Bankruptcy Spillovers

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Abstract

How do different bankruptcy approaches affect the local economy? Using U.S. Census microdata at the establishment level, 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 within a five-year period, employment declines substantially in the immediate neighborhood of the liquidated establishments, relative to reorganized establishments. Most of the decline is due to lower growth of existing plants, and lower entry into the nearby area. The spillover effects are highly localized, concentrate in the non-tradable sector, particularly when the bankrupt firm is in the non-tradable sector as well. The results are consistent with the story that liquidation leads to a reduction in consumer traffic to the local area, and that bankruptcy institutions impose important externalities on neighboring firms.

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I. Introduction

Bankruptcy institutions play a significant role in resolving insolvency and financial distress in the economy. Since 1980, the number of business bankruptcy filings in the US surpassed 1.8 million cases. Most of these cases are resolved through either reorganization (Chapter 11 under the U.S. Bankruptcy code), that attempts to rehabilitate the distressed firm, or liquidation (Chapter 7 under the U.S. Bankruptcy code), in which the firm ceases to exist and all assets are auctioned.[1] Given the importance of bankruptcy institutions, it is not surprising that a large literature discusses the design of the bankruptcy system and its implications for distressed firms and their claimholders.[2] Yet, the agglomeration literature highlights the importance of spillover effects that may arise between geographically proximate firms.[3] In that case, bankruptcy institutions may have far-reaching implications on other economically related firms that are not represented in courts. In this paper, we explore how different approaches to bankruptcy, namely liquidation and reorganization, affect the spillovers imposed by distressed firms on the local economy.

How do firms impose spillovers on other proximate firms? First, firms may affect other stores consumer traffic, as customers are likely to visit not only their store, but also to other businesses nearby. Second, the agglomeration literature has identified multiple channels through which synergies may arise between proximately located firms. For example, firms may provide cheaper and faster supply of intermediate goods and services to nearby firms. Firms may generate knowlegde spillovers, as proximate firms and employees may benefit from own knowledge and skills. And, by enhancing the depth of local labor market, firms may reduce search frictions in the labor market for nearby firms. Finally, employees may simply increase local demand through dependence on local goods and services, such as restaurants and retail, which will further increase the dependence among geographically proximate firms.

Such spillovers may be affected by the bankruptcy approaches used to resolve financial distress in courts. In liquidation, the firm ceases to exist, and all assets are sold through a cash auction.

[1] More precisely, 62% of all business bankruptcy filings occur under Chapter 7, and 24% take place under Chapter 11. Remaining cases include Chapter 13 cases with both business and personal debt, and family farm bankruptcies filed under Chapter 12. Bankruptcy court statistics are taken from official U.S. Courts filing statistics.

[2] Examples include Baird (1986, 1993); Aghion et al. (1992); Hart (2000).

[3] See Duranton and Puga (2004); Glaeser and Gottlieb (2009); Moretti (2010)\ for recent surveys.

This allows the revitalization of the neighborhood through the reallocation of the distressed firm assets such as its building, capital and labor, to better uses. Arguably, the replacement of the distressed firm could improve spillovers and synergies with local firms. However, liquidation may disrupt existing linkages with local firms, and various frictions such as search costs, or financial constrains of potential users may prevent efficient reallocation of the assets (Williamson (1988); Shleifer and Vishny (1992); Gavazza (2011)). In such cases, locations may remain vacant, or sub-optimally used, imposing negative spillovers on neighboring firms. In reorganization, however, the firm is allowed to restructure and continue operations, and thus potentially preserve existing spillover linkages. However, critics argue that reorganization may lead to inefficient continuation of the firm due to conflict of interest among claimholders, and agency problems (Bebchuk (1988); Gertner and Scharfstein (1991)). This may prevent the reallocation of the assets to better uses, limiting potential spillovers and the revitalization of the area.

Estimating the spillovers effects imposed by bankruptcy regimes on neighboring firms is empirically challenging. First, many companies have multiple establishments, which makes it difficult to determine the relevant local area, and identify all plants that are potentially affected by the bankrupt firm. Second, identifying spatial spillover effects suffers from the fact that location choices are endogenous, and locations are subject to various observed and unobserved shocks. How can one identify whether changes in economic activity of neighboring firms are due to spillover effects or unobserved economic shocks? In our context, if firms that reside in declining areas are more likely to be liquidated (rather than reorganized), then finding an association between liquidation and a subsequent decline in economic activity of neighboring firms could be spurious, merely reflecting negative trends rather than spillover effects.

We deal with these obstacles in the following manner. First, we use detailed micro data at the establishment level from the U.S. Census Bureau. Specifically, we combine the Longitudinal Business Database (LBD) with bankruptcy filings from LexisNexis Law to obtain a comprehensive dataset of 91,000 establishments belonging to bankrupt firms across all industries. This comes with a sharp contrast to almost the entire literature on agglomeration, which examines the manufacturing industry only. However, manufacturing is found disproportionately in smaller and medium size cities,

as well as the rural fringes of cities (Kolko (2000)). Exploring the entire population of bankrupt firms allows us to explore relations across industries, and better understand larger and denser cities. Moreover, using the geo-codes from the LBD, we can explore different scopes of spillovers, varying the geographical units from Census tracts, to very localized spillovers at the level of the Census block.

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 U.S. 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 the local economic conditions in the vicinity of the filer's establishments. Importantly, judges differ in their propensity to liquidate companies (Chapter 7) as opposed to keeping them alive through reorganization (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 force companies into liquidation. 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 liquidation of an establishment imposes negative spillovers on the establishment's local area. Specifically, within a five-year period following the bankruptcy filing, we find that liquidation leads to an average decrease in employment of 5.8% per year in the establishment's immediate neighborhood (Census block), relative to a reorganized plant. The effect takes place gradually and persists over the five-year period after the bankruptcy filing. We further decompose the effect into changes at the intensive and extensive margins. We find that most of the decline in employment is due to a reduction in entry, in terms of the number of new establishments ("births"), as well as slower growth of existing plants ("continuers"). Meanwhile, the employment reduction associated with an increase in plant closures ("deaths") is present but

[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)).

relatively small.

