical Strategy

My identification approach exploits within-state across-industry heterogeneity in exposure to demand shocks. To do so, I first define a measure of exposure to demand shocks through trade as motivated by a general expenditure system. I use differences in trading patterns across industries in the same state to identify which industries should be *relatively* more affected by demand shocks. This industry heterogeneity allows me to separate the trade channel from potentially confounding mechanisms.

[1] Recent examples are Di Giovanni and Levchenko (2009) [23] who study the relationship between trade openness and volatility in a panel of countries and industries, and Di Giovanni and Levchenko (2010) [22] who highlight the role of vertical linkages between industries for cross-country economic comovement.

Consider the problem of measuring the spread of consumer demand shocks through a trade network. In principle, these demand shocks will entail direct effects on producers through lower demand, and indirect effects through general equilibrium changes in wages and prices.[2]

For the empirical part, I derive a measure of exposure to demand shocks by focusing only on direct effects, similar to Autor et al. (2013) [5]. This has the advantage that I can avoid making specific modeling assumptions that the derivation of general equilibrium effects would require. It does not assume, however, that general equilibrium effects are absent. The strategy is to derive a measure of exposure to demand shocks, and then to analyze the adjustment at the state-industry level. I defer a model-based discussion of general equilibrium effects and their role for the empirical estimates to a later section of the paper.

I consider the spending side of a simple Armington trade model with N states and S industries. Each state n produces only one distinct variety in each industry, and consumes an aggregate of goods from all industries:

$$C_n = \prod_k \left(C_n^k\right)^{\alpha^k},$$

where C_n is aggregate consumption in state n and C_n^k is consumption by state n of varieties in industry k. This leads to constant expenditure shares across all industries, $X_n^k = \alpha^k X_n$, and unit income elasticities for all goods. The real consumption by state n of the good produced by state i in industry k is denoted by c_{ni}^k with price p_{ni}^k. Total consumption of the good in industry k by state n is a CES aggregate of the varieties coming from all states:[3]

$$C_n^k = \left(\sum_i \left(c_{ni}^k\right)^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}}$$

The price index for industry k in state n is denoted P_n^k. Prices differ across states due to trade costs. Expenditure by state n on the variety produced by state i in industry k is then a function of total expenditure in state n.

$$X_{ni}^k = \left(\frac{P_n^k}{p_{ni}^k}\right)^{\sigma-1} \alpha^k X_n$$

Total sales by state i, industry k, can then be written as a function of expenditure at all destinations:

$$Y_i^k = \sum_n X_{ni}^k = \sum_n \left(\frac{P_n^k}{p_{ni}^k}\right)^{\sigma-1} \alpha^k X_n$$

[2] In general equilibrium, expenditure shocks may affect wages (and therefore prices) through shifts of both the labor demand and the labor supply curve.

[3] This expenditure system could be generalized to include state-industry specific expenditure shares α_n^k, a state-industry specific elasticity of substitution σ_n^k and preference parameters ω_{ni}^k to accommodate, for instance, home bias in goods. This generalization would not affect equation 1.1.

The idea is to capture the shocks to household balance sheets in states with high leverage and strong house price declines as shocks to consumer expenditure. In terms of the model, these are shocks to X_n. Considering only the direct effect of these shocks, changes in production at the origin state i in industry k can be written as follows.

$$\mathrm{d}\log(Y_i^k) = \sum_n \frac{X_{ni}^k}{Y_i^k}\mathrm{d}\log(X_n) \tag{1.1}$$

In general equilibrium, growth of industry sales and consumer expenditure are jointly determined. In a regression framework, this leads to a simultaneity problem. In order to estimate the role of trade in transmitting the crisis, I need exogenous variation in expenditure growth. Work by Mian et al. (2011) [41] has shown that pre-crisis household leverage had a strong effect on expenditure growth during the recession. In 2006, there were large differences in household leverage across U.S. states. A large part of these differences were due to differences in leverage growth from 2002 to 2006. Strong house price growth in some U.S. states during that time led to a buildup of leverage, driven to a large extent by home equity withdrawals (Mian and Sufi (2011) [42]). At the peak of the housing bubble in 2006, states with rapid house price growth during 2002-06 also tended to have the most highly indebted households. With the reversal of house prices starting in 2006, highly levered households experienced significantly lower expenditure growth, compared to households with low leverage (Mian et al. (2011) [41]). I therefore use pre-crisis household leverage as an *initial* shock to expenditure growth, which is not subject to simultaneity concerns.

I define the trade demand shock at the state i, industry k level as follows:

$$\mathrm{TDS}_i^k = \sum_{n=1}^{N} \frac{X_{ni}^k}{Y_i^k}\mathrm{Lev}_n$$

TDS is the weighted sum of destination-state pre-crisis household leverage, where the weights are given by outgoing trade shares.[4]

I estimate the reduced form effect of the trade demand shock variable on industry outcomes.[5] In particular, I consider the following specification:

$$\mathrm{d}\log(Y_i^k) = \beta_0 + \beta_1\mathrm{TDS}_i^k + \gamma_i + \alpha^k + \epsilon_i^k \tag{1.2}$$

where Y stands for employment, earnings, or the average wage.[6] By adding a state fixed effect γ_i, the estimation makes use of differences in trading patterns across industries within

[4] I aggregate county-level leverage ratios to the state-level using the number of households in a county as weights. The fraction of total shipments in industry k from state i to destination n, $\frac{X_{ni}^k}{Y_i^k}$, is observed from shipments data detailed in the next section.

[5] I only present results for the reduced form, because no good data for expenditure growth at the state level are available.

[6] Since I do not observe industry sales, I use changes in log employment and wage payments as dependent variables. In the case of a Cobb-Douglas production function and constant markups, the percentage change in wage payments equals the percentage change in sales.

a state. The industry fixed effect α^k controls for shocks that hit all producers in a specific industry.

The main endogeneity concern for the estimation of equation 1.2 is potential omitted variable bias: In addition to a demand shock that is transmitted through trade, a particular region (i.e. a county or a state) may be subject to other shocks that also affect economic outcomes. If these shocks are unobserved and correlated with the trade demand shock, the estimated coefficient of interest will be inconsistent. For instance, employment may decline due to a contraction in local credit supply, or due to expectation shocks. If regions that trade more with high-leverage states are also subject to larger credit supply shocks or stronger adverse expectation shocks, then an approach that only links regional employment growth to a measure of exposure to demand shocks through trade is problematic.

The idea of the identification strategy is to make use of *cross-industry heterogeneity* in the exposure to demand shocks through trade: Within a state, industries that trade *relatively* more with high-leverage states should experience a stronger economic decline. The identification assumption is that these industries are not *relatively* more affected by other shocks. That is, within a state, any unobserved shocks that affect state-industry level outcomes are not higher (or lower) for industries that sell relatively more to high-leverage states pre-crisis.[7]

The validity of the identification assumption depends on what drives the variation in the measure of trade demand shocks. To put it differently, why do trading patterns differ across industries in the same state? If the source of this heterogeneity is plausibly exogenous to other shocks, the identification assumption is valid.

Figures 1.2 and 1.3 show that distance plays an important role in intra-U.S. trade and that the effect of distance varies systematically across industries. Figure 1.2 plots aggregate (i.e. state-to-state) trade flows against distance, and shows that, on average, states that are further apart trade significantly less. However, the aggregate effect of distance hides substantial variation at the industry level: Figure 1.3 plots the average distance (in miles) traveled by shipments in an industry against the log of the value-to-weight ratio, a common (inverse) measure of transportation costs.[8] Industries with higher transportation costs (i.e. lower

[7] In mathematical terms, consider the framework

$$y_{si} = \beta_0 + \beta_1 x_{si} + \alpha_s + \gamma_i + \epsilon_{si}$$

where α_s and γ_i are fixed effects and $\epsilon_{si} = z_{si} + \nu_{si}$. z_{si} is an omitted variable and ν_{si} is an i.i.d. error term with mean zero. The transformed model is:

$$\tilde{y}_{si} = \beta_0 + \beta_1 \tilde{x}_{si} + \tilde{z}_{si} + \tilde{\nu}_{si}.$$

Taking the conditional expectation:

$$E[\tilde{y}_{si}|\tilde{x}_{si}] = \beta_0 + \beta_1 \tilde{x}_{si} + E[\tilde{z}_{si}|\tilde{x}_{si}]$$

since $E[\tilde{\nu}_{si}|\tilde{x}_{si}] = 0$. The identification assumption is that $E[\tilde{z}_{si}|\tilde{x}_{si}] = 0$. In words: Within a state, industries that ship relatively more to high-leverage states are not more affected by another shock than other industries in the same state.

[8] I construct it by dividing the total value of all shipments in an industry by the overall tonnage of these

value-to-weight ratio) trade over much shorter distances. The effect of transportation costs on trade thus implies that in high-leverage states, industries with lower transportation costs should experience a lower demand shock (compared to industries with high transport costs), because they ship more out-of-state. Similarly, in low leverage states, they should be *relatively more* affected than high-transportation cost industries. For other shocks (like credit supply or expectation shocks) to play a confounding role, they would have to imply similar cross-sectional effects. Since transportation costs are arguably a *technological* characteristic of an industry, there is no immediate reason to believe that other shocks would cause a similar cross-sectional pattern.

One may worry that industries receive financing from their destination states. If industries that trade more with high-leverage states also receive more financing from these states, and credit supply shocks were larger among banks in high-leverage states, the estimated coefficient of interest would be inconsistent. However, in practice this is unlikely to be a major concern. While the distance between lenders and borrowers has generally increased over the last decades, Brevoort et al. (2010) [9] find that, in 2003, the median distance between a firm and its bank was still only 11 miles.

One last check on the identification strategy is to make sure that there is significant variation in the trade demand shock variable. This requires not only variation in trading patterns across industries in the same state, but also variation in demand shocks at the state-level. [43] use variation in household leverage at the *county* level to identify the effect of local demand shocks on local employment. This variation in county leverage is to a large degree driven by variation across states: A regression of county leverage on a set of fixed effects delivers an R^2 of 48% in an unweighted regression (72% if weighted by the number of households). Across states, household leverage ranges from 1.15 in Mississippi to 3.08 in California, thus providing substantial variation of demand shocks across states.

Figure 1.4 visualizes this variation by plotting the 5th, 25th, 75th, and 95th percentile of the trade demand shock variable within each state. The horizontal axis sorts states by 2006 HH leverage, and the boxes show the range of the trade demand shock across industries within that state. On average, industries in states with higher leverage face higher demand shocks. This is a natural consequence of the home bias in shipments: A sizable part of shipments stays within the same state. However, this variation across states will not be used in the main set of regressions, since it is controlled for by a state fixed effect. Instead, I use the variation across industries within a state, which is represented in the graph by the vertical spread of the boxes.

1.3 Data and Summary Statistics

This section describes the data used in this study and provides basic summary statistics. I put a particular emphasis on the trade flow data. These data allow me to discern heterogeneity

shipments.

in exposure to demand shocks across industries based on the direction of trade flows. I then give a brief overview of other datasets and discuss summary statistics.

Trade Flow Data

To measure trade flows across states, I turn to the Commodity Flow Survey (CFS) that is administered by the U.S. Census Bureau. Since the CFS is only conducted every five years together with the Economic Census, the latest trade flow data available are for 2007. The CFS captures data on shipments originating from selected types of business establishments located in the 50 states and the District of Columbia. The survey reports the value of shipments, where the value is defined as the net selling value (f.o.b.), exclusive of freight charges and excise taxes. Based on the shipment records of enterprises, the Census Bureau estimates trade flows between regions (states or other CFS defined areas) at the aggregate level and at the level of industries/commodities. To make the trade flows directly comparable to the employment records, I turn to the CFS origin-destination tables at the NAICS industry level.

There are several caveats of the data that require more detailed discussion. First, for some combinations of origin, destination, and industry, the Census Bureau only observes very few shipment records. In case the precision of the estimates is too low, the CFS withholds information and these entries appear as missing values in the data. I use the state-to-state industry-level table in which these missing entries only account for 15% of the value of total measured trade flows.[9] For the empirical implementation, I set these missing entries to zero.[10] For D.C., Alaska, and Hawaii, the table features many missing trade flows, which is why I disregard these states altogether. To further improve on data quality, I discard observations that have less than $25 million worth of total shipments (and which therefore have many missing values). Finally, to arrive at a more homogeneous sample of industries, I focus only on 39 of the 45 NAICS industries covered in the CFS, namely on all manufacturing (21) and all wholesale trade (18) industries.[11]

Next, the CFS data also capture shipments that are related to international trade. In the case of an international destination, the CFS records the U.S. port of exit as domestic destination.[12] Shipments that originated as imports are included to the extent that they are shipped within the U.S. to their final destination from one of the surveyed firms. Unfortunately, the state-to-state trade data from the CFS do not allow me to distinguish between

[9] Total outgoing flows for a state-industry are reported separately. This allows calculating the fraction of total flows that missing values account for.

[10] In a robustness exercise, I impute these missing values based on predicted values from a gravity model, and then re-do the main empirical analysis. I find that the results remain unchanged.

[11] I therefore exclude Mining (NAICS 212), Electronic Shopping and Mail-Order Houses (4541), Warehousing and Storage (4931), Newspaper, Periodical, Book, and Directory Publishers (5111), Fuel Dealers (45431), and Corporate, Subsidiary, and Regional Managing Offices (551114).

[12] The CFS does collect data on exports, but these information are not publicly available in the state-to-state NAICS files.

purely domestic shipments, and shipments to or from other countries. While the U.S. Census collects information on exports in the CFS, these information are not made available in the state-to-state industry table.[13] However, information on exports are made available at a higher level of aggregation. This data reveals that, compared to the total value of shipments, exports are relatively small in value. The total value of all shipments of my sample of industries in the 2007 CFS is $10.2 trillion. In comparison, the total value of exports of these industries in the 2007 CFS is $822 billion, only 8.1% of all shipments.[14]

While the CFS collects data on exports, it does not provide information on whether a shipment originated as an import. However, it is important to note that I only focus on shipments that originate from domestic manufacturing establishments or wholesale traders. According to Bernard et al. (2009) [7], the presence of wholesale and retail trade firms is more important in U.S. imports than it is in exports.[15] Wholesale traders are likely to reduce their employment when demand at their destinations dries up, but it should not matter whether the shipped products were originally imported or not. For wholesale traders, it is therefore desirable to include shipments that were imported. In contrast, manufacturers are more likely to use imported products as inputs for production (instead of resale). Imported products would then pose a problem, if they result in a domestic shipment that originates from a manufacturing firm. For instance, a manufacturing plant could ship the imported product to a downstream plant of the same firm. However, as Atalay et al. (2012) [4] note, this kind of shipments is very rare in the U.S.. It then seems more likely that manufacturers use imported products directly in the plants in which the shipments arrive. In this case, the shipments would not be part of my data. It is therefore highly unlikely that imports have a significant influence on the empirical results.

Other Datasets

Annual data for employment and wage payments of industries at the state and county level come from the U.S. Census County Business Patterns (CBP). For monthly data on employment of industries I turn to the publicly available files from the BLS Quarterly Census of Employment and Wages (QCEW). The trade data from the CFS come in a more aggregated format in terms on industries, and therefore determine the level of my analysis. Finally, I will use data on county-level household leverage from the Federal Reserve Bank of New York Consumer Credit Panel.

Summary Statistics

Table 1.1 shows summary statistics of employment growth and the trade demand shock variable at the state-industry level for the sample of manufacturing and wholesale trade

[13] In contrast, information on a potential foreign origin of a shipment are not collected in the CFS.

[14] Focusing only on manufacturing industries, this ratio is 10.3%.

[15] According to Bernard et al. (2009) [7], wholesale traders account for 63% of the number of importing firms and 27% of the total value of imports (vs. 10% of the value of exports).

industries. On average, employment fell by 8% between 2007 and 2009, with substantial heterogeneity across industries. Moreover, variation in employment growth accounts for most of the variation in changes of the total wage bill. Table 1.2 shows summary statistics of the level of employment in 2007, and the level of the total wage bill (in million $) and the average wage.

Figure 1.5 gives a slightly different view of the expenditure imbalances across states at the beginning of the crisis. It shows the state-level trade deficit (calculated from the CFS) against HH leverage: The higher expenditure of high-leverage-states are mirrored in their sizable trade deficits against other states.

Although the crisis started with demand shocks in high-leverage states, by 2009 unemployment was on the rise almost everywhere across the U.S. If trade is an important factor for the geographic spread of the crisis, we would expect that tradable industries account for a sizeable share of the jobs lost in low-leverage states. Figure 1.6 plots the jobs lost in manufacturing (2007-09) as a share of the total jobs lost in that state against the pre-crisis household leverage. It shows that manufacturing alone accounts for a large fraction of the jobs lost in low-leverage states, in some states even over 50%.

1.4 Trade Channel Results

This section shows empirically that the trade channel was important for spreading the crisis across space. First I show that industries that were relatively more dependent on markets with housing boom-bust cycles declined by more during the crisis period. Second, this comovement is not restricted to the crisis period. The same industries tended to grow relatively faster during the housing boom period preceding the recession. This result establishes empirically that, more generally, trade links business cycles across space.

The main results hold in several robustness checks. Most importantly, these patterns are specific to trade flows *to*, but not trade flows *from* states with housing bubbles. They are also robust to restricting the variation in trade flows to the part that can be explained by distance and different transportation costs across industries.

The Trade Channel During the Crisis

I find that industries selling relatively more to states with high household leverage declined by more during the crisis. This effect is more pronounced for employment than for the average wage. It holds for pooling manufacturing and wholesale trade industries, and also when I restrict attention to manufacturing industries only.

I estimate equation 1.2 using growth in employment, earnings, and the average wage (i.e. earnings divided by employment) as left hand side variables.[16] For each dependent variable, I

[16] By definition of the average wage, the coefficients in the employment and wage regressions have to sum up to the coefficient obtained in the earnings regression.

run an unweighted and a weighted least squares specification, using 2007 employment of the state-industry cell as weight. Standard errors are twoway clustered at the state and industry level. If the trade channel is relevant for transmitting shocks, we would expect $\beta_1 < 0$.

Table 1.3 shows a negative and large effect of the trade demand shock on state-industry level employment and earnings. The point estimate reveals that a one standard deviation increase in the trade demand shock causes a reduction in employment growth by approximately 3 percentage points. Given a standard deviation of employment growth of 16 percentage points, this corresponds to almost 20% of a standard deviation. The fall in employment accounts for most of the earnings adjustment (70%-80%), while the remainder is accounted for by the average wage.

The coefficient is considerably larger than the estimates in Mian and Sufi (2011) [43] who look at the relationship between household leverage and local *nontradable* employment growth. These authors report a coefficient of approximately -0.02, which is substantially smaller than my estimate of -0.09. The fact that my estimates are larger in magnitude likely reflects compositional changes in demand, i.e. a shift in spending away from tradable goods. In the context of international trade, these compositional changes have already been noted by Eaton et al. (2011) [15] and Levchenko et al. (2010) [36] and are found to explain a large part of the fall in global trade relative to GDP. My estimates are thus consistent with the findings of these authors.

Results from focusing only on manufacturing industries are similar to the previous set of results (Table 1.4).

The Trade Channel over the Recent Business Cycle

Studying the relationship between trading patterns and industry growth over a longer horizon reveals that trade caused comovement of the entire recent business cycle: Prior to the crisis, consumption grew rapidly in states with a housing boom. This led to faster growth of industries selling predominantly to these states.

To track the recent business cycle, I estimate equation 1.2 for a sequence of rolling windows of two-year employment growth. I use monthly employment data available from the BLS for a better alignment of time with the key events during the Great Recession.[17] I start with the window of Jan 2002 - Jan 2004, and end with Dec 2009 - Dec 2011 to include the evolution of the coefficient during the years of the credit boom, the crisis, and the recovery.

If trade links business cycles across states, then the coefficient should be positive before the crisis. As Mian and Sufi (2011) [42] document, high pre-crisis leverage was largely a result of high growth in leverage during the years 2002-2006. In places with high growth of house prices, homeowners extracted new debt from the rising value of their homes to finance ongoing consumption. We would then expect that industries that sold primarily to states

[17] Since data for wholesale industries are not available for the early years, this exercise is conducted using only data for manufacturing industries.

with high pre-crisis leverage were booming in the pre-crisis period.
Figure 1.7 shows that exactly this pattern holds in the data. It is only in mid-2007 that the coefficient turns negative (roughly one year after the Case-Shiller house price index peaks). Starting in 2009 it slowly reverses and approaches zero during the period of the recovery. By 2011, industries selling primarily to high-leverage states show no difference in growth from the control industries.
Figure 1.7 is also evidence that the results obtained in the previous subsection are not caused by different pre-existing trends. If that was the case, industries selling to high-leverage states would have been growing more slowly even before the crisis. The fact that the coefficient is positive confirms instead the role of trade in transmitting shocks across space.
Finally, the timing of the employment decline in figure 1.7 also suggests that the coefficient does not pick up the decline in international trade. Eaton et al. (2011) [15] document that international trade collapsed particularly in 2008Q4 and 2009Q1, which is clearly after the decline in the coefficient on the trade demand shock variable starts and after it turns negative for the first time.

Robustness

This subsection presents a set of robustness checks, each addressing a specific concern one may have about the previous estimations.

Restricting the Variation in Trade Flows

I start by restricting the source of variation in trade flows. Without a clear idea of what is driving the variation in trade flows across industries in a state, it is hard to discuss the validity of the identification strategy. In the first exercise, I therefore limit the variation in trade flows to the part that can be explained by different transportation costs across industries. I compute predicted trade flows from a structural gravity framework, using state-to-state distance and the industry-specific value-to-weight ratio to measure transportation costs. Using these predicted trade flows, I re-compute the trade demand shock. In a final step, I use the newly constructed variable as an instrument for the trade demand shock and find the main results from the previous section unchanged.
The value-to-weight measure is calculated from the commodity flow survey data as the aggregate value of all shipments of an industry divided by the aggregate tonnage of shipments. Differences in the value to weight ratio are enormous and range from $110 per ton shipped (Nonmetallic Mineral Products) to $71,000 per ton (Computer and Electronic Products). Distance is defined as the great circle distance between the population-weighted centers of two states.
To consistently estimate the effect of trade costs on trade flows, I follow the literature on estimation of gravity equations (e.g. Head and Mayer (2013) [30]). A wide variety of trade models yields an expression for trade flows that can be written in log form as follows:

$$\log(X_{ni}^k) = \log(G) + \log(S_i^k) + \log(M_n^k) + \log(\phi_{ni}^k)$$

In this expression, S_i^k denotes all factors that promote exports of industry k in state i to all destinations, and M_n^k all factors that promote imports. Pinning down the exact expressions for S_i^k and M_n^k would require more structure at this point. Finally, the variable ϕ_{ni}^k captures trade costs, and G is a constant. To consistently estimate the effect of trade costs on trade flows, I employ a fixed effects model which is standard in the gravity literature. To that end, I use exporter-industry and importer-industry fixed effects to control for S_i^k and M_n^k, respectively.

Modeling trade costs using an interaction between distance and the value-to-weight ratio parsimoniously captures the heterogeneous effect of distance on trade flows across industries. More specifically, I model trade costs as

$$\log(\phi_{ni}^k) = \beta \log(\text{Distance}_{ni}) + \delta \log(\text{Distance}_{ni}) \cdot \log(\text{Value-to-weight}^k)$$

To see that this model does a good job at fitting the heterogeneity in trade costs across industries, first consider a nonparametric approach:

$$\log(X_{ni}^k) = \alpha_i^k + \gamma_n^k + \sum_{l=1}^{S} \beta^l \cdot \log(\text{Distance}_{ni}) \cdot d^l + \epsilon_{ni}^k$$

That is, I first estimate the effect of distance on trade flows industry-by-industry, where d_l is a dummy for industry l. The effect of distance is thus allowed to vary by industry. Figure 1.8 plots the β_l coefficients against the log of the value-to-weight ratio of the respective industry.

As expected, the graph shows a positive relationship between the two: Distance is less of a barrier for trade flows in industries with a higher value-to-weight ratio. Moreover, the graph shows that a linear fit does a good job at describing the heterogeneity of the effect of distance across industries. This suggests the following estimation equation:

$$\log(X_{ni}^k) = \alpha_i^k + \gamma_n^k + \beta \log(\text{Distance}_{ni}) + \delta \log(\text{Distance}_{ni}) \cdot \log(\text{Value-to-weight}^k) + \epsilon_{ni}^k$$

Results are in table 1.5. As expected, they show a strong negative effect of distance on trade flows, which is muted in industries with high value-to-weight ratios.

I then re-construct the trade demand shock variable based on predicted trade flows $\hat{X}_{ni}^k$:

$$TDSIV \equiv \sum_n \frac{\hat{X}_{ni}^k}{\sum_n \hat{X}_{ni}^k} Lev_n$$

Table 1.6 shows the first and second stage results. The first stage yields a F-statistic of 45, showing the relevance of the instrument. The second stage yields coefficient estimates that are very similar to the simple least squares estimates.

