Asset Reallocation in Bankruptcy*

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ABSTRACT

This paper investigates the consequences of Chapter 7 liquidation and Chapter 11 reorganization on the reallocation and subsequent utilization of assets in bankruptcy. We identify 129,000 bankrupt establishments and construct a novel dataset that tracks the occupancy and utilization of real estate assets over time. Asset reallocation is widespread, as nearly 80% of real estate is not occupied by bankrupt firms five years after the bankruptcy filing. Using the random assignment of judges to bankruptcy cases as a natural experiment that forces some firms into liquidation, we find that liquidated assets are more likely to be vacant and employ less workers five years after bankruptcy than comparable assets that are reorganized. These effects are concentrated in thin markets with few potential users, in areas with low access to finance, and in areas with low economic growth.

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Asset reallocation between firms is an important determinant of the economy's overall productivity and its speed of recovery following adverse shocks (Eisfeldt and Rampini (2006); Hsieh and Klenow (2009)). In this paper we examine how the bankruptcy system affects asset reallocation of distressed firms. Bankruptcy institutions determine whether assets will remain with the bankrupt firm, and, if not, how to reallocate assets to different users. Such institutions typically fall into one of two distinct regimes: liquidation or reorganization. In liquidation (such as Chapter 7 of the U.S. bankruptcy code), the firm ceases to exist and assets are sold in cash auctions. In reorganization (such as Chapter 11 of the U.S. bankruptcy code), the firm continues to operate while a structured bargaining process determines whether assets remain under existing ownership or are divested. Despite the prevalence of bankruptcy in the economy, relatively little is known about the effects of liquidation and reorganization on asset reallocation. This paper attempts to fill this gap by comparing how assets are reallocated and ultimately utilized across these two bankruptcy regimes, and identifying under which circumstances the two regimes differ.

Theoretically, with frictionless markets, the outcomes of liquidation and reorganization should be identical and both systems will effectively allocate capital to its best use. However, various frictions in either liquidation or reorganization could reduce asset utilization. Specifically, while reorganization provides the flexibility to choose which assets to keep within a firm and which to sell, previous work has shown that conflicts of interest, information asymmetry, and coordination costs can lead to complicated and costly negotiations that erode the value of the estate and prevent assets from reallocating to better uses (Baird (1986); Gertner and Scharfstein (1991); Aghion et al. (1992); Ivashina et al. (2015)). Meanwhile, forcing the liquidation of the firm avoids both inefficient continuation and costly negotiation. However, when asset markets are illiquid, forced liquidations can prevent assets from being allocated to their highest-value use, especially when assets are specific to a particular firm or industry (Williamson (1988)). Such misallocation may be further exacerbated by the correlation of financial distress with industry-wide downturns, in which optimal buyers may not be able to bid for the bankrupt firm's assets (Shleifer and Vishny (1992)).

Since theory suggests that there are significant frictions to reallocation in both structured bargaining and forced liquidation, ultimately, their relative effect on asset utilization is an empirical question. However, answering this question is complicated by two important issues. First, there is little information on how assets are reallocated and ultimately utilized in bankruptcy, particularly when firms are dissolved. Second, firms that are liquidated through Chapter 7 may be fundamentally different from firms that are reorganized in Chapter 11, as illustrated by Maksimovic and Phillips (1998).[1] Any comparison between the two bankruptcy regimes may lead to biased estimates due to unobserved differences in firm prospects and other characteristics.

To deal with the first issue, we construct a novel dataset that tracks real estate assets over time.[2] This dataset captures the reallocation and utilization of real estate assets of bankrupt firms by tracking economic activity at specific locations, even after plant shutdowns, and across changes in occupancy. The optimal user of these assets varies significantly, depending on building features and location characteristics. For example, an industrial warehouse is unlikely to be a suitable location for a retail store, and a restaurant is unlikely to be replaced with a hotel. Moreover, specific locations benefit firms differently as they provide access to customers and suppliers, local labor markets, and knowledge spillovers (Ellison et al. (2010)).

We combine the U.S. Census Bureau's Longitudinal Business Database (LBD) with bankruptcy filings from LexisNexis Law, to obtain a dataset with rich information on 129,000 establishments belonging to 28,000 bankrupt firms employing close to 4.7 million workers. An important contribution of our work is the creation of geographic linkages that allow us to measure reallocation and utilization of real estate assets over time. While the LBD tracks plants over time and across ownership changes, our approach tracks real estate locations, thus allowing to capture also cases in which the real estate is vacant or when it is used for a different purpose than the original plant.[3]

[1] Maksimovic and Phillips (1998) find that the productivity of plants that are converted to Chapter 7 is significantly lower than the productivity of plants that remained in Chapter 11. Hence, selection into Chapter 7 is consistent with economic viability.

[2] Real estate represents a significant portion of firms' total capital. Based on Flow-of-Funds tables from the Federal Reserve, nonresidential structures (value of buildings, excluding the value of the land) accounted for $8.2 trillion of real assets, while nonresidential equipment comprised only $4 trillion at the end of 2014.

[3] For example, if an auto parts manufacturer, AutoABC, is shutting down, and the building is then occupied by a shoes manufacturer, ShoesXYZ, the LBD will consider the death of AutoABC and the birth of ShoesXYZ as two separate incidents. Our linkages will connect the two, and we will say that ShoesXYZ replaced AutoABC in this real estate location. For details on how LBD linkages are constructed, see Jarmin and Miranda (2002). We describe our linkages in detail in Section III.A and in the Appendix.

