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# Bankruptcy spillovers[☆]

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ABSTRACT

How do different bankruptcy approaches affect the local economy? Using US Census microdata, we explore the spillover effects of reorganization and liquidation on geographically proximate firms. We exploit the random assignment of bankruptcy judges as a source of exogenous variation in the probability of liquidation. We find that employment declines substantially in the immediate neighborhood of the liquidated establishments, relative to reorganized establishments. The spillover effects are highly localized and concentrate in nontradable and service sectors, consistent with a reduction in local consumer traffic and a decline in knowledge spillovers between firms. The evidence highlights the externalities that bankruptcy design can impose on nonbankrupt firms.



## 1. Introduction

Bankruptcy institutions play a significant role in resolving insolvency and financial distress in the economy. Since 1980, more than 1.8 million businesses have filed for bankruptcy in the US Most of these cases are resolved through either reorganization (Chapter 11 under the US Bankruptcy Code), which attempts to rehabilitate the distressed firm, or liquidation (Chapter 7 under the US Bankruptcy Code) in which the firm ceases to exist and all assets are auctioned. Given their importance, bankruptcy institutions have spurred a large literature that mostly focuses on how these two regimes affect the bankrupt firms and their claim holders.[1] Yet, bankruptcy institutions may

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[1] Some theoretical examples include Baird (1986, 1993), Gertner and Scharfstein (1991), Aghion et al. (1992), Shleifer and Vishny (1992),

have far-reaching implications on other economically related firms that are not represented in courts. In this paper, we explore the spillover effects these two bankruptcy regimes, liquidation and reorganization, may impose on the local economy.

In theory, the impact of liquidation–relative to reorganization–on the local economy is ambiguous. The agglomeration literature highlights the importance of complementarities that arise between geographically proximate firms (for recent surveys, see Duranton and Puga, 2004; Glaeser and Gottlieb, 2009; Moretti, 2010).[2] If agglomeration matters – i.e., if spatial concentration of economic activity benefits firms within the agglomeration–liquidation might disrupt agglomeration economies and therefore hurt neighboring firms. In this case, reorganization may lead to a more desirable outcome, as firms are allowed to restructure, continue their operations, and preserve existing agglomeration linkages.

On the other hand, liquidation could benefit the local economy. In particular, if inefficient companies are liquidated, their assets (such as buildings, capital, and labor) can be absorbed by the local economy and redeployed toward more productive uses, attracting new entrants to the area, enhancing synergies with neighboring firms, and contributing to the revitalization of the neighborhood. In other words, liquidation can initiate a "creative destruction" process in which inefficient assets are replaced by more efficient ones. Additionally, reorganization may permit the continuation of inefficient firms due to conflicts of interest and agency problems among claim holders (e.g., Bebchuk, 1988; Gertner and Scharfstein, 1991). This may prevent the reallocation of assets to better uses, limiting potential synergies with local firms and the revitalization of the area.

Ultimately, how these bankruptcy procedures affect the local economy is an empirical question. However, estimating these spillover effects is challenging. First, many companies have multiple establishments, which makes it difficult to determine the relevant local area and identify all establishments that are potentially affected by the bankrupt firm. Second, the decision to liquidate versus reorganize is not random. If, for example, liquidation is more prevalent in declining areas, then an association between liquidation and a local decline in economic activity could be spurious, merely reflecting a negative trend at the local level.

To overcome the first obstacle, we use detailed micro data at the establishment level from the US Census Bureau. Specifically, we combine the Longitudinal Business Database (LBD) with bankruptcy filings from LexisNexis Law to obtain a comprehensive data set of 91,000 establishments belonging to bankrupt firms. Using the geo-codes from the LBD, we determine the relevant local area for each bankrupt establishment and then study how bankruptcy affects other establishments at the same location.[3]

To overcome the second obstacle–the endogeneity of the decision to liquidate versus reorganize–we employ an instrumental variable (IV) approach that exploits the fact that US bankruptcy courts use a blind rotation system to assign cases to judges, effectively randomizing filers to judges within each court division. The assignment of bankruptcy judges is therefore orthogonal to the filer's characteristics and, importantly, to the local economic conditions in the vicinity of the filer's establishments. Judges differ in their propensity to force the liquidation of companies by pushing them to Chapter 7, as opposed to allowing them to reorganize in Chapter 11. The random allocation of filers to bankruptcy judges thus results in the assignment of similar companies to judges who differ in their propensity to treat the firms with different bankruptcy procedures. We exploit this heterogeneity among judges to instrument for the probability that a given company is liquidated. This, in turn, allows us to disentangle the effect of liquidation from potential confounds such as changes in local economic conditions. In essence, this identification strategy is closest to the ideal experiment in which otherwise identical companies are randomly assigned to liquidation or reorganization.[4]

Using this empirical approach, we find that the liquidation of an establishment imposes negative spillovers on the immediate neighborhood (as captured by census blocks). Specifically, relative to reorganized establishments, we find that liquidation leads to a significant decrease in employment among nonbankrupt businesses in the same census block. The effect takes place gradually and persists over the five-year period after the bankruptcy filing. Moreover, the effects are sizable only when the bankrupt establishment is fairly large relative to the size of the block. We further decompose this effect into changes at existing establishments and entry into the area. We find that the decline in employment is largely due to lower growth of existing establishments and, to a lesser extent, reduced entry into the area.

Overall, these results indicate that liquidation adversely affects the local economy and imposes negative externalities on geographically proximate firms. As such, our results are inconsistent with the creative destruction argument,

and Hart (2000), and empirical studies include Hotchkiss (1995), Strömberg (2000), Davydenko and Franks (2008), Eckbo and Thorburn (2008), Benmelech and Bergman (2011), Chang and Schoar (2013), and Maksimovic and Phillips (1998), among others.

[2] As we discuss below, there are multiple channels that may lead to economic dependency between geographically proximate firms and agglomeration spillovers. This includes common dependency on customer traffic, as stores that attract customers may benefit other nearby stores (Pashigian and Gould, 1998; Gould et al., 2005). Firms may also choose to co-locate in the same area to benefit from reductions in production costs, including transportation costs, knowledge spillovers and labor market search costs (Moretti, 2011; Glaeser and Gottlieb, 2009; Duranton and Puga, 2004).

[3] Using census data allows us to consider all industries. This is in sharp contrast to most of the literature on agglomeration that only examines the manufacturing sector. Manufacturing is found disproportionately in small- and medium-size cities, as well as the rural fringes of cities (Kolko, 2000). Hence, exploring the entire population of bankrupt firms allows us to explore relations across all industries and all geographies, including larger and denser cities.

[4] This approach follows the growing literature that takes advantage of the random assignment of judges and heterogeneity in judges' interpretation of the law (e.g., Kling, 2006; Doyle, 2008; Chang and Schoar, 2013; Dobbie and Song, 2015; Galasso and Schankerman, 2015; Bernstein et al., 2016).

which posits that forcing the liquidation of distressed firms will help revitalize the local area and induce entry by freeing up resources for healthy firms to use. Under creative destruction, we would expect higher employment following liquidation or at least higher entry into the area. Yet, neither is supported by the data.

We further show that the spillover effects are highly localized. The effects are strongest at the census block level, and decay for larger geographical areas (census block groups and census tracts). This pattern is consistent with the agglomeration literature that finds that agglomeration spillovers are large but localized (Rosenthal and Strange, 2003; Arzaghi and Henderson, 2008; Ahlfeldt et al., 2015). Importantly, it could be that some of the local employment losses we observe are offset by the reallocation of employees to different areas. As such, our estimates do not capture the effect of liquidation in a macroeconomic sense. Rather, our results establish that liquidation (compared to reorganization) imposes significant negative externalities on the local economy and they provide a quantitative estimate of these externalities.

We also explore how the spillover effects differ depending on the "fate" of the bankrupt establishment, that is, whether it continued operations, remained vacant, or was reallocated to a different user. Consistent with the notion that liquidation leads to a disruption of existing agglomeration linkages, we find that the negative spillovers on local employment are larger if the bankrupt establishment stays vacant or is redeployed to a different industry, while they are smaller if the establishment remains with its current user or remains in the same industry.

Finally, we examine three mechanisms through which liquidation may spill over to neighboring firms. First, liquidation may reduce customer traffic to the area. This will cause negative spillovers on nearby stores if those stores relied on these customers for their own demand (Pashigian and Gould, 1998; Gould et al., 2005; Benmelech et al., 2014).[5] In the second mechanism, liquidation may reduce business synergies between proximate firms. As highlighted by prior literature, such synergies may arise through the reduction of production costs pertaining to three key factors: goods, knowledge, and workers (Moretti, 2011; Glaeser and Gottlieb, 2009; Duranton and Puga, 2004). Specifically, by locating near firms in similar industries, businesses can reduce transportation costs of goods and services, increase the flow of ideas and skills between firms, and create thick labor markets to better match workers and firms. By forcing the removal of a business from a local market, liquidation may disrupt these synergies and agglomeration linkages. Finally, in the third mechanism, it might be the case that the liquidation of an establishment simply reduces local demand for goods and services, as the employees of that establishment lose their jobs and may relocate to other areas (Moretti, 2010).

[5] Liquidated stores may further deter customers if establishments are vacant and neglected. Indeed, evidence from residential real estate shows that vacancy leads to poor maintenance and increased crime (Campbell et al., 2011; Ellen et al., 2013; Cui and Walsh, 2015). Arguably, the same may apply to commercial real estate.

To examine these potential mechanisms, we decompose our sample into three broad industry sectors: (1) the nontradable sector (e.g., restaurants and retail), which relies on local demand and customer flow to the area; (2) the tradable sector (e.g., manufacturing), which is likely to rely on nonlocal demand; and (3) services (e.g., law firms, health services, and advertisement agencies, among others). Consistent with the customer traffic channel, we find that the liquidation of nontradable establishments adversely affects neighboring nontradable establishments, while the liquidation of tradable or services establishments does not. This result suggests that liquidation affects establishments that rely on local demand by reducing customer traffic to the local area. Moreover, we also find that the liquidation of services establishments adversely affects neighboring services establishments, which is consistent with the knowledge spillover and information sharing channel.

In contrast, our evidence is inconsistent with other spillover mechanisms. In particular, we find no evidence that liquidations in the tradable sector affect nontradable employment, which is inconsistent with the employee demand channel. That is, the estimates are unlikely to be explained by the reduction in demand of employees that worked in liquidated establishments. We also find no evidence that tradable employment is affected by liquidation in any sector, which is consistent with the notion that these businesses rely on nonlocal demand, and therefore may be least affected by geographically proximate liquidations. These results highlight that liquidation is not always detrimental to the local area, as its consequences depend on whether the liquidated establishment's operations are tied to the local environment. Finally, given that the liquidation spillovers we identify are highly localized, our results are likely inconsistent with the transportation cost and labor market pooling channels, as these mechanisms likely function in areas larger than a census block.

We use two exercises to quantify the magnitudes of our estimates and compare them to previous studies on agglomeration effects. First, we estimate a local multiplier of liquidation, defined as the change in number of jobs in the census block per number of jobs at the bankrupt establishment. In estimating this figure, it is important to note that there are heterogeneous treatment effects that depend on the size of the block relative to the size of the bankrupt establishment. Specifically, when the establishment is small compared to the block, we do not find any spillover effect of liquidation. Conversely, when the bankrupt establishment is large relative to the block, we estimate that liquidation leads to a reduction of 1.85–1.97 jobs in the block per job at the bankrupt establishment. In addition, we introduce a theoretical framework that allows us to estimate an agglomeration elasticity, defined as the percentage reduction in productivity at surrounding establishments caused by the exogenous liquidation of an establishment. Based on our analysis, we calculate an agglomeration elasticity of 0.21 for nontradable firms and 0.17 for services. These estimates are similar to Kline and Moretti (2013) and Gathmann et al. (2016), who estimate an agglomeration elasticity of 0.2.

The remainder of the paper is organized as follows. In Section 2, we discuss how the paper relates to the existing literature, and Section 3 provides the institutional background. Section 4 presents the data, and Section 5 describes the methodology. In Section 6, we present the results and discuss their magnitudes in Section 7. Finally, Section 8 concludes.

## 2. Related literature

Our paper contributes to several strands of the literature. First, several articles examine the costs and benefits of reorganization procedures, such as Chapter 11 e.g., (e.g., Baird, 1986; Aghion et al., 1992; Bradley and Rosenzweig, 1992; Hotchkiss, 1995; Gilson, 1997; and Bris et al. (2006)), while others consider frictions that may exist in distressed liquidations (e.g., Shleifer and Vishny, 1992; Pulvino, 1998; Pulvino, 1999; Strömberg, 2000; Thorburn, 2000; Campbell et al., 2011). However, this literature has typically ignored any spillover effects of bankruptcy on nonbankrupt firms. This paper shows that these externalities are large enough to be a first-order consideration in assessing the costs and benefits of the two bankruptcy regimes.

Second, this paper contributes to the large literature that studies the benefits of agglomeration. Ellison and Glaeser (1997), among others, show that there is significant geographic clustering of industries, supporting various theories of agglomeration. To date, the empirical literature on agglomeration spillovers has mostly focused on the expansion of agglomeration economies through entry decisions (e.g., Rosenthal and Strange, 2003; Greenstone et al., 2010).[6]

In contrast, our study examines the disruption of agglomeration economies. By focusing on disruptions that break agglomeration linkages through liquidation, and through the reliance on random variation in the assignment of bankruptcy judges, we show how agglomerations can propagate negative shocks that impose negative externalities on other firms within the cluster.[7]

Further, our detailed micro-level data and identification scheme allow us to examine more closely the various channels of agglomeration spillovers across a wide range of industries and with more precise geographic locations. This is in contrast to most of the literature on agglomeration that focuses only on the manufacturing sector, which is typically found disproportionately in small and medium-size cities, as well as their rural fringes, rather than in dense cities (Kolko, 2000).

Our paper also relates to a large literature in industrial organization that examines customer traffic externalities. Much of the work in this area estimates games of entry in which firms trade off benefits of higher customer traffic with the costs of increased competition when co-locating (e.g., Bresnahan and Reiss, 1991; Berry, 1992; Mazzeo, 2002; Ciliberto and Tamer, 2009; Vitorino, 2012). Empirically, the vast majority of retail stores are located in and around shopping centers, leading to the conclusion that the benefits of agglomeration are substantial. For example, Datta and Sudhir (2011) develop a structural model that takes into account other forces that may lead firms to co-locate, including zoning restrictions and the presence of local demand, and conclude that agglomeration benefits are a main driver of observed co-location. Similarly, Sen et al. (2011) examine the effects of opening a gas station on nearby grocery stores and find a significant increase in profitability.