Interestingly, we find that the spillover effects are particularly localized at the Census block level and the effects decay with larger geographical areas (Census block groups and Census tracts). This is consistent with prior evidence on the nature of spillover effects (Rosenthal and Strange (2003); Arzaghi and Henderson (2008)). Despite its localized effect, the aggregate consequences of these spillovers on the economy are significant due to the widespread nature of bankruptcy. Based on our sample, in a given year, there are approximately 300,000 establishments that reside in the same Census block as a firm that files for bankruptcy through Chapter 11.

Why does liquidation generate negative spillover effects relative to reorganization? We find that the decrease in local employment is concentrated entirely in the non-tradable sector (i.e., restaurants, services, retail, etc.), in contrast to tradable industries. This may suggest that the results are due to decline in demand by the employees of the liquidated firm. However, upon a closer look, the evidence is inconsistent with this explanation. We find no evidence that liquidated tradable firms affect local non-tradable firms, and similarly find no evidence that liquidated tradable firms affect tradable firms.[5] We find that the spillover effects arise almost entirely from the impact of non-tradable liquidated plants on non-tradable neighboring plants. These results are consistent with the channel that liquidation leads to a decline in consumer traffic to the local area.

Finally, in auxiliary analysis we explore how the effects correlate with the reallocation of the bankrupt plant location. Not surprisingly, we find that the negative spillovers on local employment are smaller if the establishment remains in its current operations, while they are larger if the bankrupt establishment stays vacant or is redeployed to a different industry.

Our paper contributes to several strands of the literature. First, several articles examine the costs and benefits of reorganization procedures such as Chapter 11,[6] while others consider frictions that may exist in distressed liquidations.[7] However, this literature has typically ignored any spillover effects of bankruptcy on non-bankrupt firms. This paper shows that these externalities are large

[5] Similarly, we find no evidence that the liquidation of a manufacturing plant affects local non-manufacturing firms. One potential explanation is the localized nature of our analysis, and the fact that non-tradable firms are less likely to reside right next to a manufacturing plant.

[6] Prominent examples include Baird (1986); Aghion et al. (1992); Bradley and Rosenzweig (1992); Hotchkiss (1995); Gilson (1997); Bris et al. (2006).

[7] See, e.g., Shleifer and Vishny (1992); Pulvino (1998, 1999); Strömberg (2000); Thorburn (2000); Campbell et al. (2011).

enough to be a first-order consideration in assessing the costs and benefits of the two bankruptcy regimes.

Two recent studies examine the spillover effects of the closure of large retail chains. Benmelech et al. (2014) document that following a retail chain's shutdown, stores located in the same shopping mall are more likely to close as well. Shoag and Veuger (2014) find that after a big-box store closes, consumers rapidly reduce their visits to nearby stores. While related, our paper differs in many regards. Our focus is on the externalities of the two main bankruptcy procedures found worldwide: reorganization and liquidation. 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 externalities of liquidation, but these spillovers are estimated relative to the policy-relevant option of reorganization, which may also include establishment shutdowns that are not forced. Moreover, we use establishment-level data from the U.S. Census Bureau, which allows us to study all sectors and hence provide a rich characterization of the externalities of liquidation and reorganization, and the mechanisms through which these externalities occur.

Second, this paper contributes to the large literature that studies the benefits of agglomeration. Ellison and Glaeser (1997) show that there is significant geographic clustering of industries, and many theories exist as to why firms co-locate. We discuss these theories in detail in Section II. To date, the empirical literature on agglomeration spillovers has focused on the expansion of agglomeration economies through entry decisions (e.g., Rosenthal and Strange (2003)). In particular, in a seminal article, Greenstone et al. (2010) estimate the productivity gains among incumbent plants in "winning" counties that attracted a large manufacturing plant compared to "losing" counties that were the plant's runner-up choice. In contrast to the existing literature, our study examines the *disruption* of agglomeration economies. By focusing on disruptions—specifically the breaking of agglomeration linkages through liquidation—we show how agglomerations can propagate negative shocks that impose negative externalities on other firms within the agglomeration.[8] Further, our detailed micro-level data allow us to examine more closely the various channels of agglomeration spillovers across

[8]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, liquidation may even benefit the local area if the liquidated establishment's capital and labor are redeployed efficiently within the agglomeration.

a wide range of industries, locales, and sizes of firms.

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. (2012); Carvalho (2014); Acemoglu et al. (2016)). In this vein, our paper shows how the (quasi-)random liquidation of an establishment propagates through the establishment's agglomeration and ultimately affects local employment.

The remainder of the paper is organized as follows. Section II discusses the channels through which bankruptcy can create spillovers. Section III discusses the institutional background. Section IV presents the data. Section V describes the methodology. Section VI provides the results. Section VIII concludes.

II. Spillover Mechanisms

Our goal is to understand how the liquidation or reorganization of a bankrupt establishment creates spillovers that affect nearby firms. In this section we briefly review the possible mechanisms through which bankrupt firms might affect the local economy, relying on the large literature that studies the benefits of spatial agglomeration.[9] We focus on three broad channels: customer search costs, production costs, and direct local demand.

First, stores impose spillover effects through their impact on consumers' search costs. If consumers have imperfect information about the goods market, their need to search the market creates an incentive for firms to co-locate to ease search costs. For example, customers that are drawn to an individual store are also likely to shop at other stores in the same shopping center. As a result, the demand for a particular store's goods can depend on the presence and quality of other local businesses (Pashigian and Gould (1998), Gould et al. (2005), Benmelech et al. (2014)). In our context, the extent to which liquidation or reorganization affect customer traffic will determine the magnitude and direction of the externality. If liquidation leads to more vacancy, for example, this likely reduces local demand and thus creates a negative externality on other local firms.[10] On the

[9] For reviews of this literature, see Moretti (2011), Glaeser and Gottlieb (2009), and Duranton and Puga (2004).