Out-of-state trade flows

Next, I test specifically for the diffusion of the crisis across state borders. To do so, I define a new variable, which only captures variation in demand shocks through out-of-state trade flows. That is, I define

$$\text{External TDS}_i^k = \sum_{n \neq i} \frac{X_{ni}^k}{\sum_{n \neq i} X_{ni}^k} Lev06_n$$

Just as the variable TDS, this variable is measured in leverage points, since the weights sum up to one. I then re-do the main estimations using this variable as regressor. Results can be seen in table 1.7.[18] The effect is negative and significant for both employment growth and labor income growth. The magnitude of the effect is smaller compared to the effect of the main estimations, using the TDS variable. This is expected, because out-of-state trade flows only account for 52% of all trade flows (average across state-industries), and the effect of out-of-state demand shocks should thus be lower than the effect of all demand shocks.

Reverse Trade Flows

If demand shocks in housing boom and bust states triggered the crisis, then it should be exports *to*, but not imports *from* these states that transmit the crisis across space. I use this idea to run a placebo test, constructing the right-hand-side variable with incoming, instead of outgoing trade flows. If most of bilateral trade is intra-industry (instead of inter-industry), then exports and imports would be highly correlated. As a consequence, the variables using either incoming or outgoing trade flows as weights would yield high correlation. In contrast, the two variables may differ if a substantial part of trade is *inter-industry*.

I thus construct a variable called Reverse-TDS by using incoming instead of outgoing trade flows:

$$\text{Reverse TDS}_i^k = \sum_{n} \frac{X_{in}^k}{\sum_{n} X_{in}^k} Lev06_n$$

That is, Reverse-TDS captures the weighted leverage of shipment *origin* states, where the weights are given by import shares. After controlling for industry and state fixed effects, this variable is only moderately correlated with the original trade demand shock measure. The correlation coefficient is 0.35, thus leaving room to disentangle the effect of these two variables empirically. I use the original variable and the newly constructed one in a joint regression to separate the effects. Table 1.8 presents the results of this exercise. It shows that the coefficient on the original variable is unchanged, while the coefficient on the variable using reverse trade flows is indistinguishable from zero. This suggests that trade-transmitted demand shocks spread the crisis across space.

Internal Trade Share

One concern with the previous set of estimations may be that the variable of interest only captures different exposure of industries to within-state demand shocks. In that case, there

[18] For this exercise I exclude the few state-industry pairs that do not ship across state borders. This explains the small drop in the number of observations.

would be no diffusion of the crisis across state borders, and the crisis would remain local. To test whether this is a concern, I augment specification 1.2 by the internal trade share of a state-industry, and let the effect of the internal trade share on the industry outcome vary by state. Results are in table 1.9. The coefficients on the trade shock remain unchanged. Only standard errors increase, because the rich set of interactions absorbs some of the variation in the data. Overall, the results suggest that exposure to internal shocks does not drive the effect measured in the previous section.

Industry-specific demand shocks

Next, one may worry that the main specification of an industry's exposure to demand shocks is too simplistic. In particular, industries may differ in the intensity of shocks to their particular demand, given a fall in aggregate expenditure at a destination. For instance, this can be caused by different income elasticities of consumers for goods produced by different industries.[19] In this section, I test for robustness of the main empirical results to using alternative measures of the trade demand shock adjusted for heterogeneous exposure.
In the absence of measures of expenditure elasticities at the industry level, I rely on aggregate data on the fall in economic activity for industries. More precisely, I calculate the relative gross growth rate of earnings at the national industry level as $\mu_k = \frac{1+g^k}{1+g}$. The idea is that industries that declined by more nationally (i.e. for which μ_k is low) were more exposed to demand shocks. For a given collapse in total expenditure at a destination (as proxied by leverage), the demand shock should thus be worse for industries that declined by more during the recession. I therefore divide leverage by μ_k to arrive at an industry-level demand shock. The adjusted trade demand shock variable can be written as

$$TDSI_i^k \equiv \frac{TDS_i^k}{\mu_k}$$

Results are presented in table 1.10 and are robust to this adjustment.

Splitting up by Destination

One concern with the previous estimations may be that the effect is driven by only a few destination states, that happen to be high-leverage states. In this section I investigate whether the significant effect on the trade demand shock indeed resembles a *systematic* effect of destination-state leverage, or if it merely reflects shocks to a few destinations that happen to be correlated with leverage. To do so, I split up the main regression by export destination. That is, I generalize equation 1.2 to the following setup:

$$d\log(L_i^k) = \sum_{n=1}^{50} \beta_{1n} ED_{ni}^k + \gamma_i + \alpha_k + \epsilon_i^k \tag{1.3}$$

[19] Mian et al. (2011) [41] present some evidence for this heterogeneity in consumer spending.

where ED_{sid} is defined as export dependence of industry i in state s on destination d. It is simply the industry's share of shipments that go to destination d: $ED^k_{ni} = \frac{X^k_{ni}}{Y^k_i}$. Because these shares sum to one, no constant is included in the regression. Notice that equation 1.3 collapses to equation 1.2 in the special case of $\beta_{1n} = Lev06_n$. I estimate the equation and extract the coefficient estimates $\hat{\beta}_{1n}$. Figure 1.9 plots the estimated coefficients against the state-level household leverage at the destination state. The graph shows a negative relationship, and demonstrates that the coefficient is *systematically* related to state-level household leverage. The slope is roughly in line with the results of the TDS regressions in table 1.3. The estimated coefficients can be interpreted as follows: Comparing two states on the regression line, e.g. Florida ($\beta_{FL} = -0.11$) and Texas($\beta_{TX} = -0.02$), reallocating 10 percent of total shipments (pre-crisis) from Texas to Florida would be associated with a lower employment growth of 0.9 percentage points.

Heterogeneous Effects by Industry

Differentiated vs. Homogeneous Goods

Next, I consider differential effects across manufacturing industries, depending on the type of goods they produce. The main idea of contagion through trade is inherently related to the idea that producers cannot substitute their customers easily. If an industry's output was sold on a single spot market, then heterogeneity in demand shocks across regions would not introduce heterogeneity in producer growth: Producers can substitute costlessly between buyers, and demand shocks are averaged out between producers. I use this intuition to test for the idea that geographic contagion through trade should mainly occur in industries that sell differentiated products. I use Rauch's classification (Rauch (1999) [53]) to distinguish between homogeneous and differentiated goods, as in Nunn (2007) [47]. Rauch classifies products into three categories: Those sold on organized exchange markets, products that are reference-priced, and all other products that are labeled "differentiated". I code these products as 0 for products traded on organized exchanges and reference-priced products, and 1 for differentiated products. I then use product-industry concordance tables to translate this measure into an average degree of differentiation at the level of three-digit NAICS manufacturing industries.[20] The industry measure is lowest for Petroleum and Coal Products (0.12), Primary Metal Manufacturing (0.14) and Paper Manufacturing (0.19), and highest for Transportation Equipment, Furniture, Apparel, and Printing (all with a score of 1). I then consider the following specification:

$$d\log(Y^k_i) = \beta_0 + \beta_1 \cdot TDS^k_i + \beta_2 \cdot TDS^k_i \cdot \text{Diff}^k + \gamma_i + \alpha_k + \epsilon^k_i \tag{1.4}$$

where Diff^k denotes the product differentiation measure and Y^k_i stands for either employment, earnings, or the average wage. Results are in table 1.11. All regressions indicate that the effect is driven by industries producing more differentiated products.

[20] I use both the liberal and the conservative classification of Rauch's measure and find that the results hardly differ.

Durable vs. Nondurable Goods

The size of the effect might also differ between industries producing durable vs. nondurable products. Both Mian et al. (2011) [41] and Eaton et al. (2011) [15] show that expenditure collapsed during the recession particularly for durable goods. This should have differential effects on the employment decline in industries producing durable vs. nondurable products. I use the definitions by the U.S. Census to divide manufacturing industries into durable and nondurable goods producers and look for heterogeneous effects of destination-state household leverage on employment in these industries:

$$d\log(Y_i^k) = \beta_0 + \beta_1 \cdot TDS_i^k + \beta_2 \cdot TDS_i^k \cdot \text{Durable}^k + \gamma_i + \alpha_k + \epsilon_i^k \tag{1.5}$$

where $Durable^k$ is an indicator variable for durable goods manufacturers. Results are in table 1.12, and show that results are roughly 30-50% stronger for industries producing durable goods.

County-level

In this section, I employ a different piece of variation in the data to identify the causal effect of trade relationships on employment outcomes during the crisis. For this, I compare counties within a state by their employment structure. I assume that a given industry in a state has the same trading pattern, regardless of the county in which it is located. I then construct a measure of the trade demand shock at the county-level, whose only variation within-state is coming from differences in specialization patterns across counties:

$$\text{TDSCOUNTY}_c = \sum_k \frac{L_c^k}{L_c^T} \left[\sum_n \frac{X_{nc}^k}{Y_c^k} \text{Lev06}_n \right]$$

This allows me to compare employment growth among counties for tradable and non-tradable industries.[21] To the extent that the demand shock to tradables propagates in the local economy (through lower employment and income), we might also expect effects on employment growth in nontradable industries. I consider the following specification:

$$\mathrm{d}\log(Y_c) = \beta_0 + \beta_1 \cdot \text{TDSCOUNTY}_c + \beta_2 \cdot \text{Leverage}_c + \alpha_s + \epsilon_c \tag{1.6}$$

where $\mathrm{d}\log(Y_c)$ denotes either employment growth in tradable or in nontradable industries and α_s denotes a state fixed effect. Following Mian and Sufi (2011) [43] I also include county-level pre-crisis leverage ratios in the estimation to control for the local demand shock. Tables 1.13 and 1.14 show the results using the CBP data. There is a significant effect of the county-level trade demand shock on employment growth in tradable industries, but no evidence of any further propagation in the local economy.

[21] I define tradable industries as manufacturing and wholesale, and nontradable industries as all other industries except for the few remaining industries that also have recorded shipments in the CFS.

1.5 Credit Channel Strategy and Results

An alternative mechanism for transmitting the crisis across space is the credit channel: Following the housing bust, troubles in national financial markets may have forced banks to cut lending to firms. This section tests explicitly for the role of the credit channel in spreading the crisis across space, exploiting variation across counties in pre-crisis market shares of banks. The results do not show a significant role for the credit channel, but also cannot exclude a medium-sized effect. At the same time, results on the trade channel remain unchanged.

Starting in mid-2007, there has been increased uncertainty about asset values on banks' balance sheets. Following the subprime mortgage crisis, liquidity in the asset-backed commercial paper market started to dry up in the fall of 2007. Around the same time, the TED spread started to increase and banks increasingly faced funding problems.[22] Due to the trouble in national funding markets, banks may have cut their lending to firms. The integrated national funding market for banks may thus have been another source for spreading the crisis across space.

Several papers have documented a contraction in new lending during the 2007-09 period. For instance, Ivashina and Scharfstein (2010) [33] show that new loans to large borrowers fell by 79% between the second quarter of 2007 and the fourth quarter of 2008 in their sample of syndicated loans. Naturally, a reduction in new lending can be a result of either a fall in credit demand or in credit supply.

At least part of this contraction in new lending may be due to reduced credit supply. This is suggested by several papers that study the determinants of crisis lending growth at the bank level. For instance, Gozzi and Goetz (2010) [26] find that banks that depended more on wholesale funding cut their lending by more during the crisis period. Likewise, Cornett et al. (2011) [13] report that banks that held more illiquid assets on their balance sheets were more likely to reduce lending.

To study the role of credit supply shocks on economic outcomes, I make use of cross-sectional differences across counties in the composition of local banks. This strategy assumes that firms borrow funds to a large extent from local banks, i.e. banks that operate branches in the county a firm is located in. This claim finds support in the literature: Although existing literature indicates that the role of distance for business lending has fallen over the last decades, it still seems to play a significant role. For instance, although Petersen and Rajan (2002) [49] report that over the period 1973–1993 the average borrower-lender distance has declined, the median distance in 1993 still remains very low (5 miles). In more recent work, Brevoort et al. (2010) [9] study data until 2003, and find that there was only a modest increase in distance over this period. Moreover, they report that the trend of growing borrower-lender distance has come to a halt in the second half of their study period (1998–2003). Overall, they conclude that "distance still matters".

The analysis in this part is related to papers by Gozzi and Goetz (2010) [26] and Green-

[22] A detailed documentation of events can be found in, for example, Brunnermeier (2009) [11].

stone and Mas (2012) [27]. In contrast to Gozzi and Goetz (2010) [26], I do not match local economies to banks by the bank's headquarter. Instead, I use the network of bank branches to measure banks' market shares in counties. In using bank branch data, my work also differs from the paper by Greenstone and Mas (2012) [27]. These authors focus on small business lending (firms with less than \$1 million revenue) and use lending data from the community reinvestment act. They separate credit supply from demand shocks by decomposing bank-county lending growth into a county part and a bank part. While the first measures credit demand shocks, the latter captures shocks to credit supply. I take a different approach to the identification problem by relying on variation in lending growth across banks based on differences in pre-crisis exposure to troubles in national financial markets. My work thus differs from Greenstone and Mas (2012) [27] by making use of a specific supply shock to banks.

Empirical Strategy

The main challenge for this empirical part is to separate the effect of a credit supply shock from a reduction in loan demand that may happen at the same time. The empirical strategy relies on using (i) variation in lending growth across banks that is due to different ex-ante exposure of banks to distress in national financial markets, and (ii) variation across counties in pre-crisis market shares of banks.

In order to control specifically for a credit supply shock, I consider the following specification:

$$d\log(Y_c) = \beta_0 + \beta_1 \text{TDSCOUNTY}_c + \beta_2 \text{Leverage}_c + \beta_3 \text{Loan Growth}_c + \alpha_s + \epsilon_c \tag{1.7}$$

The left hand side measures 2007-09 employment growth in county c, α_s denotes a state fixed effect. The explanatory variable of interest is Loan Growth$_c$, which is a measure of the 2007-09 growth of outstanding commercial and industrial loans at the county level. It is defined as the weighted average of loan growth of banks that own branches in that county:

$$\text{Loan Growth}_c = \sum_{i=1}^{N_c} \omega_{ci} \text{Loan Growth}_i$$

where N_c is the number of banks that own branches in county c and ω_{ci} is a measure of the market share of bank i in that county. Given that I do not have data on bank lending by county, I measure the market share as the share of deposits held by branches of bank i in county c in total deposits in county c.

Equation 1.7 presents an endogeneity concern: Shocks other than a credit supply shock may reduce employment growth in a county, leading to a reduction in loan demand and the stock of outstanding loans. I take two complementary approaches to mitigate endogeneity concerns. First, I focus only on counties that are dominated by large banks. For these counties, it is much less likely that loan growth of banks depends on county-level outcomes. Second, I use variation in loan growth of banks due to different pre-crisis exposure to troubles in national financial markets in a two-stage-least-squares setup.

Data

I use data from two sources to construct a measure of a credit supply shock at the county level. First, I use data on the bank branch network from the FDIC to construct a measure of bank pre-crisis (2007) market shares at the county level. These data give me information about the universe of bank branches in the U.S.. In particular, I use information on the location of the branch (county and state), the identity of the bank owning the branch, and the amount of deposits in the branch. I delete all branches of banks that do not file Call reports[23], all branches that do not offer full services (like drive-through facilities, administrative offices, etc.), and branches of banks that specialize in activities other than commercial lending (i.e. agriculture, credit cards, mortgages, consumer lending, and other specializations).

I then match these data with bank financial information from the Call Reports. In order to account for potential risk sharing across banks within the same holding company, I aggregate banks to the bank-holding-company (BHC) level. Finally, to avoid changes in balance sheet variables that are due to M&As as opposed to the banks normal operations, I delete all banks that were engaged in M&As across different bank holding companies. The final bank branch dataset consists of 43,000 branches belonging to 6,300 banks (or bank holding companies). I then collapse it to the county level.

At the county-level, there is large variation in loan growth, even after accounting for state fixed effects. For illustration, figure 1.10 shows a histogram of county-level loan growth, demeaned at the state level.

Results

Least Squares

I start by presenting ordinary least squares results for equation 1.7. I estimate equation 1.7 separately for employment growth in tradables and nontradables. I present results for two separate measures of loan growth, one using only commercial and industrial loans on the balance sheet, while the other also includes open lines of credit. Table 1.15 shows the results. The coefficient on the trade variable is still negative and different from zero, and very similar in magnitude to the previous estimations at the county level. The results on the credit channel are inconclusive. While some specifications hint at a positive relationship between loan growth and employment growth, standard errors are large. In particular, I cannot exclude a modest positive relationship between loan growth and employment growth.

Counties Dominated by Large Banks

A potential concern with the previous results is a reverse causality problem: Loan growth at the bank-level is endogenous to demand at the county level. In particular, this should be true for small banks which are only active lenders in very few counties. One way to reduce

[23] Prior to 2012, Office of Thrift Supervision (now OCC) institutions filed a quarterly Thrift Financial Report. All other institutions in the data file the Call report.

this endogeneity concern, therefore, is to restrict attention to counties that are dominated by large banks. In the next set of estimations, I therefore focus on counties in which large banks (defined as being in the top 10% of the asset size distribution) have a joint market share of more than 50%. This roughly reduces the sample of counties by half. Due to the size of these banks, it is unlikely that their loan growth is determined by economic conditions in a particular county.[24]

Results for this estimation are in table 1.16 and are very similar to the ones obtained from the total sample. Most importantly, the coefficient on the trade variable again remains unchanged.

Instrumental Variable Estimations

A second approach to mitigate endogeneity concerns is to an instrumental variable strategy. In particular, the idea is to use variation in loan growth across banks that comes from different pre-crisis exposure to funding problems. In particular, I use pre-crisis values of the share of illiquid assets in total assets, the share of non-core liabilities in total assets, and the share of nonperforming loans in total loans as instruments.[25] To measure the share of non-core financing, I follow Gozzi and Goetz (2010) [26]. The variable captures a bank's reliance on wholesale deposits and on non-deposit financing sources such as repos and interbank loans. I compute the share of illiquid assets in total assets as in Cornett et al. (2011) [13].[26] Finally, non-performing loans are defined as loans 90 days or more past due plus non-accruing loans. I aggregate these variables to the county level again using deposit weights.

The exclusion restriction may be violated if bank portfolios are also endogenous to local economic outcomes. One particular concern may be that some banks depend more on wholesale financing because local deposit supply in their locations is low. Low local deposit supply may in turn be correlated with economic outcomes, for instance through consumer indebtedness and reductions in consumption. Table 1.17 shows correlations of the instruments with the county-level deposit-to-employment ratio, showing no significant correlation between instruments and local deposit holdings. The table does, however, show correlations between the instruments and the pre-crisis construction share, which is why this variable is added as a control.

The first stage delivers the expected negative signs for all three measures of bank-level vulnerability, and an F-statistic of 9. Table 1.18 presents the results on the second stage. It shows that the results for the trade channel are robust to this exercise. In contrast, the table

[24] Results are very similar if I sort banks by the number of counties in which they operate branches, instead of asset size.

[25] Alternative measures that have been proposed in the literature are the capital ratio and a measure for off-balance sheet commitments. I find that the capital ratio does not have much explanatory power for loan growth, and that off-balance sheet commitments are highly positively correlated with the measure of non-core financing. I thus exclude these two variables.

[26] Specifically, illiquid assets are given by the sum of loans and leases net of unearned income, and MBS and ABS securities (held-to-maturity and available for sale).

shows no significant effect of the credit channel, but again standard errors for this estimate are relatively large.

1.6 Model

This section presents a model of the diffusion of the crisis through trade. The reduced form empirical estimates only allow me to econometrically identify the *relative* effect of the trade channel on industry outcomes. It does not allow me to capture the part of the trade channel that affects all industries in the same state (this part of the trade channel is absorbed by the state fixed effect). How important was the trade channel for the *total* spread of the crisis? Answering this question is the primary purpose of the model. Moreover, the model allows me to discuss the role of general equilibrium effects and to give a better structural understanding of the empirical results.

In the model, a decline in the dividend of a real asset causes a collapse in expenditure, and a fall in the asset value. An asset in the model is a tree that produces a stochastic dividend (an outside good), which is freely tradable. I think of this outside sector as a reduced form way of modeling the credit and expenditure dynamics related to housing: Prior to the recession, households in states with housing booms financed increasing expenditure through home equity withdrawals (Mian and Sufi (2011) [42]). At the onset of the recession, this housing-financed expenditure boom reversed, and households had to cut on consumption during the deleveraging process. The model parsimoniously captures these expenditure dynamics related to housing by letting the dividend of the outside sector vary over time. When the dividend in state i is high, total expenditure rises, and the state runs a trade deficit in manufacturing financed by the outside good. The fall in expenditure at the onset of the recession is captured by a drop in the dividend, reducing the manufacturing trade deficit in states with housing boom-bust cycles. The advantage of this modeling strategy is that it allows me to focus on the heterogeneity in trade linkages and the spread of the crisis across space, taking the expenditure dynamics generated by the housing cycle as given.

The model thus abstracts from the existence of borrowing and lending across states, because I focus on the diffusion of the expenditure decline through the economy. Instead of studying the link between the initial shock and expenditure (which would require a model focusing more on intertemporal borrowing and lending), I instead focus on the connection between the expenditure decline and the distribution of employment and wage outcomes. To focus on this connection, I model the shock in a way that it generates a cross-sectional pattern of expenditure growth as observed in the data.

Setup

There are N states denoted by $i = 1, ..., N$, each populated by a representative household that supplies labor and consumes. The only storage for wealth are N "housing" trees. The tree in state i pays income Z_{it} in the form of a freely tradable outside consumption good whose price is normalized to one. Since housing is largely locally owned, I assume that the household in state i owns 100% of the housing tree in state i.[27] As a result of this assumption, it does not matter whether dividends are stochastic or deterministic.

The interest rate faced by households in state i is then given by

$$1 + r_{it+1} = \frac{Z_{it+1} + V_{it+1}}{V_{it}},$$

where V_{it} is the value of tree i at the end of period t.

Households

The household chooses consumption and labor services to maximize expected utility

$$E_0 \left[\sum_{t=1}^{\infty} \beta^t u(C_{it}, L_{it}) \right]$$

subject to the household DBC:

$$B_{it} = (1 + r_{it}) B_{it-1} + W_{it} L_{it} - P_{it} C_{it}$$

Since the outside good is used as numeraire, the price of aggregate consumption, P_{it}, is measured in (units of outside good/units of aggregate consumption). Let the utility function be given by

$$u(C_{it}, L_{it}) = \frac{C_{it}^{1-\gamma}}{1-\gamma} - \eta \frac{L_{it}^{1+\frac{1}{\phi}}}{1+\frac{1}{\phi}}.$$

It follows that the Euler Equation is:

$$C_{it}^{-\gamma} = \beta E_t \left[(1 + r_{it+1}) \frac{P_{it}}{P_{it+1}} C_{it+1}^{-\gamma} \right]$$

The tree yields a return of $1 + r_{it+1}$ from period t to $t+1$ in units of the outside good. This needs to be adjusted by $\frac{P_{it}}{P_{it+1}}$ in order to arrive at the return in units of aggregate consumption.

Consumption: Aggregate consumption is given by a Cobb-Douglas aggregate of produced consumption goods and consumption of the outside good:

$$C_{it} = \left(C_{it}^{P} \right)^{\delta} \left(C_{it}^{O} \right)^{1-\delta}$$

[27] One can consider alternative ownership structures. The results will continue to hold if portfolio shares are heterogeneous across states, so that there is ex-ante different exposure to wealth shocks. What matters for the model is the cross-section in income shocks generated by the shocks to the housing trees. This set of income shocks is completely determined by the calibration strategy. With imperfect home bias, the calibration strategy would change the size of the shocks to tree dividends, so that the resulting shocks to household income are the same as with full home bias.

Consumption of produced goods consists of tradables (also labeled manufactures) and non-tradables.

$$C_{it}^{P} = \left(C_{it}^{M}\right)^{\delta^M} \left(C_{it}^{N}\right)^{1-\delta^M}$$

Manufacturing consumption C_{it}^{M} is a composite of consumption in different manufacturing industries:

$$C_{it}^{M} = \prod_{k=1}^{S} \left(c_{it}^{k}\right)^{\alpha_i^k}$$

Within each manufacturing industry k, trade is modeled in an Armington-structure:[28] Each state produces a unique variety of the good in industry k, and consumption c_{it}^{k} is a CES composite of the varieties from different origins.[29]

$$c_{it}^{k} = \left(\sum_j \left(c_{ijt}^{k}\right)^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}}$$

The share of expenditure that state i spends on goods form state j in industry k can be written as:

$$\pi_{ijt}^{k} = \left(\frac{P_{it}^{k}}{p_{ijt}^{k}}\right)^{\sigma-1}$$

Labor Supply and Wages: Workers can supply labor in the tradable and nontradable sector, and in different industries within the tradable sector. However, the labor market is subject to two frictions: Labor mobility frictions prevents wage equalization across sectors and industries, and there is rigidity in the adjustment of real wages. I introduce labor mobility frictions in the model in order to account for the different evolution of wages across industries in a state, as shown in the empirical part. I allow the these frictions to vary for inter-sectoral mobility (manufacturing vs. nontradables) and intra-sectoral mobility (across industries within manufacturing). Wage stickiness is added to allow for equilibrium unemployment and to improve realism of the quantitative exercise.