Several papers illustrate directly that firms are willing to pay higher rents to be located in an agglomerated area. Arzaghi and Henderson (2008) explore advertising agencies in Manhattan and find that firms would be willing to pay over two standard deviations higher rent to gain access to immediate neighbors. Similarly, Pashigian and Gould (1998) show that anchor stores, which increase customer traffic in shopping malls, receive a per foot rent subsidy of at least 72% relative to other stores. In other words, while nonanchor stores occupy 23.4% of total square footage in shopping malls, they pay 78.3% of total rental fees. This evidence further illustrates the significant value of spillover effects to firms.

Most closely to our paper are two recent studies that examine the spillover effects of the closure of large retail chains. Benmelech et al. (2014) show that following a retail chains shutdown, stores located in the same shopping mall are more likely to close as well. Shoag and Veuger (2018) show that after a big-box store closes, consumers rapidly reduce their visits to nearby stores. While related, our paper differs in several regards. Our focus is on the externalities of the two main bankruptcy procedures–reorganization and liquidation–instead of the closure of retail chain stores. We do so through an identification strategy that exploits the random allocation of bankruptcy judges. This not only allows for a tight identification of the spillovers on nearby firms, but these spillovers are estimated relative to the policy-relevant option of reorganization. Moreover, we use establishment-level data from the US Census Bureau, which allows us to study all sectors and hence provide a rich characterization of the spillovers of liquidation and reorganization and the mechanisms through which these spillovers occur.

Finally, our paper contributes to the growing literature in macroeconomics that studies the propagation of shocks across industries and firms (e.g., Acemoglu et al., 2015; Acemoglu et al., 2012; Carvalho, 2014). In this vein, our paper shows how the liquidation of an establishment propagates through the establishments agglomeration network and ultimately affects local employment.

## 3. Institutional background

Bankruptcy procedures can be broadly classified into two main categories: liquidation through a cash auction and reorganization through a structured bargaining process (Hart, 2000). The US Bankruptcy Code contains both procedures, with liquidation falling under Chapter 7 and

[6] Two exceptions include Jofre-Monseny et al. (2015), who study the closure of large manufacturing plants in Spain, and Gathmann et al. (2016), who examine the spillovers effects of mass layoffs in Germany.

[7] Note that agglomeration spillovers arising from an expansion versus disruption of agglomeration economies need not be symmetric. Indeed, several theories of agglomeration highlight agglomerations ability to absorb negative shocks (e.g., Krugman, 1991). In fact, as discussed above, liquidation may even benefit the local area if the liquidated establishment's capital and labor are redeployed efficiently within the agglomeration.

reorganization taking place under Chapter 11 of the code. Bankruptcy formally begins with the filing of a petition for protection under one of the two chapters. In nearly all cases, it is the debtor that files the petition and chooses the chapter of bankruptcy, although under certain circumstances creditors can also file for an involuntary bankruptcy. Firms can file for bankruptcy where they are incorporated, where they are headquartered, or where they do the bulk of their business (see 28 USC Section 1408); thereby giving the largest, nationwide firms some leeway in the choice of bankruptcy venue. However, once a firm files for bankruptcy, it is randomly assigned to one of the bankruptcy judges in the divisional office in which it files. This random assignment is a key part of our identification strategy, which we outline in Section 5.

Firms that file for Chapter 7 bankruptcy expect to liquidate all assets of the firm and hence face a relatively straightforward process, although it can be lengthy (Bris et al., 2006). A trustee is put in place to oversee the liquidation of the assets of the firm, and proceeds from the asset sales are used to pay back creditors according to their security and priority. According to US Bankruptcy Court filing statistics, liquidations are frequent, as about 65% of all business bankruptcy filings in the US are Chapter 7 filings.

A significant portion of firms that originally file for Chapter 11 bankruptcy also end up in Chapter 7 through case conversion. Conversion to Chapter 7 occurs when the bankruptcy judge approves a petition to convert the case. Conversion petitions are typically filed either by a creditor or the court itself (e.g., by a trustee), accompanied with a brief that outlines why liquidation will provide the highest recovery for the creditors.[8]

Importantly, while there are uniform criteria by which a judge may convert a case from Chapter 11 to Chapter 7, there is significant variation in the interpretation of these criteria across judges. The random allocation of bankruptcy judges thus results in the assignment of similar companies to judges who differ in their propensity to trigger liquidation. As we discuss in Section 5, we exploit this heterogeneity among judges to instrument for the probability that a given company is liquidated.

Firms that remain in Chapter 11 go through a structured bargaining process in which management and creditors negotiate a plan that outlines any restructuring that will be undertaken, including instituting a new capital structure or selling assets. As shown in Bernstein et al. (2016), a significant number of assets are sold, and many firms are completely shut down even if they remain in Chapter 11. The key difference between the two bankruptcy regimes is that in Chapter 7 liquidation is forced, while in Chapter 11 it is only an option. Meanwhile, negotiations in Chapter 11 are subject to a variety of bargaining costs and principal-agent conflicts that may result in inefficient outcomes. This is important to keep in mind, as establishments that remain in Chapter 11 serve as the counterfactual in our analysis. Thus, we compare spillovers of establishments that are forced to be shut down in liquidation to those of establishments that are still bankrupt–and hence may be sold or shut down–but pass through the reorganization process.

## 4. Data

### 4.1. Bankruptcy data

We gather data on Chapter 11 bankruptcy filings from LexisNexis Law, which obtains filing data from the US courts system. These data contain legal information about each filing, including the date the case was filed, the court in which it was filed, the judge assigned to the case, an indicator of whether the filing was involuntary or not, and status updates on the case. From the status updates, we are able to identify cases that were converted to Chapter 7. The LexisNexis data set contains a few bankruptcies beginning as early as 1980, but coverage is not complete in these early years, as courts were still transitioning to an electronic records system. We begin our sample in 1992, when LexisNexis coverage jumped to over 2000 bankruptcy filings per year (from 450 in 1991) across 70 different bankruptcy districts (out of 91). By 1995, LexisNexis covers essentially 100% of all court cases across all bankruptcy districts.[9]

We end our sample with cases that were filed in 2005 so as to be able to track economic activity near bankrupt firms for a five-year period after the bankruptcy filing.

### 4.2. Establishment-level data

The establishment-level data are obtained from the US Census Bureau's (LBD). An establishment is a 'single physical location where business is conducted' (Jarmin and Miranda, 2002), e.g., a retail store, supermarket, restaurant, warehouse, or manufacturing plant. The LBD covers all business establishments in the US with at least one paid employee. Importantly, the LBD allows us to identify all establishment locations of the bankrupt firms and also identify geographically proximate establishments that may be affected by the bankruptcy.

We match bankruptcy filings from LexisNexis to the bankrupt firms' establishments in the LBD using the procedure of Bernstein et al. (2016). Specifically, we match the bankruptcy filings from LexisNexis to the US Census Bureau's Business Register-the Standard Statistical Establishment List (SSEL)-using the employer identification number (EIN), which is contained in both data sets. Importantly, each legal entity of a firm can have a separate EIN, and thus there can be multiple EINs (and multiple bankruptcy filings) for each firm.

Further, an EIN can have multiple establishments in the LBD. We match bankrupt EINs to all establishments in the SSEL in the year of the bankruptcy filing to form our initial sample of 129,000 bankrupt establishments belonging to 28,000 unique firms.[10]

[8] We examine court documents for a random sample of 200 cases and found that, on average, a motion to convert a case occurs four months after the bankruptcy filing. Importantly, in nearly all cases this is the first major motion on which a judge rules.

[9] See Iverson (2018) for more information on the LexisNexis data.

[10] Note that the Census Bureau requires us to round observation counts.

**Table 1**
Summary statistics.

| | All | | | Chapter 7 (Liquidation) | | | Chapter 11 (Reorganization) | | |
|---|---|---|---|---|---|---|---|---|---|
| | *N* | Mean | Std. Dev. | *N* | Mean | Std. Dev. | *N* | Mean | Std. Dev. |
| *a. Bankrupt establishments* | | | | | | | | | |
| Employees | 91,000 | 37.0 | 193.5 | 16,000 | 28.0 | 84.4 | 75,000 | 38.8 | 209.3 |
| Payroll (000s) | 91,000 | 977 | 9,169 | 16,000 | 585 | 2,475 | 75,000 | 1,060 | 10,020 |
| Payroll / Employees (000s) | 91,000 | 22.2 | 48.0 | 16,000 | 19.6 | 45.8 | 75,000 | 22.7 | 48.5 |
| *b. Bankrupt firms* | | | | | | | | | |
| Employees | 20,000 | 220 | 2,249 | 8000 | 72 | 385 | 12,000 | 309 | 2,828 |
| Establishments | 20,000 | 6.1 | 48.3 | 8000 | 2.7 | 21.8 | 12,000 | 8.1 | 58.7 |
| *c. Census blocks* | | | | | | | | | |
| Employees | 91,000 | 1,105 | 2,521 | 16,000 | 926 | 2,327 | 75,000 | 1,143 | 2,558 |
| Establishments | 91,000 | 55.5 | 104.9 | 16,000 | 50.6 | 98.7 | 75,000 | 56.5 | 106.2 |
| Employees / Establishments | 91,000 | 18.9 | 42.5 | 16,000 | 16.1 | 32.6 | 75,000 | 19.5 | 44.3 |
| *d. Census block groups* | | | | | | | | | |
| Employees | 91,000 | 3,453 | 6,706 | 16,000 | 3,455 | 7,014 | 75,000 | 3,453 | 6,640 |
| Establishments | 91,000 | 165.3 | 266.5 | 16,000 | 171.1 | 283.8 | 75,000 | 164.1 | 262.7 |
| Employees / Establishments | 91,000 | 18.3 | 21.3 | 16,000 | 17.4 | 20.8 | 75,000 | 18.6 | 21.4 |
| *e. Census tracts* | | | | | | | | | |
| Employees | 91,000 | 6,581 | 11,079 | 16,000 | 6,862 | 11,553 | 75,000 | 6,522 | 10.976 |
| Establishments | 91,000 | 330.3 | 456.3 | 16,000 | 348.5 | 475.1 | 75,000 | 326.5 | 452.2 |
| Employees / Establishments | 91,000 | 17.8 | 14.6 | 16,000 | 17.3 | 14.3 | 75,000 | 17.9 | 14.7 |

This table provides summary statistics for the bankrupt firms, their establishments, and nonbankrupt establishments residing in the same census block, census block group, and census tract, respectively, as the bankrupt establishments. Statistics are reported for all bankrupt firms and separately for firms that are liquidated (Chapter 7) and reorganized (Chapter 11). Observation counts are rounded to the nearest thousand due to the disclosure rules of the US Census Bureau.

### *4.3. Geographical units*

In our baseline analysis, we define a location at the level of the census block. Census blocks are the smallest geographic area for which the Census Bureau reports information. In a city, the shape of a census block follows the geographic pattern of the streets (usually a rectangular grid). Census blocks in suburban and rural areas may be large, irregular, and bounded by a variety of features, such as roads, streams, and transmission lines (U.S. Census Bureau,1994). There are more than 11 million blocks in the 2010 Decennial Census.[11]

Census blocks serve as a valuable source for small-area geographic studies (e.g., Echenique and Fryer, 2007; Bayer et al., 2005). They are especially appealing in our context, since establishments are small economic entities. Arguably, blocks are likely to best approximate the area that is economically relevant to the establishment.

The Census Bureau started collecting block information for business establishments as of the 1992 Census. This coincides with the initial year of our sample. However, block coverage is incomplete in 1992 and becomes increasingly more comprehensive in subsequent census years. To fill in missing geo-codes, we use the most recent block information (e.g., if an establishment has no block information available in 1992 but does in 1997, we fill in the pre-1997 years with the 1997 block code). Out of the initial 129,000 establishments of the bankrupt firms, we obtain a final sample of 91,000 establishments (belonging to 20,000 unique bankrupt firms) with nonmissing block information.[12]

We also examine how bankruptcy regimes affect larger areas. Census block groups are the next level above census blocks in the geographic hierarchy. A census block group is a set of one or more contiguous census blocks. There are about 220,000 block groups in the 2010 Decennial Census. Finally, the largest area we consider is the census tract. A census tract usually covers a contiguous area and contains up to nine block groups. There are about 74,000 tracts in the 2010 Decennial Census.

### *4.4. Summary statistic*

Table 1 provides summary statistics for the 91,000 establishments belonging to 20,000 firms that filed for Chapter 11. Out of these establishments, 16,000 pertain to firms that were converted to Chapter 7 liquidation (8,000 firms), while the remaining 75,000 establishments belong to firms that stay in Chapter 11 reorganization (12,000 firms). Note that approximately 40% of the bankrupt firms filing for Chapter 11 convert to Chapter 7.

[11] Note that census blocks are not delineated based on population. In fact, about 45% of the census blocks do not have any population, while a block that includes an apartment complex might have several hundred inhabitants US Census Bureau, 1994.

[12] A related issue is that block boundaries are sometimes redrawn, which could lead to inconsistent block codes over time. To mitigate this issue, we replace inconsistent block codes by the most recent block code e.g., if an establishment has inconsistent block codes in 1996 and 1997, we use the 1997 block code. This correction is immaterial for our results-we obtain almost identical estimates if we use the opposite approach, that is, rely on the earliest available block code to fix inconsistencies.

As can be seen, Chapter 7 establishments are, on average, smaller compared to Chapter 11 establishments (28.0 versus 38.8 employees), have lower payroll per employee ($19,600 versus $22,700), and belong to smaller companies (2.7 versus 8.1 establishments; 72 versus 309 employees). The latter is in line with prior research documenting that Chapter 7 firms tend to be smaller than Chapter 11 firms (e.g., Bris et al., 2006).

The table also provides additional statistics at the block, block group, and tract level. As is shown, the average census block in our sample consists of 55.5 establishments corresponding to 1105 employees, capturing fairly dense areas. When we contrast the blocks of Chapter 7 and Chapter 11 establishments, again, we observe systematic differences. In particular, Chapter 7 blocks are, on average, smaller (50.6 versus 56.5 establishments; 926 versus 1143 employees), and they are populated by smaller establishments (16.1 versus 19.5 employees per establishment).

These systematic differences are apparent in Fig. 1, which plots the average log number of employees (Panel A) and log number of establishments (Panel B) over time for the blocks of Chapter 7 and Chapter 11 establishments. From three years before bankruptcy until five years after, blocks with Chapter 11 bankruptcies are larger than those with Chapter 7 bankruptcies. In fact, both groups have parallel trends prior to the bankruptcy filing.[13] After bankruptcy, blocks with Chapter 7 establishments shrink much more quickly than Chapter 11 blocks, a finding that foreshadows our main results. However, these differences could be due to selection effects, as the bankruptcy treatment is not randomly assigned and hence the need for identification in assessing the externalities of liquidation versus reorganization. We discuss our identification strategy in detail in the next section.