To explore long-run (five-year) reallocation and utilization of these assets we rely on several measures. First, we explore whether a location continues to be operated by the bankrupt firm, and if not, whether it is occupied by a new firm or remains vacant. Second, we explore the average number of employees and average total wage bill at a given location over time. While the former measure captures only whether economic activity takes place in a given location, the latter measures also capture the intensive margin of such economic activity.

Tracking asset reallocation in bankruptcy reveals several interesting stylized facts. First, both liquidation and reorganization lead to substantial asset reallocation. Second, when an asset is redeployed to a different user, it is most likely to a local firm and often remains within the same industry, suggesting a significant degree of asset specificity. These findings are consistent with Williamson (1988) and Ramey and Shapiro (2001). Finally, we find that industry conditions and, especially, local economic activity are important determinants of asset reallocation and utilization, consistent with the importance of market liquidity for asset redeployment (Shleifer and Vishny (1992); Gavazza (2011)).

In order to deal with the endogeneity of the bankruptcy regime, we employ an instrumental variables strategy 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. 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.

Our empirical strategy compares bankrupt firms that are reorganized within Chapter 11 to firms that file for Chapter 11 but are converted to liquidation within Chapter 7 due to the assignment of the judge. In effect, otherwise identical filers are randomly placed in either a structured bargaining or cash auction bankruptcy regime, thereby allowing us to compare plant outcomes across the two regimes. Our empirical strategy follows a growing set of papers that takes advantage of the random assignment of judges and variations in judge interpretation of the law such as Chang and Schoar (2013) and Dobbie and Song (2015).[4]

[4]Additional examples include Kling (2006); Doyle Jr (2007); Doyle Jr. (2008); Galasso and Schankerman (2015).

Such empirical strategy allows us to explore the following question: if a given firm had not been reorganized, how would its assets have been redeployed through liquidation?[5] We find that bankrupt plants in liquidation are 30% more likely to be shut down, relative to reorganization, during the five years following the bankruptcy filing. The subsequent reallocation of real estate to new users does not fully close this gap. Specifically, liquidated plants are 17.4% less likely to be occupied by any firm five years after the bankruptcy filing, suggesting that in liquidation, on average, assets are less utilized. The two additional measures of utilization confirm this finding: the average number of employees and total payrolls are significantly lower in liquidation relative to reorganization. Taken together, the results show that bankruptcy regimes are an important determinant of asset reallocation and utilization, and that frictions affect how these assets are redistributed to new users.

While the results described thus far show that there is a difference in utilization across the two bankruptcy regimes, this difference could be caused by frictions in reorganization that lead to over-utilization or frictions in liquidation that lead to uder-utilization. We next explore the mechanism causing lower utilization by examining three channels through which liquidation is predicted to lead to lower asset utilization. First, we create a measure of market thickness which assesses the extent to which potential users of the bankrupt plant's real estate reside locally. Second, financial constraints may prevent firms from using assets due to an inability to obtain funding. Since assets typically reallocate to new and local businesses, we explore measures that identify markets with low access to small business finance. Finally, we test whether reallocation is reduced in low economic growth regions, leading to lower utilization of assets in liquidation.

We find support for all three channels. Five years following the bankruptcy filing, plants in thick markets are equally likely to be occupied regardless of the bankruptcy regime, due to significant reallocation activity in liquidation cases. In sharp contrast, liquidated plants in thin markets are over 30% less likely to be occupied than otherwise-identical reorganized establishments. Similarly, we find no long-term differences in employment and wages across

[5] We use the terms "reorganization" and "liquidation" to refer to bankruptcy procedures similar to Chapter 11 and Chapter 7, respectively. Importantly, this usage of the terms "reorganization" and "liquidation" is separate from the ultimate outcome of the bankruptcy. Firms in a reorganization bankruptcy regime can be liquidated if that is the outcome of the bargaining process. The key difference is that liqudation is forced under a cash auction system like Chapter 7, while it is not with structured bargaining.

the two bankruptcy regimes in thick markets, while employment and wages are significantly lower at liquidated plants in thin markets.

We also find that local access to small business financing affects asset reallocation in bankruptcy. In regions with high access to small business finance, we find similar levels of utilization for both liquidated and reorganized establishments. But in markets with low access to finance, liquidated plants are less likely to be occupied and have significantly lower employment and payroll relative to plants in Chapter 11 reorganization. Similarly, in high-growth regions, both bankruptcy regimes generate similar levels of asset utilization. However, in markets with low economic growth, we again find that liquidated establishments experience significantly lower long-run utilization.[6]

Overall, the concentration of the effects of liquidation in areas with thin markets, low access to finance, and low employment growth is consistent with Shleifer and Vishny (1992) and suggests that frictions in liquidation can lead to significantly lower utilization. On the other hand, we also find that liquidation can lead to *better* outcomes than reorganization in particular markets that combine thick asset markets, high access to capital, and strong economic growth. Thus, our results should not be interpreted as showing that reorganization is universally better than liquidation, but rather that local market conditions play an important role in determining the long-term effects of liquidation relative to reorganization.[7]

Our analysis builds on Maksimovic and Phillips (1998) who explore how industry conditions affect the reorganization and redeployment of bankrupt plants. Our paper differs in several important manners. First, by linking real estate assets over time, we can measure how assets are reallocated and utilized even after firms are dissolved and plants are shut down, thus avoiding selection and attrition issues which may be important in bankruptcy. Second, our sample includes close to 30,000 bankruptcy cases, covering not only publicly traded firms, but rather a representative set of the firms filing for Chapter 11. Finally, we rely on random judge assignment as an instrument, in order to mitigate selection issues into bankruptcy regimes. In that regard, our paper is also related to Chang and Schoar

[6]The correlation between the three channels is small (below 0.10), suggesting that each of these channels captures different frictions that are responsible for the gap between liquidation and reallocation.