[10] In particular, 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)). Similar factors in commercial real estate

other hand, liquidation could create a positive externality if the bankrupt locations are reallocated to new firms that generate higher customer traffic.

Second, it is possible that economic activity is geographically concetrated because agglomerated economies have lower production costs. Beginning with Marshall (1890), prior literature has posited that industry agglomeration reduces the costs of three key factors in the production function: knowledge, goods, and workers. 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 implicitly 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)). Focusing on manufacturing firms, Ellison et al. (2010) find empirical support for all three of these explanations for lower production costs. Greenstone et al. (2010) also find support for these theories among manufacturing firms. However, these benfits of agglomeration need not be limited to manufacturing, as to some extent all firms use knowledge, goods, and labor in their production functions.

Finally, the treatment of bankrupt firms could create spillovers by directly affecting local demand. For example, Moretti (2010) shows that a new job created in manufacturing creates 1.6 new jobs in the nontradable sector in the same city. In our case, if liquidation reduces overall employment this will reduce demand for local goods and services, such as restaurants and retail.

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could reduce local demand dramatically.

III. 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 U.S. Bankruptcy code contains both procedures, with liquidation falling under Chapter 7 and 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 § 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 V.

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 U.S. Court filing statistics, liquidations are frequent, as about 65% of all business bankruptcy filings in the U.S. 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 which outlines why liquidation will provide the highest recovery for the creditors. 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 V.B, we exploit this heterogeneity among judges to instrument

for the probability that a given company is liquidated.

Firms that remain in Chapter 11 pass 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 plants that remain in Chapter 11 serve as the counterfactual in our analysis. Thus, as opposed to Benmelech et al. (2014) and Shoag and Veuger (2014), we compare spillovers of plants that are forced to be shut down in liquidation to those of plants that are still bankrupt—and hence may be sold or shut down—but pass through the reorganization process.

IV. Data

A. Bankruptcy Data

We gather data on Chapter 11 bankruptcy filings from LexisNexis Law, which obtains filing data from the U.S. 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 dataset 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 2,000 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.[11] We end our sample with cases that were filed in 2005 so as to be able to track bankrupt firms for a five-year period after the bankruptcy filing.

[11] See Iverson (2015) for more information on the LexisNexis data.

B. Establishment-Level Data

The establishment-level data are obtained from the U.S. Census Bureau's Longitudinal Business Database (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 U.S. with at least one paid employee.

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 U.S. Census Bureau's Business Register—the Standard Statistical Establishment List (SSEL)—using the employer identification number (EIN), which is contained in both datasets. 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.[12][13]

C. 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. In remote areas, Census blocks may encompass hundreds of square miles (U.S. Census Bureau (1994)). There are more than 11 million blocks in the 2010 Decennial Census.[14]

Census blocks serve as a valuable source for small-area geographic studies (e.g., Echenique and

[12]For more details on the matching process and sample selection, see Bernstein et al. (2016).

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

[14]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 (U.S. Census Bureau (1994)).

Fryer (2007); Bayer et al. (2008)). 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 non-missing block information.[15]

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.

D. Summary Statistics

Table I 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.

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

[15] 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.

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 level. As is shown, the average Census block in our sample consists of 55.5 establishments corresponding to 1,105 employees. When we contrast the blocks of Chapter 7 and Chapter 11 establishments, we observe again systematic differences. In particular, Chapter 7 blocks are on average smaller (50.6 versus 56.5 establishments; 926 versus 1,143 employees), they are populated by smaller establishments (16.1 versus 19.5 employees per establishment) and those establishments have lower payroll per employee ($27,700 versus $33,500).

Overall, the differences in Table I highlight the importance of selection into the bankruptcy regimes, 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.

V. Methodology

A. OLS Specification

Quantifying the externalities 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 externalities. 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 externalities of liquidation by estimating the following specification:

$$y_l = \alpha + \beta \cdot Liquidation + \gamma \cdot X_{lpi} + \mu_k + \epsilon_{lpi} \tag{1}$$

where l indexes locations (e.g., blocks, block groups, and tracts), p indexes establishments ("plants"), i indexes firms, and k indexes industries. The main dependent variable y is the annualized percentage change in employment at the location of the bankrupt establishment (excluding employment of the

bankrupt establishment itself) in the five years following the bankruptcy filing.[16] Other dependent variables are similarly defined as percent changes from their level in the year of the bankruptcy filing.[17] *Liquidation* is a dummy variable equal to one if the establishment belongs to a company whose Chapter 11 filing is converted into Chapter 7 liquidation. X is a vector of pre-bankruptcy characteristics at the establishment, firm, and location level.[18] We further include 2-digit NAICS industry fixed effects to account for unobserved heterogeneity at the industry level. The coefficient of interest is β which captures the local externalities of liquidation relative to reorganization.

B. IV-2SLS Specification

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 as 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' 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 U.S., each of which pertains to one

[16] More precisely, $y = \frac{\#emp_5 - \#emp_0}{\#emp_0}$, where $\#emp$ is the total number of employees at the location of 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.

[17] To mitigate the impact of outliers, we trim all dependent variables at the 5th and 95th percentiles of their empirical distribution.

[18] 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).

of 94 U.S. 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.[19] We can 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:

$$Liquidation_{pi} = a + b \cdot ShareCasesConverted_j + c \cdot X_{lpi} + \delta_{dt} + \mu_k + \eta_{lpi} \tag{2}$$

where $ShareCasesConverted_j$ is the share of Chapter 11 cases that judge j ever converted to Chapter 7, excluding the current case.[20] Importantly, the inclusion of division by year fixed effects, δ_{dt}, ensures that we exploit the random variation in judge assignment within a division-year cell. The coefficient b 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.[21]

We then estimate the following second-stage regression:

$$y_l = \alpha + \beta \cdot \overline{Liquidation_{pi}} + \gamma \cdot X_{lpi} + \delta_{dt} + \mu_k + \epsilon_{lpi} \tag{3}$$

where $\overline{Liquidation_{pi}}$ are the predicted values from the first-stage regression. The second-stage regression mirrors the OLS regression in equation (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

[19] As an example, consider the bankruptcy district of New Jersey, which is divided into three divisions: Camden, Newark, and Trenton. The Local 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 U.S. 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 (see, e.g., Federal Judicial Center (2016)).