## 5. Identification strategy

### 5.1. Empirical design

Quantifying the local spillovers of liquidation (Chapter 7) relative to reorganization (Chapter 11) is challenging due to the inherent selection into bankruptcy regimes. For example, companies filing for Chapter 7 directly may operate in declining areas, which could bias our estimate of local spillovers. To mitigate this selection issue, we focus only on firms that filed for Chapter 11 reorganization and exploit the fact that a significant fraction (40%) of these firms are converted to Chapter 7 liquidation subsequently. We then quantify the local spillovers of liquidation by estimating the following specification:

$$Y_{l,t+m} = \alpha + \beta \cdot Liquidation_{pit} + \gamma \cdot X_{lpit} + \mu_k + \epsilon_{lpi}, \qquad (1)$$

where $l$ indexes location (e.g., blocks, block groups, and tracts) around bankrupt establishment $p$, which belongs to bankrupt firm $i$. The year of the bankruptcy filing is $t$, and $k$ defines the industry of the bankrupt firm. The main dependent variable $Y_{l,t+m}$ is the annualized percentage change in employment at the location $l$ of the bankrupt establishment (excluding employment of the bankrupt establishment itself) in the $m$ years following the bankruptcy filing year. In most of the analysis we will focus on the five years after the bankruptcy filing.[14]

In most specifications, we also examine the change in number of establishments, which is similarly defined as the percent change from its level in the year of the bankruptcy filing.[15] $Liquidation_{pit}$ is a dummy variable equal to one if establishment $p$ belongs to a company $i$ whose Chapter 11 filing (in year $t$) is converted into Chapter 7 liquidation. The decision of whether the case is converted to Chapter 7 liquidation or remains in Chapter 11 reorganization is typically taken in the bankruptcy filing year.[16] $X_{lpit}$ is a vector of prebankruptcy characteristics at the establishment, firm, and location level.[17] We further include two-digit The North American Industry Classification System (NAICS) industry fixed effects to account for unobserved heterogeneity at the industry level. The coefficient of interest is $\beta$, which captures the local externalities of liquidation relative to reorganization.

A caveat of specification (1) is that, even among Chapter 11 filers, there might be a substantial amount of selection among firms that convert to Chapter 7. Symptomatic of this issue are the differences in Table 1 – e.g., Chapter 7 firms have fewer establishments, fewer employees, and operate in smaller census blocks. Naturally, these differences raise concerns that Chapter 7 firms may differ based on unobservables as well. For example, firms that are converted to Chapter 7 may typically reside in less resilient areas. Under this scenario, a negative shock at the local level may trigger both the conversion to Chapter 7 and the decline of the local area.

To mitigate this concern, we use an instrumental variable that exploits the heterogeneity among bankruptcy judges in their propensity to convert Chapter 11 filings into Chapter 7 liquidation. This instrument does not rely on differences in actual bankruptcy laws, as the bankruptcy code is uniform at the federal level. Rather, the instrument makes use of the fact that bankruptcy judges'

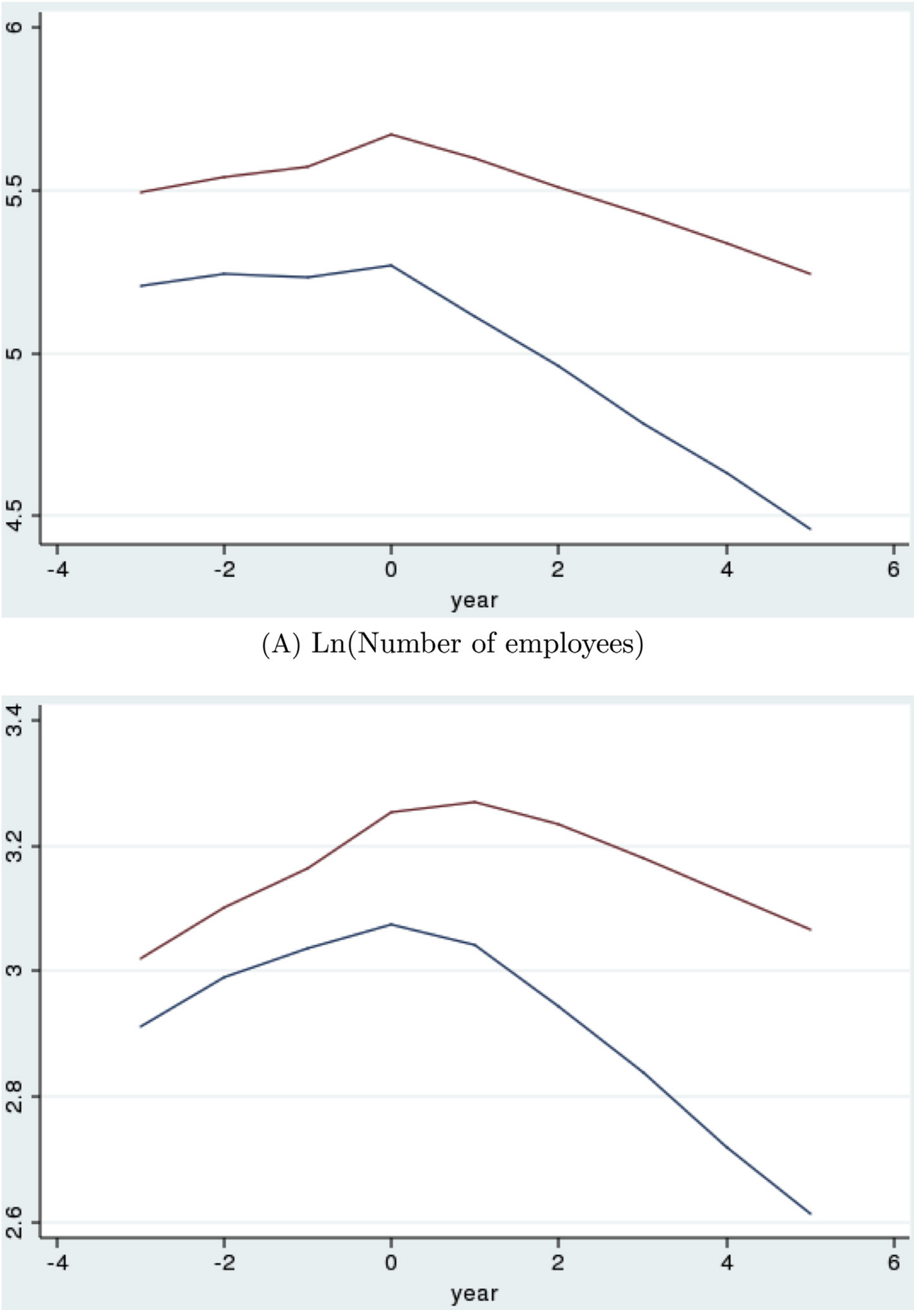
[13] The number of establishments increases for both groups in the years leading up to the bankruptcy, but this is mechanical, since we require establishments to exist in year 0 to be included in our sample.

[14] More precisely, $y = \frac{\#emp_5 - \#emp_0}{\#emp_0}$, where $\#emp$ is the total number of employees in the same block, block group, or tract as the bankrupt establishment (net of the employees of the bankrupt establishment). Year 0 is the year of the bankruptcy filing. Year 5 is five years after the bankruptcy filing. For ease of exposition, we annualize this five-year growth rate.

[15] In addition to employment and number of establishments, in auxiliary analyses we examine the impact of liquidation on wages per employee and for manufacturing establishments, output, productivity, operating margin, and investment (see Sections 6.4 and 7.2.1).

[16] To verify this, we examined the court documents of 200 randomly selected cases in our sample and found that for the median case, the time between case filing and a decision on whether the case will remain in Chapter 11 or be converted to Chapter 7 is four months.

[17] The firm-level controls include (i) log(employment) of the bankrupt firm, (ii) log(establishments) of the bankrupt firm, and (iii) a dummy variable indicating whether other related firms (e.g., subsidiaries of the same firm) also filed for bankruptcy at the same time. The establishment-level control is log(employment) of the bankrupt establishment. Finally, the block-level control is log(employment) in the block of the bankrupt establishment. All controls are measured in the year of the bankruptcy filing (year 0).




(A) Ln(Number of employees)



(B) Ln(Number of establishments)

**Fig. 1.** Evolution of block employment and number of establishments around bankruptcy filing. These figures use the raw data to plot the evolution of block log total employment (Panel A) and log number of establishments (Panel B) from three years before the bankruptcy until five years after. In both panels the blue line plots the average across all census blocks with a Chapter 7 liquidation, and the red line plots the average across blocks with a Chapter 11 reorganization. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

interpretation of the law varies significantly (e.g., LoPucki and Whitford, 1993; Bris et al., 2006; Chang and Schoar, 2013).

Bankruptcy judges work in 276 divisional offices across the US, each of which pertains to one of 94 US bankruptcy districts. A firm filing for bankruptcy may choose to file either where it is (i) headquartered, (ii) incorporated, or (iii) does most of its business. Once a filing is made in a particular division, judge assignment is random.[18] We can

[18] As an example, consider the bankruptcy district of New Jersey, which is divided into three divisions: Camden, Newark, and Trenton. The Local

then rely on this random assignment to generate exogenous variation in the probability that a given case is converted to Chapter 7, since judges vary in their propensity to convert filings. We implement this instrumental variable approach by estimating the following first-stage regression:

$$Liquidation_{pit} = \rho + \pi \cdot ShareCasesConverted_j + \lambda \cdot X_{lpit} + \delta_{dt} + \mu_k + \eta_{lpi}, \quad (2)$$

where $ShareCasesConverted_j$ is the share of Chapter 11 cases that judge $j$ ever converted to Chapter 7, excluding the current case.[19] Importantly, the inclusion of division-by-year fixed effects, $\delta_{dt}$, ensures that we exploit the random variation in judge assignment within a division-year cell. The coefficient $\pi$ captures the extent to which a judge $j$'s propensity to convert a case to Chapter 7 affects the probability that a given case is converted into Chapter 7 liquidation.

We then estimate the following second-stage regression:

$$y_{l,t+m} = \alpha + \beta \cdot \overline{Liquidation}_{pit} + \gamma \cdot X_{lpit} + \delta_{dt} + \mu_k + \epsilon_{lpi}, \quad (3)$$

where $\overline{Liquidation}_{pi}$ are the predicted values from the first-stage regression. The second-stage regression mirrors the Ordinary Least Squared regression in Eq. (1), except that it relies on the exogenous component of Liquidation–i.e., the component that is induced by the randomization of bankruptcy judges.

In all regressions, we cluster standard errors at the division by year level. Doing so accounts for any arbitrary correlation of the error terms within bankruptcy courts. Lastly, we weight all regressions by the inverse of the number of establishments operated by the bankrupt firm to ensure that each firm receives the same weight and hence avoid overweighting large bankruptcy cases.

If the conditions for a valid instrumental variable are met, $\beta$ captures the causal effect of Chapter 7 liquidation on local employment and other outcomes of interest, relative to reorganization. It is important to note that the estimates in the instrumental variables analysis are coming only from the sensitive firms–i.e., firms that switch bankruptcy regimes because they were randomly assigned to a judge who commonly converts cases to Chapter 7 (Imbens and Angrist, 1994). Clearly, some firms will stay in Chapter 11 no matter the judge, and other firms will convert to Chapter 7 regardless of the judge. Thus, the instrumental variables estimates only capture the local average treatment effect on the sensitive firms and should be interpreted as such.

### 5.2. Validity of the instrument

To be valid, our instrument needs to strongly affect the probability of converting a Chapter 11 filing into Chapter 7 liquidation (first-stage condition). Moreover, the instrument needs to be unrelated to the evolution of the bankrupt establishment's local area (exclusion restriction). In the following section, we discuss both conditions.

#### 5.2.1. First stage

Table 2 presents the results of the first-stage regression, which confirms that the instrument strongly affects the probability of conversion to Chapter 7 liquidation. In column (1), the regression includes division-by-year fixed effects. In column (2), we also include control variables. In column (3), we further include industry fixed effects. As is shown, the coefficient of share of cases converted is economically large and highly significant in all specifications. The estimates of 0.58–0.59 imply that a one-standard deviation increase in the instrument (0.13) corresponds to an increase in the probability of Chapter 7 liquidation by 7.5–7.6%, or a 12.2–12.3% increase compared to the unconditional probability of 40%. In addition, the instrument is "strong" in a statistical sense. The $F$-statistic ranges between 75.7 and 80.0, which is well above the $F = 10$ threshold of Staiger and Stock (1997) and the critical values of Stock and Yogo (2005). This alleviates concerns about weak instruments.

#### 5.2.2. Exclusion restriction

The exclusion restriction requires that our instrument, judge leniency, has no direct effect on postbankruptcy changes in employment at the location of the bankrupt establishment other than through the effect on the probability of conversion to Chapter 7 liquidation. The random allocation of bankruptcy judges, while not sufficient, strongly supports that the exclusion restriction is satisfied–analogous to the ideal setting of randomized experiments. In Table 3, we conduct randomization tests showing that the instrument is uncorrelated with a large set of covariates and industry fixed effects.

Column (1) shows that the $R^2$ from regressing the share of cases converted on division-by-year fixed effects alone is 0.78, suggesting that there is substantial variation in judge conversion propensities between divisions and over time. However, the random assignment of judges occurs within court divisions, and therefore we want to verify that covariates are orthogonal to the instrument conditional on the division-by-year fixed effects. In columns (2)–(5), we include industry fixed effects and controls. Column (2) includes the baseline controls. In columns (3)–(5), we further include pre-trends in employment (that is, the change in employment in the three years preceding the bankruptcy filing in the block, block group, and tract, respectively, of

Rules of the New Jersey Bankruptcy Court lay out exactly which counties pertain to each division, and firms must file in the division "in which the debtor has its principal place of business." Once a case is filed in a particular division, the Local Rules state that "case assignments shall be made by the random draw method used by the Court" (D.N.J. LBR 1073–1). More broadly, the random assignment of bankruptcy judges within districts is an important feature of the US bankruptcy process. The rationale is to help ensure a fair distribution of cases and prevent "judge shopping," or parties' attempts to have their cases heard by the judge who they believe will act most favorably.

[19] This standard leave-one-out measure deals with the mechanical relationship that would otherwise exist between the instrument and the conversion decision for a given case. We have experimented with alternative definitions of the instrument as well: (i) the share of cases that judge $j$ converted to Chapter 7 in the five years prior to the current case and (ii) judge fixed effects. Both the first- and second-stage results are unaffected by the choice of the instrument.