[7]The welfare implications of our findings are nevertheless still somewhat ambigious. We discuss this and other caveats related to the interpretation of our results in Section V.C.

(2013) who explore the impact of pro-debtor versus pro-creditor judges on firms outcomes in Chapter 11. Our paper complements theirs, by focusing on a different question, that is, the effect of bankruptcy regimes on asset reallocation, and by directly comparing Chapter 7 and Chapter 11 cases. Finally, Dobbie and Song (2015) use a similar instrumental variable approach to estimate the impact of personal bankruptcy on earnings, mortality, and home foreclosure.

More broadly, this paper contributes to several strands of the literature. First, it highlights the importance of local market characteristics towards the consequences of liquidation and reorganization on asset reallocation, and thus contributes to an extensive body of theoretical and empirical literature that discuss the optimal design and frictions of the bankruptcy process.[8] Second, a large literature explores the existence and implications of fire sales.[9] This paper adds to this literature by relying on random variation that forces liquidation, which allows exploring subsequent reallocation and utilization of assets separately from reasons that initially lead to the forced sale. Finally, this paper also contributes to the literature that highlights the importance of labor and asset reallocation to economic activity, by studying frictions that may impede reallocation.[10]

The remainder of the paper is organized as follows. Section I discusses the bankruptcy process. Section II discusses the data construction. Section III introduces the measurement of asset reallocation and Section IV presents the empirical strategy. Section V provides the main results in the paper and Section VI concludes.

I. The Bankruptcy Process

Bankruptcy procedures can be broadly classified into two main categories: cash auctions and structured bargaining (Hart (2000)). The U.S. Bankruptcy code contains both procedures,

[8] Some theoretical examples include Baird (1986, 1993); Gertner and Scharfstein (1991); Aghion et al. (1992); Shleifer and Vishny (1992); Hart (2000), and empirical studies include Hotchkiss (1995); Strömberg (2000); Davydenko and Franks (2008); Eckbo and Thorburn (2008); Benmelech and Bergman (2011) among others.

[9] For example, see Pulvino (1998, 1999); Ramey and Shapiro (2001); Campbell et al. (2011). Shleifer and Vishny (2011) surveys this literature.

[10] See, for example, Davis and Haltiwanger (1992); Eisfeldt and Rampini (2006); Hsieh and Klenow (2009); Ottonello (2014).

with cash auctions falling under Chapter 7 and structured bargaining taking place in 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 below.

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, 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. As we discuss in Section IV, the judge plays an important role in the decision to convert the case to Chapter 7. However, once a case has been converted, the responsibility to liquidate the estate is passed to a trustee, and thus the judge plays little role in the reallocation of assets for these cases from that point forward. Meanwhile, firms that remain in Chapter 11 go through a structured bargaining process governed by specific rights, voting rules, and the judge himself.

Importantly, Chapter 11 allows for some or all of the assets of the firm to be liquidated should that be the outcome of the bargaining process. The key difference from Chapter 7 is that it is not forced. Assets that are owned by the firm can be sold via "Section 363 sales," in which some or all of the firm's assets are auctioned off while the firm remains in bankruptcy.[11] Similarly, in Chapter 11 there is negotiation that determines whether assets that are leased (as much commercial real estate is) should be retained or returned to their owners. Firms in Chapter 11 have the ability to choose which leases to accept and which to reject, thereby terminating the contract. In Chapter 7, leases are automatically rejected, thereby forcing the lessor to find a new tenant. Thus, regardless of whether an asset is owned or leased, Chapter 11 allows for negotiation surrounding which assets are kept in the firm, while a new buyer or user must be found for assets in Chapter 7.

In this paper, we compare asset reallocation and utilization across these two bankruptcy procedures. The key difference between the procedures for our purposes is that in Chapter 7 all assets are potentially reallocated, while in Chapter 11 there is negotiation over which assets remain with the bankrupt firm, or whether that firm survives at all.

II. Data

A. *Bankruptcy Filings*

We gather data on Chapter 11 bankruptcy filings from LexisNexis Law, which obtains filing data from the U.S. Courts system. This data contains 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.[12] The comprehensive nature of the LexisNexis data makes this one of the largest empirical studies on bankruptcy to date, including both public and private firms

[11] Alternatively, some or all of the assets of the firm can be liquidated through a formal plan of reorganization. Creditors are allowed to vote on these plans.

[12] Iverson (2015) provides more details of the LexisNexis data.

from all bankruptcy districts and across all industries. 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.

B. *Census Data and Local Market Heterogeneity Measures*

We match bankruptcy filings from LexisNexis to their establishments in the U.S. Census Bureau's Business Register (BR), which we then link to the Longitudinal Business Database (LBD). The LBD includes all non-farm tax-paying establishments in the U.S. In the LBD, an establishment is a physical location where economic activity occurs. This serves as the main unit of observation in our study.