Aggregate labor is a CES composite of labor services in manufacturing and nontradables:

$$L_{it} = \left(\left(L_{it}^{M}\right)^{\frac{1+\rho}{\rho}} + \left(L_{it}^{N}\right)^{\frac{1+\rho}{\rho}}\right)^{\frac{\rho}{1+\rho}}$$

where $\rho \in [0, \infty)$ measures the degree of worker mobility across sectors. The case $\rho = \infty$ denotes perfect labor flexibility, under which wages equalize across the tradable and

[28]The particular within-industry structure that is driving trade is not crucial for the model. For instance, using an Eaton-Kortum structure for each industry would deliver the same simulation results as the present Armington structure if their comparative advantage parameter is set to the value of $\sigma - 1$ in the present model.

[29]One could add preference parameters to this consumption index to account, for instance, for home bias in consumption. As long as these parameters are constant, there would be no change to any of the results in this paper.

nontradable sector. Labor supply for tradables is given by supply for different industries k:

$$L_{it}^{M} = \left(\sum_{k} \left(L_{it}^{k} \right)^{\frac{1+\nu}{\nu}} \right)^{\frac{\nu}{1+\nu}}$$

The parameter denoting labor immobility within the tradable sector potentially differs from labor immobility across sectors and is denoted by ν. The wage in state i, industry k, is given by

$$w_{it}^{k} = (w_{it-1}^{k})^{\lambda} (w_{it}^{k*})^{1-\lambda}$$

where the parameter λ governs the degree of real wage rigidity and varies between 0 (perfect wage flexibility) and 1 (full wage rigidity). An analogous equation holds for the wage in the nontradable sector, W_{it}^{N}. The variable $W_{it}^{*} = \left(\sum_{k} \left(w_{it}^{k*} \right)^{1+\nu} \right)^{\frac{1}{1+\nu}}$ is the wage at which workers are willing to supply an aggregate labor input L_{it}, i.e. it is given by the marginal rate of substitution:

$$\eta \frac{L_{it}^{\frac{1}{\phi}}}{C_{it}^{-\gamma}} = \frac{W_{it}^{*}}{P_{it}}$$

Labor supply across industries follows the rule:

$$L_{it}^{k} = \left(\frac{w_{it}^{k*}}{W_{it}^{*}} \right)^{\nu} L_{it}$$

and the aggregate manufacturing wage solves:

$$W_{it}^{M} L_{it}^{M} = \sum_{k} w_{it}^{k} L_{it}^{k}$$

Finally, the aggregate wage index solves:

$$W_{it} L_{it} = W_{it}^{M} L_{it}^{M} + W_{it}^{N} L_{it}^{N}$$

Firms

In each industry and state, there are many identical firms that face perfectly competitive output and labor markets within their industry. Firms hire workers in the local labor market and produce output with a constant returns technology.[30]

$$Q_{it}^{k} = A_{i}^{k} L_{it}^{k}$$

so that sales $Y_{it}^{k} \equiv p_{it}^{k} Q_{it}^{k}$ equal earnings $w_{it}^{k} L_{it}^{k}$. Again, analogous expressions hold for the nontradable sector.

Shipments are subject to iceberg transportation costs τ_{ni}^{k}, so that the price p_{nit}^{k} can be written as $p_{nit}^{k} = \tau_{ni}^{k} \frac{w_{it}^{k}}{A_{i}^{k}}$.

[30] All model implications remain unchanged if I assume decreasing returns to labor (with constant elasticity) and that firms are owned locally. In that case, earnings growth still equals sales growth. The only difference is that total income growth is divided into profit growth and growth in earnings (which would be the same).

Equilibrium

An equilibrium is an allocation (labor services, production and consumption) and prices (output prices and wages) such that (i) households optimize, (ii) firms optimize, and (iii) markets clear. In equilibrium, total expenditure by households in state i equal the sum of income and the asset's dividend:

$$X_{it} = W_{it}L_{it} + Z_{it}$$

and household wealth equals the value of the domestic asset: $B_{it} = V_{it}$. Expenditure on manufacturing goods are a constant fraction of total expenditure

$$X_{it}^{M} = \delta^{M}\delta(W_{it}L_{it} + Z_{it})$$

so that a state runs a manufacturing trade deficit whenever the dividend Z_{it} is high. Wages in manufacturing industries are determined in the trade equilibrium,

$$w_{it}^{k}L_{it}^{k} = \delta^{M}\delta\sum_{n}\pi_{nit}^{k}\alpha_{n}^{k}(W_{nt}L_{nt} + Z_{nt}),$$

and trade flows have a structural gravity representation as defined in Head and Mayer ([30]):

$$X_{ni}^{k} = \frac{Y_{i}^{k}}{\Omega_{i}^{k}}\frac{X_{n}^{k}}{\left(P_{n}^{k}\right)^{1-\sigma}}\left(\tau_{ni}^{k}\right)^{1-\sigma},$$

where Ω_{i}^{k} is given by $\Omega_{i}^{k} = \sum_{n}\left(\frac{\tau_{ni}^{k}}{P_{n}^{k}}\right)^{1-\sigma}X_{n}^{k}$.

Total income in the economy is tied down by the supply of the outside good:

$$\sum_{i}W_{it}L_{it} = \frac{\delta}{1-\delta}\sum_{i}Z_{it}$$

Finally, the value of asset i at time t can be found by iterating on the Euler equation:

$$V_{it} = E_{t}\left[\sum_{j=1}^{\infty}\beta^{j}\left(\frac{C_{it+j}}{C_{it}}\right)^{-\gamma}\left(\frac{P_{it}}{P_{it+j}}\right)Z_{it+j}\right]$$

Solution and Calibration

To analyze the role of the trade channel for the transmission of shocks, I study the model's response to a shock in dividends Z. In the model, a reduction in the dividend Z_i for state i reduces total expenditure, including expenditure on manufactured goods. It therefore leads to lower demand for producers selling to state i. At the same time, it has no direct effect on production in state i, but only indirect effects through general equilibrium adjustments.
In general equilibrium, wages and prices will respond. In particular, a reduction in demand

for an industry's product exerts downward pressure on wages. In a labor market with perfect worker mobility across industries, we should see only differential employment growth, but not differential wage growth across industries. Yet we observe differential wage growth empirically. The model allows for this possibility by introducing imperfect labor mobility across industries. While there is reallocation of employment to other industries and potentially to the nontradable sector, total employment changes as well through endogenous labor supply.

Identifying Labor Mobility Parameters

The parameters ν and ρ determine how easily labor can shift between manufacturing industries, and between the tradable and nontradable sector, respectively. They can take on values between zero (labor completely immobile) and infinity (perfect mobility). The parameters can also be interpreted as elasticities of labor supply to individual manufacturing industries and to the tradable and nontradable sector, respectively.
I identify the the parameter governing labor mobility frictions across manufacturing industries from the reduced form estimates. The parameter ν measures this degree of labor mobility frictions, where a larger ν means a higher degree of mobility. The model implies that the parameter can be identified directly from the reduced form estimations on employment and average wages. Writing the labor allocation rule of households in growth rates delivers the following equation:[31]

$$\hat{L}_i^k = \frac{\nu}{1-\lambda}\left(\hat{w}_i^k - \hat{W}_i^M\right) + \hat{L}_i^M$$

That is, conditional on a state fixed effect, employment and wage growth are directly proportional. This allows me to identify $\frac{\nu}{1-\lambda}$ as the ratio of coefficients on the trade demand shock from the employment and wage regressions.[32] I use the preferred estimates of these coefficients from the weighted least squares regressions using only manufacturing industries (Table 1.4 columns 2 and 6) and obtain

$$\frac{\nu}{1-\lambda} = \frac{\hat{\beta}_1^L}{\hat{\beta}_1^w} = 3.$$

I calibrate the wage rigidity parameter following Shimer (2010) [57] and Gorodnichenko et al. (2012) [25] as $\lambda = 0.79$, and thus infer that $\nu = 0.63$.

Next, I identify the parameter ρ, which governs frictions to labor mobility across the tradable and nontradable sector. To identify this parameter, I restrict the model to reproduce the cross-sectional correlation between employment growth in manufacturing and non-manufacturing at the state-level, which equals 0.55 in the data. In the model, this moment is

[31] I assume that the economy starts from a state with no endogenous wage dynamics, i.e. $w_{it-1}^k = w_{it-1}^{k*}$. This implies that the growth rate of wages is a fixed share of the growth rate of the MRS: $\hat{w}_{it}^k = (1-\lambda)\hat{w}_{it}^{k*}$.

[32] Subtracting state and industry fixed effects, we can write the regression equation for employment growth as $\tilde{\hat{L}}_i^k = \beta_0^L + \beta_1^L \widetilde{\text{TDS}}_i^k + \tilde{\epsilon}_i^k$, and $\tilde{\hat{L}}_i^k = \frac{\nu}{1-\lambda}\tilde{\hat{w}}_i^k$ from the model equation. It then follows that $\hat{\beta}_1^L = \frac{\nu}{1-\lambda}\hat{\beta}_1^w$.

sensitive to the labor immobility parameter between manufacturing and non-manufacturing. In particular, if $\rho = 0$ (complete immobility), the correlation between manufacturing and non-manufacturing employment is one, and it is declining as ρ increases. Using a simulated method of moments approach to determine this parameter delivers a value of $\rho = 0.42$, implying higher rigidity between manufacturing and non-manufacturing than across manufacturing industries.

How do the parameter values inferred from the data compare to the literature? Ashenfelter et al. (2010) [3] review several studies from the micro literature that estimate the labor supply elasticity faced by an individual *firm*. In principle, one would expect labor mobility to be higher across firms within an industry compared to mobility across different industries, for instance because of lower search frictions or more similar skill requirements. Consequently, the labor supply elasticity faced by individual firms is likely higher than the elasticity faced by an industry. The studies reviewed by Ashenfelter et al. (2010) [3] estimate very low short-run labor supply elasticities at the firm level, in the range between 0.1 and 2, and higher long run elasticities (between 2 and 4). Considering the short-run nature of my study and that firm-level labor supply elasticities are most likely upper bounds for industry-level labor supply elasticities, my estimates for ν and ρ seem reasonable.

Calibration

In addition to the parameters above, I need to make choices for other model parameters. I set both the coefficient of relative risk aversion, γ and the labor supply elasticity, ϕ, equal to 2. These values are in line with existing literature. Next, I need to specify values for δ, the share of produced goods and services in expenditure, and δ^M, the share of manufactured goods among produced goods. I set the value for δ equal to 0.8, which matches a share of consumer expenditure on housing (i.e. shelter) equal to 20%, as found in the consumer expenditure survey and in, for example, Piazzesi et al. (2007) [50]. The value for δ^M is set to 0.18 as in Eaton et al. (2011) [15], and g is set to -0.044 to roughly match the decline of real GDP from peak to trough during the recession. Instead of calibrating trade parameters τ_{ni}^k, I plug observed trade shares π_{ni}^k directly into the model, following the work by Dekle et al. (2007) [14]. Finally, I calibrate the consumption parameters α_i^k to match the expenditure shares observed in the trade data.

Modeling the Shock

The model asks what share of the spread of the crisis can be explained by the diffusion of the consumer demand shocks at the beginning of the recession. I therefore think of the shocks in the model, dZ_i, as being directly related to household leverage. To pin down this relationship, I employ a simulated method of moments approach: In general equilibrium, I require that the model can reproduce exactly the same regression relationship between expenditure growth and pre-crisis leverage that has been identified by Mian et al. (2011) [41].

Since expenditure growth in the model can be expressed as $\hat{X}_i = \frac{Y_i}{X_i}\hat{Y}_i + \frac{dZ_i}{X_i}$, I start by requiring that $\frac{dZ_i}{X_i}$ is linear in leverage, i.e. $\frac{dZ_i}{X_i} = a + b \cdot Lev_i$. The parameter a can be expressed as a function of the growth rate of aggregate expenditure, g. This delivers $a = g(1-\delta) - b\sum_i \frac{X_i}{X} Lev_i$. The parameter b is determined in a simulated method of moments procedure: After general equilibrium adjustments, the model reproduces the Mian et al. (2011) [41] regression relationship between expenditure growth and leverage at the state level. That is, b is implicitly determined through the relationship $\frac{Cov(\hat{X}_i, Lev_i)}{Var(Lev_i)} = \chi$, where χ denotes the coefficient on household leverage in Mian et al. (2011) [41]. Notice that this approach does not require expenditure growth to be exactly linear in leverage: A state that sells relatively more to high-leverage states will experience lower income growth, and therefore be below the regression line. Mian et al. (2011) [41] estimate the effect of household leverage on consumer expenditure for four different spending categories. In particular, their results differ among durable and non-durable consumption goods. To arrive at a single statistic summarizing the effect of household leverage on expenditure growth, I first take simple averages of their coefficients within the durable and nondurable category, and then aggregate the two effects using employment weights for durable and nondurable manufacturing.[33] Using this procedure, I arrive at a coefficient of $\chi = -0.11$.

Interpreting the Empirical Results

Decomposing earnings growth in the model delivers an equation that is similar to the one written down in the empirical part. Comparing this model equation to the empirical specification helps in interpreting the empirical results.
In the model, earnings growth can be written as follows

$$\hat{Y}_i^k = \sum_n \frac{X_{ni}^k}{Y_i^k}\left(\hat{\pi}_{ni}^k + \hat{X}_n\right)$$

The variable $\hat{\pi}_{ni}^k$ is the change in the market share by industry k, located in state i, in market n. The changes in the market share are driven by changes in factor costs:

$$\hat{\pi}_{ni}^k = (\sigma - 1)\left(\sum_{j=1}^{N} \pi_{nj}^k \hat{w}_j^k - \hat{w}_i^k\right)$$

The market share of industry k from state i in market n rises, if its factor cost fall by more than the factor cost of its competitors in market n. Given a relative fall in factor costs, the market share rises relatively more for industries selling more substitutable products. Aggregating over all markets n, $\sum_n \frac{X_{ni}^k}{Y_i^k}\hat{\pi}_{ni}^k$ measures the change in earnings driven by changes in competitiveness.

[33] I use the U.S. Census definitions of durable and nondurable manufacturing to sort manufacturing industries into these categories.

The expression $\sum_n \frac{X_{ni}^k}{Y_i^k}\hat{X}_n$ measures a demand effect: Because total expenditure in destination n collapses, earnings of the producers fall. This effect includes both a direct effect through the shock dZ_n as well as the change in income at destination n.
Using the fact that the normalized shock, $\frac{dZ_n}{X_n}$ is linear in leverage, we can write earnings growth and employment growth as follows:

$$\widetilde{\hat{Y}}_i^k = \theta + b \cdot \widetilde{\mathrm{TDS}}_i^k + \widetilde{\mathrm{GE}}_i^k \tag{1.8}$$

where the tilde denotes that variables are written as net of state and industry fixed effects. General equilibrium effects GE_i^k can be written as the sum of two terms:

$$\mathrm{GE}_i^k = \underbrace{\sum_n \frac{X_{ni}^k}{Y_i^k}\frac{Y_n}{X_n}\hat{Y}_n}_{\text{Income}} + \underbrace{\sum_n \frac{X_{ni}^k}{Y_i^k}\hat{\pi}_{ni}^k}_{\text{Competitiveness}}$$

An exogenous shock affects expenditure directly, and indirectly through income changes. It also changes competitiveness through changes in factor costs of industries.

In the model, the trade demand shock has a partial equilibrium effect (b) and a general equilibrium effect on industry outcomes. However, when taking the model to the data we can only estimate the total effect of the trade demand shock. This is the way the equation was estimated in the empirical part. Controlling for general equilibrium effects would not be appropriate, since the general equilibrium effects are themselves outcomes of the trade demand shock. This is a typical case of a *bad control.* The estimations in the empirical part are reduced form equations.

I now compare the magnitude of the estimated effect from the data to the one predicted by the model. Solving for the response of the model to the shock provides me with model-implied data on employment growth and wage growth for manufacturing industries at the state level. I use this new data series to re-run the reduced form estimations from the empirical part. Results for this set of regressions are in table 1.20. I run weighted-least squares regression on the simulated data and compare them to the estimates using the same specifications for manufacturing industries only (since the model is calibrated to only these industries), i.e. columns 2, 4, and 6 in table 1.4.

Overall, the magnitude of the coefficients is broadly consistent with the empirical findings. The coefficients using the model-implied data are roughly 25% smaller than the results using the actual data. Note that the model parameters only pin down the *relative* size of the estimated coefficients, but not their *absolute* size. In particular, the relative size between the coefficients in the employment and wage regression is determined by the choice of the labor mobility parameter ν.[34] However, the absolute size of the coefficients is not determined through the choice of model parameters. It is rather a result of the interaction between the shocks dZ_i and the trade shares π_{ni}^k that I take from the data.

[34]Of course, this also pins down the size of these coefficients relative to the earnings regression. Since $\hat{Y}_i^k = \hat{W}_i^k + \hat{L}_i^k$, we must have $\hat{\beta}_1^Y = \hat{\beta}_1^w + \hat{\beta}_1^L$.

How important are general equilibrium effects for these results? Estimating the parameter b (the partial equilibrium effect associated with the exposure to demand shocks) via a simulated method of moments gives a value of -0.035. Given a coefficient equal to -0.098 in the earnings equation suggests that roughly two thirds of the effect can be attributed to general equilibrium adjustments through changes in income and competitiveness.

The Role of Trade in the Spread of the Crisis

To assess the role of trade in spreading the crisis across space, I ask: What would have been the distribution of the crisis across space in a world without trade? To that end, I compute the model's response if each state was a closed economy. I define the spread of the crisis as the difference between the closed economy model and the data. I then gauge the contribution of trade to the spread of the crisis by asking how much closer the trade model, compared to the closed economy model, is to the data.

The Closed Economy Case

I first compute the response if each state was a closed economy. That is, I use the shocks dZ_i and compute the model's response if each state i was in autarky, using the same model parameters as before (except the trade parameters). Computing the state-level one-period employment response in the closed economy case delivers the following equation:

$$\hat{L}_i^{Closed} = \frac{\frac{1}{1-\lambda} + \delta(\gamma - 1) - \gamma}{\frac{1}{1-\lambda} + \delta(\gamma - 1) + \frac{1}{\phi}} \hat{Z}_i$$

For the parameter values chosen, the employment response is procyclical.[35]

Comparing Closed Economy, Open Economy, and Data

I can now compare the outcomes predicted on employment growth by the closed economy model, the model featuring trade, and the data. In particular, I assess the role of trade for the spread of the crisis by comparing the distribution of employment losses across states between the trade model and the data relative to the closed economy model and the data.

Figure 1.11 shows a graph comparing the model predictions on state employment growth to the data. It shows regressions lines for regressions of total employment growth on leverage for each the closed economy model, open economy model, and data. Compared to the data and the open economy model, the closed economy model implies a much steeper relationship between employment growth and leverage. In the closed economy model, all of the reduction in demand is a reduction in demand for domestically produced goods. The reduction in

[35] The employment response can only be countercyclical if the numerator of the ratio is negative, since the denominator is always positive. For very high degrees of risk aversion, the employment response may be countercyclical, because the fall in consumption during a recession would make the labor supply curve shift out wide enough to more than offset the fall in labor demand.

local labor demand is thus stronger compared to the open economy model. In this setting, trade works like insurance. Part of the negative shocks to high-leverage states is absorbed by other states, making the cross-sectional relationship between employment growth and leverage flatter compared to the closed economy world.

The *spread* of the crisis refers to the distribution of the recession intensity across space, starting from a set of initial shocks. If there was no diffusion of initial shocks across space, employment growth in the data should be close to employment growth in the closed economy model. I therefore define the *spread* of the crisis as the deviation in growth at the state level between the data and the growth that would have prevailed if each state was a closed economy. Focusing on employment growth, it is written as follows:

$$Spread = \sum_{i=1}^{N} |\hat{L}_i^{Data} - \hat{L}_i^{Closed}|$$

That is, I take the absolute deviation of employment growth in the closed economy model from its counterpart in the data for each state i, and then sum across states to arrive at an aggregate statistic.

I now compute how much of this spread is accounted for by the trade channel. Analogous to the *spread*, I define the *trade shortfall* as the difference between the *open economy* model and the data:

$$TradeShortfall = \sum_{i=1}^{N} |\hat{L}_i^{Data} - \hat{L}_i^{Open}|$$

The *trade shortfall* thus describes the gap between model and data that remains after accounting for the diffusion of local shocks through the trade channel.[36]

I now ask: Compared to the closed economy model, how much closer does the trade model get to the data? I define the trade share as the fraction of the total spread that the trade channel can account for.

$$TradeShare = 1 - \frac{TradeShortfall}{Spread}$$

Intuitively, if the trade model implies state-level employment growth rates that are very similar to the closed economy model, then the trade share will be close to zero. On the other hand, the closer it can replicate the data, relative to the closed economy model, the more the trade share approaches one. In the simulations, I obtain a number of

$$TradeShare \approx 0.32$$

Accounting for the trade channel can thus explain roughly a third of the total spread of the crisis.

[36]Results hardly change when I use employment-weighted absolute distances instead of simple absolute distances for the *Spread* and the *Trade Shortfall*.

1.7 Conclusion

This paper provides evidence that trade has contributed to the geographic spread of the Great Recession, i.e. the shift of the recession away from states with housing boom-bust cycles. Empirically, I identify the trade channel by comparing economic outcomes of industries with different shipment patterns that are located in the same state. Industries that sold relatively more to states with housing boom-bust cycles grew by more before the crisis and declined faster during 2007-09. This *relative effect* of the trade channel is sizable: One standard deviation in the trade demand shock explains a 3 percentage point difference in 2007-09 employment growth between industries.

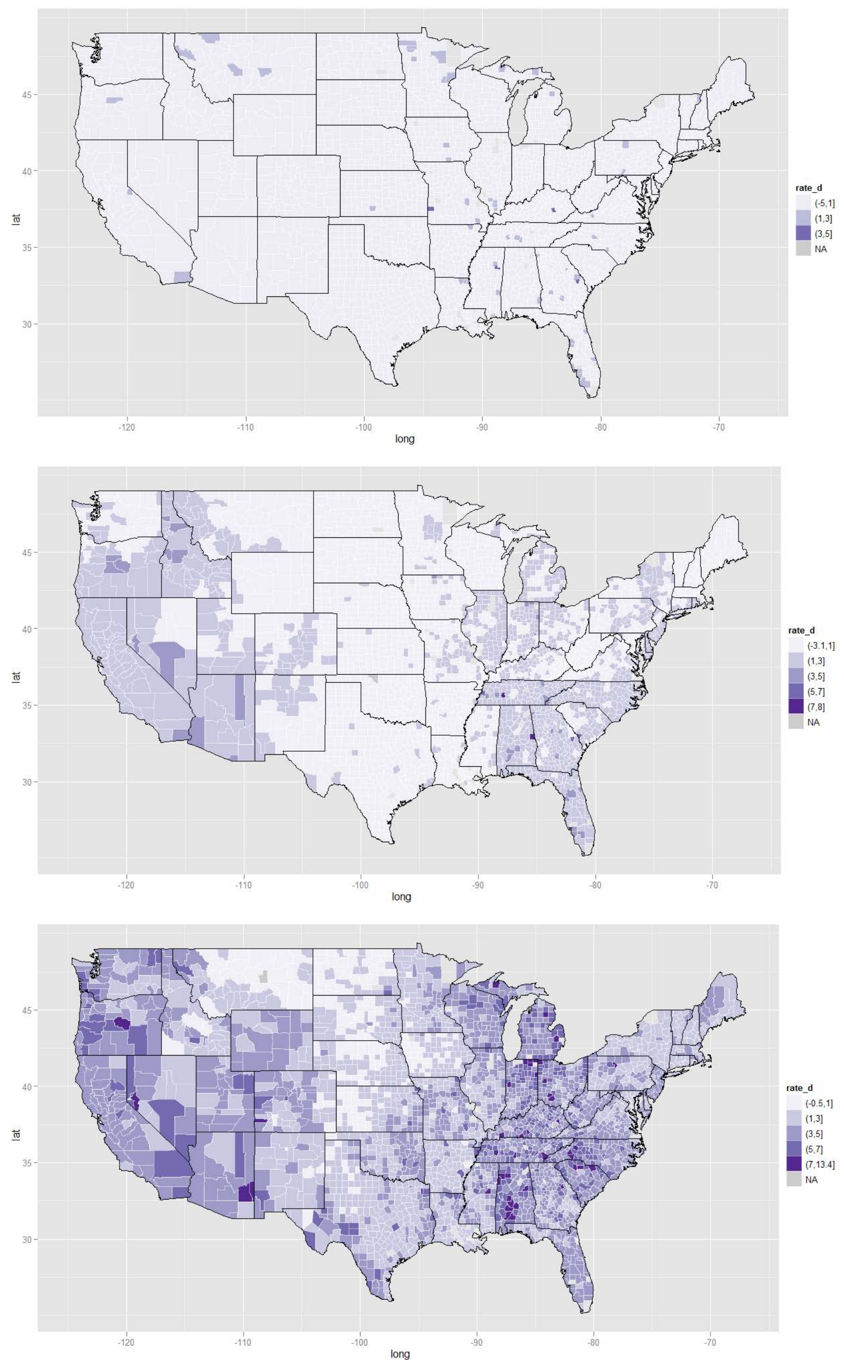
To assess the *aggregate* role of trade for the spread of the crisis, I build a model of the crisis diffusion through trade. I then shock the model using the [41] relationship between leverage and consumer expenditure. The model implies a relative effect of the trade channel that is of similar magnitude as the one found empirically. Finally, I use the model to gauge the overall contribution of trade to the spread of the crisis. I define this *spread* of the crisis as the difference in the distribution of the recession intensity across space between the data and a closed economy world. While the 2007-09 recession was concentrated in states with housing boom-bust cycles, the model suggests that this degree of concentration would have been much higher in the absence of trade between U.S. states. Comparing the implications of the closed economy model, the model including trade, and the data, reveals that trade is responsible for roughly a third of the overall spread.