**Table 2**
First stage.

| Dependent variable: | Liquidation | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Share of cases converted | 0.578*** | 0.589*** | 0.588*** |
| | (0.066) | (0.066) | (0.066) |
| *a. Firm-level controls* | | | |
| log(employees of bankrupt firm) | | −0.032*** | −0.029*** |
| | | (0.004) | (0.004) |
| log(establishments of bankrupt firm) | | −0.008 | −0.016*** |
| | | (0.005) | (0.006) |
| *b. Establishment-level control* | | | |
| log(employees of bankrupt establishment) | | 0.012*** | 0.010*** |
| | | (0.003) | (0.003) |
| *c. Block-level control* | | | |
| log(employees at block of bankrupt establishment) | | −0.027*** | −0.026*** |
| | | (0.002) | (0.002) |
| Division-year fixed effects | Yes | Yes | Yes |
| Industry fixed effects | No | No | Yes |
| *F*-stat for instrument | 75.73 | 79.34 | 80.01 |
| Adjusted *R*-squared | 0.140 | 0.164 | 0.173 |
| Observations | 91,000 | 91,000 | 91,000 |

This table reports estimates from the first-stage regression. The dependent variable, liquidation, is a dummy variable that indicates whether the establishment belongs to a company whose bankruptcy filing is converted from Chapter 11 reorganization to Chapter 7 liquidation. Share of cases converted is the share of all other Chapter 11 cases that a judge converted to Chapter 7. The controls are self-explanatory. All regressions include division-by-year fixed effects. The regression in column (3) further includes two-digit NAICS industry fixed effects and a dummy indicating whether other related firms (e.g., subsidiaries of the same firm) also filed for bankruptcy at the same time. The sample includes all establishments belonging to companies that filed for Chapter 11 bankruptcy between 1992 and 2005. Standard errors, clustered at the division-by-year level, are reported in parentheses. *, **, and *** denotes statistical significance at the 10%, 5%, and 1% level, respectively.

the bankrupt establishment), controls for the block's industry composition (which include the share of employment in the block that is in tradable and nontradable industries, compared to the omitted category of services), and an indicator for whether the bankrupt firm had multiple establishments. None of the controls is statistically significant and all point estimates are close to zero. Further, the industry fixed effects are jointly insignificant, and the $R^2$ remains unchanged in all specifications. Overall, this evidence lends strong support to the randomization assumption.[20]

The exclusion restriction assumption might still be violated if a judge's tendency to liquidate is correlated with other judge characteristics that affect neighboring firms. For example, it could be that lenient judges are also more likely to internalize spillovers and thus take actions that mitigate externalities on nonbankrupt firms. If this is how firms are liquidated (or reorganized) in the economy, then naturally, this is also the liquidation treatment in our setting–that is, we cannot separate the law from the way it is implemented. In that case, the liquidation treatment should not be viewed more broadly than just the motion to convert to Chapter 7, and rather as the package of motions and judge characteristics that typically lead to conversion. While this would not invalidate our results, it would affect the interpretation of the results.

To explore this broader interpretation, we regress changes in local employment directly on the judge leniency instrument in reduced-form specifications separately for firms that are reorganized and those that are liquidated (Table A.2 in the Appendix). Arguably, if judge leniency is systematically correlated with other actions taken by judges (aside from conversion to liquidation) that cause spillovers on local firms, then we should continue to find a relationship between our instrument and local economic growth, even when conditioning the sample in this way. However, we find that the coefficient of the instrument is small and insignificant in both bankruptcy regimes, which is inconsistent with such broader interpretation.

In further support of this, we also examine whether judges have a large effect on bankruptcy cases before making a decision on whether to convert a case or not. Based on a random sample of 200 cases, we calculate that the median time between the bankruptcy filing and the selection of the bankruptcy regime (either liquidation or reorganization) is only four months. Further, we find that typically no significant motions are passed in the case prior to a ruling on a motion to convert the case. Lastly, we also note that Chang and Schoar (2013) – who use detailed data on court motions to perform a principal component analysis on a set of the most important rulings of a bankruptcy judge in an effort to identify pro-debtor judges – find that the motion to convert a case receives by far the

[20] In Appendix Table A.1 we show that the first stage is unaffected when we control for employment pre-trends and block composition. Figs. 2 and 3 also demonstrate that there are no pre-trends in the IV analysis.

**Table 3**
Randomization.

| Dependent variable: | Share of cases converted | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *a. Firm-level controls* | | | | | |
| log(employees of bankrupt firm) | | 0.0009 | 0.0009 | 0.0009 | 0.0009 |
| | | (0.001) | (0.001) | (0.001) | (0.001) |
| log(establishments of bankrupt firm) | | −0.0015 | −0.0015 | −0.0014 | −0.0012 |
| | | (0.001) | (0.001) | (0.001) | (0.001) |
| Multi-establishment firm | | | | | −0.0008 |
| | | | | | (0.002) |
| *b. Establishment-level control* | | | | | |
| log(employees of bankrupt establishment) | | −0.0001 | −0.0001 | −0.0001 | −0.0001 |
| | | (0.001) | (0.001) | (0.001) | (0.001) |
| *c. Block-level control* | | | | | |
| log(employees at block of bankrupt establishment) | | 0.0000 | 0.0000 | 0.0000 | 0.0000 |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| *d. Employment change in the three years prior to bankruptcy* | | | | | |
| % change in employment (block level) | | | −0.0000 | | −0.0000 |
| | | | (0.000) | | (0.000) |
| %change in employment (block-group level) | | | 0.0003 | | 0.0003 |
| | | | (0.000) | | (0.000) |
| % change in employment (tract level) | | | 0.0001 | | 0.0000 |
| | | | (0.001) | | (0.001) |
| *e. Block composition* | | | | | |
| % employment in nontradable | | | | 0.0002 | 0.0002 |
| | | | | (0.002) | (0.002) |
| % employment in tradable | | | | 0.0015 | 0.0015 |
| | | | | (0.002) | (0.002) |
| Division-year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Industry fixed effects | No | Yes | Yes | Yes | Yes |
| *F*-test for joint significance of industry FE | – | 0.439 | 0.438 | 0.442 | 0.442 |
| Adjusted *R*-squared | 0.777 | 0.777 | 0.777 | 0.777 | 0.777 |
| Observations | 91,000 | 91,000 | 91,000 | 91,000 | 91,000 |

This table reports randomization tests to illustrate the random assignment of bankruptcy judges within a division. The dependent variable, share of cases converted, is the share of all Chapter 11 cases that a judge converted to Chapter 7, excluding the current case. The right-hand side variables are self-explanatory. All regressions include division-by-year fixed effects. The regressions in columns (2)-(5) further include two-digit NAICS industry fixed effects and a dummy indicating whether other related firms (e.g., subsidiaries of the same firm) also filed for bankruptcy at the same time. The sample includes all establishments belonging to companies that filed for Chapter 11 bankruptcy between 1992 and 2005. Standard errors, clustered at the division-by-year level, are reported in parentheses. *, **, and *** denotes statistical significance at the 10%, 5%, and 1% level, respectively.

lowest weight in the first principal component. This suggests that the decision to convert may be mostly unrelated to a judge's overall pro-debtor or pro-creditor bias, as opposed to other motions. Hence, while we cannot fully reject the broader interpretation of the liquidation treatment, we find little evidence in support thereof.

## 6. Results

### 6.1. Main results

Table 4 presents the main results in which we focus on how liquidation affects neighboring firms, relative to reorganization. In columns (1) and (2), the dependent variable is the annualized percentage change in employment in the block of the bankrupt establishment within the five-year period following the bankruptcy filing (excluding employment of the bankrupt establishment itself). All regressions include the baseline controls, industry fixed effects, and division-by-year fixed effects. The OLS estimate reported in column (1), which does not account for selection, shows that liquidation is associated with an annual employment growth rate that is 2.5 percentage points lower relative to reorganization. The IV-2 Stage Least Square (SLS) estimate in column (2), which relies on the random assignment of bankruptcy judges, is somewhat larger in magnitude.[21] It implies that liquidation leads to an annual employment growth rate that is 4.0 percentage points lower relative to reorganization.

In columns (3) and (4), the dependent variable is the annualized percentage change in the number of establishments in the block of the bankrupt establishment within the five-year period following the bankruptcy filing (again excluding the bankrupt establishment). The results mirror those in columns (1) and (2). Specifically, the estimates

[21] While the IV coefficient is somewhat larger in magnitude than the OLS estimate, the two are not statistically significantly different from each other. Further, it is unclear in which direction the OLS coefficient might be biased due to endogeneity. On one hand, liquidation might be more prevalent in areas with poor economic growth, which would bias the OLS coefficient downwards. On the other hand, it is possible that liquidation is more likely to occur in areas with strong economic growth with many potential buyers of the assets, which would bias the OLS coefficient upwards.

**Table 4**
Main results.

| Dependent variable: | Employment | | Establishments | |
|---|---|---|---|---|
| Model | OLS (1) | IV-2SLS (2) | OLS (3) | IV-2SLS (4) |
| Liquidation | −0.025*** (0.002) | −0.040** (0.017) | −0.037*** (0.002) | −0.047*** (0.016) |
| Control variables | Yes | Yes | Yes | Yes |
| Division-year fixed effects | Yes | Yes | Yes | Yes |
| Industry fixed effects | Yes | Yes | Yes | Yes |
| Adjusted $R$-squared | 0.154 | 0.190 | 0.322 | 0.162 |
| Observations | 91,000 | 91,000 | 91,000 | 91,000 |

In columns (1) and (2), the dependent variable is the annualized percentage change in employment in the census block of the bankrupt establishment (excluding employment of the bankrupt establishment) in the five years following the bankruptcy filing. The dependent variable in columns (3) and (4) is defined similarly with respect to the number of establishments. Liquidation is a dummy variable that indicates whether the establishment belongs to a company whose case is converted from Chapter 11 reorganization to Chapter 7 liquidation. The regressions in columns (1) and (3) are estimated by OLS; the regressions in columns (2) and (4) are estimated by 2SLS using as instrument the share of cases converted. All regressions contain the full set of controls used in column (3) of Table 2. The sample includes all establishments belonging to companies that filed for Chapter 11 bankruptcy between 1992 and 2005. Standard errors, clustered at the division-by-year level, are reported in parentheses. *, **, and *** denotes statistical significance at the 10%, 5%, and 1% level, respectively.

imply that the number of establishments drops by 3.7–4.7% per year. Overall, the results in Table 4 indicate that liquidation imposes large negative externalities on the immediate surroundings of the liquidated establishment.[22]

In Figs. 2 and 3, we examine the dynamics of the local externalities at the block level. Specifically, we estimate variants of the regressions in columns (2) and (4) of Table 4, but instead of using as the dependent variable the annual change in employment (and number of establishments, respectively) over a five-year period, we now consider horizons of 3 years before to 5 years following the filing date and report the cumulative change in employment.

Fig. 2 plots the coefficients (along with the 95% confidence bounds) pertaining to the employment regressions. Importantly, we find no pre-trends in employment dynamics prior to the bankruptcy filing, consistent with judge random assignment. After bankruptcy, the local externalities take time to materialize. After one year, the decrease in employment is relatively modest. It is only after two years that it becomes sizable, with further increases in years three and four, at which point the estimate becomes statistically significant. Finally, the effect stabilizes somewhat after four to five years. Similarly, in Fig. 3, we repeat this analysis for the number of establishments and find that the number of establishments declines monotonically and becomes statistically significant in year 2 and stabilizes in years 4 and 5.[23]

The estimates in Table 4 and Figs. 2 and 3 represent averages across all census blocks in our sample. But intuitively, one would expect that the effect of a liquidated plant on a block will be significantly smaller if the bankrupt plant is small relative to the size of the block. We explore this idea in Table 5. In this table, we estimate our main specification separately for blocks with different sizes relative to the size of the bankrupt plant, that is, different levels of $\frac{emp_{0,block}}{emp_{0,estab}}$, where $emp_{0,\,block}$ is the number of employees in the block at the time of bankruptcy (excluding the bankrupt establishment) and $emp_{0,\,estab}$ is the number of employees at the bankrupt establishment. In column (2), we show that when $\frac{emp_{0,block}}{emp_{0,estab}} > 100$, which roughly corresponds to the top quartile of the relative size distribution, the effect of liquidation is indistinguishable from zero. This is expected, since the bankrupt establishment is small relative to the size of the block. Meanwhile, when $\frac{emp_{0,block}}{emp_{0,estab}} < 100$, in column (3) we estimate that liquidation reduces employment growth by 5.2 percentage points per year. Columns (4) and (5) show similar results when we use a lower threshold of $\frac{emp_{0,block}}{emp_{0,estab}} < 70$, which corresponds to the top tercile of the relative size distribution. These findings help put the economic magnitudes in proper perspective. In particular, they show that the 20% five-year decline in employment shown in Fig. 2 would not be expected in relatively larger blocks; indeed, we would expect a little overall decline in employment in these blocks. Meanwhile, when the block is small relative to the bankrupt establishment, we estimate a larger cumulative decline in employment of 26%, but since the block is small in this case, this corresponds to a relatively small number of lost jobs in the block. We use these estimates to calculate a local multiplier of liquidation in Section 7.1.

[22] One might expect that these effects are largest in more densely populated areas, where firms are closer together and thus can more easily form connections. We test this idea in Appendix Table A.7, finding that coefficients are larger in denser areas. However, the differences are not statistically significant.

[23] In Table A.3 in the Appendix we explore the robustness of these main results along several dimensions. For example, we apply a more conservative trimming of the dependent variables, remove blocks in which employment drops to zero by year 5, split the sample by block size, and remove blocks that contain more than a single establishment owned by the bankrupt firm. In all cases, the results remain unchanged.