We match the bankruptcy filings from LexisNexis to the BR 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 connected to it in the LBD. We match bankrupt EINs to all establishments in the BR in the year of the bankruptcy filing to form our initial sample of bankrupt plants. We further reduce the sample due to missing addresses (which are necessary to track economic activity at a location), resulting in a final sample of 129,000 establishments belonging to 28,000 unique firms.[13]

Table 1 presents summary statistics for our final sample. Panel A shows that the average firm in our sample has 4.7 establishments and employs 169 individuals. In total, firms employ 4.7 million individuals at the time of the bankruptcy filing. Approximately 40% of the bankruptcy filings in our sample convert to Chapter 7 liquidation. Further, there are stark differences between firms that stay in Chapter 11 and those that are converted to Chapter 7. The average Chapter 11 firm has nearly three times as many establishments and over four times as many employees. These differences are apparent also at the level of the plant, where plants of Chapter 11 firms employ almost 50% more workers than those of firms that convert to Chapter 7. In addition, Chapter 11 firms have higher payroll per employee ($26,000 per year versus $20,200 at Chapter 7 firms) and are about two years older than Chapter 7 firms. The differences between Chapter 11 and Chapter 7 firms highlight

[13]We provide extensive details of the matching process and sample selection in Appendix A.

the importance of selection into bankruptcy regimes, and hence the need for identification in assessing the impacts of the regimes.

In Section V.B, we explore three measures of heterogeneity of local market characteristics: market thickness, access to capital, and economic growth. Following Gavazza (2011), we first focus on market thickness as a principal driver of the ability to redeploy assets. Given that reallocation is typically done locally and within the same industry (as we show below), we expect that counties which contain many firms in the same or similar industries as the bankrupt plant will have lower search costs and hence a higher probability of finding a user of the vacated real estate. We use the full LBD to measure market thickness for industry i in county c in year t as

$$Thickness_{ict} = \sum_j \tau_{ij} s_{jct},$$

where τ_{ij} is the observed probability across our full sample that a plant in industry i transitions to industry j after closure, and s_{jct} is industry j's share of total employment in county c in year t. $Thickness_{ict}$ is essentially a weighted index of market concentration, where each industry is weighted by τ_{ij}. τ_{ii}, the probability that a plant remains in the same industry, is substantially higher than any other τ_{ij} for all industries, implying that it is often difficult to transition a location to a new industry. Thus, $Thickness_{ict}$ will be highest when a given county has a high concentration of plants in the same or similar industries, thereby making it easier to find a user of a given real estate asset.[14] In Panel B of Table 1, we show that levels of market thickness are similar for both Chapter 11 and Chapter 7 firms.

Second, we focus on access to capital as a determinant of asset reallocation. Because the majority of new occupants of bankrupt locations are local or new firms, we expect that small business loans will be the principal source of capital for these firms (Petersen and Rajan (1994)). Accordingly, we use the share of loans going to small businesses in a county as a proxy for access to finance. We measure this share using the Community Reinvestment Act (CRA) disclosure data from the Federal Financial Institutions Examination Council (FFIEC), which contains data on loan originations by commercial banks for loans under \$1

[14] An alterntaive hypothesis is that thick markets are already saturated with a given industry type. This would imply the opposite prediction: that reallocation is harder in thicker markets.

million.[15] Specifically, we proxy for access to capital by measuring the share of small business loan originations going to small businesses, defined as firms with less than $1 million in annual gross revenue.[16] In Panel B we find that the share of small business loans in regions of Chapter 11 firms is similar to those in regions of firms that were converted to Chapter 7.

Lastly, as in Shleifer and Vishny (1992), we expect that when a bankrupt firm's peers are also experiencing poor economic condtions it will be difficult to find new users of assets. Accordingly, for each year of our sample we aggregate the full LBD to measure the cumulative three-year growth in total employment in each county, and identify county-years with above-median growth as those with high growth. Chapter 11 firms reside in regions with slightly higher past economic growth than Chapter 7 firms.

III. Asset Reallocation Measurement

A. *Tracking Real Estate Assets Over Time*

In this section we describe the construction of geographical linkages that track bankrupt firms' real estate locations over time. We track assets even when plants are sold or shut down, thereby capturing whether real estate is occupied (by either a bankrupt firm or a different occupier), and if so, how intensively it is utilized, as captured by the asset's total employment and payroll. To do so, we rely on the Census LBD, which covers all nonfarm private sector establishments in the United States.

To track real estate occupancy, employment, and payroll outcomes over time, we create a careful address matching algorithm. First, we clean all addresses and address abbreviations using the United States Postal Service formal algorithm.[17] Then, for each shut down plant,

[15] The CRA requires banks above a certain asset threshold to report small business lending each year. During our sample period, the asset threshold was $250 million. Greenstone et al. (2014) estimate that CRA eligible banks accounted for approximately 86% of all loans under $1 million.

[16] Following Greenstone et al. (2014), we define small business loans as those up to $1 million, and small businesses as firms with less than $1 million in annual gross revenue. Ideally, we would measure the share of all lending that goes to small banks, rather than just the share of loans under $1 million, but county-level data on all loans is not available. Given that over 50% of loans less than $1 million go to large firms, it is likely that nearly all loans greater than $1 million go to large firms, and thus the share of CRA loans going to small businesses is a reasonable proxy for the share of all lending going to small businesses.