[20] 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 including all dismissed cases in the denominator; ii) the share of cases that judge j converted to Chapter 7 in the five years prior to the current case; and iii) judge fixed effects. Both the first and second stage results are unaffected by the choice of the instrument.

[21] Note that liquidation can also occur through reorganization. Not surprisingly, however, the probability of liquidation is substantially smaller in reorganization (Bernstein et al. (2016)).

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.

C. Validity of the Instrument

To be valid, our instrument needs to bring about significant changes in the probability of converting a Chapter 11 filing into Chapter 7 liquidation (rank condition). Moreover, the instrument needs to be unrelated to the evolution of the bankrupt establishment's local area (exclusion restriction). In the following, we describe how our instrument fulfills both conditions.

C.1. Rank Condition

Table II presents the results of the first-stage regression, which confirm 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 further include industry fixed effects and the full set of controls. As is shown, the coefficient of share of cases converted is economically large and highly significant in both 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%, 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 is 75.7 in column (1) and 80.0 in column (2). Both values are well above the $F = 10$ threshold of Staiger and Stock (1997) and the critical values of Stock and Yogo (2005) to alleviate concerns about weak instruments.

C.2. Exclusion Restriction

The exclusion restriction requires that our instrument, the judge leniency, has no direct effect on post-bankruptcy changes in employment at the location of the bankrupt establishment other than through the increased 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 III we conduct random-

ization tests showing that the instrument is uncorrelated with the full set of controls and industry fixed effects.

Column (1) shows that the R^2 from regressing the share of cases converted on the division by year fixed effects is 0.78, suggesting that there is substantial variation in judge conversion propensities between divisions and over time. In column (2), we include industry fixed effects and controls. As can be seen, none of the controls is statistically significant, the industry fixed effects are jointly insignificant, and the R^2 remains unchanged. Overall, this evidence lends strong support to the randomization assumption.

VI. Results

A. Main Results

Table IV presents the main results. In columns (1)-(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 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 3.3 percentage points lower relative to reorganization. The IV-2SLS estimate in column (2), which relies on the random assignment of bankruptcy judges, is larger in magnitude. It implies that liquidation leads to an annual employment growth rate that is 5.8 percentage points lower relative to reorganization. Both coefficients are significant at all conventional statistical levels. Since the average number of employees in the blocks of Chapter 7 establishments is 926 (Table I), these coefficients imply that block-level employment decreases by 31 to 54 employees per year.

In columns (3)-(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)-(2). Specifically, the estimates imply that the number of establishments drops by 3.7% to 4.7%

per year. Since there are on average 50.6 establishments in the blocks of Chapter 7 establishments (Table 1), these coefficients correspond to a reduction by 1.9 to 2.4 establishments per year.

Finally, in columns (5)-(6), we show that our results are similar if we use payroll in lieu of employment. Overall, the results in Table IV indicate that liquidation imposes large negative externalities on the immediate surroundings of the liquidated establishment.[22]

B. Decomposition

In this section we examine the sources of the decline in employment and establishments. In particular, following the decomposition below, we break changes in employment into changes in: i) employment of establishment openings ("births"), ii) employment of continuing establishments ("continuers"), and iii) employment of establishment closures ("deaths"):

$$\Delta emp_{pi5} = \frac{\#emp_5 - \#emp_0}{\#emp_0} = \frac{(\#emp_5^{new} - 0) + (\#emp_5^{cont} - \#emp_0^{cont}) + (0 - \#emp_0^{dead})}{\#emp_0}$$

Similarly, we decompose changes in the number of establishments into establishment openings ("births") and establishment closures ("deaths") according to the following decomposition:

$$\Delta plants_{pi5} = \frac{\#plants_5 - \#plants_0}{\#plants_0} = \frac{(\#plants_5^{new} - 0) + (0 - \#plants_0^{dead})}{\#plants_0}$$

The results are presented in Table V.[23] In columns (1)-(3), we estimate variants of the specification in column (2) of Table IV, decomposing the change in employment into its three parts. The decline in employment operates mostly through lower growth of continuing plants and lower entry into the region. The 2.3 percentage point decrease in employment at continuer establishments indicates that about 40% of the overall employment externalities comes from the intensive margin (i.e., changes in employment within existing establishments), whereas the remaining 60% comes from the extensive margin (i.e., changes in employment due to establishment closures and births).

[22] These results are robust to a variety of specifications. Specifically, we find similar results if we weight the regressions by the block-level employment, showing that they are not driven by small blocks with few employees. Further, we verify that outliers are not affecting the estimates by trimming at the 10th and 90th percentiles of the employment change distribution or removing blocks whose employment drops to zero within five years of the bankruptcy. Neither of these changes affects the results.

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

Focusing on these extensive margin changes, we find that reductions in establishment births account for 1.8 percentage points of the decline in employment, while only 1.1 percentage points is due to accelerated plant closure.

In columns (4)-(5), we examine the extensive margin more directly by estimating variants of the specification in column (4) of Table IV, decomposing the change in the number of establishments into establishment openings ("births") and establishment closures ("deaths"). The estimates confirm that liquidation leads to both an increase in establishment closures and a decrease in establishment openings. While the likelihood of establishment closure increases significantly (4.7%), these are only the smallest plants that have fairly marginal effect on the overall employment changes, as illustrated in columns (1) through (3).

In the remainder of the analysis, we focus on the employment specification—the specification from column (2) of Table IV (henceforth, the "baseline specification")—since it incorporates both the intensive and extensive margins.