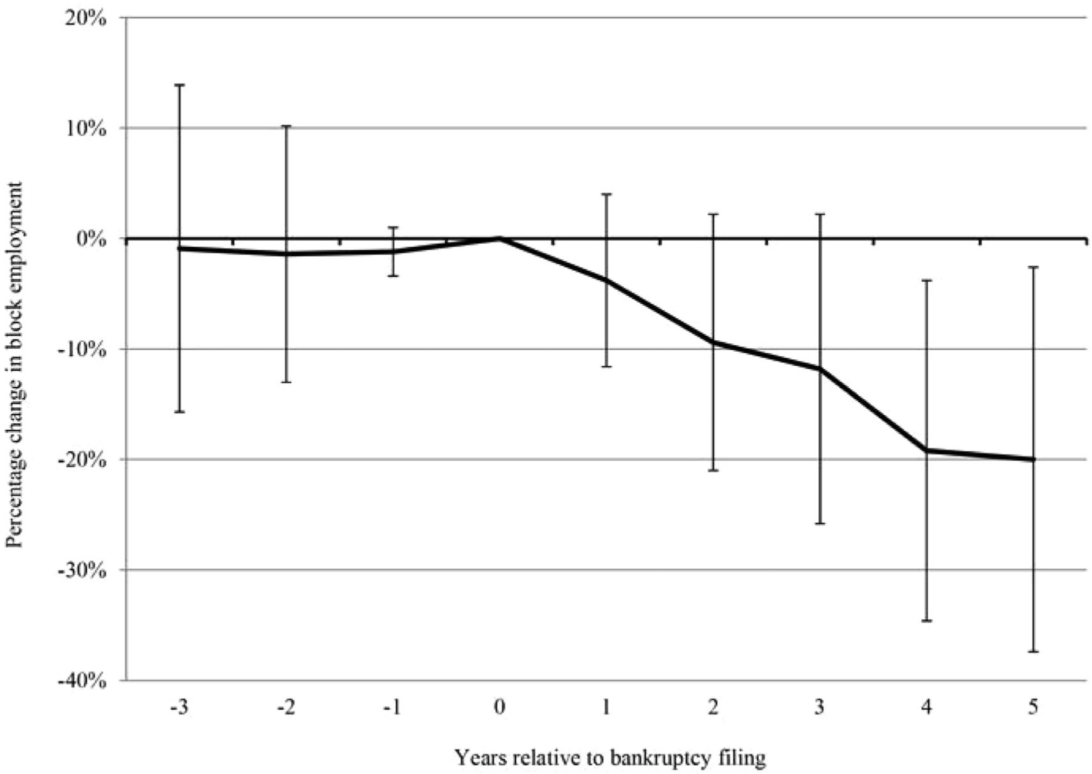



**Fig. 2.** Evolution of employment in the liquidated establishments' blocks. This figure plots the evolution of employment in the census blocks of Chapter 7 bankruptcies (i.e., establishments belonging to companies that are liquidated) compared to the census blocks of Chapter 11 establishments (i.e., establishments belonging to companies that are reorganized) from three years before the bankruptcy filing until five years after. The *y*-axis indicates the (cumulative) percentage change in employment compared to the year of the bankruptcy filing (year 0). The *x*-axis indicates the year relative to the bankruptcy filing. Error bars show the 95% confidence bounds.

**Table 5**
Heterogeneity by relative size.

| Dependent variable: | Employment | | | | |
|---|---|---|---|---|---|
| Block-to-estab. emp ratio | All<br>(1) | Top quartile<br>(2) | Excluding top quartile<br>(3) | Top tercile<br>(4) | Excluding top tercile<br>(5) |
| Liquidation | −0.040** <br>(0.017) | −0.004<br>(0.030) | −0.052**<br>(0.020) | −0.016<br>(0.028) | −0.057***<br>(0.022) |
| Control variables | Yes | Yes | Yes | Yes | Yes |
| Division-year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Industry fixed effects | Yes | Yes | Yes | Yes | Yes |
| Observations | 91,000 | 27,000 | 64,000 | 31,000 | 60,000 |

This table presents estimates from regressions similar to those in column (2) of Table 4 for various subsamples of our data. In column (1) we repeat the regression from Table 4 on the full sample for reference. In the remaining columns we test whether the treatment effects vary by the size of the census block relative to the bankrupt establishment. In particular, we calculate the ratio of block employment to bankrupt establishment employment at the time of the bankruptcy. In column (2) we include only blocks that have over 100 times more employees than the bankrupt establishment, which corresponds approximately to the top quartile of the distribution and in column (3) blocks that are less than 100 times the size of the establishment. In columns (4) and (5) we use a threshold of 70, which corresponds to the top tercile, for robustness. Standard errors, clustered at the division-by-year level, are reported in parentheses. *, **, and *** denotes statistical significance at the 10%, 5%, and 1% level, respectively.

Additionally, it is important to note that the effect we capture is at the block level, a very localized geographical unit (we consider larger geographical units in Section 4.3 and find that the effect decays with distance). This pattern is consistent with the agglomeration literature that typically finds that agglomeration spillovers are large but localized (Rosenthal and Strange, 2003; Arzaghi and Henderson, 2008; Ahlfeldt et al., 2015).[24]

[24] The same applies to other types of spillovers, e.g., default spillovers in the residential mortgage market (Campbell et al., 2011; Gupta, 2016) and knowledge spillovers through patenting (Jaffe et al., 1993).

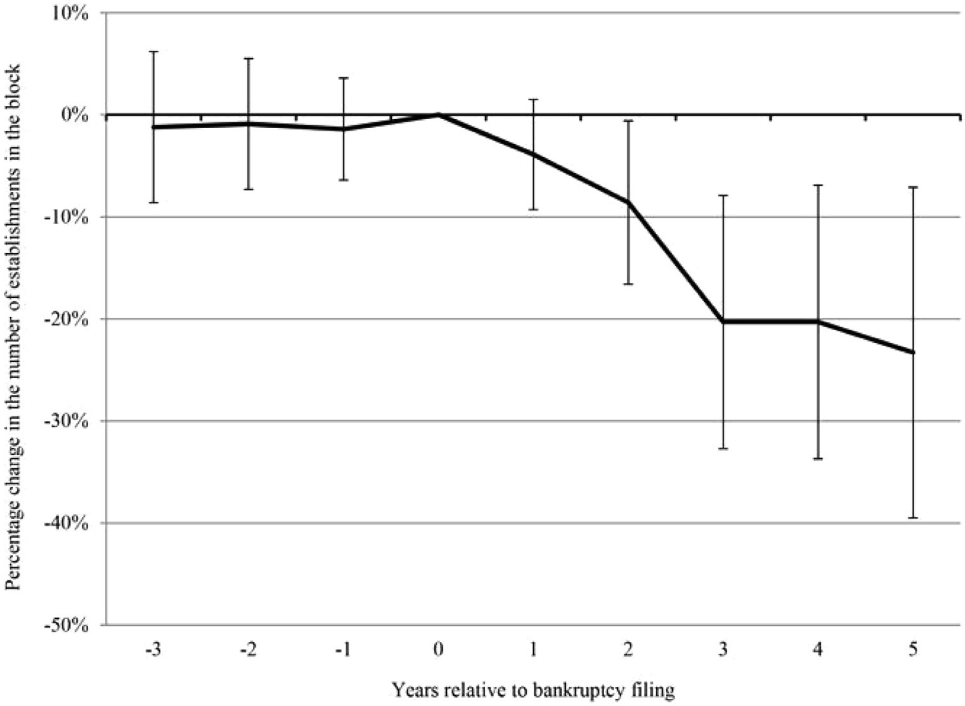



**Fig. 3.** Evolution of the number of establishments in the liquidated establishments' blocks. This figure plots the evolution of the number of establishments in the census blocks of Chapter 7 bankruptcies (i.e., establishments belonging to companies that are liquidated) compared to the census blocks of Chapter 11 establishments (i.e., establishments belonging to companies that are reorganized) from three years before the bankruptcy filing until five years after. The *y*-axis indicates the (cumulative) percentage change in the number of establishments compared to the year of the bankruptcy filing (year 0). The *x*-axis indicates the year relative to the bankruptcy filing. Error bars show the 95% confidence bounds.

Naturally, it could be that some of the employment losses we observe at the local level are offset by the reallocation of employees into broader areas. As such, our results do not speak to the question of whether liquidation is good or bad in a macroeconomic sense. Rather, our results establish that liquidation (compared to reorganization) imposes large negative externalities on the local economy and quantify such externalities. We discuss the magnitudes of our estimates in depth in Section 7.

### 6.2. Decomposing the effect into entrants and existing establishments

The previous section explored the spillover effects of liquidation on aggregate employment and the number of establishments, relative to reorganization. In this section we decompose these aggregate effects to examine separately how liquidation spillovers affect existing establishments and how it affects the entry of new establishments to the area. In particular, we are interested in understanding the extent to which the aggregate negative spillovers of liquidation are driven by the disruption of agglomeration linkages among existing firms and whether liquidation leads to a "creative destruction" process by increasing entry to the area. This creative destruction may occur if liquidation frees up resources, such as employees, machinery, and real estate, that were previously unavailable. To do so, we decompose the overall changes in employment into changes in (i) employment of existing establishments ("existing") and (ii) employment of new establishments ("new"):

$$\Delta emp_{pi5} = \frac{\#emp_5 - \#emp_0}{\#emp_0} = \frac{(\#emp_5^{new} - 0) + (\#emp_5^{existing} - \#emp_0^{existing})}{\#emp_0}, \tag{4}$$

where $\#emp_m$ is the number of employees in a given census block in year m after the bankruptcy filing (excluding the bankrupt establishment). Similarly, $\#emp_m^{existing}$ is the number of employees working at establishments that existed in the year of the bankruptcy filing, and $\#emp_m^{new}$ is the number of employees that are working at establishments that did not exist in the year of the bankruptcy filing and thus entered later into the area. Therefore, this decomposition separately explores the effect of liquidation on the share of employment growth due to existing establishments and the portion of employment growth due to the entry of new establishments.

We decompose the change in the number of establishments analogously. Specifically, we distinguish between changes in the number of existing establishments due to establishment closures and changes in entry due to establishment openings:

**Table 6**
Decomposition.

| Panel A: Change in employment | | | |
|---|---|---|---|
| Dependent variable: | Employment | | |
| | All establishments (1) | Existing establishments (2) | New establishments (3) |
| Liquidation | −0.040** (0.017) | −0.030** (0.014) | −0.010 (0.010) |
| Control variables | Yes | Yes | Yes |
| Division-year fixed effects | Yes | Yes | Yes |
| Industry fixed effects | Yes | Yes | Yes |
| Model | IV-2SLS | IV-2SLS | IV-2SLS |
| Adjusted $R$-squared | 0.190 | 0.025 | 0.012 |
| Observations | 91,000 | 91,000 | 91,000 |
| **Panel B: Change in number of establishments** | | | |
| Dependent variable: | Establishments | | |
| | All establishments (1) | Existing establishments (2) | New establishments (3) |
| Liquidation | −0.047*** (0.016) | −0.046*** (0.012) | −0.002 (0.010) |
| Control variables | Yes | Yes | Yes |
| Division-year fixed effects | Yes | Yes | Yes |
| Industry fixed effects | Yes | Yes | Yes |
| Model | IV-2SLS | IV-2SLS | IV-2SLS |
| Adjusted $R$-squared | 0.162 | 0.129 | 0.061 |
| Observations | 91,000 | 91,000 | 91,000 |

The regressions in Panel A are variants of the regression in column (2) of Table 4, which is reproduced in column (1). Columns (2) and (3) decompose the change in employment into (i) employment changes from existing establishments and (ii) employment changes from new establishments. Similarly, Panel B contains regressions that are are variants of the regression in column (4) of Table 4, with columns (2) and (3) decomposing the number of establishments into i) the number of establishment closures ("deaths") and ii) the number of establishment openings ("births"). The sample includes all establishments belonging to companies that filed for Chapter 11 bankruptcy between 1992 and 2005. Standard errors, clustered at the division-by-year level, are reported in parentheses. *, **, and *** denotes statistical significance at the 10%, 5%, and 1% level, respectively.

$$\Delta plants_{pi5} = \frac{\#plants_5 - \#plants_0}{\#plants_0} = \frac{(\#plants_5^{birth} - 0) + (0 - \#plants_0^{death})}{\#plants_0}. \quad (5)$$

The results are presented in Panels (a) and (b) of Table 6.[25] In columns (2) and (3) of Panel (a), we estimate variants of the baseline specification -reproduced in column (1)-, decomposing the change in employment into its two components. As can be seen, the decline in employment operates mostly through a decrease in employment within existing establishments of 3 percentage points per year, which captures 75% of the overall effect. To a lesser extent, the effect is also driven by a decline in employment due to less entry into the region of 1 percentage point per year (which captures 25% of the overall effect). We obtain a similar pattern in Panel (b) where we decompose the number of establishments into closures and openings. As is shown, most of the effect comes from the closure of existing establishments.

These results are inconsistent with the creative destruction argument, according to which liquidation could help revitalize the local area by freeing up resources of distressed establishments to induce entry. Instead, our estimates indicate that, if anything, liquidation slightly deters entry into the area while simultaneously reducing employment at existing establishments.

### 6.3. Different geographical areas

In the analysis so far, we examined the spillovers of liquidation at the census block level–the smallest geographic unit used by the Census Bureau. This choice is intuitive given the small size of the average bankrupt establishment. Nevertheless, it is instructive to study larger geographies as well to explore how far reaching the relative effect of liquidation is. To do so, we explore spillover effects of bankruptcy in census block groups and census tracts. Following the terminology of Section 6.2, we separately explore the effect of liquidation on aggregate employment in Panel (a) of Table 7, employment in existing establishments in Panel (b), and employment associated with entry in Panel (c).

[25] As in the main analysis, we annualize the changes in employment (and number of establishments, respectively).

**Table 7**
Geographies.

| Panel A: Overall effect | | | | | |
|---|---|---|---|---|---|
| Dependent variable | Employment | | | Employment net of block of bankrupt establishment | |
| Geography | Blocks (1) | Block-groups (2) | Tracts (3) | Block-groups (4) | Tracts (5) |
| Liquidation | −0.040** (0.017) | −0.017 (0.010) | 0.001 (0.007) | −0.006 (0.014) | 0.005 (0.008) |
| Control variables | Yes | Yes | Yes | Yes | Yes |
| Division-year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Industry fixed effects | Yes | Yes | Yes | Yes | Yes |
| Observations | 91,000 | 91,000 | 91,000 | 91,000 | 91,000 |
| **Panel B: Existing establishments** | | | | | |
| Dependent variable | Employment | | | Employment net of block of bankrupt establishment | |
| Geography | Blocks (1) | Block-groups (2) | Tracts (3) | Block-groups (4) | Tracts (5) |
| Liquidation | −0.030** (0.014) | −0.008 (0.010) | 0.008 (0.007) | 0.003 (0.014) | 0.007 (0.008) |
| Control variables | Yes | Yes | Yes | Yes | Yes |
| Division-year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Industry fixed effects | Yes | Yes | Yes | Yes | Yes |
| Observations | 91,000 | 91,000 | 91,000 | 91,000 | 91,000 |
| **Panel C: New establishments** | | | | | |
| Dependent variable | Employment | | | Employment net of block of bankrupt establishment | |
| Geography | Blocks (1) | Block-groups (2) | Tracts (3) | Block-groups (4) | Tracts (5) |
| Liquidation | −0.010 (0.010) | −0.009 (0.007) | −0.007 (0.006) | −0.009 (0.008) | −0.002 (0.005) |
| Control variables | Yes | Yes | Yes | Yes | Yes |
| Division-year fixed effects | Yes | Yes | Yes | Yes | Yes |
| Industry fixed effects | Yes | Yes | Yes | Yes | Yes |
| Observations | 91,000 | 91,000 | 91,000 | 91,000 | 91,000 |

The regressions in Panel A are variants of the regression in column (2) of Table 4, which is reproduced in column (1). In columns (2) and (3), changes in employment are computed for the census block group and census tract, respectively. The regressions in columns (4) and (5) are analogous except that employment in the census block group (and census tract, respectively) is net of employment in the census block of the bankrupt establishment. The regressions in Panels B and C are analogous, except that the change in employment is decomposed into i) employment changes from existing establishments,and ii) employment changes from new establishments. The sample includes all establishments belonging to companies that filed for Chapter 11 bankruptcy between 1992 and 2005. Standard errors, clustered at the division-by-year level, are reported in parentheses. *, **, and *** denotes statistical significance at the 10%, 5%, and 1% level, respectively.