[17] See the following link (valid as of September 2015) for details of the postal addressing standards used: http://pe.usps.gov/text/pub28/

we attempt to match its address location with subsequent LBD years (up to five years following the bankruptcy filing), to track the next occupant of the real estate location.[18] Our address matching algorithm forces a perfect match on both zipcode and street numbers for each location, and then allows for (almost perfect) fuzzy matching on street name and city name. The details of the address matching algorithm are provided in Appendix B.

With these geographical linkages, we categorize each plant outcome in the following manner. First, if a plant continues to operate after the bankruptcy filing under its original ownership we classify the plant as "continued." Second, if a real estate location is occupied and active (i.e. has positive payroll), and is owned by a different firm from the original bankrupt occupier, we classify it as "reallocated." Such reallocation may not necessarily take place immediately. Therefore, in a given year, we say that a plant is "vacant" if the original plant has previously shut down and no active plant is currently occupying the real estate location.

B. *Measurement Issues and Verification Tests*

Address matching is inherently imperfect for various reasons, such as slight differences in reported street names. One general concern is that we may overstate vacancy rates due to imperfections in the matching algorithm. We conduct several verification tests for our geographical linkages that we discuss in detail in Appendix B.D. Following a manual check of the algorithm, we find that in at least 97% of the cases in which there was no match, it is indeed because there was no match in the LBD universe. In addition, manual checks verify that matched addresses are correct in essentially all cases.

Reassuring evidence of the validity of the geographical linkages matching comes from results discussed below. Consistent with intuition, we find that plant and local economic characteristics can predict whether real estate is likely to be reallocated subsequent to plant closure, as illustrated in Table 2 and discussed in more detail in Section III.C. Moreover,

[18]The LBD includes plant identifiers that link establishments over time. These plant linkages broadly rely on name and address matching (see Jarmin and Miranda (2002) for a detailed description of the construction of the plant linkages). Hence, plant linkages are maintained as long as a plant remains active under existing ownership or is sold and the new owner keeps the same plant name, and address. Otherwise, the plant identifier link is not maintained. Our goal is to construct location-based linkages which are robust to any change in name, and follow plant locations more broadly.

assets are significantly more likely to be reallocated within an industry, as expected. If matching were noisy, such strong patterns would not emerge in the data.

An additional concern is that unmatched real estate assets, which we classify as vacant because they do not appear in the LBD, are in fact converted to a different use, such as residential homes or parks. We explore whether this is the case using data from CoreLogic, a data vendor that compiles the universe of all real estate transactions in the US. Reassuringly, we find that in the U.S., real estate assets are converted into different types of real estate (residential, parks, etc.) in less than 1.5% of the cases.

A final complication that arises when constructing geographical linkages is how to deal with cases in which addresses include multiple establishments, such as office buildings or shopping malls. We construct a careful algorithm that deals with such cases, as described in detail in Appendix B.E. But in fact, this issue does not affect the results. Appendix Table A.5 shows that the results hold for various subsamples of the data that exclude addresses that have mutiple establishments within the same location.

C. Stylized Facts about Asset Reallocation in Bankruptcy

A novel contribution of this paper is the construction of reallocation and utilization measures of real estate assets of bankrupt firms. In this section we describe three stylized facts that guide our main analysis in Section V below.

Stylized Fact 1: Asset Reallocation is Prevalent in Both Bankruptcy Regimes

In Panel A of Figure 1, we explore whether plants continue to operate under their initial ownership following the bankruptcy filing under either liquidation or reorganization. We find that when a bankruptcy filing is converted to Chapter 7 only 54% of plants continue to operate under original ownership after one year, and only 8% by year three. While it is expected that Chapter 7 plants' operations will not continue, non-continuation is also prevalent in Chapter 11. Specifically, 70% of Chapter 11 plants continue after one year, and that figure drops to 39% by year three and 26% by year five. In comparison, Headd et al. (2010) report that on average across the LBD, the establishment survival rate after one year is 80%, and by year five it is 50%.

Panel B of Figure 1 provides novel evidence on the importance of reallocation in bankruptcy.

The figure compares the probability that a location is occupied by the bankrupt firm (red bar) or occupied by any firm (gray bar). The gap between the two bars illustrates the extent to which assets are reallocated. Five years after bankruptcy filing, occupancy rates with reallocation are more than three times higher than the occupancy rates of the bankrupt firms.[19] A similar picture arises when exploring utilization in terms of total employment, as illustrated in Panel C of Figure 1. When focusing on employment by bankrupt firms only, employment drops from more than 4.5 million workers at the time of the bankruptcy filing, to only slightly over one million workers by year 5. However, when taking into account asset reallocation, these locations employ close to 3.25 million workers by year 5. Both figures illustrate that asset reallocation plays an important role in the utilization of these bankrupt firms assets.

Relatedly, we find that reallocation, when it takes place, occurs almost immediately. Panel D of Figure 1 illustrates the pace at which a closed plant is reallocated, conditional on reallocation taking place. As is evident from the figure, approximately 65% of the reallocation happens in the same year a plant is shut down, and the probability that the real estate is redeployed falls drastically subsequently. The pattern is almost identical for both bankruptcy regimes.