C. Intensity of Treatment

In Table VI, we examine whether our baseline results are amplified for larger "treatments". We code a treatment as large if the size of the bankrupt establishment relative to the Census block (based on the number of employees in the filing year) is above the median across all bankrupt establishments in our sample. Intuitively, the liquidation of establishments that are economically more important within a block is likely to have more impact on the surrounding establishments. As can be seen, we indeed find that the drop in block-level employment is larger and highly significant for above-median treatments, while it is smaller and insignificant for below-median treatments.

D. Dynamics

In Figures 1 and 2, 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 dependent variable the annual change in employment (and number of establishments, respectively) over a five-year period, we now consider horizons of 1 to 5 years following the filing date and report

the cumulative change in employment.

Figure 1 plots the coefficients (along with the 95% confidence bounds) pertaining to the employment regressions. As can be seen, 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, and after four years that it becomes statistically significant.

In Figure 2, we repeat this analysis for the number of establishments. The results mirror those for employment, although that statistical significance is higher throughout.

E. Geographies

In the analysis so far, we examined the externalities 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 (37 employees). Nevertheless, it is instructive to study larger geographies as well.

In column (1) of Table VII, we examine how liquidation affects 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. As is shown, the drop in employment is smaller (–1.8%) and marginally significant ($t = 1.75$). 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 (2), 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.[24]

F. Tradable and non-tradable industries

In Table VIII, we probe potential mechanisms through which liquidation may impose negative spillovers on the local economy. The liquidation of an establishment may hurt nearby businesses

[24]Note that the latter can be interpreted as a placebo test. Indeed, within a tract, the economic relevance of a liquidated establishment is trivial. Hence, finding any effect at the tract level would be symptomatic of omitted variables or other form of non-randomness in the allocation of bankruptcy judges.

that rely on local demand (i.e., the non-tradable sector such as restaurants and retail stores). For example, employees losing their jobs at the liquidated establishments may cut back on their local grocery shopping and restaurant visits, or alternatively, liquidated stores no longer attract customers that used to shop elsewhere. If liquidated establishments remain vacant, that may further deter potential customers from the neighborhood. We examine this channel in Table VIII. Specifically, we re-estimate our baseline specification decomposing block-level employment into non-tradable industries (in column 1) and the remaining industries (in column 2), respectively. We classify 4-digit NAICS industries as non-tradable if the Herfindahl-Hirschman index (HHI) of geographic concentration from Mian and Sufi (2014) is less than 10%.[25] As can be seen, the drop in employment is large and highly significant in non-tradable industries, while is it small and insignificant in other industries. These findings lend support to the local demand channel as well as the impact on consumer traffic.

In an additional analysis, we explore separately the consequences of the liquidation of both non-tradable bankrupt plants and tradable bankrupt plants on both tradabe and non-tradabe firms. Interestingly, we find that while the liquidation of a non-tradable plants significantly lead to the decline in employment of non-tradable plants, all other cases are statistically insignificant when the coefficient are close to zero. Particularly interesting is the effect of the liquidation of tradable plants on non-tradable neighboring plants. The fact that we find insignificant effect suggests that local demnad generated by the workers of the liquidated plant is unlikely to be a key explanation to our findings. In fact, it may suggest that mostly search costs are important in driving the results.

G. Fate of the Bankrupt Establishments

Finally, in Table IX we examine how the local externalities of liquidation vary depending on the "fate" of the bankrupt establishment. Broadly speaking, we classify three potential outcomes for the bankrupt establishment: i) continuer—an establishment that remains operated by the bankrupt establishment and maintains the same operations (either in reorganization, or in the years until it winds down in liquidation); ii) reallocated—an establishment that is acquired by another company (which may or may not be in the same industry); and iii) vacant—we observe no economic activity

[25] Our results are robust if we use alternative cutoffs such as 5% or 15%.

at the location of the establishment.[26] 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 form connections to local firms easily, they may also have lower negative externalities.

We examine this heterogeneity by regressing the change in block-level employment on a set of indicator variables that capture the post-bankruptcy status of the bankrupt establishment. Note that the reallocation status is split into two sub-categories depending on whether the establishment is reallocated to the same or a different 2-digit NAICS industry.[27] 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 IX. 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)-(8), where we include all indicators jointly, using vacancy as base group. Overall, these 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).

VII. Discussion of Results

In this section, we first discuss how our empircal results correspond to theories of agglomeration and the channel through which bankruptcy may affect local economies. In addition, we consider how these findings relate to the efficiency of each bankruptcy regime.

[26] We track establishments' post-bankruptcy status using the methodology of Bernstein et al. (2016).
[27] We also consider reallocation to the same or a different 3-digit NAICS industry.

A. *Spillover Mechanisms*

In Section II we discussed three separate mechanisms that may explain how bankruptcy affects local firms: customer search costs, production costs, and direct local demand. Because each of these channels has distinct empircal predictions, our results can help identify the channel through which banruptcy spillovers operate. We begin with the direct local demand channel (Moretti (2010)), which predicts that the liquidation of a bankrupt firm will reduce local demand for goods and services and hence lead to a reduction in employment of local nontradable firms only. While we do find that our effects are driven principally by the nontradable sector, it is important to note that we do not find that the liquidation of a *tradable* establishment leads to a drop in employment among *nontradable* firms.[28] Indeed, Moretti (2010) focuses precisely on this spillover from tradable to nontradable employment, finding that one new manufacturing job creates 1.6 jobs in the nontradable sector. The fact that we fail to find this relationship suggests that this is not a major channel in our setting. This is further backed up by the fact we only find spillovers at the very local block level, when local demand is likely to spread to a wider economic region, such as a city. In addition, the magnitude of our effects are likely too large to be explained by a local demand effect, as we find that liquidating one plant (with 28 employees on average) leads to a decline of 269 jobs in the same census block after 5 years, implying a multiplier of 9.6. Lastly, the timing of the effect is inconsistent with this hypothesis, as we find that the spillover takes about 4 years to materialize. If the effect were due to a direct decline in local demand, one would expect a fairly quick reaction, since employment at the bankrupt plant drops within one year after bankruptcy (Bernstein et al. (2016)).