In column (2) of Panel (a), we examine how liquidation affects aggregate employment at the block group level. Census block groups are the next level above census blocks in the geographic hierarchy and consist of a set of contiguous blocks. The drop in employment is smaller (−1.7%) and marginally insignificant ($t = 1.60$). This suggests that the externalities of liquidation are localized–they are substantial in the immediate neighborhood of the liquidated establishment and decay with distance. In column (3), we further examine the impact of liquidation on employment at the tract level. Not surprisingly, no effect is found within such large areas–the coefficient is virtually zero and highly insignificant. The results are consistent with prior literature that illustrates that spillover effects decay quickly with geographic distance e.g., (e.g., Rosenthal and Strange, 2003; Arzaghi and Henderson, 2008; Ahlfeldt et al., 2015).

In columns (4) and (5) of Panel (a), we examine the potential reallocation of employees within broader geographies. To do so, we consider the change in employment in the block group (and tract, respectively), net of the block of the bankrupt establishment. As is shown, we find some evidence of reallocation within the tract. Specifically, the coefficient in column (5) is positive (0.005), although not statistically significant. This might suggest that some of the employment losses in the block of the liquidated establishment are offset by reallocation elsewhere within the tract. Indeed, given that tracts are more than seven times larger than blocks, taking this point estimate at face value would suggest that roughly 80% of the effect at the block level is offset by increases in employment elsewhere in the tract.[26]

We repeat the analysis separately for existing establishments in Panel (b) and for new establishments in Panel (c). As expected, we find that the results in Panel (b) mirror the results in Panel (a), and the spillover effects on existing establishments decay for larger geographical units. In Panel (c) we can further explore the creative destruction hypothesis that liquidation triggers entry, but this time, using larger geographical units such as census block groups and census tracts. Similar to the evidence in Section 6.2, we find no evidence for increased entry following liquidation events. In fact, in all geographical units we find that the coefficient of liquidation is negative, as shown in columns (1) through (3). In columns (4) and (5) we focus on census block groups and census tracts, respectively, net of the block itself. In both instances the coefficient of liquidation remains negative. In sum, even when exploring larger geographical units, we find no evidence for increased entry following the liquidation of the bankrupt firms, when compared to reorganization.

### 6.4. Industry sectors and underlying mechanisms

In this section we attempt to shed light on the underlying mechanisms through which bankruptcy generates spillovers on neighboring firms. We focus on three potential mechanisms. Specifically, liquidation may affect (1) customer traffic, (2) synergies between businesses, and (3) local demand for goods and services.

To examine these mechanisms, we decompose employment in each block into three broad industry sectors: (1) the nontradable sector (e.g., restaurants and retail) that relies on local demand and customer flow to the area, (2) the tradable sector (e.g., manufacturing) that is likely to rely on nonlocal demand, and (3) services (e.g., publishers, lawyers, accountants, and advertisement agencies, among others).[27] This decomposition into employment growth in each of these three sectors is provided in Panels (a), (b), and (c), respectively, of Table 8. In each Panel, column (1) considers the effect of all bankruptcies on employment growth in each of these three sectors, whereas columns (2)–(4) only consider bankruptcies in the nontradable, services, and tradable sectors, respectively, as treatments. In other words, the first column of each panel shows how the overall effect of −0.040 from column (2) of Table 4 is split across each of these three broad industry sectors: −0.014 comes from a decline in nontradable employment, −0.029 from services, and 0.003 from the tradable sector. Thus, the overall decline in employment is driven by the nontradable and services sectors. The results in columns (2)–(4) of Table 8 then provide all 3 × 3 = 9 combinations of sector-specific block-level employment and sector-specific bankruptcies. This helps shed light on which mechanism is driving the overall effects by examining how bankruptcies affect employment within and across these industry sectors.

We begin with the first channel, that is, the possibility that liquidation reduces customer traffic to a specific area. In Panel (a), the dependent variable is employment growth at the block level in the nontradable sector. The nontradable sector, which includes establishments such as restaurants and retail stores, depends on local demand and therefore may be affected by a decline in customer traffic to the area triggered by liquidation. In column (1) we find that, on average, liquidation leads to a decline in employment growth in the nontradable sector. In column (2), we focus only on bankruptcies in the nontradable sector and find that the liquidation of nontradable establishments (which likely attracts customers) strongly affects other nontradable establishments in the area. In contrast, liquidation of establishments in the services or tradable sectors has no spillover effect on nontradable employment.

The finding in Panel (a) that nontradable liquidations affect nontradable employment – but not liquidation in other sectors – is consistent with the customer traffic channel. Indeed, the effect is found in cases in which customer traffic is likely to be reduced but only among firms that rely on customer traffic for demand (i.e., restaurants and retail). Intuitively, the liquidation of, say, a retail store may deter customers from driving to the area, which in turn may hurt nearby restaurants. In addition, two other findings are supportive of this channel. First, the fact that the effect dissipates quickly at larger geographies is consistent with the customer traffic argument, as customers who are looking to consolidate shopping trips typically do so by concentrating shopping within a single shopping center, which would be within a single census block.[28] Second, in Section 6.5 we show that the employment losses are stronger when the bankrupt establishment remains vacant. As shown in Bernstein et al. (2016), liquidation increases the likelihood of vacancy at a location. While a vacancy will certainly not attract customers, it is possible that it actually deters customers due to low maintenance, neglect, and even crime. Evidence from residential real estate shows that vacant homes and apartments have poor maintenance (Campbell et al., 2011) and cause higher crime in the immediate area (Spelman, 1993; Ellen et al., 2013; Cui and Walsh, 2015). Arguably, the same may apply to commercial real estate, which could further deter customers.

[26] The average block has 926 total employees, and the average tract has 6,862 total employees (corresponding to 898 and 6,834 employees, respectively, net of employment of the liquidated establishment). Accordingly, employment losses in the block of the liquidated establishment amount to 4% × 898 = 36 employees, while the reallocation within the tract amounts to 0.5% × (6834−898) = 29.7 employees. However, this is a rough figure, as the point estimates are not tightly estimated.

[27] We identify service industries using the classification of the census (www.census.gov/econ/services.html). In keeping with Mian and Sufi (2014), we define the nontradable sector as retail trade (NAICS 44–45) and accommodation and food services (NAICS 72). The tradable sector consists of all remaining industries, which are essentially manufacturing (NAICS 31–33). Our results are virtually identical if we strictly restrict the tradable sector to manufacturing.

[28] Note that nearly all of the bankruptcies in our sample are not those of name-brand "anchor" stores that have been examined in previous studies (Pashigian and Gould, 1998; Gould et al., 2005; Benmelech et al., 2014; Shoag and Veuger, 2018). We show in Appendix Table A.5 that our results are unaffected if we remove blocks composed of shopping malls, thereby demonstrating that spillover effects occur even in locations where retail stores are not explicitly connected.

**Table 8**
Industry sectors.

| Panel A: Nontradable industries | | | | |
|---|---|---|---|---|
| Dependent variable: | Employment in nontradable industries | | | |
| Treatment | All (1) | Nontradable (2) | Services (3) | Tradable (4) |
| Liquidation | −0.014* (0.008) | −0.040** (0.020) | −0.007 (0.013) | −0.013 (0.010) |
| Control variables | Yes | Yes | Yes | Yes |
| Division-year fixed effects | Yes | Yes | Yes | Yes |
| Industry fixed effects | Yes | Yes | Yes | Yes |
| Model | IV-2SLS | IV-2SLS | IV-2SLS | IV-2SLS |
| First-stage *F*-test | 81.59 | 19.48 | 39.20 | 49.20 |
| Observations | 91,000 | 47,000 | 32,000 | 12,000 |
| **Panel B: Services** | | | | |
| Dependent variable: | Employment in services industries | | | |
| Treatment | All (1) | Nontradable (2) | Services (3) | Tradable (4) |
| Liquidation | −0.029** (0.012) | −0.045* (0.025) | −0.050** (0.023) | -0.011 (0.017) |
| Control variables | Yes | Yes | Yes | Yes |
| Division-year fixed effects | Yes | Yes | Yes | Yes |
| Industry fixed effects | Yes | Yes | Yes | Yes |
| Model | IV-2SLS | IV-2SLS | IV-2SLS | IV-2SLS |
| First-stage *F*-test | 81.59 | 19.48 | 39.20 | 49.20 |
| Observations | 91,000 | 47,000 | 32,000 | 12,000 |
| **Panel C: Tradable industries** | | | | |
| Dependent variable: | Employment in tradable industries | | | |
| Treatment | All (1) | Nontradable (2) | Services (3) | Tradable (4) |
| Liquidation | 0.003 (0.009) | −0.007 (0.016) | −0.003 (0.012) | 0.002 (0.017) |
| Control variables | Yes | Yes | Yes | Yes |
| Division-year fixed effects | Yes | Yes | Yes | Yes |
| Industry fixed effects | Yes | Yes | Yes | Yes |
| Model | IV-2SLS | IV-2SLS | IV-2SLS | IV-2SLS |
| First-stage *F*-test | 81.59 | 19.48 | 39.20 | 49.20 |
| Observations | 91,000 | 47,000 | 32,000 | 12,000 |

This table presents variants of the regression in column (2) of Table 4, except that the dependent variable is employment in nontradable industries (Panel A), services (Panel B) and tradable industries (Panel C). In each panel, column (1) considers all bankruptcies, whereas columns (2)–(4) only consider bankruptcies in the nontradable sector, services, and the tradable sector, respectively. Standard errors, clustered at the division-by-year level, are reported in parentheses. *, **, and *** denotes statistical significance at the 10%, 5%, and 1% level, respectively.

The second potential mechanism is synergies between businesses. Does liquidation reduce business synergies among neighboring firms? Beginning with Marshall (1920), prior literature has posited that industry agglomeration can benefit other firms by (i) increasing the transfer of knowledge and skills, (ii) reducing transportation costs of goods, and (iii) creating larger labor market pools. More specifically, if ideas and knowledge are more easily transmitted face-to-face, or if informal interaction creates more sharing of knowledge and skills, then geographic proximity can increase the productivity of similar firms. Empirical evidence for this channel includes the spread of knowledge in agriculture (Griliches, 1958), patents (Jaffe et al., 1993), and high-tech firms (Saxenian, 1994). Similarly, geographic proximity reduces transportation costs for goods when customers and suppliers co-locate (Krugman, 1991; Glaeser and Kohlhase, 2004)). Finally, agglomeration can create positive spillovers by reducing search frictions in the labor market, thereby providing a better worker-firm match. In addition, large labor markets can provide implicit insurance against idiosyncratic shocks on both the firm and worker side, as workers who are laid off can more easily find a new job, while firms that lose employees face lower costs in hiring replacements (Krugman, 1991).

To explore these potential explanations, we turn to Panels (b) and (c) that explore the services and tradable sectors. In Panel (b) the dependent variable is the employment growth at the block level in the services sector. In column (1) we find that liquidation leads to a significant decline in employment growth in the services sector. More specifically, as illustrated in columns (2) and (3), the decline in employment in the services sector is triggered by the liquidation of nontradable establishments and, especially, the liquidation of establishments in the services sector. In contrast, column (4) shows that the liquidation of establishments in the tradable sector does not affect employment growth in the services sector. Meanwhile, in Panel (c) we find that the tradable sector is not affected by liquidation in any sector.[29]

These results suggest that the liquidation spillovers we identify are unlikely to arise due to changes in the labor market or transportation costs. First, the benefits of labor market pooling should arguably apply to all sectors, yet we find no effect among tradable firms. Similarly, transportation costs likely matter, especially for producers of tradable goods, while they play a small role in production costs for services firms. Our findings are in the opposite direction of this prediction. Second, our effect decays quickly at larger geographies and is essentially zero at the tract level. Labor markets are much larger than even a census tract, and similarly, agglomeration due to transportation costs likely occurs at a geographic level much larger than a census block. Hence, it seems unlikely that a decline in labor market pooling or increase in transportation costs may explain our results.

Instead, knowledge spillovers may occur on a very local level. For example, Arzaghi and Henderson (2008) use data on the location of advertising agencies to show that the information benefits of agglomeration begin to decay at a distance as small as 500m. This is especially sensible if information transfers rely on informal meetings and face-to-face communication. Thus, if the negative spillovers from liquidation are driven by a loss of business synergies, it is likely due to the loss of knowledge transfers rather than a disruption of the supply chain or labor market of a firm. To the extent that knowledge spillovers are especially important in services, our finding in Panel (b) that services bankruptcies affect local services employment is potentially consistent with this mechanism.[30]

The third potential mechanism is the direct local demand channel triggered by the bankrupt firm's employees (Moretti, 2010), which predicts that the liquidation of a bankrupt firm will reduce local demand for goods and services and hence hurt nontradable establishments. For example, if a manufacturing plant is liquidated, employees losing their jobs at the plant may cut back on their local grocery shopping and restaurant visits. We find no statistically significant evidence of this mechanism. As can be seen in column (4) of Panels (a) and (b), the liquidation of tradable establishments does not significantly reduce either nontradable or services employment. Hence, the evidence is inconsistent with the direct local demand channel.[31] In sum, while it is difficult to precisely distinguish between these different potential mechanisms, our results appear to be most consistent with liquidation harming knowledge transfers and reducing customer traffic to the area.

### 6.5. Fate of the bankrupt establishments

Finally, in Table 9 we examine how the local spillovers of liquidation vary depending on the “fate” of the bankrupt establishment. Broadly speaking, we distinguish between four potential outcomes for the bankrupt establishment: (i) continuer – an establishment that remains operated by the bankrupt firm and maintains the same operations (either in reorganization, or in the years until it winds down in liquidation); (ii) reallocated to the same industry – an establishment that is acquired by another company in the same two-digit (or three-digit) NAICS industry as the original bankrupt firm; (iii) reallocated to a different industry–an establishment that is acquired by another company in a different industry as the original bankrupt firm; and (iv) vacant – we observe no economic activity at the location of the establishment.[32] Intuitively, we would expect the negative externalities to be lowest for continuers, since this outcome does not disrupt the local agglomeration network. However, if reallocated plants are able to employ more workers, or more easily form connections to local firms, they may be less prone to negative externalities.