At the aggregate level, therefore, trade works like insurance: Local adverse shocks are distributed across space, thereby causing business cycle comovement. In particular, this paper suggests that trade is still important for transmitting crises across space, even within a country like the U.S., where the manufacturing share in GDP has declined for years, and is low in international comparison. Moreover, my estimates may understate the importance of trade because the available data have only flows for manufactured goods and do not have information on trade in services.

The large pre-crisis U.S. current account deficit and the particularly sharp contraction in imports during the Great Recession suggest that trade may have also played a role in the international transmission of the crisis. This may have contributed to the recession particularly in key trading partners of the U.S., such as Canada and Mexico.

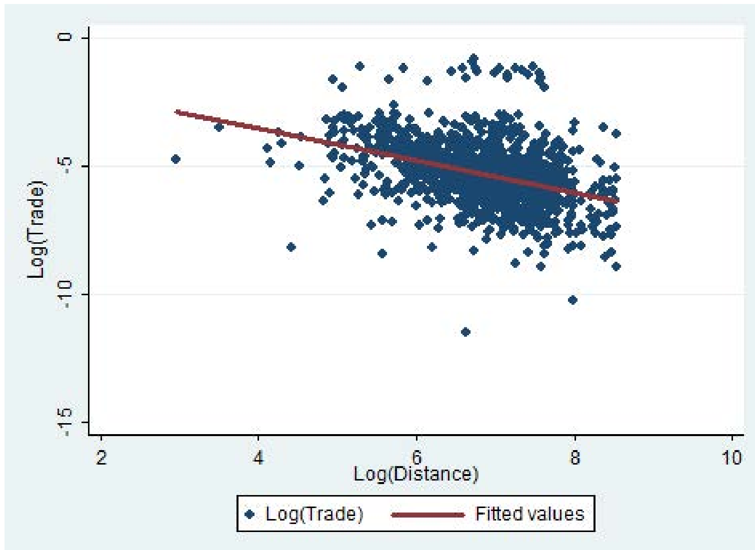
Finally, developing a model that captures richer dynamics of diffusion of the crisis across space is left for future work.

Figure 1.1: Yearly change in unemployment rate across US counties, 2006-07, 2007-08, and 2008-09



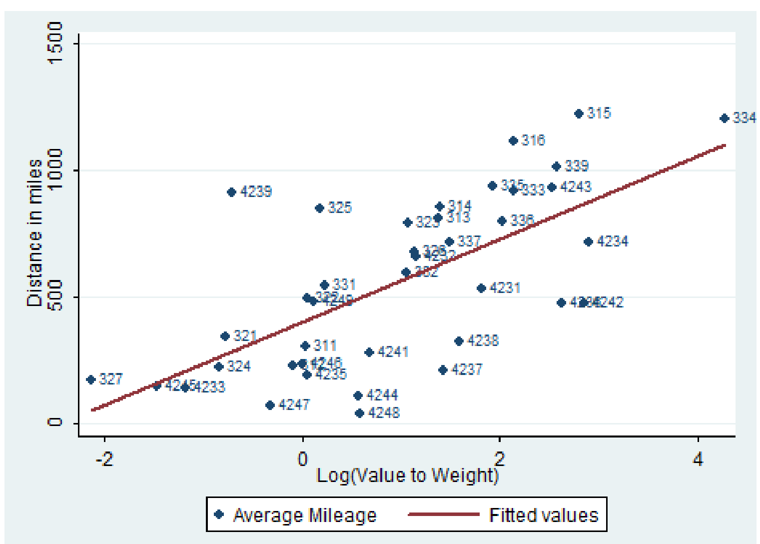
Notes: The maps show the year-on-year change in the unemployment rate from 2006-07, 2007-08, and 2008-09 across U.S. counties. Data are from the BLS.

Figure 1.2: Trade Linkages vs. Distance



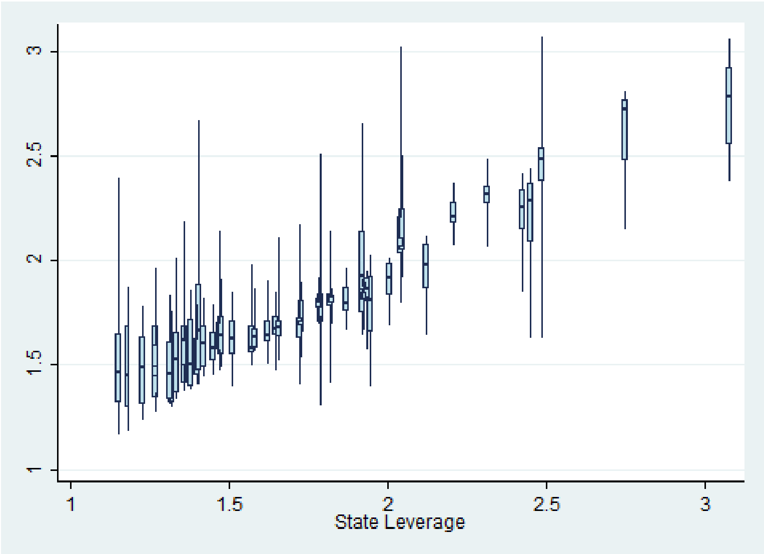
Notes: This figure plots the results from a bivariate regression of bilateral state-level trade flows on bilateral distance.

Figure 1.3: Transport costs drive shipment patterns across industries



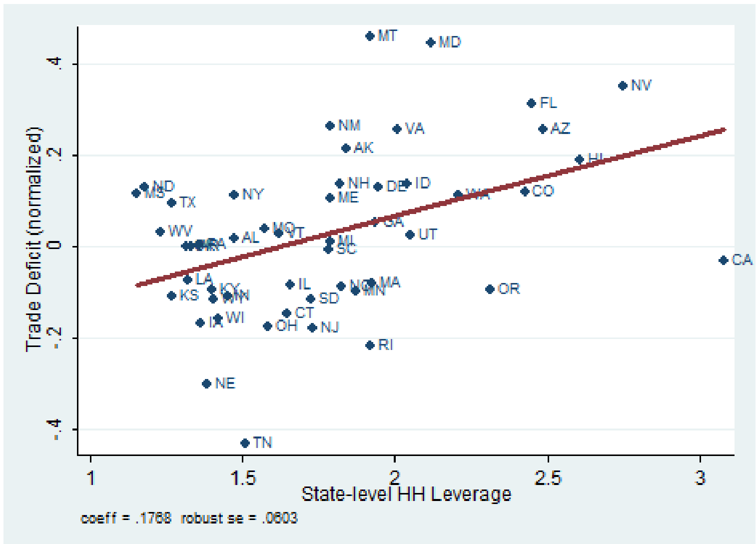
This graph shows the average mileage traveled by shipments in a particular industry, plotted against the (log of) the ratio of total value shipped over total tonnage shipped. Data are from the 2007 round of the CFS.

Figure 1.4: Intra-state heterogeneity in the trade demand shock



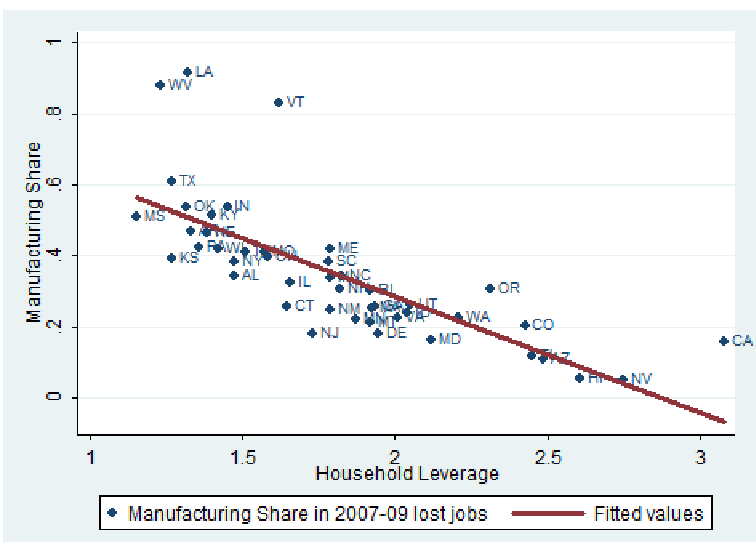
Notes:This figure shows a box plot of the trade demand shock at the state-industry level vs. state-level leverage. Each vertical bar represents one state. The thick boxes show the range of values spanned by the 25th and 75th percentile. The thin lines (whiskers) measure the values spanned by the 5th and 95th percentile. The most extreme values in each state are excluded.

Figure 1.5: State-level trade deficit vs. state-level HH leverage



Notes: This figure shows the relationship between the state -level (normalized) trade deficit, calculated as total expenditure minus total sales, divided by the average of total expenditure and total sales: $TradeDeficit_i = \frac{\sum_n X_{in} - \sum_n X_{ni}}{0.5 \cdot (\sum_n X_{in} + \sum_n X_{ni})}$

Figure 1.6: Share of Jobs Lost in Manufacturing vs. HH Leverage

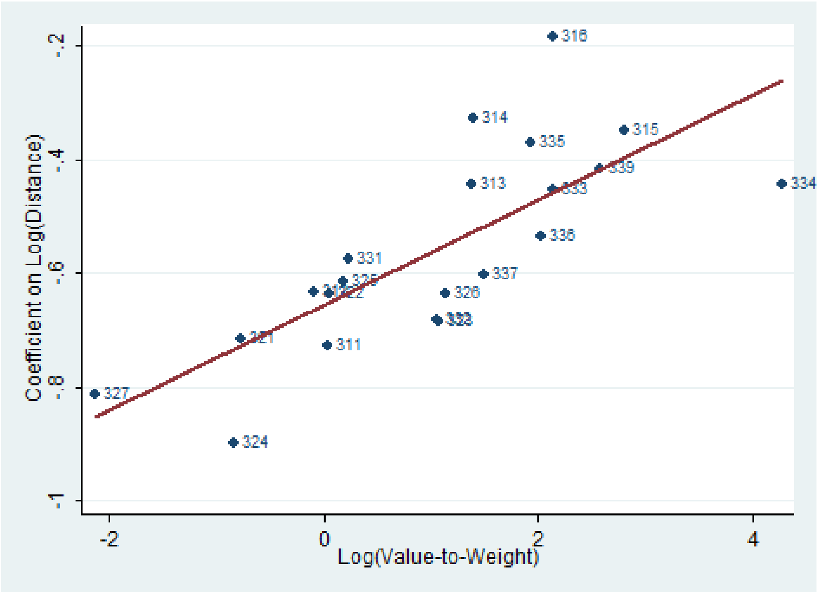


Notes: This figure shows the jobs lost in manufacturing between 2007 and 2009 as a share of the total jobs lost in that state.

Figure 1.7: The transmission of boom and bust through trade

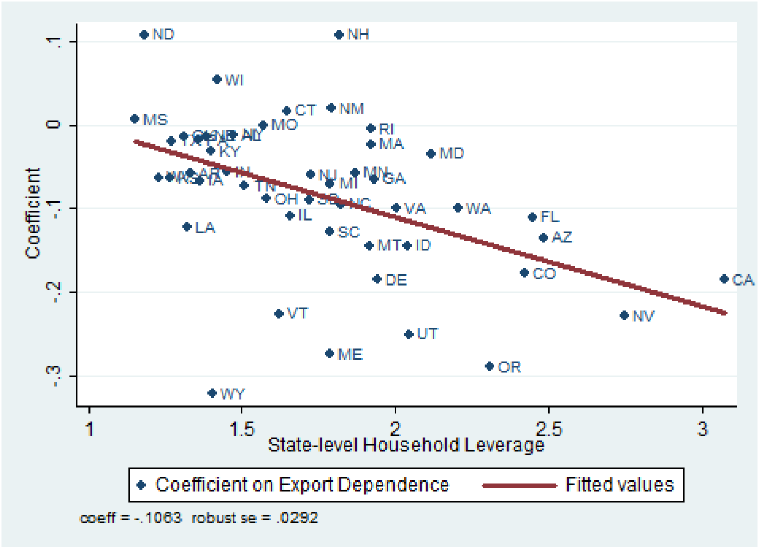
Notes: This graph shows coefficients from WLS regressions of employment growth on the trade demand shock, including state and industry fixed effects. Each regression is estimated for a 2-year interval of employment growth. The light grey lines show the 95% confidence intervals for twoway-clustered standard errors. The red lines show important events during the Great Recession: (i) The time at which the Case-Shiller house price index peaks (May 2006), (ii) the fall of Lehman (Sept 2008), and (iii) the month at which the national unemployment rate peaks at 10% (Oct 2009).

Figure 1.8: Distance Effect on Trade vs. Value-to-Weight Ratio



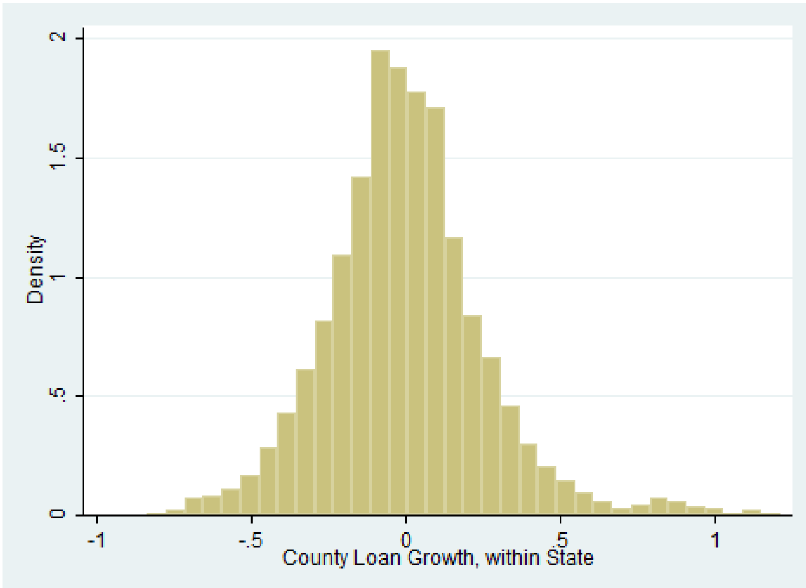
Notes: This figure plots the distance effect on trade flows against the industry-level value-to-weight ratio.

Figure 1.9: Export Dependence, Employment Growth and HH Leverage



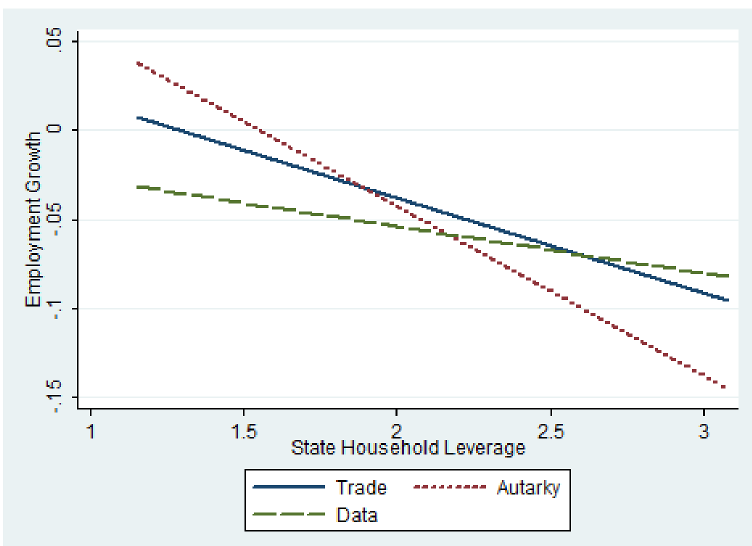
Notes: The graphs shows the coefficients on export dependence from the estimation of equation 1.3, plotted against state-level leverage.

Figure 1.10: County Loan growth, within State



Notes: This figure shows a histogram of loan growth at the county level, demeaned at the state-level.

Figure 1.11: Employment Response in Trade Model, Autarky Model, and Data



Notes: This graph shows fitted regression lines estimating the relationship between state-level household leverage and employment growth. The dashed, solid, and dotted line use employment growth in the data, trade model, and closed economy model, respectively.

Table 1.1: Summary statistics of growth rates and TDS

	Mean	Median	StdDev	10th	90th
Employment Growth 2007-09	-.08	-.08	.16	-.26	.11
Earnings Growth 2007-09	-.06	-.08	.2	-.28	.17
Av. Wage Growth 2007-09	.02	.01	.1	-.08	.12
TDS	1.82	1.75	.35	1.44	2.34

Notes: This table shows summary statistics for variables at the state-industry level: Employment growth, growth of the total wage bill, growth of average wages, and the trade demand shock (as defined in the text).

Table 1.2: Summary statistics of employment and wages in 2007

	Mean	Median	StdDev	10th	90th
Employment 2007	12005	5445	18305	1039	29745
Earnings 2007 (Mil.)	594	246	1057	45	1437
Av. Wage 2007	47220	45044	13371	32910	64152

Notes: This table shows summary statistics for variables at the state-industry level: Employment, total wage bill, and the average wage.

Table 1.3: The Effect of the Trade Demand Shock on Industry Growth

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment 2007-09		Earnings 2007-09		Av. Wage 2007-09	
TDS	-0.090***	-0.095***	-0.115***	-0.135***	-0.025	-0.040***
	(0.027)	(0.017)	(0.032)	(0.023)	(0.021)	(0.014)
Observations	1,519	1,519	1,519	1,519	1,519	1,519
R-squared	0.402	0.568	0.428	0.548	0.232	0.280
Industry FE	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓
Specification	OLS	WLS	OLS	WLS	OLS	WLS

Notes: This table shows results for regressions of industry growth on the trade demand shock. An observation is a state-industry cell. Weighted least squares specifications use 2007 employment levels as weights. Standard errors are clustered at the state and industry level.

Table 1.4: The Effect of the Trade Demand Shock on Manufacturing Industries

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment 2007-09		Earnings 2007-09		Av. Wage 2007-09	
TDS	-0.089***	-0.100***	-0.129***	-0.134***	-0.040**	-0.033
	(0.034)	(0.031)	(0.042)	(0.045)	(0.020)	(0.023)
Observations	793	793	793	793	793	793
R-squared	0.383	0.547	0.401	0.509	0.168	0.283
Industry FE	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓
Specification	OLS	WLS	OLS	WLS	OLS	WLS

Notes: This table shows results for regressions of industry growth on the trade demand shock, restricting attention to manufacturing industries. An observation is a state-industry cell. Weighted least squares specifications use 2007 employment levels as weights. Standard errors are clustered at the state and industry level.

Table 1.5: Effect of transport costs on trade flows in gravity model

	(1)
	Log(Trade Flow)
Log(Distance)	-0.667***
	(0.00748)
Log(Distance) · Log(Value-to-Weight)	0.0803***
	(0.00438)
Constant	5.576***
	(0.117)
Observations	18,594
R-squared	0.798
Origin-Industry FE	Yes
Destination-Industry FE	Yes

Notes: This table shows results for a gravity regression of within-U.S. trade flows.

Table 1.6: Using Variation in Transport Cost to Identify the Trade Channel

	(1) TDS	(2) Employment 2007-09	(3) Earnings 2007-09
TDS-IV	0.752***		
	(0.112)		
TDS		-0.105***	-0.109**
		(0.0295)	(0.0484)
Observations	793	793	793
R-squared	0.938	0.547	0.509
Industry FE	✓	✓	✓
State FE	✓	✓	✓
Specification	WLS	IV-WLS	IV-WLS

Notes: This table shows results for the first and second stage, using the trade demand shock constructed from predicted trade flows as an instrument for the trade demand shock using actual trade flows. The first column shows the first stage relationship, and columns 2 and 3 show second stage relationships, using employment and earnings growth, respectively.

Table 1.7: The external shock

Dependent Variable	(1) Employment 2007-09	(2) Earnings 2007-09	(3) Av. Wage 2007-09
External TDS	-0.070***	-0.113***	-0.043**
	(0.023)	(0.035)	(0.019)
Observations	1,455	1,455	1,455
R-squared	0.566	0.548	0.286
Industry FE	✓	✓	✓
State FE	✓	✓	✓
Specification	WLS	WLS	WLS

Notes: This table shows results for regressions of growth of employment growth and earnings growth at the state-industry level on the external trade demand shock. The external trade demand shock is defined as External $\text{TDS}_i^k = \sum_{n \neq i} \frac{X_{ni}^k}{\sum_{n \neq i} X_{ni}^k} Lev_n$. Standard errors are clustered at the state and industry level.

Table 1.8: Placebo Trade Demand Shock Through Reverse Trade Flows

	(1) Employment 2007-09	(2) Earnings 2007-09	(3) Av. Wage 2007-09
TDS	-0.087***	-0.134***	-0.047**
	(0.023)	(0.033)	(0.023)
Reverse TDS	-0.014	-0.001	0.012
	(0.029)	(0.042)	(0.029)
Observations	1,518	1,518	1,518
R-squared	0.568	0.548	0.281
Industry FE	✓	✓	✓
State FE	✓	✓	✓

Notes: The variable Reverse-TDS conducts a placebo test. It is calculated just like the trade demand shock variable at the state-industry level, but using import flows instead of export flows. That is, $ReverseTDS_i^k = \sum_n \frac{X_{in}^k}{\sum_n X_{in}^k} Lev06_n$, whereas $TDS_i^k = \sum_n \frac{X_{ni}^k}{\sum_n X_{ni}^k} Lev06_n$. All estimations are carried out weighting with 2007 employment. Standard errors are twoway-clustered at the state and industry level.

Table 1.9: Robustness: Internal Shipments Share

	(1) Employment 2007-09	(2) Earnings 2007-09	(3) Av Wage 2007-09
TDS	-0.090**	-0.143**	-0.053
	(0.042)	(0.060)	(0.036)
Observations	1,519	1,519	1,519
R-squared	0.583	0.566	0.321
Industry FE	✓	✓	✓
State FE	✓	✓	✓
State FE × Int. Share	✓	✓	✓
Specification	WLS	WLS	WLS

Notes: This table shows results for regressions of growth of employment growth, earnings growth and wage growth at the state-industry level on the trade demand shock. The estimations control for the share of output shipped internally, interacted with a full set of state fixed effects. Standard errors are clustered at the state and industry level.

Table 1.10: Robustness: Industry-specific Demand Shocks

	(1) Employment 2007-09	(2) Earnings 2007-09	(3) Av Wage 2007-09
TDSI	-0.112***	-0.143***	-0.032**
	(0.015)	(0.022)	(0.012)
Observations	1,519	1,519	1,519
R-squared	0.573	0.551	0.279
Industry FE	✓	✓	✓
State FE	✓	✓	✓
Specification	WLS	WLS	WLS

Notes: This table shows results for regressions of growth of employment growth, earnings growth, and wage growth at the state-industry level on the trade demand shock with industry-specific shocks. It is defined as $TDSI_i^k = \sum_n \frac{X_{ni}^k}{Y_i^k} \frac{Lev_n}{\mu^k} = \frac{TDS_i^k}{\mu^k}$, where μ^k is the relative growth of labor payments of industry k in all of the U.S.. Standard errors are clustered at the state and industry level.