Stylized Fact 2: Asset Specificity Matters for Reallocation

We find that asset specificity is an important feature of the reallocation process in bankruptcy. In Panel C of Table 1 we explore the characteristics of reallocated bankrupt plants. We find that most assets are reallocated to local firms, either newly created businesses (52.0%) or existing firms that already have at least a single plant in the same county (34.4%). Non-local entrants account for only a small fraction of total reallocations. The local nature of reallocation is similar in both Chapter 7 and Chapter 11 bankruptcy regimes. We also

[19] Even after accounting for reallocation, vacancy rates are still over 30% in year 5. For reference, statistics collected by the National Association of Realtors indicate that commercial real estate vacancy rates nationwide average over 10%, with levels as high as 20% not being uncommon during economic downturns and in rural areas (see http://www.realtor.org/reports/commercial-real-estate-outlook, link valid as of September 2015). Our sample includes only bankrupt firms, which are more likely to reside in poorly performing regions, and assets may be more likely to be neglected, thus explaining the higher vacancy rates. In a series of paper, Steven Grenadier (Grenadier (1995, 1996)) finds evidence for vacancy rates as high as 30% in the Denver and Houston areas in the 1980s, and shows that the level of equilibrium vacancy rates is predominately determined by local factors. Moreover, he illustrates a significant persistence in vacancy rates in commercial real estate.

find a high degree of reallocation within industries, as the probability that reallocated asset will remain within the same 3-digit industry NAICS is 46.4%. Note that if assets were to randomly transition between industries, we would expect the probability of within-industry reallocation to be less than 1%, as there are 111 3-digit industry codes. These results are consisitent with the literature documenting the importance of asset specificity in asset reallocation (Ramey and Shapiro (2001); Eisfeldt and Rampini (2006); Gavazza (2011)), as discussed above.

Stylized Fact 3: Industry and Local Economic Conditions Affect Reallocation

Finally, we find that industry and local economic conditions are important in determining the degree of asset reallocation. Table 2 reports regression results in which we limit the sample to 101,000 plants that do not continue with the bankrupt firm, and explore what affects the probability that real estate assets will be reallocated and utilized by a new owner. The dependent variable is an indicator equal to one if a new establishment occupies the real estate location within five years of the bankruptcy filing, and zero if the plant was closed but not replaced.

In column 1, we find that county-level characteristics are significant predictors of real estate reallocation. In particular, we find that a high number of local plants, high local economic growth (measured by three-year employment growth in a county), and high payroll per employee in a county, are significantly correlated with higher probability that a discontinued plant will be reallocated.

We find that industry-level conditions matter as well in column 2, which illustrates that real estate in high-growth industries is more likely to be reallocated. In column 3, we also report industry dummies to illustrate heterogeneity across industries in reallocation likelihood. For example, real estate in accomodation, food and entertainment, is much more likely to be reallocated (conditional on plant closure) realtive to the mining and construction omitted category. This evidence suggests that the degree of asset specificity, and the number of potential buyers for commercial real estate may vary across industries.

In columns 4 and 5 of Table 2, we control simultaneously for county-level and industry characteristics. All county-level characteristics remain highly significant in these regressions as well as industry fixed effects, but the effect of industry growth rates falls to zero. Motivated by this, and by the second stylized fact, in the main analysis we focus on local market conditions, and in particular the presence of local firms in similar industries, as important determinants of reallocation in bankruptcy.

IV. Identification Strategy

A. Empirical Design

Identifying the effect of Chapter 7 liquidation on asset reallocation relative to Chapter 11 reorganization is challenging given the inherent selection into bankruptcy regimes. Firms filing directly for Chapter 7 may have worse prospects, and this will be reflected also in the way its assets are reallocated and subsequently utilized. To mitigate the selection in Chapter 7, 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. Hence, the baseline specification of interest is:

$$Y_{pit} = \alpha + \beta \cdot Liquidation_{pi} + \gamma X_{pi} + \epsilon_{pit}$$

where p indexes an individual plant belonging to firm i, and t indexes a year of observation (ranging from one to five years after the bankruptcy filing). The dependent variable Y_{pit} is a measure of post-bankruptcy plant outcomes and real estate asset utilization such as the total number of workers employed at plant p in year t. We are interested in estimating β, which captures the impact of conversion to the Chapter 7 liquidation on Y_{pit}, after controlling for a set of firm- and plant-level variables, X_{pi}, such as pre-bankruptcy filing employment and plant age. Under the null hypothesis that liquidation has similar effect on asset utilization as reorganization, β should not be statistically different from zero.

Even within Chapter 11 filers there may be a significant amount of selection among firms that convert to Chapter 7 liquidation. Table 1 illustrates this point, as firms converted into Chapter 7 liquidation tend to have a smaller number of plants, employ fewer workers, and are slightly younger. Therefore, to identify the causal effect of liquidation on plant outcomes and asset reallocation, we rely on judge heterogeneity in their propensity to convert Chapter

11 filings to Chapter 7 as an instrumental variable.[20] 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 have a large amount of leeway in their interpretation of the law and the level of influence they choose to exert on each case (LoPucki and Whitford (1993); Bris et al. (2006); Chang and Schoar (2013)).