We examine this heterogeneity by regressing the change in block-level employment on a set of indicator variables that capture the postbankruptcy status of the bankrupt establishment. We caution that this analysis does not necessarily warrant a causal interpretation. Indeed, while the random assignment of bankruptcy judges provides exogenous variation in the probability of Chapter 7 liquidation or Chapter 11 reorganization, we do not have an instrument for the post-bankruptcy status (e.g., reallocation versus vacancy). Because of this, these specifications are estimated with regular OLS.

The results are provided in Table 9. In columns (1)–(6), we include each indicator separately. As shown, the decrease in block-level employment is smallest for continuers, while it is largest for vacant establishments and establishments that are reallocated to a different industry. This pattern also emerges from columns (7) and (8),

[29] In Appendix Table A.9, we show that liquidation does not have a detectable effect on output (total value of shipments), total factor productivity, operating margin, or investment among manufacturing establishments in the same block as the liquidated plant. This is not surprising, given that there is no effect on employment in the tradable sector (which is mostly made up of manufacturing firms).

[30] Given the broad range of services, it is also possible that the customer traffic channel matters for some of these firms. This could explain why the liquidation of nontradable establishments has a marginally significant effect on services firms. However, in Appendix Table A.6 we recategorize service sectors that may rely on foot traffic (specifically, NAICS 71 “Arts, entertainment, and recreation” and NAICS 81 “Other services,” which includes dry cleaning and laundry services, repair and maintenance, etc.) as nontradables and repeat the analysis, finding similar results.

[31] Importantly, some of the industries classified as services include onsite contractors (such as janitorial services or food preparation), which would be affected by the liquidation of their customer via a direct-demand channel. If services firms are more likely to hire these on-site contractors, our findings would be consistent with the direct-demand effect. However, these industries compose only 4.6% of the total employment in our sample, and removing them from the regressions has no effect on our estimates in any of the panels of Table 8.

[32] We track establishments' postbankruptcy status using the methodology of Bernstein et al. (2016). Summary statistics displaying plant outcomes are provided in Appendix Table A.8.

**Table 9**
Fate of the bankrupt establishments.

| Dependent variable: | Employment | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Continuer | 0.072*** | | | | | | 0.071*** | 0.071*** |
| | (0.003) | | | | | | (0.003) | (0.003) |
| Reallocated in same three-digit NAICS | | −0.005** | | | | | 0.012*** | |
| | | (0.002) | | | | | (0.002) | |
| Reallocated in different two-digit NAICS | | | −0.035*** | | | | −0.010*** | |
| | | | (0.002) | | | | (0.002) | |
| Reallocated in same three-digit NAICS | | | | −0.001 | | | | 0.015*** |
| | | | | (0.002) | | | | (0.002) |
| Reallocated in different three-digit NAICS | | | | | −0.036*** | | | −0.009*** |
| | | | | | (0.002) | | | (0.002) |
| Vacant | | | | | | −0.023*** | | |
| | | | | | | (0.002) | | |
| Control variables | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Division-year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Model | OLS | OLS | OLS | OLS | OLS | OLS | OLS | OLS |
| Adjusted *R*-squared | 0.341 | 0.274 | 0.291 | 0.273 | 0.293 | 0.282 | 0.344 | 0.345 |
| Observations | 91,000 | 91,000 | 91,000 | 91,000 | 91,000 | 91,000 | 91,000 | 91,000 |

This table presents estimates from regressions of the annualized change in employment in the block of the bankrupt establishment (excluding employment of the bankrupt establishment) in the five years following the bankruptcy filing on a set of dummy variables that capture the "fate" of the bankrupt establishment. Continuer indicates whether the establishment remains in its current operations. Reallocated in same two-digit NAICS indicates whether the establishment is reallocated to the same two-digit NAICS industry. The other reallocation indicators are defined analogously. Vacant indicates whether the establishment stays vacant throughout the five-year period. In columns (7) and (8), vacant is the base group. The sample includes all establishments belonging to companies that filed for Chapter 11 bankruptcy between 1992 and 2005. Standard errors, clustered at the division-by-year level, are reported in parentheses. *, **, and *** denotes statistical significance at the 10%, 5%, and 1% level, respectively.

where we include all indicators jointly, using vacancy as the base group. While this evidence should be interpreted cautiously, the findings indicate that the disruption of existing operations, either by switching into another industry or full vacancy, is associated with negative externalities. Such disruptions are more common in liquidation, rather than reorganization, as documented in Bernstein et al. (2016).

These relationships are important to keep in mind when interpreting the magnitude of our estimates, as we discuss below in Section 7. It is not necessarily the case that the spillover effects we identify stem solely from a decline in employment at the liquidated establishment. Instead, spillover linkages can be disrupted even if an establishment is reallocated to a different user and employment is maintained. Indeed, by definition, any linkages between firms will be disrupted in liquidation because the existing firm is discontinued. Meanwhile, in reorganization the disruption of these linkages is not abrupt, it results from a reorganization plan, and economic linkages may be taken into account when deciding whether to close an establishment. Because we lack exogenous variation that affects the outcome of bankrupt establishments, our empirical setting does not allow us to quantify how much of the effect arises from vacancy, reallocation to different industries, or reallocation to a different user in the same industry. Our treatment effect measures the combined impact of these forces.

## 7. Discussion

We find that liquidation leads to a significant decline in the employment of geographically proximate firms, and the spillover effects decay with distance. Moreover, the results are consistent with agglomeration effects that relate to customer search and information spillovers, as discussed in Section 6.4. To further understand how to interpret the magnitudes of the findings, in this section we calculate the size of the multiplier of liquidation – the number of jobs lost in a census block per employee at the bankrupt establishment – and discuss how this relates to the overall magnitudes estimated in Table 4. In addition, we set up a simple theoretical model in the spirit of Kline and Moretti (2013), Greenstone et al. (2010), and Gathmann et al. (2016). The model allows us to compare our estimates to previous work on agglomeration spillovers, by estimating an agglomeration elasticity. This elasticity measures how the exogenous liquidation of an establishment may affect the productivity of geographically proximate firms and can be compared to estimates from the agglomeration literature. Finally, we discuss several important considerations that may affect the interpretation of our magnitudes, including the potential reallocation of labor to other census blocks, the presence of fragile firms in close proximity to bankrupt firms, and how rigidity of wage adjustments might impact the size of the spillover effects.

### 7.1. Calculation of the multiplier

While the cumulative effect of liquidation, after five years, is estimated to be a 20% decline in employment in the affected block, it is important to put this magnitude in the context of the size of the bankrupt establishment. One straightforward way to do this is by calculating the size of the multiplier, defined as the change in number of jobs in the block per number of jobs at the bankrupt establishment:

$$Multiplier = \beta \cdot \frac{emp_{0,block}}{emp_{0,estab}}. \quad (6)$$

In this equation, $\beta$ is the cumulative percent change in employment estimated from our main regression specifications. For example, column (2) of Table 4 indicates that liquidation leads to 4% lower employment growth in the affected block and thus over a five-year period $\beta = -0.04 \times 5 = -0.2$.[33] Meanwhile, $emp_{0,\ block}$ is the number of employees in the block at the time of the bankruptcy (excluding the bankrupt establishment), and $emp_{0,\ estab}$ is the number of employees at the bankrupt establishment.

Based on our full sample, it is tempting to apply the estimate of $\beta = -0.2$ to an average block with $emp_{0,block} = 1105$ and an average bankrupt establishment with $emp_{0,estab} = 37$ (see the summary statistics in Table 1), resulting in a multiplier of $0.2 \times (1105/37) = 5.97$.[34] This would indicate that for each liquidated job, an additional six jobs are lost in the same census block. However, this provides a misleading view of the magnitude of our estimates because it assumes that 20% of employment is lost in all blocks regardless of the block-to-establishment employment ratio. However, results in Table 5 show that the effect of a liquidated plant on a block is significantly smaller when the bankrupt plant is small relative to the size of the block. This insight dramatically reduces the multiplier estimate.

In particular, column (2) of Table 5 shows that when $\frac{emp_{0,block}}{emp_{0,estab}} > 100$, which corresponds to the top quartile of the relative size distribution, the point estimate of $\beta$ is indistinguishable from zero. Meanwhile, when $\frac{emp_{0,block}}{emp_{0,estab}} < 100$, shown in column (3) we estimate $\beta = -0.052$. While the estimated impact of liquidation is larger in this subsample, the block-to-establishment employment ratio is significantly lower at (402/53)=7.58, meaning that the multiplier over a five-year period is estimated to be $0.26 \times 7.58 = 1.97$. Columns (4) and (5) show similar results when we use a lower threshold of $\frac{emp_{0,block}}{emp_{0,estab}} < 70$, which corresponds to the top tercile of the relative size distribution. Using this cut-off, we estimate a multiplier of 1.85 over a five-year period, while there is no significant effect when $\frac{emp_{0,block}}{emp_{0,estab}} > 70$.[35]

Taken together, these results indicate that our main estimate of a 4% decline in annualized employment growth is not appropriate in cases when the bankrupt establishment is inconsequential relative to the census block.[36]

For this reason, we estimate that the multiplier of liquidation is about 1.85–1.97 jobs lost over a five-year period for each liquidated job. Further, it is important to note that in Section 6.2 we estimate that about three-fourths of the overall effect is due to a loss of existing jobs, and one-fourth is due to a reduction of entry. Thus, the multiplier of liquidation on jobs that were existing at the time of bankruptcy is roughly 1.39–1.49. Meanwhile, for each liquidated job, we estimate that entry into the block is reduced by about 0.47 jobs.[37] In that case, given that blocks at the bottom three quartiles correspond to 29.7% of the overall employment in the sample, this implies an overall unconditional multiplier of 29.7%*1.97=0.58 across all sample blocks. A similar calculation using the tercile threshold yields an aggregate unconditional multiplier of 0.47.

We also aggregate the multiplier to calculate an unconditional effect, under the assumption that when the effects are not statistically significant (in the top quartile or tercile of the relatize size distribution), the multiplier is effectively zero.

### 7.2. Calculation of the agglomeration elasticity

In this section, we introduce a simple theoretical framework that allows us to estimate an agglomeration elasticity based on our estimates. This will then allow us to compare the magnitude of the effects that we find to previous work in the agglomeration literature that has estimated such elasticities in different settings. Consider a local economy in which local labor markets are small relative to national markets, and hence, local shocks affect only local labor market, even if individuals move out of the region. Consider a firm $j$ that is located in area r with a Cobb–Douglas production function. Following Glaeser and Gottlieb (2009), Kline and Moretti (2013), and Gathmann et al. (2016), we assume that there are two types of capital, capital that is fixed to the region denoted by F (such as resource endowment, proximity to city center, or other specific location characteristic) and flexible capital denoted by $K$. Hence, the production function is

$$Y_j = \rho_j A_r L_j^{\alpha} F_j^{(1-\alpha)(1-\mu)} K_j^{(1-\alpha)\mu}, \quad (7)$$

where $\alpha$ is the share of labor in production, and $\mu$ is the share of fully flexible capital. Moreover, we denote $\rho_j$ as a firm-specific productivity shifter and $A_r$ is the local area productivity shifter that captures the agglomeration forces. Following Greenstone et al. (2010) and Moretti (2010), and

[33] Fig. 2 shows that this 20% cumulative effect is present by year 4 and levels off by year 5, so one should not extrapolate a 4% decline per year past the five-year horizon.

[34] Note that we use the average size across all bankrupt establishments rather than the average size of a liquidated establishment. This is because the natural experiment ensures that we are comparing equally sized liquidated and reorganized establishments, due to random judge assignment.

[35] In Table A.10 in the Appendix we divide the sample into quintiles of relative size (i.e., the size of the block relative to the size of the bankrupt establishment) and separately repeat the analysis in each quintile bin. We find that liquidation leads to statistically significant annual employment declines of 4.96% and 7.66% in the first two quintiles of the relative size distribution. In the first quintile, the average block has 274 employees, while the average bankrupt establishment has 47 employees, leading to a multiplier estimate of $(274 \times 0.248)/47 = 1.45$. A similar calculation in the second quintile yields a multiplier estimate of $(471 \times 0.383)/76 = 2.38$. Thus, the employment-weighted average multiplier conditional on being in one of the two statistically significant quintiles is $(274/745) \times 1.45 + (471/745) \times 2.38 = 2.04$, an estimate similar to the multipliers of 1.97 and 1.85 estimated when excluding the top tercile and quartile of the relative size distribution.

[36] We note further that our estimates are not driven by cases when the bankrupt establishment comprises almost all of the employment in the block. For example, removing all cases in which $\frac{emp_{0,block}}{emp_{0,estab}} < 1$ has no appreciable effect on our estimates.

[37] This assumption is conservative, as there are likely some spillover effects in larger blocks that we are unable to measure due to the size of the block.

as is common in the literature, we define the local shifter as a concave function of overall employment in the region, $A_r = L_r^\lambda$, where $L_r$ is the aggregate local employment, and $\lambda$ is the agglomeration elasticity parameter, measuring the percentage decline in productivity due to a change in local employment. In our setting, we will measure agglomeration elasticity relative to an exogenous liquidation shock. The parameter $L_r^\lambda$ captures the effect of agglomeration on productivity, under the simplifying assumption that the benefits of agglomeration can be measured by the size of local employment. In reality, agglomeration benefits from the local economy are more nuanced, varying with the particular mix of firms and industries and other such characteristics. In addition, for simplicity, we assume that local firms are price takers, and we normalize prices such that $p = 1$.[38]

The local supply of labor in area r is given by $L_r = f(w_r, w_r')$, where $w_r$ are the wages paid in the local area $r$ and $w_r'$ are wages paid outside the area. Hence, changes in local wages may further affect the adjustment of labor and subsequently the productivity of firms in the area. This adjustment will depend on $\eta$, the inverse of the local labor supply elasticity with respect to local wages, $\eta = \frac{1}{\frac{\partial f()}{\partial w_r}\frac{w_r}{f()}}$. We further assume, for the sake of simplicity, that the cost of capital $K_j$ is fixed and equal to $i$. The profit maximization objective of firm $j$ is therefore given by

$$\pi_j = \rho_j A_r L_j^\alpha F_j^{(1-\alpha)(1-\mu)} K_j^{(1-\alpha)\mu} - iK_j - w_r L_j. \tag{8}$$

Firms maximize profits by adjusting their labor $L_j$ and flexible capital $K_j$ (nonflexible capital $F_j$ cannot adjust), taking into account local productivity $A_r$ and local wages $w_r$. In a competitive equilibrium, production factors are paid at their marginal product. Therefore, as we illustrate in the appendix, first-order conditions for capital and labor generate the firm's labor demand curve $L_j$. Using this, we can aggregate local demand across all firms in region $r$ to generate aggregate demand for labor in the area[39]:

$$\begin{aligned}\log L_r &= \log \sum L_j \\ &= \log \sum \rho_j^{\frac{1}{(1-\alpha)(1-\mu)}} + \frac{\log A_r}{(1-\alpha)(1-\mu)} \\ &\quad - \frac{1-(1-\alpha)\mu}{(1-\alpha)(1-\mu)} \log w_r + \kappa.\end{aligned} \tag{9}$$

Note that the aggregate labor demand curve is downward sloping with respect to $w_r$.[40]

*7.2.1. The effect of liquidation on equilibrium wages and employment*

If agglomeration forces are important, then the liquidation of an establishment may trigger additional job losses in the local economy. Broadly speaking, as we discuss in Section 6.5, the liquidation of an establishment causes an abrupt disruption of economic linkages, in contrast to the case of reorganization, in which existing economic linkages may be taken into account when deciding whether or not to close an establishment. The abrupt disruption of economic linkages may impose adverse effects on nearby firms because it causes vacancy or because it leads to the reallocation of the assets to different users, within or outside the industry. Regardless of the channel, the size of the liquidated plant proxies for the intensity of the disruption of economic linkages in the area. Therefore, in the model, we assume that the size of the shock to agglomeration varies with the size of the liquidated plant, and the agglomeration elasticity $\lambda$ measures the effect of such disruption on local firms.