Table 1.11: Differential Effects: Industries producing differentiated vs. homogeneous products

	(1) Employment 2007-09	(2) Earnings 2007-09	(3) Av Wage 2007-09
TDS	-0.061**	-0.086**	-0.026
	(0.025)	(0.037)	(0.026)
TDS $\cdot$ Diff	-0.069***	-0.082**	-0.014
	(0.021)	(0.032)	(0.025)
Observations	793	793	793
R-squared	0.550	0.513	0.283
Industry FE	✓	✓	✓
State FE	✓	✓	✓
Specification	WLS	WLS	WLS

Notes: This table shows results for regressions of growth of employment and earnings at the state-industry level on the trade demand shock and the interaction of the trade demand shock and the index of product differentiation. The first two columns give results for a employment growth, columns 3-4 give results for labor income growth. Standard errors are clustered at the state and industry level.

Table 1.12: Differential Effects: Industries producing durable vs. nondurable products

	(1) Employment 2007-09	(2) Earnings 2007-09	(3) Avg Wage 2007-09
TDS	-0.084***	-0.116***	-0.032
	(0.026)	(0.038)	(0.022)
TDS · Durable	-0.041***	-0.044**	-0.003
	(0.013)	(0.018)	(0.014)
Observations	793	793	793
R-squared	0.550	0.512	0.283
Industry FE	✓	✓	✓
State FE	✓	✓	✓
Specification	WLS	WLS	WLS

Notes: This table shows results for regressions of growth of employment, earnings, and the average wage at the state-industry level on the trade demand shock and the interaction of the trade demand shock and the index of product durability. Standard errors are clustered at the state and industry level.

Table 1.13: County-Level Employment Growth in Tradable Industries

	(1)	(2)
	Tradable Employment 2007-09	
TDS-County	-0.220**	-0.246***
	(0.096)	(0.067)
HH Leverage	-0.013	0.000
	(0.009)	(0.009)
Observations	2,114	2,114
R-squared	0.101	0.189
State FE	✓	✓
Specification	OLS	WLS

Notes: This table shows results for regressions of tradable employment growth at the county-level on the trade demand shock. Tradable industries are defined as industries producing or shipping tradable goods, i.e. manufacturing and wholesale trade. Standard errors are clustered at the state level.

Table 1.14: County-Level Employment Growth in Nontradable Industries

	(1)	(2)
Dependent Variable	Nontradable Employment 2007-09	
TDS-County	-0.019	0.012
	(0.027)	(0.042)
HH Leverage	-0.014***	-0.018***
	(0.004)	(0.005)
Observations	2,114	2,114
R-squared	0.158	0.267
State FE	✓	✓
Specification	OLS	WLS

Notes: This table shows results for regressions of tradable employment growth at the county-level on the trade demand shock. Nontradable industries are defined as all industries except manufacturing and wholesale trade. Standard errors are clustered at the state level.

Table 1.15: Least Squares Results

	(1)	(2)	(3)	(4)
	Employment Trad. 2007-09		Employment Nontrad. 2007-09	
TDS-County	-0.245***	-0.246***	0.002	0.003
	(0.066)	(0.067)	(0.033)	(0.034)
HH Leverage	0.001	-0.000	-0.021***	-0.021***
	(0.009)	(0.009)	(0.005)	(0.005)
Loan Growth	0.009		-0.001	
	(0.015)		(0.005)	
Loan + OLC Growth		-0.006		0.003
		(0.007)		(0.005)
Observations	2,114	2,114	2,114	2,114
R-squared	0.190	0.190	0.396	0.397
State FE	✓	✓	✓	✓
Specification	WLS	WLS	WLS	WLS

Notes: The table shows regression results from OLS regressions of county employment growth in tradables and nontradables. Loan Growth measures loan growth in a county using the outstanding stock of loans of banks that have branches in that county. Loan + OLC Growth measures the growth of outstanding loans plus open lines of credit. Standard errors are clustered at the state level.

Table 1.16: Least Squares Results: Counties Dominated by Large Banks

	(1)	(2)	(3)	(4)
	Employment Trad. 2007-09		Employment Nontrad. 2007-09	
TDS-County	-0.225***	-0.225***	0.008	0.008
	(0.079)	(0.079)	(0.039)	(0.040)
HH Leverage	-0.009	-0.009	-0.024***	-0.024***
	(0.011)	(0.011)	(0.006)	(0.007)
Loan Growth	0.010		0.002	
	(0.023)		(0.007)	
Loan + OLC Growth		-0.001		0.010
		(0.014)		(0.010)
Observations	1,174	1,174	1,174	1,174
R-squared	0.239	0.238	0.457	0.460
State FE	Yes	Yes	Yes	Yes
Specification	WLS	WLS	WLS	WLS

Notes: The table shows regression results from OLS regressions of county employment growth in tradables and nontradables, restricting attention to counties in which large banks account for more than half of the market. Loan Growth measures loan growth in a county using the outstanding stock of loans of banks that have branches in that county. Loan + OLC Growth measures the growth of outstanding loans plus open lines of credit. Standard errors are clustered at the state level.

Table 1.17: Instrument Correlations

Instrument	Construction-Share	Leverage	TDS-County	Deposits-to-Emp
NCF-Share	-0.048***	0.278	0.048	33.259
	(0.014)	(0.316)	(0.029)	(34.300)
NP-Loans	-0.557**	1.281	0.378	712.238
	(0.220)	(3.518)	(0.399)	(618.233)
Illiq-Assets	0.055*	0.300	-0.002	-70.078
	(0.032)	(0.420)	(0.042)	(43.014)

Notes: The table shows regression coefficients from regressions of the column variables on the row variables (i.e. the instruments) and a set of state fixed effects. Each cell corresponds to a coefficient from a separate regression. All regressions are at the county-level, weighted by total county employment. Standard errors are clustered at the state level.

Table 1.18: IV Results for Credit Channel

	(1) Employment Trad. 2007-09	(2) Employment Nontrad. 2007-09
TDS-County	-0.245***	0.009
	(0.063)	(0.036)
HH Leverage	-0.006	-0.012**
	(0.010)	(0.006)
Loan Growth	-0.030	0.063
	(0.054)	(0.039)
Construction Share	0.158	-0.230***
	(0.193)	(0.074)
Observations	2,114	2,114
R-squared	0.182	0.350
State FE	Yes	Yes
Specification	IV-WLS	IV-WLS

Notes: This table shows results for regressions of tradable and nontradable employment growth at the county-level. Tradable industries are defined as industries producing or shipping tradable goods, i.e. manufacturing and wholesale trade. Standard errors are clustered at the state level.

Table 1.19: State-Industry-Level Employment Growth

	(1) Emp Growth	(2) Emp Growth
Loan growth · EFD (IV)	0.032	-0.117
	(0.186)	(0.125)
TDS	-0.088***	-0.093***
	(0.030)	(0.032)
Observations	793	793
R-squared	0.358	0.536
State FE	Yes	Yes
Industry FE	Yes	Yes
Specification	IV-OLS	IV-WLS

Notes: This table shows results for regressions of employment growth at the state-industry level. EFD stands for the index of external finance dependence as in [52].

Table 1.20: Regressions on Model-Generated Data

	(1) Employment Growth	(2) Earnings Growth	(3) Wage Growth
TDS	-0.074***	-0.098***	-0.025***
	(0.008)	(0.010)	(0.003)
Observations	793	793	793
R-squared	0.904	0.934	0.988
Industry FE	✓	✓	✓
State FE	✓	✓	✓
Specification	WLS	WLS	WLS

Notes: This table shows results for regressions of model-generated data on employment, earnings and wage growth on the trade demand shock variable.

Table 1.21: List of Industries in Sample

311	Food Manufacturing
312	Beverage and Tobacco Product Manufacturing
313	Textile Mills
314	Textile Product Mills
315	Apparel Manufacturing
316	Leather and Allied Product Manufacturing
321	Wood Product Manufacturing
322	Paper Manufacturing
323	Printing and Related Support activities
324	Petroleum and Coal Products Manufacturing
325	Chemical Manufacturing
326	Plastics and Rubber Products Manufacturing
327	Nonmetallic Mineral Products Manufacturing
331	Primary Metal Manufacturing
332	Fabricated Metal Product Manufacturing
333	Machinery Manufacturing
334	Computer and Electronic Product Manufacturing
335	Electrical Equipment, Appliance and Component Manufacturing
336	Transportation Equipment Manufacturing
337	Furniture and Related Product Manufacturing
339	Miscellaneous Manufacturing
4231	Motor Vehicle and Motor Vehicle Parts and Supplies Merchant Wholesalers
4232	Furniture and Home Furnishing Merchant Wholesalers
4233	Lumber and Other Construction Materials Merchant Wholesalers
4234	Professional and Commercial Equipment and Supplies Merchant Wholesalers
4235	Metal and Mineral (except Petroleum) Merchant Wholesalers
4236	Electrical and Electronic Goods Merchant Wholesalers
4237	Hardware, and Plumbing and Heating Equipment and Supplies Merchant Wholesalers
4238	Machinery, Equipment and Supplies Merchant Wholesalers
4239	Miscellaneous Durable Goods Merchant Wholesalers
4241	Paper and Paper Product Merchant Wholesalers
4242	Drugs and Druggists' Sundries Merchant Wholesalers
4243	Apparel, Piece Goods, and Notions Merchant Wholesalers
4244	Grocery and Related Product Merchant Wholesalers
4245	Farm Product Raw Material Merchant Wholesalers
4246	Chemical and Allied Products Merchant Wholesalers
4247	Petroleum and Petroleum Products Merchant Wholesalers
4248	Beer, Wine, and Distilled Alcoholic Beverage Merchant Wholesalers
4249	Miscellaneous Nondurable Goods Merchant Wholesalers

Chapter 2

Financial Reforms and Aggregate Productivity: The Microeconomic Channels

2.1 Motivation

Recent research suggests that resource misallocation is an important factor of cross-country differences in productivity and income per worker (Hsieh and Klenow (2009) [31]). Since financial markets play a large role in the allocation of capital, financial repression might lead to economic underdevelopment. In this paper, we argue that financial sector reforms improve resource allocation across firms and lead to higher aggregate productivity and income.

In particular, we study financial market liberalization in 10 Eastern European countries[1] during the late 1990s and early 2000s–a time when these countries drastically reduced government control of the financial sector. State-owned banks, for example, were privatized. Barriers to foreign bank entry were lifted. As a result, banks started making loans based on profitability rather than political connections. We use a large firm-level dataset, including listed and unlisted firms, and estimate the effects of financial reform on resource allocation across firms and aggregate productivity. The main contribution of our paper is to provide the first empirical assessment of the link between financial sector reforms, reallocation of capital across firms, and aggregate productivity.

We find that financial reforms lead to higher industry total factor productivity (TFP). More importantly, we find that this effect is driven entirely by improvements in the within-industry allocation of resources across firms, as opposed to within-firm productivity improvements. Our results show that previously constrained firms that, after reform, are able to increase their market share, drive this more efficient allocation. Before reform, these firms produce at sub-optimal levels with a marginal product of capital (MPK) above the cost of capital. According to our findings, financial reforms allow these firms to borrow more and

[1] Bulgaria, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Russia, and Ukraine.

produce closer to their optimal level, at a point where the gap between MPK and cost of capital is smaller.

The results are not only statistically, but also economically significant. Our estimations, as explained below, can only identify the differential effect of reform on TFP across industries. However, we employ a back-of-the-envelope calculation to trace the aggregate effect. Using a conservative approach, we find that financial reform increases aggregate productivity by 17%.

In order to separate the effects of financial sector reforms from other concurrent reforms, we exploit cross-sectoral differences in external financial dependence (Rajan and Zingales (1998) [51]). Since financial reform alleviates financing constraints, it should benefit particularly firms producing in industries that, for technological reasons, require more external finance. Our identification strategy is based on exploiting the *within-country* variation *across* industries over time. The cross-sectoral comparison deals with reforms that affect all industries uniformly. We also explicitly control for other reforms to deal with potential heterogeneous effects across industries.

We argue that since the reforms were motivated by pressures from outside governing bodies such as the European Union (EU), the International Monetary Fund (IMF), and the Organization for Economic Co-operation and Development (OECD), they are plausibly exogenous to industry productivity developments. For example, during the 1990s, transition economies became candidate countries for EU accession. However, joining the EU required satisfying specific institutional requirements, including a significant reduction of government involvement in the financial sector. Likewise, many countries expressed their commitment to undertake financial sector reforms in exchange for financial support from the IMF.

We start our analysis by estimating firm-level productivity as the residual from a Cobb-Douglas production function. We then calculate industry TFP as the weighted average of productivity of all firms in the industry. Our first set of results indicates that financial reforms increase TFP in industries with high financial needs by 10.5% more than in industries with low financial needs.[2]

Next, to pin down the factor driving productivity gains, we decompose industry productivity into the sum of the average within-firm productivity and the within-industry size-productivity covariance (Olley and Pakes (1996) [48]). Inefficient resource allocation depresses TFP by allowing less productive firms to assume a high market share. This shows up as a low size-productivity covariance. According to our results, TFP gains are driven entirely by an increase in the size-productivity covariance, that is, by an improvement in resource allocation.

We then analyze the channels by which the improved allocation takes place. Firm-level measures of financing constraints are likely to be endogenous. Hence, we exploit cross-sectoral variation in external financial dependence, which is likely to be exogenous, and

[2]High financial needs means an industry at the 75th-percentile of the Rajan and Zingales (1998) [51] financial dependence index (motor vehicles); low financial needs means an industry at the 25th-percentile of the index (dressing of leather).

analyze industry-level measures of resource allocation. According to our findings, financial sector reforms reduce the within-industry variance of the marginal product of capital, particularly in financially dependent industries.[3] This indicates that after reform, previously constrained firms produce closer to their optimal level, with a smaller gap between MPK and cost of capital.

We also find that financial reforms decrease the within-industry covariance between firm productivity and MPK, more so in industries dependent on external finance. Finally, we find that banks play a fundamental role in the capital reallocation process. In particular, financial reforms increase the within-industry covariance between firm debt and MPK disproportionately in industries with high external financial dependence. This indicates that financially constrained firms finance their expansion towards their optimal level through increased bank borrowing.

Related literature. This paper contributes to a growing literature that studies the relationship between finance and economic development.[4] In particular, our paper relates closely to Wurgler (2000) [58], who argues that financial markets improve the allocation of capital. The author finds that financially developed countries increase investment more in their growing industries and decrease investment more in their declining industries. Since the paper uses sectoral data, he can only analyze the allocation of capital across industries. By using firm-level data, we can go one step further and analyze *within-industry* capital allocation.

A recent group of papers has highlighted that misallocation of resources across firms can lead to lower aggregate TFP and income (e.g. Hsieh and Klenow (2009) [31], Bartelsman et al. [6]. We follow these papers in measuring misallocation by the within-industry variance in the marginal product of factors and the within-industry size-productivity covariance. A subset of this literature analyzes the links between finance, misallocation, and productivity through the lens of quantitative models (e.g. Midrigan and Xu (2013) [45], Buera et al. (2011) [12], Moll (2012) [46]. Our paper contributes to this literature by linking concrete financial sector policies to reallocation and aggregate productivity.

Our paper also builds on the literature analyzing the relationship between financial sector repression and financing constraints. Khwaja and Mian (2005) [35] show that state-owned banks give preferential treatment to politically connected firms. They find that connected firms borrow substantially more and exhibit higher default rates than non-connected firms. Bank privatization therefore tightens the link between lending and profitability. Similarly, Giannetti and Ongena (2009) [21] show that foreign banks help to mitigate connected-lending problems and to improve capital allocation. They find that when restrictions to foreign bank entry are lifted, non-connected firms receive more and cheaper loans, while connected firms receive fewer loans at a higher cost.

Finally, our paper identifies the effects of financial reforms by exploiting cross-sectoral variation in external financial needs, as in Rajan and Zingales (1998) [51]. Gupta and Yuan

[3]We assume Cobb-Douglas production functions, so our measure of marginal product of capital is proportional to the average product of capital.

[4]See Levine (2005) [38] for a detailed review of this literature.

(2009) [28] and Levchenko et al. (2009) [37] also follow this methodology and find that financial reform increases output and TFP, particularly in financially dependent sectors. Since these papers use sectoral data, they cannot analyze the factors leading to industry TFP gains. We extend this literature by using firm-level data and providing evidence that industry productivity gains are driven entirely by a more efficient within-industry allocation of capital.[5]

2.2 Analytical framework

This section provides a very simple analytical framework to understand the connections between finance, misallocation, and aggregate productivity. Consider an industry with firms that are heterogeneous in productivity (z) and net worth (a). The cost of capital (r) is given. Each firm produces according to:

$$y = zf(k),$$

where y denotes output, z firm productivity, and k capital. In the first-best, each firm demands capital until the marginal product of capital equals the cost of capital, $MPK \equiv z\partial f/\partial k = r$. The optimal capital demand, $k^*(z)$, is increasing in productivity. Capital is financed in part by net worth and the remainder with bank debt, $b = k^*(z) - a$.

Consider now the case where firm i might be constrained in its ability to demand capital. For example, a firm without political connections might face a limit on the loan it can take, $b < \bar{b}_i$. Alternatively, a non-connected firm might face a relatively high interest rate. In either case, we can write the marginal product of capital as:

$$MPK_i = r + \mu_i \tag{2.1}$$

where μ may be interpreted as either the Lagrange multiplier of a borrowing limit constraint or the interest rate markup. If the firm is constrained ($\mu > 0$), it will produce at a sub-optimal scale with a MPK exceeding the cost of capital.

Next, we define industry TFP as a weighted-average of firm productivities:

$$Z \equiv \sum_i \omega_i z_i,$$

where ω denotes market share of a firm, i.e., $\omega = y/Y$, and Y denotes the industry output. We can further decompose industry TFP into two elements:

$$Z = E[z] + Cov[\omega, z]$$

[5]Galindo et al. (Galindo et al. (2007) [20] and Abiad et al. (2008) [2] use firm-level data to analyze the effect of financial reforms on investment allocation, but they do not make the link to aggregate productivity. Moreover, by exploiting only variation in the timing of reforms across countries, they cannot establish a causal link between reforms and allocation. Finally, they use a small sample of publicly traded firms, which should be the least affected by the reforms.

The first term captures average firm productivity. The second term, which we call *size-productivity covariance*, captures how well existing capital is allocated across firms. A large covariance means that more productive firms have a higher market share, which indicates a more efficient allocation of resources.

In this setting, a financial reform is a policy that either relaxes the constraint on borrowing or directly equalizes interest rates across firms. The simple model delivers a series of testable implications.

Prediction 1. *A financial reform increases the size-productivity covariance leading to higher total factor productivity.* Since the borrowing constraint becomes less binding, productive firms can borrow more and expand towards their optimal level. More capital is allocated towards more productive firms, so the size-productivity covariance increases. Since firm productivity is fixed by assumption, this leads to an increase in industry TFP.

Prediction 2. *A financial reform reduces the covariance of the marginal product of capital across firms.* When the borrowing constraint is binding, MPK is not equalized across firms. Financial reform alleviates the constraint and allows constrained firms to produce closer to their optimal level. This closes the gap between marginal product of capital and the cost of capital, reducing the dispersion in MPK.

Prediction 3. *A financial reform reduces the covariance between the marginal product of capital and productivity.* In a perfect capital market, the covariance between the MPK and productivity is zero, since MPK is equal to the cost of capital for all firms. In the presence of borrowing constraints this covariance will be positive. Consider two firms whose capital input is restricted by $\bar{k}$, which is lower than the optimal level. The two firms only differ in productivity levels. For a given capital input $\bar{k}$, the MPK is increasing in productivity. The borrowing constraint will thus generate a positive covariance between productivity and the MPK. A financial reform, by relaxing the borrowing constraint, should bring this covariance closer to zero.[6]

Prediction 4. *A financial reform increases the covariance between the marginal product of capital and debt.* As with the previous measure, this covariance should be zero in an economy with a perfect capital market. If firms are restricted in the amount they can borrow (i.e., low level of debt), they will produce at a sub-optimal level with a high MPK. The covariance between debt and the MPK will therefore be negative. Financial reform allows constrained firms to borrow more, bringing this covariance closer to zero.

The analytical framework developed in this section is deterministic and static. In Section 2.6, we explore the consequences of adding uncertainty and dynamics to the framework.

[6]This measure is related to a point made by Restuccia and Rogerson (2008) [54]. They find that the "wedges" between marginal products and factor costs have to be positively related to productivity in order to generate sizable productivity losses.

2.3 The reforms

Reform process

Starting in the early 1990s, Eastern European countries undertook dramatic reforms in their transition from centrally planned to free market economies. Financial sector liberalization was a key component of the second phase of the transition, which was designed to be market deepening.[7] State-owned banks were privatized. Ceilings on interest rates and credit controls were lifted. Barriers to foreign bank entry and foreign capital flows were reduced.

Our sample consists of eight Eastern European EU member countries (Bulgaria, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, and Romania) together with Russia and Ukraine. The composition of the sample is driven by data availability.[8] Importantly, *external* pressures from outside governing bodies such as the EU, the IMF, and the OECD induced financial sector reforms in these countries. For example, to join the EU, a state needs to fulfill economic and political conditions summarized in the Copenhagen criteria. These economic criteria require a sizable reduction of government intervention in the financial sector and had a substantial influence on policies in transition economies. This external pressure from the EU emerged as a dominant factor for domestic policymaking.[9]

In their request for financial support from the IMF, many countries expressed their commitment to undertake financial sector reforms.[10] Finally, three of the countries examined (Czech Republic, Hungary, and Poland) became members of the OECD. The OECD also imposed pressure on its prospective members to eliminate capital controls.[11]

Reform data

The data on financial sector reforms comes from Abiad et al. (2010) [1]. The authors create a time-varying index of financial reform that measures the removal of government control of the financial sector. Recognizing the multifaceted nature of financial reforms, the index is an aggregation along seven dimensions: (1) credit controls, (2) interest rate controls, (3) bank entry barriers, (4) state ownership of banks, (5) capital account restrictions, (6) prudential bank regulations, and (7) securities market policy.

[7] The first phase of transition consisted of market-enabling reforms (such as privatization and price liberalization) while the third phase consisted of market-sustaining economic reforms (such as competition policy). See EBRD (2007) [16] for details.

[8] The coverage of additional Eastern European countries in our firm-level dataset, Amadeus, is extremely poor.

[9] Schimmelfennig and Sedelmaier (2004) [56] argue that "The credibility that the EU will reward rule adoption with membership . . . emerges as the most important factor influencing the cost-benefit calculations of CEEC governments . . . the fulfillment of EU acquis conditions became the highest priority in CEEC policy-making, crowding out alternative pathways and domestic obstacles."

[10] For example, Bulgaria's 1998 letter of intent stated, "Our priorities in the structural areas are to complete privatization of the state banks and enterprises and to develop and deepen financial markets."

[11] According to a number of former officials interviewed, the process of eliminating capital controls was largely driven by a country's prospective accession to the OECD (IMF (2005)[32]).

The first item measures the extent of credit controls and excessively high reserve requirements for banks. Additionally, the government can control interest rates, either setting them directly or letting them fluctuate within a band. Next, the government may restrict the entry of foreign banks entirely or control the credit market through direct ownership of banks. Capital account restrictions measure direct transaction taxes and restrictions on capital inflows and outflows. Prudential banking regulations include whether the banking supervisory agency is independent from the executive's influence. The last category focuses on government policies towards debt and equity markets.[12]

Along each dimension, a country is given a final score on a graded scale from zero to three, with zero corresponding to the highest degree of repression and three indicating full liberalization. The index therefore ranges from 0 to 21. Figure 2.1 plots the cumulative change of the financial reform index for the 10 transition economies in our sample.[13]

Among all countries, Ukraine presents the highest level of financial repression during this period. In 2005, Ukraine's index score of reform was less than 70% of the maximum achievable level. Estonia, on the other hand, exhibits the least government intervention in the financial sector. Hungary is the economy that most rapidly liberalized its financial market during this time period. Its reform index score almost doubled between 1994 and 2005.

In many cases, a financial reform (defined as a change in the aggregate financial reform index score of a country) involves policy changes along several dimensions. We therefore make no attempt to disentangle their effects and only estimate an average return to financial reform. Nevertheless, we highlight that the privatization of state-owned banks and the elimination of restrictions on foreign bank entry constituted a fundamental pillar in all reforms episodes.