Bankruptcy judges work in 276 divisional offices across the United States, each of which pertains to one of 94 US Bankruptcy Districts. A firm filing for bankruptcy may choose to file either where it is (1) headquartered, (2) incorporated or (3) does most of its business, thereby giving the largest firms some leeway in the bankruptcy venue. However, once a filing is made in a particular division, judge assignment is random.[21] We can then rely on this random assignment to generate exogenous variation in the probability that a given case is converted, since judges vary in their propensity to convert filings. To implement the instrumental variables approach, we estimate the following first stage regression:

$$Liquidation_{pi} = \rho + \pi \cdot \phi_j + \lambda X_{pi} + \delta_{dt} + \mu_k + \epsilon_{pit}$$

where $Liquidation_{pi}$ is an indicator variable equal to one if the bankruptcy case was converted to Chapter 7 liquidation and zero otherwise. Importantly, we include division by year fixed effects, δ_{dt}, to ensure that we exploit judge random variation within a division-year. We also include plant-level controls X_{pi} and industry fixed effects, μ_k. The coefficient on the instrumental variable, π, represents the impact of judge j's tendency to convert a case to Chapter 7, ϕ_j, on the probability that a case is converted to Chapter 7 liquidation. We experiment with several versions of the instrument. First, we estimate ϕ_j as the share of Chapter 11 cases that judge j ever converted to Chapter 7, excluding the current case. This standard leave-one-out measure deals with the mechanical relationship that would otherwise

[20] This approach was pioneered by Kling (2006), and has been applied in a variety of settings since then to convincingly answer a number of important economic questions (Doyle Jr (2007); Doyle Jr. (2008); Maestas et al. (2013); Di Tella and Schargrodsky (2013); Dahl et al. (2014); Galasso and Schankerman (2015); Chang and Schoar (2013); Dobbie and Song (2015)).

[21] As an example, consider the bankruptcy district of New Jersey, which is divided into 3 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."

exist between the instrument and the conversion decision for a given case. We also consider in the Appendix alternative measures of our instrument: (a) the share of cases that judge j converted to Chapter 7 in the five years prior to the current case; (b) judge fixed effects. Both the first and second stage results are unaffected by the choice of the instrument.

The second stage equation estimates the effect of Chapter 7 on plant outcomes asset reallocation:

$$Y_{pit} = \alpha + \beta \cdot \widehat{Liquidation}_{pi} + \gamma X_{pi} + \delta_{dt} + \mu_k + \epsilon_{pit}$$

where $\widehat{Liquidation}_{pi}$ are the predicted values from the first stage regression. In all regressions we cluster standard errors at the division-by-year level, to account for any correlation within bankruptcy court.

If the conditions for a valid instrumental variable are met, β captures the causal effect of Chapter 7 liquidation on plant outcomes and asset reallocation, relative to Chapter 11 reorganization. It is important to note that the estimates in the instrumental variables analysis are coming only from the sensitive firms - those firms which switch bankruptcy regimes because they were randomly assigned a judge that commonly converts cases (Imbens and Angrist (1994)). Clearly, there are some firms that will stay in Chapter 11 no matter the judge and there are other firms that 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.

B. Judge Heterogeneity and Conversion to Liquidation

For the instrument to be valid, it must strongly affect the likelihood of conversion to Chapter 7 liquidation. This can be illustrated in Figure 2, which plots the nonparametric kernel regression between the probability that a case is converted to liquidation and ϕ_j, the share of Chapter 11 cases that a judge ever converted, excluding the current case. We confirm this evidence in our first stage regression, presented in Table 3, which demonstrates that there is a strong and tightly estimated relationship between all versions of the instrument and the probability of conversion to liquidation, even after introducing a comprehensive set of controls.

In column 1 of Table 3 the unit of observation is a bankruptcy filing. The result illustrates that the instrument, *share of other cases converted*, is strongly and significantly correlated with conversions to liquidation. In particular, a one standard deviation (12.9%) increase in our instrument increases the likelihood of conversion by 7.49%, a 18.37% increase from the unconditional propensity of 40.74%.

In the remaining columns of Table 3, and in fact in the entire analysis below, the unit of observation is at the plant location level rather than the bankruptcy case level. In these regressions each observation is weighted by the inverse of the number of plants operated by the firm, to ensure that each firm receives the same weight in the regression and avoid overweighting large bankruptcy cases. In column 2 we repeat the specification in column 1, and verify that the first stage results are identical to column 1 in which the unit of observation is at the bankruptcy case level. In column 3 we add additional control variables, such as the plant age, and number of employees per plant at the year of the bankruptcy filing. The results remain unchanged. In Table A.1 of the Appendix we illustrate that the results are robust to the instrumental variable specifications discussed above. In all specifications, the F-stat is above 100, well above the required threshold of $F = 10$ to alleviate concerns about weak instrument (Staiger and Stock (1997)).

Another identifying assumption is monotonicity, which requires that the assignment of a judge has a monotonic impact on the probability that a given Chapter 11 case is converted into Chapter 7. This means that while the instrument may have no effect on some firms, all those who are affected are affected in the same way. The assumption would be violated if we observe certain types of firms for which the likelihood of conversion increases after being assigned to a given judge, and other firms treated with the same judge for which the likelihood of conversion decreases. This implies that the first stage estimates should be non-negative for all subsamples. In unreported regressions, we estimate the first stage regression for samples split by the median for the following characteristics: number of employees at plant or firm, number of plants in firm, county, or industry, plant age, three-year employment growth in county or industry, and payroll per employee in county or industry. These estimates are positive and sizeable in all subsamples, in line with the monotonicity assumption.