From Eq. (9), we can now consider the employment adjustment of local, nonliquidated firms, in the economy, following the liquidation event. We assume that there are $J$ firms in the region:

$$\begin{aligned} d\log L_{j\neq liq} &= \frac{J-1}{J} d\log L_j \\ &= \frac{\lambda}{(1-\alpha)(1-\mu)} d\log(L_r) \\ &\quad - \frac{1-(1-\alpha)\mu}{(1-\alpha)(1-\mu)} d\log w_r. \end{aligned} \tag{10}$$

The two terms of this equation highlight that liquidation can affect local employment either by affecting the size of the agglomeration benefit (first term) or equilibrium wages (second term). Note that if agglomeration effects do not exist, that is $\lambda = 0$, employment in other firms may in fact increase as long as wages are not rigid and can adjust. An observed decline in employment in other firms in the region, in contrast, indicates agglomeration effects that are large enough to dominate any positive effects through wage adjustments.

The extent to which labor demand of local firms will adjust with changes in wages depends on the intersection of the local labor supply and demand curves and labor elasticity, since $d\log w_r = \eta d\log L_r$. In one extreme case, if wages are fully flexible, equilibrium wages will decline and local employment will remain unchanged, since all displaced workers will be hired by other firms (hence, local firms will increase labor demand). In the other extreme case, if wages are completely rigid (i.e., $\eta = 0$), local wages are unchanged, and the full adjustment occurs through a decline in local employment.

Of course, wages are unlikely to be fully flexible due to labor market institutions and contractual arrangements. Indeed, if wages are rigid, then the full adjustment to liquidation event is borne by the quantity of local employment. We explore the effect of liquidation events on local wages in column (1) in Table A.9 of the Appendix. We find that liquidation does not affect local wages, as the relationship is not statistically significant and economically small. This is intuitive, given that the effects of liquidation seem to be highly localized at the census block level only, and displaced workers will look outside of the block for new employment opportunities. Hence, the effect of liquidation on local labor demand seems to take place mostly through the agglomeration channel, rather than the wage

[38] We believe this assumption is realistic given our focus on small geographical units, such as census blocks.

[39] As we define in the Appendix, $\kappa = -\frac{\mu}{(1-\mu)}\log i + \log F_j + \frac{1-(1-\alpha)\mu}{(1-\alpha)(1-\mu)}\log\alpha + \frac{\mu}{(1-\mu)}\log[(1-\alpha)\mu]$.

[40] To ensure that this is always the case we need to assume that $\lambda < (1-\alpha)(1-\mu) + \eta(1-(1-\alpha)\mu)$. To see this, substitute $\lambda \log L_r$ for $\log A_r$ and solve Eq. 9 for $\log L_r$.

adjustment channel, and thus this second term plays a small role in explaining our results.[41]

*7.2.2. Estimating agglomeration elasticity*

Using the structure of the model, we can back out the agglomeration elasticity, $\lambda$. Starting with Eq. (10), we first assume that wages are rigid and do not adjust following the liquidation event; hence, we assume that $d \log w_r = 0$. As discussed earlier, this assumption is consistent with the small geographical size of a census block as well as our empirical findings. In addition, for simplicity, we assume that $\frac{J-1}{J} = 1$. Hence, by simply dividing Eq. (10) with $d\log L_r$, we find that the agglomeration elasticity is

$$\lambda = \frac{d \log L_{j \neq liq}}{d \log L_r}(1-\alpha)(1-\mu). \quad (11)$$

That is, the agglomeration elasticity equals the ratio of the change in employment of nonbankrupt plants relative to the change in total employment in the block, multiplied by the share of fixed capital. Based on our estimates in Table 5, employment losses of nonbankrupt plants in the census block after five years accumulate to 26%, as reported in column (3).[42] This is equivalent to 104 jobs in an average block. When incorporating the average liquidated plant, total job losses, $d\log L_r$, amount to 157 in an average block. Following Kline and Moretti (2013), we set the share of fixed capital $(1-\alpha)(1-\mu)$ to 0.47.[43] Hence, the initial estimate of the agglomeration elasticity, based on our findings, is 0.311.

This estimate is larger than the one estimated by Kline and Moretti (2013) or Gathmann et al. (2016), who report an agglomeration elasticity of 0.2. There are several reasons for which our estimate of agglomeration elasticity should be larger. First, note that the parameters used to generate the agglomeration elasticity rely on labor share, $\alpha$, and share of fixed capital $\mu$. For the sake of comparability, we used similar parameters as in Kline and Moretti (2013) who focus on the manufacturing sector. However, our key results arise in the services and nontradable sectors, in which the share of labor is likely to be significantly larger, while the share of capital should be lower. For example, multi-factor productivity tables from the BLS indicate that the share of labor, $\alpha$, for manufacturing is 0.3, while in retail (NAICS 44–45) and services (NAICS 54–81) it is 0.49 and 0.57, respectively.[44] If we adjust $\alpha$ according to the BLS estimates in retail and services (while keeping $\mu$ unchanged), the agglomeration elasticity is, in fact, 0.21 for retail and 0.17 for services, significantly closer to Kline and Moretti (2013).

Moreover, the empirical evidence in the agglomeration literature suggests that agglomeration elasticities are larger in the services sector. Melo et al. (2009) conduct a meta-analysis of agglomeration elasticities incorporating 729 elasticities taken from 34 different studies. They find that agglomeration elasticities are significantly higher in the services sector, when compared to the manufacturing sector. This is consistent with Glaeser and Kohlhase (2004) who find that services tend to be located in dense areas because they are more dependent on proximity to customers than manufacturing. In that regard, the underlying agglomeration forces that drive our estimates may be quite different from Kline and Moretti (2013) and most of the agglomeration literature, which typically focuses on manufacturing. For example, Graham (2007, 112) compares agglomeration elasticities across multiple sectors and finds that manufacturing has the lowest agglomeration elasticity, equal to 0.041, while business services and banking finance services have the largest elasticities, with 0.176 and 0.251, respectively.[45]

Ultimately, the agglomeration literature acknowledges that there is a significant variation in agglomeration elasticities, as illustrated in the meta-analysis study of Melo et al. (2009). Moreover, Kline and Moretti (2013) conclude that "[...] part of the variation in these estimates is due to the fact that models, data, time periods and industries used in the studies are vastly different." In that regard, a unique characteristic of our study, when compared to the agglomeration literature, is that we focus on firm bankruptcies, and so, rather than focusing on job creation, we focus on the spillover effects of job destruction. Moreover, bankruptcies are more likely to arise in areas in which other firms are somewhat more fragile and thus may be more sensitive to disruptions in local agglomeration linkages. We explore this notion in the next section.

*7.3. Interpretation and external validity*

We conclude our discussion of the magnitude of bankruptcy spillovers by highlighting several considerations that affect the interpretation of our coefficient estimates.

First, we note that any estimation of spillover effects is highly dependent on the geographic region considered. As shown in Table 7, we find that the spillover effects of bankruptcy dissipate quickly as we move to larger geographies and are highly localized, similar to Benmelech et al. (2014) and Arzaghi and Henderson (2008). Because studies on agglomeration linkages use different geographical

[41] This is consistent with the macro literature showing that wages rarely decrease during downturns (see, e.g., Bewley, 1999; Campbell and Kamali, 1997).

[42] As described in Section 7.1, when the block is more than 100 times the size of the establishment, we do not find significant agglomeration effects.

[43] Kline and Moretti (2013) assume that the labor demand elasticity, in our model given by $\frac{1-(1-\alpha)\mu}{(1-\alpha)(1-\mu)}$, is equal to 1.5, and that the share of flexible capital in production, in our model given by $(1-\alpha)\mu$, is equal to 0.3. It follows that the share of fixed capital is $(1-\alpha)(1-\mu) = 0.47$.

[44] The statistics are taken from the BLS website: https://www.bls.gov/mfp/mprdload.htm.

[45] One could also calculate the unconditional agglomeration elasticity that takes into account not only blocks that are small relative to the size of the bankrupt establishment but instead incorporates all blocks. To do so, note that for blocks in the top quartile of the relative size distribution, we detect no spillover effects. Given that the top quartile contains 70.3% of total employment in our sample (due to few very large blocks), the estimate of the unconditional agglomeration elasticity is 29.7%*0.311+(1–29.7%)*0=0.09. This estimate is very close to the mean of 0.058 across the 729 elasticities examined in Melo et al. (2009). But to some degree, the elasticity may be downward biased because of a few very large blocks in which we are unable to detect spillover effects due to the large relative size of the block.

regions, ranging from counties down to census blocks, it is difficult to compare magnitudes across studies. For example, the magnitudes found in other studies that examine larger regions (e.g., Greenstone et al., 2010; Moretti, 2010) could be significantly larger if the effects were measured in smaller regions, given that agglomeration linkages seem to dissipate with distance.

A related point is that the data used in this study do not allow us to consider the reallocation of workers outside of the census block. Blocks are small geographic areas, and employees can easily reallocate outside of the affected block. Thus, we find significant spillovers in a very local area, which suggests that liquidation imposes significant effects on neighboring firms. However, to estimate its full effect on employment loss, one needs to take into account the potential reallocation of jobs. If we take into account the effect of liquidation at the tract level as measured in Panel A of Table 7, the magnitude of the estimated liquidation spillover is reduced by 80%. Because we cannot track individual workers in our data, we cannot fully account for reallocation of workers across geographic regions, and this limits our ability to estimate the aggregate magnitude of the spillover effect on job loss.

It is also important to consider the external validity of the experiment. While our sample contains 91,000 census blocks that are widely dispersed across the US, it is important to keep in mind that this sample, by definition, contains only blocks with bankrupt firms. This selection does not invalidate the experiment, as this is precisely the set of regions one should consider when measuring the spillovers of bankruptcy. However, firms that are located near bankrupt firms are likely more fragile than firms in other regions, and thus the estimated spillover effects may be larger. For example, if bankruptcies occur in areas with many small and young firms, or less financially sound firms in general, the removal of the bankrupt firm could cause large employment losses at the fragile firms that are in this area.

Following this logic, in Appendix Fig. A.1 we use the universe of establishments in the LBD and show that smaller firms are more fragile (especially those with less than ten employees), in the sense that they have a much lower likelihood of surviving for five years than larger firms.[46] Based on this evidence, we divide the blocks in our sample by the median percentage of firms in the block with less than ten employees and test whether our results are strongest in areas with a high share of small firms. The results are shown in Appendix Table A.4. Consistent with the magnitudes being larger in areas with more fragile firms, we find that our results are mostly driven by blocks with many small firms. In column (2), we show similar results if we divide the sample into blocks with a high share of both small and young firms. These findings support the idea that the presence of fragile firms will increase the magnitude of the spillover.

It is also important to note the dynamics of the spillover effect. Both in the raw data (Fig. 1) and (using instrumental variables (Figs. 2 and 3, we find that liquidation spillovers materialize gradually over time. This suggests that the total cumulative estimate of a 20% decline in employment may be the result of a snowball effect in which initial employment losses spur further reductions in employment. For example, consider employment losses in year 4 after the bankruptcy. In this year, Fig. 2 shows that there is roughly a 7% decline in employment (moving from a cumulative 12% decline in year 3 to a 19% loss in year 4). This marginal reduction of 7% is not responding only to the liquidated establishment but to the total loss of 12% of total employment over the previous three years. Put differently, the initial liquidation will cause a reduction in employment among the most sensitive firms first (for example, the most fragile firms), but then these firms will have further effects on other related firms, which can therefore result in a fairly large multiplier effect. The gradual evolution of employment losses in Figs. 2 and 3 is consistent with this interpretation.

## 8. Conclusion

The results presented in this paper show that the liquidation of bankrupt firms imposes large negative externalities on the local economy, when compared to reorganization, an alternative approach to resolve distress in courts. Using the random assignment of bankruptcy judges as a source of exogenous variation in the probability of Chapter 7 liquidation (versus Chapter 11 reorganization), we find that, within a five-year period, employment decreases substantially in the census block of the liquidated establishment. Most of the decline is due to lower growth of existing establishments and, to a lesser extent, reduced entry into the area. This evidence is inconsistent with a “creative destruction” argument, according to which liquidation would contribute to the revitalization of the area and induce entry.

We further show that the spillover effects are highly localized and concentrate in the nontradable and service sectors, particularly when the bankrupt firm operates in the same sector. These results are consistent with liquidation leading to a reduction in consumer traffic to the local area and reducing knowledge spillovers between firms.

These findings leave a number of important areas open for future research, of which we highlight two here. First, our study examines local spillovers from liquidation. Spillovers can be nonlocal as well (e.g., the liquidation of an important customer could hurt nonlocal suppliers). A challenge for future research is to establish the relevance and magnitude of such nonlocal spillovers. Second, we caution that, while our estimates are an important step toward understanding the welfare implications of liquidation and reorganization, there are still many aspects of the welfare question that are not included in our analysis. For example, liquidation could have ex ante benefits by disciplining managers, and it clearly has effects on the firm’s creditors that are not considered here. Extending our analysis to consider these aspects is a difficult, yet exciting, avenue for future research.

[46] This is similar to Fort et al. (2013), who show that small and young firms are more sensitive to business cycle fluctuations.

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