2.4 Empirical strategy and data

Identification strategy

Our empirical analysis faces two main endogeneity problems. First, there may be a problem of omitted variables. During the transition period, these countries undertook several other economic reforms, including trade reforms, competition reforms, and others. If these reforms occurred at the same time as financial reforms, they might confound our estimates. To disentangle the effects of a financial reform from other reforms, we exploit cross-sectoral variation in external financing needs (Rajan and Zingales (1998) [51]). Since financial sector reforms alleviate financial frictions, they should benefit particularly firms in industries that inherently need a lot of external finance to produce. To the extent that other reforms do

[12] In the Appendix, we show that the results do not change if we exclude each individual reform component and re-estimate the main estimations.

[13] Our sample spans the years 1994 and 2005 as the Amadeus firm-level dataset begins in 1994 and the financial reform data ends in 2005.

not change industry productivity differentially according to the industry pattern of external financial dependence, their effect will be canceled out with the cross-sectoral comparison.

However, there might be other reforms that affect industries differentially according to external financial needs. We address this issue by controlling explicitly for the interaction of a large set of observable reforms with external financial dependence. In particular, we use the reform indicators published by the European Bank for Reconstruction and Development (EBRD (2005) [16]): competition policy, infrastructure, price liberalization, privatization, restructuring, and trade.[14] Our identification assumption is that after controlling for these additional reforms, there are no other policies that increase productivity particularly in financially dependent industries.

The second threat to identification comes from a reverse causality concern. If industries that depend strongly on external finance experience high productivity growth, they may lobby for financial reform to accommodate their capital needs. We think this argument is not a concern for three reasons. First, as discussed in the previous section, pressures from external organizations such as the EU, IMF, and OECD drove reforms. It is therefore unlikely that the decision to undertake reform was led by industry-specific developments. Second, in order to successfully lobby for financial reform, an industry would need to exhibit a high level of political strength. We identify industry political strength by looking at the concentration of output. If an industry is highly concentrated, it is easier for firms to coordinate and lobby the government. We use two measures of industry concentration: market share of the four largest firms and the Herfindahl index.[15] Figures 2.2 and 2.3 show that neither measure of concentration is correlated with external financial dependence. This suggests that financially dependent industries do not exhibit a higher degree of political strength than other industries.

Finally, the argument that increased productivity leads to reform would only seem reasonable if individual firms becoming more productive drove industry productivity growth. If industry TFP growth is coming from improved resource allocation across firms, the reform creates losers and winners within an industry. Coordinated lobbying for reform seems then very unlikely. According to our results, the main source of productive growth is a more efficient allocation of resources. This is a final reason that makes the reverse causality argument very unlikely to apply.

External financial dependence

Our measure of dependence on external finance comes from Rajan and Zingales (1998) [51]. It is defined as capital expenditures minus cash flow from operations divided by capital

[14] Competition policy summarizes efforts targeted at lowering entry restrictions. The infrastructure component captures the degree of private sector involvement in utilities and transport. Price liberalization measures the share of administratively set prices. Privatization summarizes the degree of state ownership in enterprises. Governance restructuring refers to actions designed to promote corporate governance. Trade reform captures changes in import and export restrictions.

[15] Both measures are computed at the beginning of the sample.

expenditures. For technological reasons, some industries require more external finance than others. Industries that operate in large scales, with long gestation periods or high R&D, will tend to be highly dependent on external finance. The authors construct a financial dependence index using the median of financial dependence for US publicly traded firms in each manufacturing industry. Table 2.1 reports the index for the 22 two-digit manufacturing industries analyzed in our paper. Industries with low financial dependence include tobacco and textiles; industries with high dependence include machinery and professional equipment. This lines up with the intuition that high external dependence is derived from large minimum scales or high product development requirements.

In this paper we extrapolate the US-based financial dependence measure to transition economies based on the assumption that the sectoral technological differences persist across countries. Note that measuring financial dependence with large publicly traded US firms has the advantage of capturing the true demand component for external finance, since these firms should be largely unconstrained. In addition, for identification, we do not require each country to have the same value of financial dependence in each sector. We only require that the *ranking* of financial dependence across sectors be the same in each country. Also, since we use a proxy for the true value of financial needs, we introduce measurement error that might result in attenuation bias, which could lead to underestimate the true effect. Finally, in Section 2.6 we show that our results are robust to using asset intangibility as an alternative measure of industry financial constraints.

Firm-level dataset

The firm-level data we use comes from Amadeus. Amadeus is a commercial dataset provided by Bureau van Dijk (BvD). It contains financial information on millions of publicly traded and private firms across Western and Eastern European countries. BvD collects data from local information providers, which in most cases are the local company registers. The dataset comes in yearly versions and each vintage includes up to 10 years of information per firm.[16] If a firm has stopped filing, it is kept in the dataset for four subsequent years and then deleted. This creates a survivorship bias. For our study, it is essential to follow firms for consecutive years. We overcome this bias by appending two versions (2006 and 2002) of the dataset. Firms that exited prior to 2002 and were deleted in the 2006 version of the dataset are reported in the 2002 vintage and are, therefore, included in our appended dataset.

Following the literature on productivity and misallocation, we focus exclusively on manufacturing firms.[17] We clean the dataset based on our main variables of interest, firm TFP and MPK and their respective growth rates. First, we delete all observations with clearly wrong

[16] Amadeus series are available in current US dollars. We deflate all series to 2000 US dollars using producer price indices.

[17] It is harder to conceptualize and measure productivity outside manufacturing, since service industry firms typically provide a wide variety of different services. Moreover, in our setting the filing requirements exclude the smallest firms. This should bias the selection of firms against service industries, since manufacturing typically requires a larger minimum scale.

values, i.e., observations with negative values for assets, revenue, or employment. Second, we pool all data and trim the 1st and 99th percentile of the four variables. If data quality varies across countries, this step will produce cleaner data for those countries with more outliers. Next, we repeat this procedure within each country-year cell, to ensure that we do not miss outliers in countries with relatively higher quality data. Finally, we delete all observations with missing data for any of these four variables. We are left with roughly 470,000 observations for 135,000 companies from 1995 to 2005.[18] Table 2.2 reports the coverage of firms for the 10 transition economies.

The differences in the number of firms across countries is due to varied filing requirements for companies. In most cases, these filing requirements are related to size criteria or to the mode of incorporation.[19] The large number of Romanian firms is attributed to the exceptional coverage of small firms: 60% of the observations in Romania come from firms with 10 or less employees, while this fraction is only 20% on average in all other countries.[20]

Due to the nature of the filing requirements, we are unable to capture entry or exit if entrants are either too small to meet the filing requirements or if they start their business in a mode of incorporation that excludes them from the filing requirement. Similarly, we cannot distinguish between firms that exited the market and firms that fell below the size restrictions for filing or changed their mode of incorporation. Therefore, in this paper we are not able to provide a detailed analysis of the extensive margin of reallocation.

Amadeus includes a large number of small and medium-sized enterprises (SMEs). Since financial frictions are particularly binding for smaller firms, this represents a distinct advantage over datasets that only contain listed companies (e.g., Worldscope). Table 2.3 reports the distribution of employment across firms in different size bins. The two bottom rows compare the average across countries in Amadeus with data on the universe of firms from Eurostat.

Productivity measurement and decomposition

We assume Cobb-Douglas production functions and measure firm-level TFP residually:

$$\log(z)_{ist} = \log(y)_{ist} - \alpha_s^k \log(k)_{ist} - \alpha_s^l \log(l)_{ist}, \tag{2.2}$$

where i denotes a firm, s a sector, and t a year. z corresponds to firm TFP, y to revenues, k to fixed assets, and l to number of employees.[21] We omit a subscript c for country in this section to keep the notation simple.

[18] In principle, the survivorship bias of the data also applies to the pre-1998 data. When we restrict the estimations to the time period 1998-2005, however, our results remain unchanged.

[19] For instance, in Bulgaria, all companies that match at least two out of the following three criteria have to file: at least 50 persons staff, total assets of at least EUR 500,000, or turnover at least EUR 1,000,000. In Hungary, all companies except private entrepreneurs have to file records.

[20] To verify that a single country is not driving the results, in the Appendix we exclude each country individually and re-estimate our core estimations. The main results remain unchanged.

[21] Our TFP measure is a measure of revenue productivity, since we don't have physical output data. Similarly, we only observe the marginal revenue product of capital. The difference between revenue and

Estimating the factor elasticities is challenging because unobserved productivity shocks may affect factor demands, which leads to simultaneity bias. Olley and Pakes (1996) [48] and Levinsohn and Petrin (2003) [39] deal with this issue by employing a control function approach, using investment and intermediate inputs, respectively. However, these methods assume that factor prices are equalized across firms, so that there is a unique mapping from productivity to investment/intermediate inputs. Since we are inherently worried about firms facing different factor prices, we refrain from a control function method to estimate the factor elasticities.

Instead, we back out factor elasticities from industry-level income shares. Under the assumption of constant returns to scale, we can measure the labor elasticity for each industry as the average labor share of value added across all countries and years, i.e. $\alpha_s^l = (wl/va)_s$. Because factor prices may also differ across industries and distort the labor income shares, we use data from the US[22] Having inferred the factor shares, we measure firm TFP from equation (2.2). In section 2.6, we show that our results are robust to using labor productivity instead of TFP as a measure of firm productivity.

Next, we define industry productivity as a weighted average of firm-level productivities:

$$\log(Z_{st}) = \sum_i \omega_{ist} \log(z_{ist}),$$

where ω_{ist} is the share of revenues of firm i in total revenues of sector s. We use the Olley and Pakes (1996) [48] methodology to decompose industry productivity into two components:

$$\log(Z)_{st} = \overline{\log(z)}_{st} + \sum_i (\omega_{ist} - \overline{\omega}_{st})[\log(z)_{ist} - \overline{\log(z)}_{st}],$$

where $\overline{\omega}$ and $\overline{\log(z)}$ denote the unweighted mean share and unweighted mean (log) productivity. The first component measures the average within-firm productivity. The second component measures the within-industry *size-productivity covariance*. A large covariance means that a higher share of production is allocated to more productive firms. This indicates a more efficient allocation of resources.

Allocation measures based on marginal products

Under the assumption of a Cobb-Douglas technology, the marginal product of capital is proportional to its average product. We calculate the log MPK as:

$$\log(MPK)_{ist} = \alpha_s^k + \log(y)_{ist} - \log(k)_{ist}$$

physical measures has been pointed out in recent literature (Foster et al. (2008) [18]). While it would be preferable to have a physical measure of TFP when we look at changes in sector-level productivity, a revenue-based measure is the correct variable when we study the dispersion of marginal products.

[22] Following Hsieh and Klenow (2009) [31], we use data from the NBER Manufacturing Industry Productivity Database, which is based on the Census and Annual Survey of Manufacturers (ASM).

We calculate the within-industry variance of MPK as a measure of the intensity of distortions in the capital market and denote it by $VMPK_{st} = Var_{st}(\log MPK_{ist})$. The calculations for the marginal product of labor (MPL) are analogous. We will also analyze the effect of financial reform on the within-industry covariance between MPK and firm TFP and the within-industry covariance between MPK and firm debt.

Table 2.4 shows basic summary statistics for the main variables at the country-sector-year level. Note that on average, the VMPK exceeds VMPL, suggesting a larger degree of capital misallocation in the economy relative to labor. The other sample moments have the expected signs. The covariance between the MPK and firm TFP within an industry is positive. This is in line with the idea that, given a firm's borrowing constraint, the firm will be more constrained (higher MPK) the greater its productivity. Finally, the covariance between the MPK and the level of debt is negative. Intuitively, a constrained firm produces below its optimal scale with a relatively high MPK due the limit it faces on borrowing.

Figure 2.4 shows the relationship between the degree of capital misallocation (measured by the variance of the MPK) and external financial dependence in a cross-section of industries.[23] The graph depicts a strong positive relationship between the two measures. In an economy with financial repression, capital misallocation is more severe in industries that rely more heavily on external finance.

2.5 Main results

Aggregate results

We start by analyzing the effects of financial reform on aggregate outcomes. While this analysis entails several endogeneity issues, it provides a useful first reference point. To do so, we collect data on real GDP per capita (PPP) and total investment from the Penn World Tables and total employment from the World Development Indicators. We calculate the capital stock from investment through the perpetual inventory method.[24] For simplicity, we compute TFP residually from a Cobb-Douglas aggregate production function with factor shares of 0.3 and 0.7 for capital and labor.

We then consider the following specification:

$$\log(Y)_{ct} = \alpha Ref_{ct} + X_{ct}'\beta + \eta_c + \eta_t + \epsilon_{ct}, \tag{2.3}$$

where Y measures either GDP, employment, capital, or TFP of country c in year t. The variable Ref_{ct} denotes the financial reform index, which ranges from 0 to 21. The vector X_{ct} includes the other reforms from the EBRD transition indicators. The specification includes country (η_c) and year (η_t) fixed effects. Country fixed effects control for all time-invariant

[23] To arrive at an industry measure of $VMPK_{cst}$, we average all observations across countries and time. That is, the graph shows $\overline{VMPK}_s \equiv \frac{1}{N_s}\sum_c \sum_t VMPK_{cst}$ on the vertical axis.

[24] For some countries, we have relatively few observations of investment, so it is likely that the capital stock is measured with error. We employ a depreciation rate of 6%.

country characteristics and year fixed effects for global shocks that affect all countries equally. The parameter of interest is α, which is identified from the variation in the timing of reforms across countries. It estimates the time change in the outcome variable (e.g., TFP) in a reforming country relative to the same change in a country not reforming at that time. The standard errors are clustered at the country level.[25]

Results are reported in Table 2.5. The effect of financial reform on GDP is positive and significant at the 5% level, driven by an increase in aggregate productivity, not by factor accumulation. To interpret the magnitude of the effect, consider a country implementing an averaged-sized reform, which we define as an increase in the financial reform index score by two units.[26] According to the results, financial reform is associated with an increase in GDP of 4% and an increase in aggregate TFP of 3.5%.

Industry effects

To analyze the causal effect of the reform, we exploit cross-sectoral variation in external financial needs and estimate:

$$\log(Y)_{cst} = \alpha Ref_{ct} \cdot EFD_s + \beta X_{ct} \cdot EFD_s + \eta_{ct} + \eta_{cs} + \eta_{st} + \epsilon_{cst}, \tag{2.4}$$

where Y_{cst} denotes either output, capital, employment or TFP of country c in sector s in year t. Ref_{ct} is the financial reform index. EFD_s measures sector s's external financial dependence. X_{ct} is a vector containing other transition reforms. The coefficient of interest is α and is identified from the *within-country* variation *across* sectors. It estimates the time change in the outcome variable (e.g., TFP) in an industry with high financial dependence in a reforming country relative to the same change in an industry will low financial dependence within the same country.

The specification includes a full set of country-year (η_{ct}), country-industry (η_{cs}), and industry-year (η_{st}) fixed effects. Country-year effects control for country-specific time trends, country-industry effects control for country-specific industry characteristics, and industry-year effects control for industry-specific time trends. By including country-industry and country-year effects, we exploit the variation across industries within a country over time. Adding sector-year fixed effects ensures that our estimates are not driven by global shocks to financially dependent industries (such as global technology shocks) whose timing might coincide with the reform. The standard errors are clustered at the country level.[27]

The results are presented in Table 2.6.[28] Financial reform increases output and TFP relatively more in industries with high financial needs, while there are no statistically significant effects on either capital or employment. The absence of labor and capital movements

[25] This allows any serial correlation within a country across time.

[26] The standard deviation of the financial reform index is 2.3.

[27] This allows for any correlation across industries within a country and any serial correlation within a country across time. In the Appendix, we show that the results are robust to clustering at the country-industry level, at the year level, at both the country and year level, and to block bootstrapping.

[28] To ensure that our results are not driven by some industries with very few observations, we restrict our analysis to only those country-industry-year cells with more than 10 observations.

to industries with higher dependence might reflect the (short-run) inter-sectoral immobility of factors. In terms of labor, it takes time to release workers, train them for jobs in other industries, and re-hire them. In terms of capital, banks may have pre-set diversification targets across industries. In that case, a financial reform would only induce them reallocate loans across firms within an industry, and not across industries.

The point estimate implies that an average-size reform increases output in the 75th-percentile industry by financial dependence (motor vehicles) by 12% more than in the 25th-percentile industry (dressing of leather).[29] The differential effect on TFP across industries is 10.5%.

We can provide a graphical depiction of the results by estimating the industry-specific effects of the reform on productivity:

$$\log(TFP)_{cst} = \sum_s \alpha_s Ref_{ct} \cdot D_s + \beta X_{ct} \cdot EFD_s + \eta_{ct} + \eta_{cs} + \eta_{ts} + \epsilon_{cst},$$

where D_s is a dummy variable for each sector that is equal to one if the sector is s and zero otherwise. Figure 2.5 plots the estimated coefficients $\hat{\alpha}_s$ against the index of external financial dependence. The figure shows that the linear functional form imposed in equation (2.4) is a good approximation. It also shows that the estimated effect is not driven by any particular sector.

Productivity decomposition

To analyze the factor driving industry TFP gains, we decompose industry productivity into the sum of the average within-firm productivity and the within-industry size-productivity covariance. We then analyze the effect of financial reform on each of the two components. We employ the same specification as in equation (2.4) but use the efficiency and allocation terms as the dependent variables.

Table 2.7 reports the results. We find that financial reform increases the within-industry allocation term disproportionately in financially dependent industries. However, there is no significant effect on the within-firm productivity term. In other words, industry TFP gains are driven entirely by a more efficient allocation of resources.

Note that the within-firm productivity component simply captures the average productivity of firms within a country-sector-year cell. It could therefore increase if either individual firms become more productive or if the set of observed firms changes due to entry and exit. To rule out the latter possibility, we analyze the effect of financial reform on the productivity of the *same* firm by employing a firm-level regression with firm fixed effects:[30]

$$\log(z_{csit}) = \alpha Ref_{ct} \cdot EFD_s + \beta X_{ct} \cdot EFD_s + \eta_{ct} + \eta_{st} + \eta_i + \epsilon_{csit}$$

[29] The differential effect of the reform is calculated as $\hat{\alpha} \cdot 2 \cdot (EFD_h - EFD_l)$.

[30] As shown before, our sample sizes differ considerably across countries. An unweighted firm-level regression would give excessive weight to the countries with large sample sizes. To avoid this problem, and to make the results of the firm-level estimations comparable to the ones at the industry level, we weight each observation by the inverse of the number of firms in the given country-industry-year cell.

Here z_{csit} denotes productivity of firm i in country c in sector s in year t. The coefficient of interest is α, which is identified from the *within-firm* variation across time. It estimates the time change in productivity within a firm in an industry with high external dependence, relative to the same change within a firm in an industry with low dependence. The results are reported in column (1) of Table 2.8. They confirm that financial reform had no significant effect on individual firm productivity.

Likewise, the size-productivity covariance could increase if either the allocation becomes more efficient or if productivity in large firms increases more than in small firms. To deal with this potential concern, we estimate:

$$\begin{aligned}\log(z_{csit}) =& \alpha_1 Ref_{ct} \cdot EFD_s + \alpha_2 Ref_{ct} \cdot Large_{csit-1} + \alpha_3 Ref_{ct} \cdot EFD_s \cdot Large_{csit-1} \\ &+ \beta X_{csit} + \eta_{ct} + \eta_{st} + \eta_i + \epsilon_{csit},\end{aligned}$$

where $Large_{csit-1}$ is a dummy equal to one for firms above the median of employment size in a country-industry-year cell and zero otherwise. The vector X_{csit} includes the set of interactions between reform, external finance dependence, and firm size for the other reform indicators. The coefficient of interest is α_3, which estimates the time change in productivity of large firms relative to small firms within industries with high dependence, and compares it to the same relative change within industries with low dependence. Column (2) of Table 2.8 presents the results. The coefficient is not statistically different from zero. This shows that larger firms in financially dependent sectors experiencing faster productivity growth cannot explain the increase in the allocation term.

Forces behind reallocation

Next, we analyze the channels by which financial reform leads to better allocation of resources and productivity. We first focus on the within-industry dispersion in the marginal product of factors across firms.[31] We use the empirical specification (2.4), now using the variance of the MPK and the MPL as dependent variables.

While the variance of the MPK is expected to fall after financial reform, the effect on the variance of the MPL is ambiguous. On the one hand, reform may increase the variance of MPL if labor market rigidities prevent labor to reallocate from less productive to more productive firms. On the other hand, financial frictions may also extend to labor if employing workers requires some upfront cost. In that case, the variance of the MPL may fall after reform.

The estimation results are reported in Table 2.9. Financial reform decreases the variance of the marginal product of capital particularly in industries with high external financial dependence (column (1)). Reform has no differential effect on the variance of the MPL across industries (column (2)). According to the point estimate, an average-sized reform

[31]Hsieh and Klenow (2009) [31] derive an expression that links industry TFP directly to the variance of the marginal products of factors.

lowers the variance of the MPK by 10.6% more in financially dependent industries. This is a sizable effect, amounting to 10% of the average sample variance.

Next, we turn to the covariance between marginal products and firm-level productivity. In the presence of financial frictions, the covariance between MPK and firm TFP is positive since more productive firms are constrained and must produce with a larger gap between MPK and cost of capital. Table 2.10 reports the results. According to column (1), financial reform reduces the covariance between MPK and firm TFP disproportionately in financially dependent industries. From column (2), we can see that there is no significant effect on the covariance between the MPL and firm TFP.

Finally, if financial reform makes financing frictions less severe, constrained firms should be able to borrow more and produce with a lower gap between MPK and cost of capital. We analyze this channel by studying the evolution of the covariance between the marginal product of factors and firm debt. The results of these estimations are reported in Table 2.11. The results in column (1) show that reform increases the covariance between MPK and debt covariance particularly in industries dependent on external finance. Again, there is no significant effect on the covariance between MPL and debt (column (2)).

2.6 Additional results

In this section, we present a series of additional exercises that further support the main results.

Firm-level evidence on reallocation

The industry evidence suggests that financial reform allows constrained firms to produce closer to their optimal level, with a MPK closer to the cost of capital. In this section, we analyze this channel directly using firm-level information. Since financing constraints are binding when there is a gap between MPK and cost of capital, we proxy financing constraints by the lagged value of MPK. While this measure is likely to be endogenous, we estimate a specification that addresses the most urgent endogeneity concerns:

$$\begin{aligned}\log(Y)_{csit} = {} & \beta_1 MPK_{csit-1} + \beta_2 Ref_{ct} \cdot EFD_s + \beta_3 EFD_s \cdot MPK_{csit-1} + \beta_4 Ref_{ct} \cdot MPK_{csit-1} \\ & + \beta_5 Ref_{ct} \cdot EFD \cdot MPK_{csit-1} + \gamma X_{csit} + \eta_{ct} + \eta_{st} + \eta_i + \epsilon_{csit},\end{aligned}$$

where Y denotes either MPK, market share, or debt of firm i. The coefficient of interest is β_5. It estimates the time change in the outcome variable (e.g. MPK) in firms with high pre-reform MPK relative to firms with low pre-reform MPK within industries with high financial dependence, and compares it to the same relative change within industries with low dependence.

Suppose that firms face productivity shocks before financial reform, and that firms cannot adjust their capital stock immediately. In that case, the productivity shocks will generate cross-sectional dispersion in MPK pre-reform. If productivity shocks are persistent, then firms that received a positive productivity shock before the reform will want to expand their capital stock (possibly financed by new debt) and increase their market share. Thus a firm with high pre-reform MPK might produce after reform with a lower level of MPK for reasons unrelated to financial liberalization. Under the assumption that this situation is not particularly strong in industries with high external finance dependence, our cross-sectoral comparison deals with this problem.

Our industry-level analysis suggests that financial reform allows constrained firms to increase their market share through more borrowing and to produce closer to their optimal level. We therefore expect β_5 to be negative when we look at the MPK, and positive when we study the behavior of the market share and debt. Results are presented in Table 2.12. The sign of the β_5 coefficient is as expected in all three estimations. Constrained firms (i.e., firms with a high pre-reform MPK) can take on new debt in order to finance investment, which reduces their post-reform MPK.

Wages

Next, we study the effect of financial reform on industry-level wages. The idea is that financial reform alleviates financial frictions allowing constrained firms to demand more capital. Since labor complements capital, the reform also increases labor demand and should increase wages in equilibrium. If labor is not fully mobile across industries, the reform can have a differential effect on wages across sectors. Higher wages constitute a general equilibrium mechanism that induces large unproductive firms to reduce their size. We construct firm wages as the ratio between the wage bill and the number of employees, and then use output shares to aggregate to the industry level. We employ the specification of equation (2.4) using the log of industry-level wages as the dependent variable. Results are reported in Table 2.13.[32]

The results show some evidence consistent with this general equilibrium effect. According to the point estimate, financial reform increases wages in industries with high financial dependence by 7% more than in industries with low dependence. This is of a similar magnitude as the differential change in productivity across industries, although it suggests that the pass-through of productivity changes to wages is not complete.