Are the results consistent with the effect being driven by a loss of business synergies? Under this theory, similar firms agglomerate in order to increase the transfer of knowledge and skills, to reduce transportation costs of goods, and to create larger labor market pools. Empirically, this channel predicts that switching a plant to a new industry will generate negative spillovers, as this disrupts the network and reduces the synergies shared by all similar plants. Consistent with this idea, in Table IX we find that the liquidation spillover is significantly more negative when a plant

[28] This result has not yet been disclosed from the Census and so the exact coefficient estimates are unreported at this time.

is reallocated to a different 2-digit or 3-digit NAICS than if it remains in the same industry.[29] However, Table VII shows that our effect decays very quickly at larger geographies, such that it is nonexistant at the census tract level. Labor markets are much larger than even a census tract, and so it seems unlikely that a decline in labor market pooling plays a significant role in our estimates. Similarly, agglomeration due to transportation costs likely occurs at a geographic level much larger than a census block, suggesting that an increase in transportation costs are not the principal cause of the negative externalities. Meanwhile, 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 500 meters. This makes sense 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.

The final channel is that liquidation could cause negative externalities by reducing customer traffic to a specific area. Several pieces of evidence are consistent with this theory. First, Table VIII shows that nearly all of the overall effect comes from a reduction of employment at nontradable firms (such as retail and restaurants). In addition, in unreported results[30] we find further that our findings are driven by the liquidation of nontradable bankrupt firms on other nontradable firms. Meanwhile, when bankrupt firms in the tradable sector are liquiated we find little effect on either tradable or nontradable employment in the same block. Thus, the effect is found in cases where customer traffic is likely to be reduced, and only among firms that rely on customer traffic for demand. Second, the fact that the effect dissipates quickly at larger geographies is also consistent with this hypothesis, 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. It is important to 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 (2014)). This suggests that even small stores play a significant role

[29] This fact is also consistent with the consumer search channel, since changing industries will reduct the concentration of shops in a single industry.

[30] Results have not yet been disclosed from the census.

in attracting customers. Table IX suggests that one mechanism for this is store vacancy, which is correlated with larger drops in block-level employment. As shown in Bernstein et al. (2016), liquidation increases the likelihood of vacancy at a location by 17.4%. While a vacant location will certainly not attract customers, it is possible that vacancy actually *deters* customers. 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)). If vacancy in commercial real estate is similar, this could be a key mechanism by which liquidation reduces local consumer demand.

Although our analysis does not distinguish precisely between these different possible channels, it appears to be most consistent with liquidation harming knowledge transfer and reducing customer traffic to nontradable firms.

B. Welfare Discussion

While our estimates show that liquidation causes a reduction in local employment, the welfare consequences are less clear. A key factor that is unobserved in our analysis is how employees and other assets of affected firms are reallocated to other locations. To these extent that there are frictions that make it difficult for displaced employees to find new jobs, liquidation could have far-reaching consequences.[31] Further, our estimates only capture the local spillover effects of bankruptcy, ignoring any effects in more distant areas. Because of these limitations, we do not make strong claims as to the welfare consequences of liquidation versus reorganization.

Importantly, one should not interpret our estimates as applying to all bankruptcy liquidations. Our empirical set-up estimates a local average treatment effect of liquidation, and thus focuses on marginal firms that are affected by judge assignment.[32] On average, we find a sharply negative effect of liquidation on local employment for these marginal firms, but clearly this does not imply that firms should never be liquidated or that liquidation will always have negative spillovers for all firms.

[31] Jacobson et al. (1993) and Von Wachter et al. (2009) show significant earnings losses for displaced workers. At distressed firms, Graham et al. (2013) find that employees of bankrupt firms experience significant and long-lasting earnings declines.

[32] However, a large number of bankrupt firms are marginal, as discussed in detail in Bernstein et al. (2016).

VIII. 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. This effect decays with distance—it is marginally significant at the Census block group level, and no longer significant at the Census tract level. We further find that most of the decline in employment operates through a decrease in employment growth of existing establishments, and lower entry of new establishments into the local region.

When we examine the mechanisms through which liquidation affects local employment, we find that the decrease in employment is larger in the non-tradable sector, suggesting that liquidation hurts nearby businesses that rely on local demand. In addition, we find evidence of agglomeration spillovers. Specifically, the decrease in employment is larger at establishments that are related to the liquidated establishment's industry through input-output linkages as well as labor market linkages. These findings indicate that liquidation hurts the surrounding firms by breaking relevant linkages within the agglomeration.

These findings leave a number of important areas open for future research, of which we highlight two here. First, our study examines the local externalities of liquidation. Externalities can be non-local as well (e.g., the liquidation of an important customer could hurt non-local suppliers). A challenge for future research is to establish the relevance and magnitude of such non-local externalities. Second, the general equilibrium implications of our findings remain to be explored. Our results indicate that employment declines in the Census block of the liquidated establishment. Where do these employees go? Do they migrate to other locations? If they find new jobs, how long do they remain unemployed in between? Addressing these questions would require matched employer-employee data to track employees across different employers and geographies. These are exiting avenues for future research.

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Figure 1
Evolution of Employment in the Liquidated Establishments' Blocks

This figure plots the evolution of employment in the Census blocks of Chapter 7 establishments (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). 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. The dashed lines plot the 95% confidence bounds.

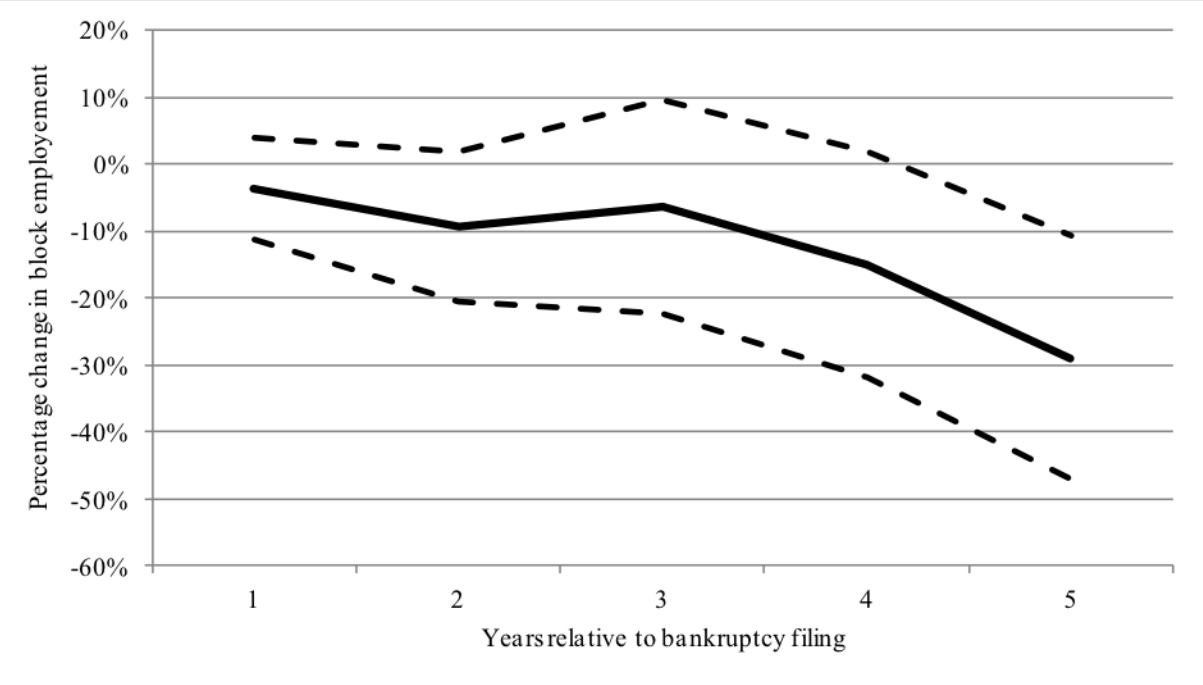

Figure 2

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 establishments (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). 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. The dashed lines plot the 95% confidence bounds.

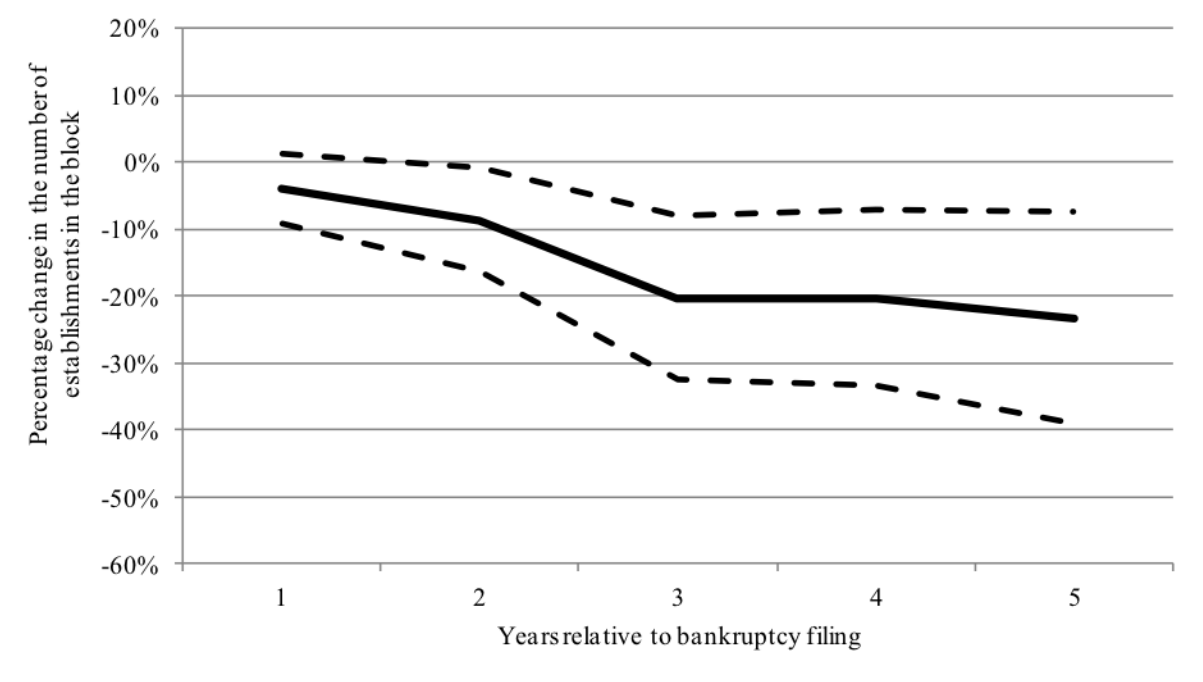

Table I
Summary Statistics

The table provides summary statistics for the bankrupt firms, their establishments, and non-bankrupt establishments residing in the same Census block as bankrupt plants. 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 U.S. Census Bureau.

	All			Chapter 7 (Liquidation)			Chapter 11 (Reorganization)		
	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.
a. Establishment-level characteristics									
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. Firm-level characteristics									
Employees	20,000	220	2,249	8,000	72	385	12,000	309	2,828
Establishments	20,000	6.1	48.3	8,000	2.7	21.8	12,000	8.1	58.7
c. Block-level characteristics									
Employees	91,000	1,105	2520.8	16,000	926	2327	75,000	1,143	2558.2
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
Payroll / Employees (000s)	91,000	32.5	1357.9	16,000	27.7	23	75,000	33.5	1494.2

Table II
First Stage

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. Both regressions include division by year fixed effects. The regression in column (2) further includes 2-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.