Uncertainty and risk premia

Uncertainty. The analytical framework developed in Section 2.2 is deterministic and therefore abstracts from any issues related to uncertainty. Consider an economy where productivity is stochastic and firms are risk-neutral. In the first-best, a firm demands capital until the point where the *expected* marginal product of capital equals the cost of capital. In the data, we only observe the ex-post MPK, which can be expressed as the sum of an expected

[32]Due to lower data availability for the wage bill (only available in eight out of 10 countries), the number of country-industry-year observations drops by approximately 20%.

component and an innovation term:

$$MPK_i = E[MPK_i|\Omega_{i,-1}] + \nu_i,$$

where ν_i is a random error term and $\Omega_{i,-1}$ denotes the information set for firm i in the previous period. Therefore, even in the first-best, where $E[MPK_i|\Omega_{i,-1}] = r$, the ex-post MPK is not equalized across firms. If the economy exhibits uncertainty and financial frictions, equation (2.1) becomes $E[MPK_i] = r + \mu_i$. As a result, the ex-post MPK can be re-expressed as:

$$MPK_i = r + \nu_i + \mu_i$$

We have shown that financial reform reduces the dispersion in ex-post MPK, which we have interpreted as a reduction in financing frictions (lower variance in μ_i). However, the results might be driven by a reduction in the dispersion of risk (lower variance in ν_i). To rule out this alternative, we decompose ex-post MPK into its two elements and analyze the effect of financial reform in the cross-sectional variance of the two elements. We assume a linear functional form for the conditional expectation:

$$E[MPK_{it}|\Omega_{it-1}] = x'_{it-1}\gamma_t,$$

where x_{it-1} includes a large range of firm-level variables in period $t-1$.[33] We then estimate the following specification for each country-industry-year cell:[34]

$$MPK_{it} = x'_{i,t-1}\gamma_t + \nu_{it}$$

Next, we extract the predicted and residual components and calculate their variances. We use these variances as dependent variables in the same specification as in equation (2.4). Table 2.14 reports the results.[35] Column (1) shows that the variance of the MPK remains unchanged. Since we can only do the decomposition for firms with available information in the previous period ("stayers"), we re-calculate the variance of the MPK only for stayers. Column (2) presents the results for this sub-sample, showing only a minor change in the coefficient. Finally, columns (3) and (4) present the results for the two components of the variance of ex-post MPK.[36] According to the results, our results are not driven by a reduction in the variance of risk.

Risk premia.[37] With risk-averse firms, the ex-post marginal product of capital becomes:

$$MPK_i = r + \nu_i + \mu_i + \theta_i,$$

[33] In particular, we include lagged values of MPK, revenue, TFP, debt, market share, and the share of current debt in total debt. We also analyzed alternative specifications and found similar results.

[34] Since the equation is estimated separately for each country-industry-year cell, there is no need to include reform indicators. Their effect will be captured by the regression constant.

[35] Since we can only consider firms with available lagged information, we lose the first year for each country-industry observation, which reduces sample size.

[36] Since OLS imposes that $Cov(x'\hat{\gamma}, \hat{\nu}) = 0$, we have that $Var(MPK) = Var(x'\hat{\gamma}) + Var(\hat{\nu})$. Therefore, the coefficients of columns (3) and (4) have to add up the coefficient of column (2).

[37] We thank Matteo Maggiori for his suggestions on how to deal with risk aversion in our framework.

where θ_i denotes risk premium of firm i. An additional concern is that the reduction in the variance of ex-post MPK might be driven by a reduction in the variance of risk premia. We argue that this is unlikely since the cross-sectional variance of risk premia is an order of magnitude lower than the variance of MPK.

We consider a very simple one-factor production model. For each firm i we run the following time-series regression:

$$MPK_{it} = a_i + \beta_i M_t + \eta_t, \quad for \quad t = 1, ..., T$$

where M_t is the average MPK across firms, i.e., $M_t \equiv \frac{\sum_i MPK_{it}}{N_t}$. With the estimates of $\hat{\beta}_i$, we run the following cross-sectional regression:

$$\overline{MPK}_i = \lambda \hat{\beta}_i + \epsilon_i, \quad for \quad i = 1, ..., I$$

With the estimate of $\hat{\lambda}$, we can compute the firm risk premium as the product between the market price of risk and the amount of risk, $\theta_i = \hat{\lambda}\hat{\beta}_i$. The cross-sectional variance is $Var(\theta_i) = Var(\hat{\lambda}\hat{\beta}_i)$. The Amadeus data provides too few observations per firm to estimate the time-series regressions. Instead, we use data on the universe of manufacturing firms in Ukraine, provided by Brown et al. (2006) [10]. The data includes 3,000 firms that have 20 or more years of information.

After running the time-series and cross-sectional regressions for Ukraine, we find that $Var(\theta_i) = 0.05$. From Table 2.4, we know that $Var(MPK) = 1.06$. Since the variance in risk premia accounts for less than 5% of the variance of MPK, it is unlikely that a reduction in the variance in risk premia is driving our results.

Adjustment Costs

In our main framework, we have also abstracted from any dynamics. In a dynamic model of capital, firms face adjustment costs to installing new capital. Even with no frictions, the MPK could not be equalized across firms. To deal with this issue, we analyze the return on *investment*, which includes the costs from capital adjustment:[38]

$$r^I = \frac{MPK - \phi_2(i,k) + (1-\delta)(1+\phi_1(i,k))}{1+\phi_1(i_{-1},k_{-1})}$$

The function $\phi(i,k)$ denotes adjustment costs and depends on investment i and capital k. ϕ_1 and ϕ_2 denote the derivatives with respect to the first and second argument. We make the standard assumption of quadratic adjustment costs: $\phi(i,k) = \frac{a}{2}(\frac{i}{k})^2 k$. Finally, δ denotes the depreciation rate, which we set to 6%. We calculate investment returns and their cross-sectional variance for values of $a \in \{1, 2, 3, 10, 15, 30\}$, which are in line with previous literature. Results are presented in Table 2.15. The estimates show that even after accounting for adjustment costs, financial reform reduces the variance of investment returns.

[38] See Liu et al. (2009) [40] for a derivation of the return on investment in a dynamic model of capital. Note that return on investment equals the MPK in the absence of capital adjustment costs.

Aggregate Effect

With our empirical analysis, we can only identify the differential effect of financial reform on TFP across industries, not the overall effect. In this section we provide a back-of-the-envelope calculation to estimate the aggregate effect. We define aggregate TFP as a weighted average of industry-level TFP:

$$\log(Z_c) = \sum_s \omega_{cs} \log(Z_{cs})$$

To back out the *level* effect from the *cross-sectional* estimates, we take the index of external finance dependence literally. We assume that a reform has no effect on productivity for an industry that can finance all of its capital expenditures with cash flow from operations. That is, we set the effect of the reform to zero for a (hypothetical) industry with a value of external financial dependence that equals zero. Among our 22 industries, the index is negative for only one industry (tobacco), which we assume is also not affected by the reform. We consider this assumption to be plausible and conservative. There is no immediate reason why TFP in the low-dependence industries should fall due to changes of TFP in high-dependence industries through general equilibrium effects.[39]

As seen in Figure 2.1, several countries undertook very sizable reforms equivalent to changes in the financial liberalization index of 5 or more points. For the aggregate effect, we therefore consider the effect of a "large" reform that we define as an index score change of 3 points.[40]

Since the effect for an industry with $EFD = 0$ is set to zero, the effect of the reform on TFP of sector s is:

$$\Delta \log(Z_{cs}) = \hat{\alpha} \cdot 3 \cdot EFD_s$$

We can then back out the effect of financial reform on aggregate TFP for country c as follows:

$$\Delta \log(Z_c) = \hat{\alpha} \cdot 3 \sum_s \omega_{cs} EFD_s$$

The weights, which are based on revenues, are measured for the year 2000. Results are reported in Table 2.16. Results vary by country due to different industry composition. On average, the increase in aggregate TFP is 17%. Note that this effect is only for manufacturing. According to the estimation of the cross-country equation (2.3), the effect of financial reform on the whole economy was 5.2%.[41] For the countries in our sample, the manufacturing sector accounts for about 1/5 of GDP. Assuming no effect on TFP for the agriculture and service sectors, this overall effect implies an increase in TFP in manufacturing of roughly 26%. This figure is roughly consistent with the back-of-the-envelope calculation provided in

[39] This would be different if we were looking at capital or labor. These are scarce resources that have to be allocated across industries. As a result, general equilibrium effects are important.

[40] This is in line with the definition of a large reform in Abiad et al. (2010) [1].

[41] Before we reported the effect of a two-point reform, which was 3.5%.

this section, but also suggests that an estimation using only variation across countries and time may slightly overestimate the effect of reform.

2.7 Robustness Checks

In this section we present a series of robustness checks for our main set of results.

Labor productivity. Throughout the paper, we have used TFP as a measure of efficiency. An alternative measure of efficiency is labor productivity.[42] The main advantage of this measure is its simplicity, since it is calculated simply as the ratio between output and employment. In Table 2.17 we show that the productivity decomposition produces the same results when we use this alternative productivity measure. The differential effect across industries is very similar in magnitude. Reallocation remains the factor entirely driving the productivity gains.

Asset Tangibility. An alternative measure of industry financial constraints is asset tangibility (Braun (2003) [8]). For technological reasons, some industries employ a higher fraction of tangible assets over total assets than others.[43] In Table 2.18, we show the results of exploiting cross-sectoral variation in tangibility. Overall, the results are very similar to the ones obtained using external financial dependence. Since asset tangibility is an *inverse* measure of financial constraints, all coefficients have the opposite sign. The magnitude of the effects lines up very well with the effects obtained previously. The differential effect on TFP across industries with low and high asset tangibility (25th vs. 75th percentile) is 11.4% (compared to 10.5% when we use financial dependence). Again, most of this differential effect is accounted for by a more efficient resource allocation.

Additional industry characteristics. We also consider a variety of industry characteristics that may be related to the ease of obtaining external finance. First, since industries that manufacture durable goods tend to be highly dependent on external finance, our interaction term could be picking up variation in the durability of the goods produced rather than financial dependence. To check for this, we include in specification (2.4) an indicator of whether the industry manufactures predominantly durable goods, using the classification by the US Bureau of Economic Analysis (BEA). Next. Next, we control for the fact that financially dependent sectors tend to be R&D intensive. We collect data for industry R&D expenditure and value added from the OECD STAN database, using data for Germany for the year 2000.[44] We then add a measure of R&D intensity, defined as the ratio of industry R&D expenditure to value added.

[42]Several papers, including Bartelsman et al. (2013) [6], use this measure to study misallocation.

[43]For instance, a significant part of assets in the manufacturing of medical, precision, and optical instruments consists of intangible assets, due to a high R&D component. In contrast, asset tangibility is much larger for manufacturing of basic metals.

[44]We use data for Germany instead of U.S. data, because of many missing values in the U.S. data series of R&D expenditure.

Finally, we have controlled for trade reform very roughly by interacting a country-level reform indicator with an industry's need for external finance. However, there is a possibility that tariffs are falling, particularly for sectors with high financial dependence. We control for this possibility by including country-sector-year measures of tariffs in our specification. We assemble data on ad valorem (or ad valorem equivalent) tariff data from the UNCTAD TRAINS database.[45] We interact our reform indicators with the measures of R&D intensity and durability, and also include the tariff data as an additional control. Table 2.19 reports the results. As the table illustrates, our results our not driven by any of these industry characteristics.

2.8 Conclusions

In an economy with a well-functioning financial market, productivity is the main determinant of firm size. Under financial sector repression, some productive firms may not be able to borrow enough to achieve their optimal size. As a result, capital is not allocated towards its most efficient use, which results in low aggregate productivity. Low productivity in turn leads to economic underdevelopment.

Financial sector reforms tighten the link between lending and productivity and should therefore tighten the link between productivity and firm size. In this paper, we use a large firm-level dataset to analyze the microeconomic channels by which financial reforms in 10 transition economies affect capital allocation across firms and aggregate productivity. To identify the causal effect of the reforms, we exploit differences in external financial needs across industries. We argue that common concerns about the endogeneity of financial reforms do not apply in our setting, due to the particular history of the reform process: financial liberalization in Eastern Europe was largely a process of externally imposed reforms, driven by the EU and OECD accession process, and conditional financial support from the IMF.

Our findings indicate that financial reforms increase TFP particularly in financially dependent industries. To pin down the factor driving productivity gains, we decompose industry productivity into the sum of the average within-firm productivity and the within-industry size-productivity covariance. We find that industry TFP gains are driven entirely by more efficient resource allocation. This improved allocation is manifested in a reduction in the within-industry variance of the MPK across firms, a reduction in the covariance between firm productivity and MPK, and an increase in the covariance between firm debt and MPK. Our analysis indicates that financial reforms allow financially constrained firms to take on new debt, increase market share, and produce closer to optimal level, with a smaller gap between MPK and cost of capital.

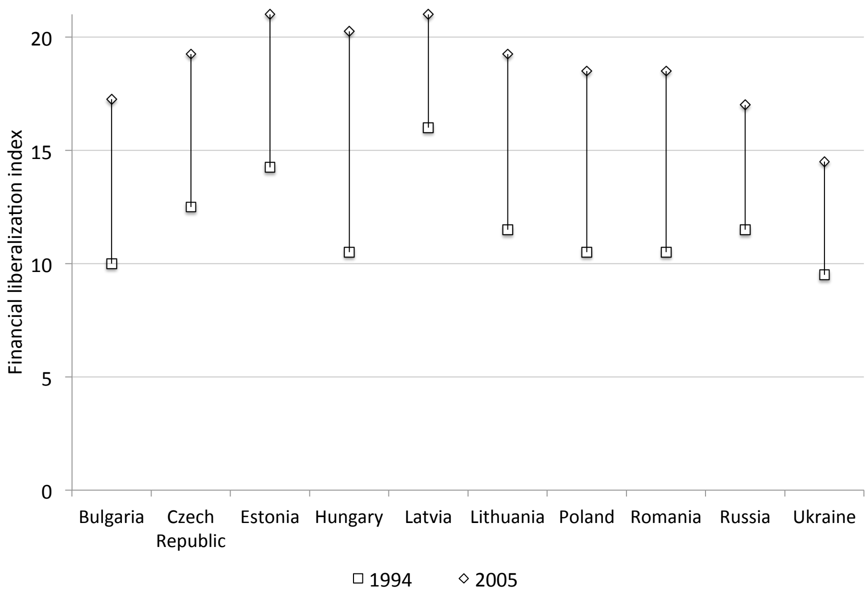
Hsieh and Klenow (2009) [31] show that firm-level distortions lead to misallocation of factors across firms and reduce aggregate TFP. Our paper links firm distortions to a particu-

[45] We collect data on average import and export tariffs by industry. Export tariffs are the tariffs that other countries impose on shipments from our sample countries. To aggregate tariffs from the product level to the industry level, tariffs are weighed by trade flows.

lar policy, financial sector repression. We show that a reform that alleviates these distortions leads to a significant process of reallocation and aggregate TFP gains. Since cross-country differences in income per capita are driven primarily by cross-country differences in TFP (Hall and Jones (1999) [29]), financial sector reforms have the potential of helping reduce the per capita income gap across countries. Our results suggest that financial sector policies can potentially play a large role in curbing the misallocation of resources. In a conservative approach, we find that a large financial reform increases manufacturing TFP by 17%. This appears sizable compared to the 30-50% TFP gains for China (40-60% for India) that Hsieh and Klenow (2009) [31] calculate, if total misallocation in these countries was reduced to the level observed in the US.

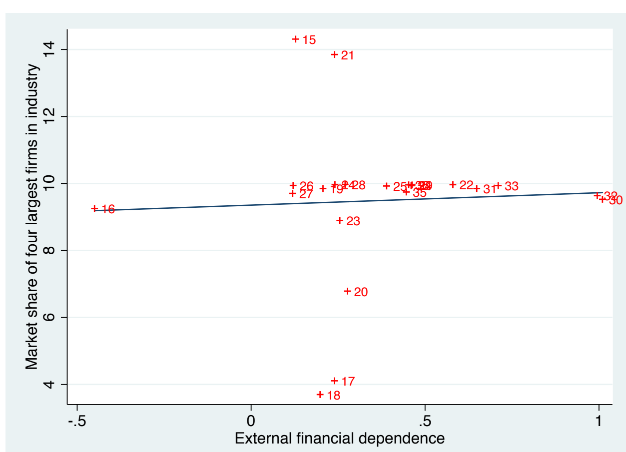
Finally, our analysis shows that financial market imperfections generate sizable TFP losses by distorting the *within-industry* allocation of resources. Although we do not find evidence for reallocation of capital and labor across industries following a reform, we do not conclude that financial repression does not lead to a misallocation of factors across industries. Production factors are presumably more mobile within industries and reallocation across industries may take more time, so we may potentially miss this reallocation in our 10-year study window. By focusing on the within-industry margin of misallocation, we thus view our paper as complementary to Wurgler (2000) [58]. Considering a much longer time horizon of more than 30 years, that paper establishes a link between less developed financial markets and capital misallocation *across* industries. To the extent that reallocation of resources across industries may further increase manufacturing TFP, our estimate of 17%, by considering only the within-industry component of reallocation, would be an underestimate of the long-term gains from reform.

Figure 2.1: Cumulative change in financial reform index for the ten transition economies, 1994-2005



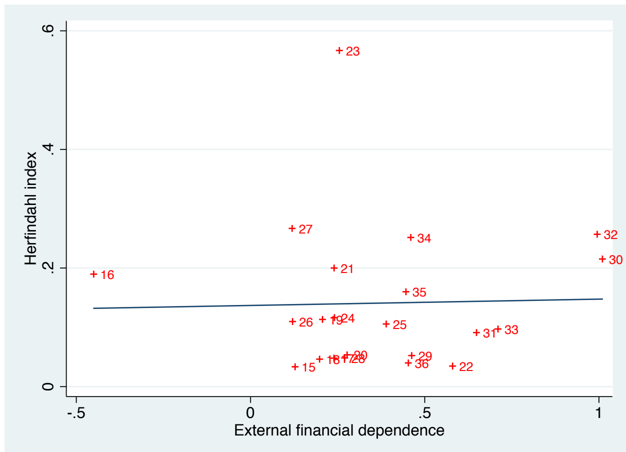
Notes: The figure plots the cumulative change of the [1] financial reform index for the ten transition economies between 1994 and 2005. The index takes values between zero and 21. The index aggregates seven financial sector dimensions: (1) credit controls, (2) interest rate controls, (3) bank entry barriers, (4) state ownership of banks, (5) capital account restrictions, (6) prudential bank regulations, and (7) securities market policy. Source: own calculations based on [1].

Figure 2.2: Industry Market Concentration and financial dependence: Market share of four largest firms



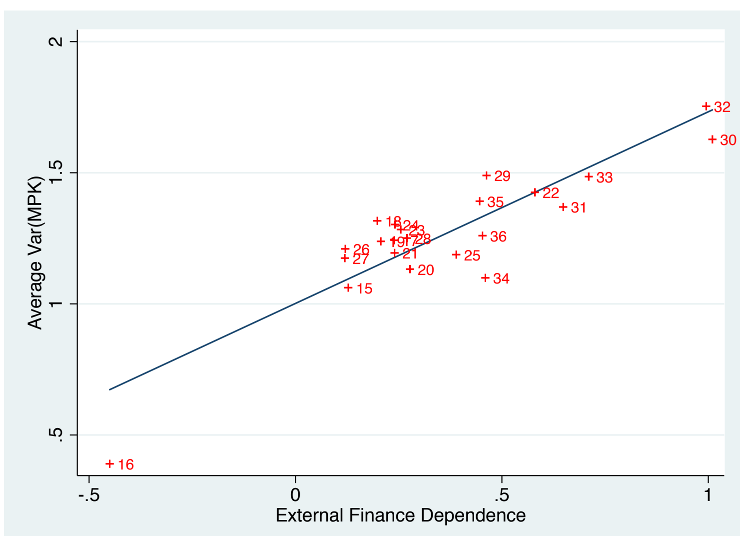
Notes: The figure plots the relationship between external financial dependence and industry concentration. Industry concentration is measured as the market share of the four largest firms. Source: own calculations based on Amadeus dataset and Rajan and Zingales (1999) [51] financial dependence index.

Figure 2.3: Industry Market Concentration and financial dependence: Herfindahl Index



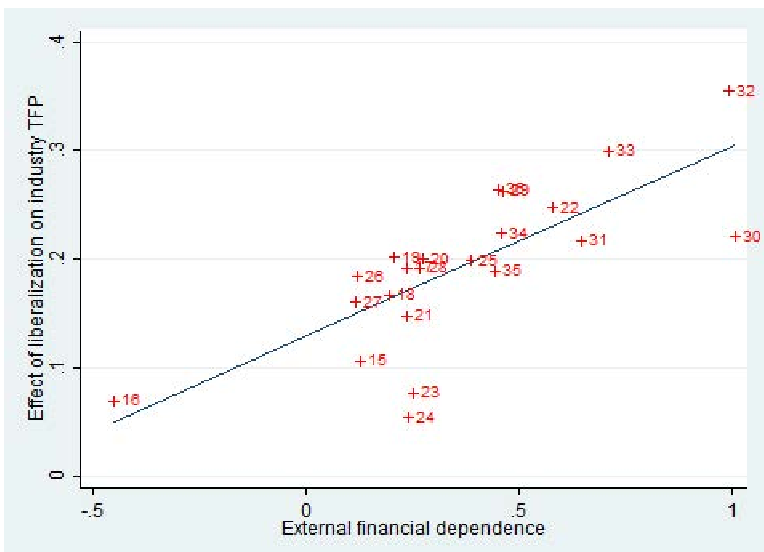
Notes: The figure plots the relationship between external financial dependence and industry concentration. Industry concentration is measured as the Herfindahl index. Source: own calculations based on Amadeus dataset and Rajan and Zingales (1999) [51] financial dependence index.

Figure 2.4: Variance of the marginal product of capital external finance dependence in a cross-section of industries



Notes: The figure plots the average variance of the marginal product of capital in a given industry against its index of external finance dependence. The average variance is calculated by first computing the variance of the log MPK within a country-sector-year cell, and then averaging it across all countries and years for that industry. Each industry is indexed with its two-digit ISIC rev. 3 code. Source: own calculations based on Amadeus dataset and [51] financial dependence index.

Figure 2.5: Effect of financial reform on industry-level TFP



Notes: The figure plots the estimated coefficient α_s from equation $\log(TFP_{cst}) = \sum_{s=1}^{S} \alpha_s Ref_{ct} \cdot D_s + \beta X_{ct} \cdot EFD_s + \eta_{ct} + \eta_{cs} + \eta_{ts} + \epsilon_{cst}$ against its index of external finance dependence. See main text for details of specification. Each industry is indexed with its two-digit ISIC rev. 3 code. Source: own calculations based on Amadeus dataset and [51] financial dependence index.

Table 2.1: External financial dependence index across industries

Industry name	(1) ISIC rev 3.	(2) External financial dependence
Manufacture of food products and beverages	15	0.13
Manufacture of tobacco products	16	-0.45
Manufacture of textiles	17	0.24
Manufacture of wearing apparel	18	0.20
Tanning and dressing of leather	19	0.21
Manufacture of wood	20	0.28
Manufacture of paper and paper products	21	0.24
Publishing, printing and rep. of media	22	0.58
Manufacture of coke, refined petroleum products	23	0.26
Manufacture of chemicals and chemical products	24	0.24
Manufacture of rubber and plastics products	25	0.39
Manufacture of other non-metallic mineral products	26	0.12
Manufacture of basic metals	27	0.12
Manufacture of fabricated metal products	28	0.27
Manufacture of machinery and equipment	29	0.46
Manufacture of office, acc. and comp. machinery	30	1.01
Manufacture of electrical machinery and apparatus	31	0.65
Manufacture of radio, television and comm. equipment	32	1.00
Manufacture of medical, precision and optical inst.	33	0.71
Manufacture of motor vehicles, trailers and semi-trailers	34	0.46
Manufacture of other transport equipment	35	0.45
Manufacture of furniture; manufacturing n.e.c.	36	0.45

Notes: The table reports the external financial dependence index for the manufacturing industries used in the sample. Financial dependence is defined as the fraction of capital expenditures not financed by cash flow from operations. The table includes the two-digit ISIC rev. 3 code for each industry. Source: own calculations based on [51].

Table 2.2: Coverage of firms in Amadeus dataset

Country	Number of Firms
Bulgaria	9,787
Czech Republic	7,039
Estonia	4,840
Hungary	432
Latvia	838
Lithuania	1,710
Poland	6,844
Romania	58,046
Russia	41,163
Ukraine	4,933
Total	135,632

Notes: The table reports the coverage of firms for the ten countries in our sample during the 1994-2005 period. Source: own calculations based on Amadeus dataset.

Table 2.3: Employment distribution across different size bins in Amadeus dataset

Country	1< L <9	10< L <49	50< L <249	L >250
Bulgaria	2.3%	10.0%	26.4%	61.3%
Czech Republic	0.8%	7.1%	28.7%	63.4%
Estonia	7.7%	25.1%	37.0%	30.2%
Hungary	0.4%	4.4%	36.0%	59.1%
Latvia	0.3%	5.8%	39.9%	54.1%
Lithuania	0.4%	9.8%	34.7%	55.1%
Poland	0.2%	3.3%	29.5%	67.0%
Romania	5.9%	13.9%	27.0%	53.2%
Russia	2.1%	6.2%	19.4%	72.2%
Ukraine	0.1%	1.0%	15.1%	83.8%
Average Amadeus	2.0%	8.7%	29.4%	60.0%
Average Eurostat	7.6%	17.6%	31.2%	43.6%

Notes: The table reports the employment distribution across different size bins for the ten countries in our sample during the 1994-2005 period. L stands for employment. It also compares the Amadeus average employment distribution with the Eurostat distribution, which includes the universe of firms. Source: own calculations based on Amadeus dataset and Eurostat.

Table 2.4: Summary statistics of the main variables of interest at the country-sector-year level

	(1) Mean	(2) Median	(3) 10thPt	(4) 90thPt	(5) SD
TFP	6.175	6.395	4.608	7.251	1.002
Covariance(share,TFP)	0.243	0.198	-0.095	0.678	0.324
Variance(MPK)	1.065	1.038	0.438	1.689	0.591
Variance(MPL)	0.912	0.801	0.359	1.509	0.695
Covariance(MPK,TFP)	0.554	0.522	0.125	0.980	0.364
Covariance(MPL,TFP)	0.520	0.472	0.089	0.953	0.378
Covariance(MPK,debt)	-0.349	-0.330	-0.813	0.053	0.443
Covariance(MPL,debt)	0.511	0.449	0.000	1.079	0.523

Notes: The table reports summary statistics of the main variables of interest calculated within a country-sector-year cell. Columns (1)-(5) depict the mean, median, 10th percentile, 90th percentile, and standard deviation, respectively. TFP denotes (log) total factor productivity, MPK is (log) marginal product of capital, MPL is (log) marginal product of labor, and debt is (log) firm outstanding debt. Source: own calculations based on Amadeus dataset

Table 2.5: Effects of financial reform on aggregate output, capital, labor, and TFP

	(1) GDP	(2) Labor	(3) Capital	(4) TFP
Financial Reform	0.020**	-0.011	0.033	0.017*
	(0.008)	(0.006)	(0.029)	(0.009)
Competition	0.067	-0.017	0.224	0.012
	(0.048)	(0.040)	(0.189)	(0.096)
Infrastructure	-0.062	0.004	-0.086	-0.039
	(0.052)	(0.026)	(0.136)	(0.071)
Price Lib	-0.090**	-0.025	-0.189	-0.016
	(0.039)	(0.017)	(0.176)	(0.062)
Privatization	-0.102**	-0.030	-0.184	-0.026
	(0.041)	(0.022)	(0.189)	(0.057)
Restructuring	-0.085	-0.002	0.114	-0.118**
	(0.070)	(0.016)	(0.194)	(0.051)
Trade	0.025	-0.011	0.042	0.020
	(0.028)	(0.010)	(0.111)	(0.034)
C, Y fixed effects	Yes	Yes	Yes	Yes
Observations	133	133	133	133
R-squared	0.998	0.999	0.988	0.954

Notes: The table presents the estimates of the effects of financial reform on country-level GDP, labor, capital and total factor productivity. The specifications include country and year fixed effects. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10% level.

Table 2.6: Effects of financial reform on industry output, capital, labor, and TFP

	(1) Output	(2) Capital	(3) Labor	(4) TFP
Financial Reform · EFD	0.237***	0.026	0.040	0.210***
	(0.060)	(0.086)	(0.040)	(0.039)
Competition · EFD	0.041	0.037	0.026	0.037
	(0.064)	(0.068)	(0.065)	(0.039)
Infrastructure · EFD	-0.068	-0.008	-0.021	-0.056
	(0.074)	(0.065)	(0.056)	(0.036)
Price Lib · EFD	0.087	-0.075	0.036	0.115
	(0.095)	(0.084)	(0.047)	(0.084)
Privatization · EFD	-0.180	-0.042	0.040	-0.180**
	(0.099)	(0.098)	(0.063)	(0.064)
Restructuring · EFD	-0.009	0.052	0.052	-0.079
	(0.132)	(0.123)	(0.082)	(0.088)
Trade · EFD	-0.042	-0.090*	-0.048*	-0.001
	(0.046)	(0.047)	(0.026)	(0.022)
CY-CS-SY fixed effects	Yes	Yes	Yes	Yes
Observations	1,448	1,448	1,448	1,448
R-squared	0.984	0.978	0.989	0.974

Notes: The table presents the estimates of the effects of financial reform on industry output, capital, labor and total factor productivity. EFD stands for external financial dependence. The specifications include country-year, country-sector, and sector-year fixed effects. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Table 2.7: Effects of financial reform on industry TFP and its efficiency and allocation components

	(1) Overall TFP	(2) Efficiency term	(3) Allocation term
Financial Reform · EFD	0.210***	0.016	0.194***
	(0.039)	(0.020)	(0.039)
Competition · EFD	0.037	-0.044**	0.081*
	(0.039)	(0.017)	(0.044)
Infrastructure · EFD	-0.056	-0.004	-0.052
	(0.036)	(0.042)	(0.043)
Price Lib · EFD	0.115	-0.001	0.116
	(0.084)	(0.023)	(0.092)
Privatization · EFD	-0.180**	-0.007	-0.174**
	(0.064)	(0.041)	(0.063)
Restructuring · EFD	-0.079	0.024	-0.103
	(0.088)	(0.037)	(0.116)
Trade · EFD	-0.001	0.037*	-0.038
	(0.022)	(0.018)	(0.032)
CY-CS-SY fixed effects	Yes	Yes	Yes
Observations	1,448	1,448	1,448
R-squared	0.974	0.991	0.797

Notes: The table presents the estimates of the effects of financial reform on industry total factor productivity and its efficiency and allocation components. The efficiency element is measured as the average within-industry firm productivity. The allocation element is measured as the within-industry size-productivity covariance. EFD stands for external financial dependence. The specifications include country-year, country-sector, and sector-year fixed effects. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Table 2.8: Effects of financial reform on firm-level TFP

	(1) Direct effect	(2) Effect by size
Financial reform · EFD	-0.002	0.007
	(0.015)	(0.014)
EFD · size		-0.346
		(0.274)
Financial reform · size		0.000
		(0.005)
Financial reform · EFD · size		-0.009
		(0.021)
Competition · EFD	-0.028***	-0.028**
	(0.008)	(0.010)
Infrastructure · EFD	0.008	-0.001
	(0.019)	(0.023)
Price Lib · EFD	0.017	0.012
	(0.016)	(0.024)
Privatization · EFD	0.011	0.015
	(0.012)	(0.014)
Restructuring · EFD	0.028*	0.033
	(0.013)	(0.020)
Trade · EFD	0.025***	0.037**
	(0.007)	(0.014)
CY-IY-firm fixed effects	Yes	Yes
Controls	NA	Yes
Observations	468,895	468,895
R-squared	0.931	0.931

Notes: The table presents the estimates of the effects of financial reform on firm-level total factor productivity. Column (1) documents the direct effect and column (2) the effect by size. EFD stands for external financial dependence. Large is a dummy variable indicating whether the firm was larger than the median firm in its country-sector-year cell in the previous year. The specifications include country-year, industry-year, and firm fixed effects. Column (2) also controls for the interaction between all reform indicators with EFD and firm size. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Table 2.9: Effects of financial reform on within-industry variance of marginal product of factors

	(1) Variance MPK	(2) Variance MPL
Financial Reform · EFD	-0.212***	0.005
	(0.041)	(0.041)
Competition · EFD	0.046	0.025
	(0.039)	(0.033)
Infrastructure · EFD	0.081	-0.035
	(0.048)	(0.038)
Price Lib · EFD	-0.150**	0.001
	(0.062)	(0.046)
Privatization · EFD	0.338***	0.015
	(0.061)	(0.086)
Restructuring · EFD	0.051	-0.096
	(0.084)	(0.052)
Trade · EFD	-0.126***	-0.097***
	(0.030)	(0.022)
CY-CS-SY fixed effects	Yes	Yes
Observations	1,448	1,448
R-squared	0.857	0.841

Notes: The table presents the estimates of the effects of financial reform on the within-industry variance of the marginal product of capital (column (1)) and the variance of the marginal product of capital labor (column (2)). MPK stands for marginal product of capital and MPL for marginal product of labor. EFD stands for external financial dependence. The specifications include country-year, country-sector, and sector-year fixed effects. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Table 2.10: Effects of financial reform on the within-industry covariance between marginal product of factors and firm productivity

	(1) Covariance MPK and firm TFP	(2) Covariance MPL and firm TFP
Financial Reform · EFD	-0.142***	-0.040
	(0.035)	(0.028)
Competition · EFD	0.039	-0.003
	(0.029)	(0.025)
Infrastructure · EFD	0.069*	0.021
	(0.033)	(0.039)
Price Lib · EFD	-0.098*	-0.028
	(0.045)	(0.032)
Privatization · EFD	0.241***	0.099**
	(0.051)	(0.040)
Restructuring · EFD	0.041	-0.015
	(0.060)	(0.045)
Trade · EFD	-0.109***	-0.079**
	(0.022)	(0.026)
CY-CS-SY fixed effects	Yes	Yes
Observations	1,448	1,448
R-squared	0.874	0.891

Notes: The table presents the estimates of the effects of financial reform on the within-industry covariance between the marginal product of capital and firm productivity (column (1)) and the covariance between the marginal product of labor and firm productivity (column (2)). MPK stands for marginal product of capital, MPL for marginal product of labor, and TFP for total factor productivity. EFD stands for external financial dependence. The specifications include country-year, country-sector, and sector-year fixed effects. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Table 2.11: Effects of financial reform on the within-industry covariance between marginal product of factors and firm debt

	(1) Covariance MPK and firm debt	(2) Covariance MPL and firm debt
Financial Reform · EFD	0.193**	0.124
	(0.085)	(0.072)
Competition · EFD	0.075	0.053
	(0.049)	(0.057)
Infrastructure · EFD	-0.017	-0.064
	(0.048)	(0.046)
Price Lib · EFD	0.093**	0.100*
	(0.041)	(0.052)
Privatization · EFD	-0.052	-0.101
	(0.115)	(0.072)
Restructuring · EFD	-0.018	-0.153
	(0.058)	(0.095)
Trade · EFD	0.032	0.006
	(0.034)	(0.025)
CY-CS-SY fixed effects	Yes	Yes
Observations	1,448	1,448
R-squared	0.747	0.835

Notes: The table presents the estimates of the effects of financial reform on the within-industry covariance between the marginal product of capital and firm debt (column (1)) and the covariance between the marginal product of labor and firm debt (column (2)). MPK stands for marginal product of capital and MPL for marginal product of labor. EFD stands for external financial dependence. The specifications include country-year, country-sector, and sector-year fixed effects. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Table 2.12: Effects of financial reform on reallocation at the firm-level

	(1) MPK	(2) Market Share	(3) Debt
MPK(-1)	0.411***	0.209*	0.108
	(0.069)	(0.094)	(0.104)
Financial Reform · EFD	-0.011	-0.259***	-0.014
	(0.023)	(0.040)	(0.030)
EFD · MPK(-1)	0.188**	-0.393**	-0.439
	(0.080)	(0.153)	(0.262)
Financial Reform · MPK(-1)	0.003	-0.004	-0.011
	(0.003)	(0.006)	(0.006)
Financial Reform · EFD · MPK(-1)	-0.011**	0.020*	0.028*
	(0.005)	(0.010)	(0.015)
Controls	Yes	Yes	Yes
FE	Yes	Yes	Yes
Observations	317,437	317,437	294,417
R-squared	0.916	0.979	0.969

Notes: This table presents the estimates of financial reform using firm-level data. MPK(-1) stands for the lagged marginal product of capital. EFD stands for external finance dependence. The specifications include country-year, sector-year, firm fixed effects, and control for the interaction between all reform indicators with EFD and MPK(-1). Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Table 2.13: Effects of financial reform on industry wages

	(1) Wages
Financial Reform · EFD	0.144*
	(0.075)
Controls	Yes
CY-CS-SY fixed effects	Yes
Observations	1,145
R-squared	0.920

Notes: The table presents the estimates of the effects of financial reform on industry wages. EFD stands for external financial dependence. The specifications include country-year, country-sector, sector-year fixed effects, and control for the interaction between all reform indicators and EFD. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Table 2.14: Effects of financial reform accounting for uncertainty

	(1) Var(MPK) All	(2) Var(MPK) Stayers	(3) Var(E[MPK])	(4) Var(Error)
Financial Reform · EFD	-0.220***	-0.197***	-0.194***	-0.003
	(0.031)	(0.037)	(0.035)	(0.005)
Controls	Yes	Yes	Yes	Yes
CY-CS-SY fixed effects	Yes	Yes	Yes	Yes
Observations	1,158	1,158	1,158	1,158
R-squared	0.892	0.874	0.844	0.917

Notes: The table presents the estimates of the effects of financial reform on the within-industry variance of the MPK and its components. Column (1) shows results for the variance of the MPK computed for all firms. Column (2) shows results for the variance of the MPK computed only among stayers. Column (3) uses the variance of the predicted MPK, and column (4) the variance of the error term. By construction, the coefficients of column (3) and column (4) have to add up to the coefficient in column (2). EFD stands for external financial dependence. The specifications include country-year, country-sector, sector-year fixed effects, and control for the interaction between all reform indicators and EFD. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Table 2.15: Effects of financial reform accounting for capital adjustment costs

	(1) a=1	(2) a=2	(3) a=3	(4) a=10	(5) a=15	(6) a=30
Financial Reform · EFD	-0.152*	-0.148*	-0.271**	-0.207*	-0.318**	-0.444**
	(0.071)	(0.070)	(0.116)	(0.099)	(0.138)	(0.165)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
CY-CS-SY fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,151	1,151	1,151	1,150	1,150	1,148
R-squared	0.769	0.739	0.779	0.618	0.612	0.584

Notes: The table presents the estimates of the effects of financial reform on the within-industry variance of investment returns. The capital adjustment functional form is assumed to be $\phi(i,k) = \frac{a}{2}(\frac{i}{k})^2 k$. Columns (1)-(6) show results for investment returns calculated for parameter values of $a \in \{1, 2, 3, 10, 15, 30\}$. EFD stands for external financial dependence. The specifications include country-year, country-sector, sector-year fixed effects, and control for the interaction between all reform indicators and EFD. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Table 2.16: Back-of-the-envelope effect of financial reform on aggregate TFP

Country	TFP gain
Bulgaria	16.0%
Czech Republic	19.7%
Estonia	19.1%
Hungary	17.3%
Latvia	15.4%
Lithuania	19.6%
Poland	18.3%
Romania	16.8%
Russia	16.4%
Ukraine	11.8%
Average	17.0%

Notes: This table presents back-of-the-envelope estimates of the effect of an average-sized financial reform on aggregate manufacturing TFP. The calculations have been based on the assumption that the reform does has no effect on the industry with lowest needs for external finance (tobacco). See main text for details.

Table 2.17: Effects of financial reform on industry labor productivity and it efficiency and allocation components

	(1) Overall productivity	(2) Efficiency term	(3) Allocation term
Financial Reform · EFD	0.243***	0.016	0.227***
	(0.071)	(0.022)	(0.055)
Controls	Yes	Yes	Yes
CY-CS-SY fixed effects	Yes	Yes	Yes
Observations	1,448	1,448	1,448
R-squared	0.952	0.986	0.806

Notes: the table presents the estimates of the effects of financial reform on industry labor productivity and its efficiency and allocation components. The efficiency element is measured as the average within-industry firm productivity. The allocation element is measured as the within-industry size-productivity covariance. EFD stands for external financial dependence. The specifications include country-year, country-sector, sector-year fixed effects, and control for the interaction between all reform indicators and EFD. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Table 2.18: Effects of financial reform using asset tangibility as a measure of industry financial vulnerability

	(1) TFP	(2) Efficiency	(3) Allocation	(4) Var(MPK)	(5) Cov MPK&TFP	(6) Cov MPK&Debt
Financial Reform · Tang	-0.356***	-0.063	-0.294***	0.150	0.137*	-0.322***
	(0.088)	(0.083)	(0.068)	(0.095)	(0.069)	(0.089)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
CY-CS-SY fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,448	1,448	1,448	1,448	1,448	1,448
R-squared	0.973	0.991	0.792	0.852	0.870	0.744

Notes: The table presents the estimates of the effects of financial reform using asset tangibility as a measure of industry financial vulnerability. Tang stands for asset tangibility. The specifications include country-year, country-sector, sector-year fixed effects, and control for the interaction between all reform indicators and Tang. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Table 2.19: Effects of financial reform controlling for additional industry characteristics (durability, R&D Intensity, and tariffs)

	(1) TFP	(2) Efficiency	(3) Allocation	(4) Var(MPK)
Financial Reform · EFD	0.214***	-0.022	0.236***	-0.267**
	(0.048)	(0.024)	(0.051)	(0.093)
Financial Reform · Durability	0.028	0.023	0.006	0.005
	(0.016)	(0.014)	(0.005)	(0.027)
Financial Reform · R&D	-0.017	-0.011	-0.006	0.228
	(0.122)	(0.065)	(0.164)	(0.179)
Import Tariff	0.006	0.005	0.001	-0.001
	(0.005)	(0.003)	(0.004)	(0.005)
Export Tariff	0.001	-0.003	0.003	0.007**
	(0.003)	(0.002)	(0.003)	(0.003)
Controls	Yes	Yes	Yes	Yes
CY-CS-SY fixed effects	Yes	Yes	Yes	Yes
Observations	1,207	1,207	1,207	1,207
R-squared	0.978	0.992	0.836	0.886

Notes: The table presents the estimates of the effects of financial reform on industry TFP, the efficiency and allocation terms, and the variance of the marginal product of capital, controlling for additional industry characteristics. EFD stands for external financial dependence. Durability is a dummy for industries producing durable goods. R&D measure the share of R&D expenditure in value added by industry. Import tariff is the average import tariff that the country imposes on imports in a particular industry and year. Export tariff is the average tariff that trade partners impose on shipments from the sample country in a given industry and year. The specifications include country-year, country-sector, sector-year fixed effects, and control for the interaction between all reform indicators and EFD. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Appendix A

Appendix to Chapter 2

The appendix provides additional robustness checks to the main results obtained in the text.

Excluding countries. We now check whether our main results are robust to excluding individual countries. Table A.1 presents the results for our four main industry-level estimations when we exclude one country at a time. Overall, the results do not seem to be driven by any single country.

Excluding reform components. Next, we conduct a similar exercise by excluding one reform component of financial reform at a time. That is, we now obtain a financial reform measure ranging from 0 to 18, and re-calculate the interaction with external finance dependence. Results are in Table A.2 and hardly show any changes, regardless of which reform component we exclude.

Clustering. In the text we have clustered standard errors at the county level. We analyze whether the results are robust to clustering at the country-industry level, at the year level, at both the country and year level, and to block bootstrapping. According to Table A.3, all results remain highly significant independent of the level of clustering used.

Sample of Stayers. In a final robustness exercise, we drop all observations of firms that stay in our dataset for less than three years.[1] We then recalculate the industry-level variables of TFP, efficiency, allocation, and the variance of the MPK. Results are presented in Table A.4 and show estimates that are similar to the ones obtained before.

[1] This is the time that the median firm stays in the dataset.

Table A.1: Effects of financial reform excluding one country at a time

Excluded Country	(1) TFP	(2) Efficiency	(3) Allocation	(4) Var(MPK)
Bulgaria	0.149	-0.029	0.178*	-0.150
	(0.089)	(0.023)	(0.080)	(0.088)
Czech Republic	0.199***	0.026	0.173***	-0.183**
	(0.035)	(0.021)	(0.034)	(0.063)
Estonia	0.208***	0.017	0.192***	-0.213***
	(0.038)	(0.019)	(0.036)	(0.047)
Hungary	0.210***	0.016	0.194***	-0.211***
	(0.040)	(0.020)	(0.039)	(0.041)
Latvia	0.213***	0.018	0.195***	-0.234***
	(0.044)	(0.022)	(0.043)	(0.029)
Lithuania	0.209***	0.011	0.198***	-0.208***
	(0.039)	(0.019)	(0.041)	(0.044)
Poland	0.192***	0.033	0.158***	-0.205***
	(0.054)	(0.020)	(0.045)	(0.038)
Romania	0.184**	-0.001	0.185**	-0.225***
	(0.057)	(0.023)	(0.057)	(0.054)
Russia	0.206***	-0.005	0.211***	-0.212***
	(0.048)	(0.019)	(0.044)	(0.036)
Ukraine	0.255***	0.022	0.233***	-0.197***
	(0.023)	(0.018)	(0.034)	(0.038)

Notes: The table presents the estimates of the effects of financial reform by excluding one country at a time. Each entry in the matrix corresponds to a separate regression and shows the estimated coefficient of the interaction of the financial reform index and external finance dependence. All specifications include country-year, country-sector, sector-year fixed effects, and control for the interaction between all reform indicators and financial dependence. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Table A.2: Effects of financial reform excluding one reform component at a time

Excluded Component	(1) TFP	(2) Efficiency	(3) Allocation	(4) Var(MPK)
Credit Controls	0.220***	0.026	0.193***	-0.251***
	(0.059)	(0.024)	(0.052)	(0.063)
Interest Rate Controls	0.200***	-0.006	0.206***	-0.209***
	(0.038)	(0.015)	(0.041)	(0.048)
Entry Barriers	0.202***	0.005	0.197***	-0.209***
	(0.038)	(0.018)	(0.041)	(0.041)
Supervision	0.172***	0.020	0.152***	-0.175***
	(0.038)	(0.020)	(0.037)	(0.042)
Bank Privatization	0.267***	0.040	0.227***	-0.240***
	(0.057)	(0.034)	(0.059)	(0.060)
International Capital	0.239***	0.022	0.217***	-0.220***
	(0.040)	(0.019)	(0.040)	(0.038)
Security Markets	0.207***	0.015	0.192***	-0.205***
	(0.037)	(0.021)	(0.036)	(0.039)

Notes: The table presents the estimates of the effects of financial reform by excluding one reform component of the reform index at a time. Each entry in the matrix corresponds to a separate regression and shows the estimated coefficient of the interaction of the financial reform index and external finance dependence. All specifications include country-year, country-sector, sector-year fixed effects, and control for the interaction between all reform indicators and financial dependence. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Table A.3: Effects of financial reform under clustering at different levels

	(1) TFP	(2) Efficiency	(3) Allocation	(4) Var(MPK)
A. Cluster country-sector level				
Financial reform · EFD	0.221***	0.037	0.185***	-0.151**
	(0.052)	(0.027)	(0.056)	(0.059)
B. Cluster year level				
Financial reform · EFD	0.221***	0.037	0.185***	-0.151***
	(0.043)	(0.026)	(0.046)	(0.038)
C. Cluster country and year level				
Financial reform · EFD	0.221***	0.037	0.185***	-0.151***
	(0.041)	(0.026)	(0.042)	(0.040)
D. Cluster country-sector and year level				
Financial reform · EFD	0.221***	0.037	0.185***	-0.151**
	(0.063)	(0.036)	(0.070)	(0.069)
Controls	Yes	Yes	Yes	Yes
CY-CS-SY fixed effects	Yes	Yes	Yes	Yes

Notes: The table presents the estimates of the effects of financial reform under different levels of clustering of standard errors. Panel A clusters at the country-sector level, panel B at the year level, panel C at the both the country and year level, and panel D at both the country-sector and year level. The specifications include country-year, country-sector, sector-year fixed effects, and control for the interaction between all reform indicators and financial dependence. ***, **, * denote statistical significance at 1%, 5%, and 10%.

Table A.4: Effects of financial reform restricting sample to stayers

	(1) TFP	(2) Efficiency	(3) Allocation	(4) Var(MPK)
Financial Reform · EFD	0.228***	0.028	0.200***	-0.169**
	(0.050)	(0.032)	(0.048)	(0.055)
Controls	Yes	Yes	Yes	Yes
CY-CS-SY fixed effects	Yes	Yes	Yes	Yes
Observations	1,396	1,396	1,396	1,396
R-squared	0.975	0.991	0.800	0.855

Notes: The table presents the estimates of the effects of financial reform for the sample of stayers only. A stayer is defined as a firm with at least 3 years of information. EFD stands for external finance dependence. The specifications include country-year, country-sector, sector-year fixed effects, and control for the interaction between all reform indicators and EFD. Standard errors in parentheses are clustered at the country level. ***, **, * denote statistical significance at 1%, 5%, and 10%.