Dependent variable	Liquidation	
	(1)	(2)
Share of cases converted	0.578***	0.588***
	(0.066)	(0.066)
a. Firm-level controls		
log(employees of bankrupt firm)		-0.029***
		(0.004)
log(establishments of bankrupt firm)		-0.016***
		(0.006)
b. Establishment-level control		
log(employees of bankrupt establishment)		0.010***
		(0.003)
c. Block-level control		
log(employees at block of bankrupt establishment)		-0.026***
		(0.002)
Division-Year FE	Yes	Yes
Industry FE	No	Yes
F-test	75.73	80.01
Adjusted R-squared	0.140	0.173
Observations	91,000	91,000

Table III
Randomization

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. Both regressions include division by year fixed effects. The regression in column (2) further includes 2-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.

Dependent variable	Share of cases converted	
	(1)	(2)
a. Firm-level controls		
log(employees of bankrupt firm)		0.0009
		(0.0006)
log(establishments of bankrupt firm)		-0.0015
		(0.0009)
b. Establishment-level control		
log(employees of bankrupt establishment)		-0.0001
		(0.0005)
c. Block-level control		
log(employees at block of bankrupt establishment)		0.0000
		(0.0002)
Division-Year FE	Yes	Yes
Industry FE	No	Yes
F-test for joint significance of industry FE		0.439
R-squared	0.782	0.782
Adjusted R-squared	0.777	0.777
Observations	91,000	91,000

Table IV
Main Results

In columns (1)-(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 variables in columns (3)-(4) and (5)-(6) are defined similarly with respect to the number of establishments and payroll, respectively. 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), (3) and (5) are estimated by OLS; the regressions in columns (2), (4), and (6) are estimated by 2SLS using as instrument share of cases converted. All regressions contain the full set of controls used in column (2) 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.

Dependent variable	Employment		Establishments		Payroll	
Model	OLS	IV-2SLS	OLS	IV-2SLS	OLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation	-0.033***	-0.058***	-0.037***	-0.047***	-0.038***	-0.062***
	(0.002)	(0.019)	(0.002)	(0.016)	(0.002)	(0.021)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Division-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.292	0.097	0.322	0.162	0.296	0.106
Observations	91,000	91,000	91,000	91,000	91,000	91,000

Table V
Decomposition

The regressions in columns (1)-(3) are variants of the regression in column (2) of Table 4, except that employment is decomposed into i) employment from establishment openings ("births"), ii) employment from continuing establishments ("continuers"), and iii) employment from establishment closures ("deaths"). The regressions in columns (4)-(5) are variants of the regression in column (4) of Table 4, except that the number of establishments is decomposed into i) the number of establishment openings ("births") and ii) the number of establishment closures ("deaths"). 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.

	Employment			Establishments	
Dependent variable	Employment from establishment openings (births)	Employment from continuing establishment (continuers)	Employment from establishment closures (deaths)	Establishment openings (births)	Establishment closures (deaths)
	(1)	(2)	(3)	(4)	(5)
Liquidation	-0.018***	-0.023*	-0.011*	-0.017*	0.047***
	(0.007)	(0.014)	(0.006)	(0.010)	(0.012)
Control variables	Yes	Yes	Yes	Yes	Yes
Division-Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Model	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS	IV-2SLS
Adjusted R-squared	0.019	0.136	0.008	0.059	0.142
Observations	91,000	91,000	91,000	91,000	91,000

Table VI
Intensity of Treatment

This table presents variants of the regression in column (2) of Table 4, except that the sample is split into "large" and "small" bankruptcies. A bankruptcy is coded as large if the size of the bankrupt establishment relative to its Census block (based on the number of employees in the filing year) is above the median across all bankrupt 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.

Dependent variable	Employment	
Sample	Large bankruptcies (above median)	Small bankruptcies (below median)
	(1)	(2)
Liquidation	-0.076***	-0.040
	(0.027)	(0.025)
Control variables	Yes	Yes
Division-Year FE	Yes	Yes
Industry FE	Yes	Yes
Adjusted R-squared	0.048	0.110
Observations	46,000	45,000

Table VII
Geographies

his table presents variants of the regression in column (2) of Table 4, except that the changes in employment are computed at the Census block group and Census tract level, respectively. 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.

Dependent variable	Employment		
Geography	Block groups	Census block groups	Census tracts
	(1)	(2)	(3)
Liquidation	-0.058***	-0.018*	-0.003
	(0.019)	(0.010)	(0.009)
Control variables	Yes	Yes	Yes
Division-Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Adjusted R-squared	0.097	0.006	0.023
Observations	91,000	91,000	91,000

Table VIII
Tradable and non-tradable industries

This table presents variants of the regression in column (2) of Table 4, except that the dependent variable is employment in non-tradable industries only (column (1)) and tradable industries only (column (2)). Non-tradable industries are 4-digit NAICS industries for which the Herfindahl-Hirschman index (HHI) of geographic concentration from Mian and Sufi (2014) is less than 10%. 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.

Dependent variable	Employment in non-tradable industries	Employment in tradable industries
Bankruptcy type	All	All
	(1)	(2)
Liquidation	-0.051***	-0.005
	(0.017)	(0.004)
Control variables	Yes	Yes
Division-Year FE	Yes	Yes
Industry FE	Yes	Yes
Model	IV-2SLS	IV-2SLS
Adjusted R-squared	0.097	0.007
Observations	91,000	91,000

Table IX
Fate of the Bankrupt Establishments

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 2-digit NAICS indicates whether the establishment is reallocated to the same 2-digit NAICS industry. The other reallocation indicators are defined analogously. Vacant indicates whether the establishment stays vacant throughout the 5-year period. In columns (7)-(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.

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 2-digit NAICS		-0.005**					0.012***	
		(0.002)					(0.002)	
Reallocated in different 2-digit NAICS			-0.035***				-0.010***	
			(0.002)				(0.002)	
Reallocated in same 3-digit NAICS				-0.001				0.015***
				(0.002)				(0.002)
Reallocated in different 3-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 FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	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