C. The Exclusion Restriction Condition

Our identification strategy is designed to overcome the fact that selection into liquidation is endogenous. For the instrument to be valid, it must not only strongly affect the probability of conversion to liquidation, but also, importantly, must satisfy the exclusion restriction condition. Specifically, it is required that judge assignment only affects the outcomes of interest (e.g. whether a plant location is occupied five years after bankruptcy filing) via its impact on the probability that a case is converted to liquidation. As evidence in partial support of our identification assumption, Table 4 reports randomization tests that show that our instrument is uncorrelated with a comprehensive set of firm and plant level characteristics, as well as local and industry conditions.

Column 1 of Table 4 shows that the R^2 when we regress ϕ_j on the full set of division by year fixed effects and no other controls is 0.777, suggesting that there is substantial variation in judge conversion propensities between divisions and over time. In the next column, we explore whether within a division-year, such variation is correlated with the bankruptcy case characteristics by adding controls for plant size and age, firm size, an indicator for whether there were multiple associated bankruptcy filings, and industry fixed effects. None of these variables is statistically significant and the R^2 is unaffected by their addition. In the next columns we explore whether the local market heterogeneity measures (as defined in Section II) are correlated with the instrument. In columns 3, 4, and 5 we separately add dummy variables indicating if a plant was in a county with above-median market thickness, share of small business loans, or three-year cumulative employment growth. In Column 6 we add all three measures together. In none of the specifications are any of these measures statistically significant. In Column 7 we also add additional variables that capture local economic activity and industry conditions such as the number of plants in the county and industry, payroll per employee in the county and industry, and three-year employment growth in an industry. Once again, all controls are insignificant and the overall R^2 remains basically unchanged. The evidence in Table 4 suggests that there is indeed random assignment of judges to bankruptcy filings within court divisions, thus alleviating the concern that ϕ_j might be related to other factors that might influence future plant outcomes.

The exclusion restriction assumption might still be violated if the judge affects plant outcomes through channels other than the bankruptcy regime, outside the liquidation or reorganization treatments. That is, it is violated if ϕ_j is correlated with judge characteristics which affect plant outcomes regardless of whether a plant is liquidated or not. For example, if less sophisticated judges tend to convert more cases to Chapter 7, then judge skill would be correlated with ϕ_j. This may be a problem *only* if judge skill affects reallocation and utilization outside its impact on case conversion.

While we cannot test the exclusion restriction directly, indirect tests support the identifying assumption and alleviate the above concern. Specifically, we first run a set of reduced-form regressions which directly relate ϕ_j to plant outcomes:

$$y_{pit} = \alpha + \beta \cdot \phi_j + \gamma X_{pi} + \delta_{dt} + \mu_k + \epsilon_{pit}.$$

When we run this regression on the full sample (as reported in Table A.2 in the Appendix), we find a strong relationship between ϕ_j and y_{pit} for all of our outcome variables, arguably, because ϕ_j leads to liquidation, which subsequently affects asset reallocation. If ϕ_j is correlated with judge skill (or other judge attributes), then ϕ_j should affect y_{pit} also when limiting the sample to only firms that remain in Chapter 11 reorganization, or only to firms that are liquidated. However, as reported in Table A.3 in the Appendix, when we run reduced-form regressions on these two subsets of firms we find no significant relationship between the instrument and plant outcomes. Further, we also find that within Chapter 11 reorganization, ϕ_j is uncorrelated with bankruptcy refiling rates, a proxy for bankruptcy resolution success. This evidence suggests that the instrument is likely to capture the effect of liquidation rather than some other impact of the judge through an alternative channel.[22]

[22]Note that if judges propensity to approve a motion to convert a case to Chapter 7 is systematically correlated with other motions then this does not violate the exclusion restriction, rather, affects the interpretation of the liquidation treatment. Nevertheless, if this is the case, and the additional motions affect asset reallocation, then such relationship should lead to statistically significant correlations in Table A.3 in the Appendix, in contrast to our findings. Further explanation for the lack of such statistical correlation can be found in 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. Interestingly, the motion to convert a case receives by far the lowest weight in the first principal component, suggesting 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.

V. Results

A. Full-sample Results

We first focus on how liquidation affects reallocation and utilization in our full sample by testing its impact on four main outcome variables. *Continues* is an indicator variable equal to one if the plant is active (has positive payroll) and continues to be occupied by the original bankrupt firm five years after the bankruptcy filing. The purpose of this variable is to test whether the bankruptcy regime has a significant impact on whether a real estate asset is reallocated or not. The other three variables are measures of the utilization of the real estate, regardless of who the occupant is. *Occupied* is an indicator equal to one if the plant is active five years after the bankruptcy filing. *Ln(average employment)* and *Ln(average total wages)* are defined as averages of employment or payrolls at a specific location over all five years after bankruptcy. Because vacant establishments by definition have zero employment and payrolls, these two measures account for any interim years in which a plant is not occupied, even if it is occupied in year five. Further, they have the advantage of accounting for the intensive margin of employment or wages as well as the extensive margin, since they reflect plants that are reallocated but have fewer employees or lower payrolls. For all three measures of utilization, the geographical linkages discussed in Section III.A allow us to account for reallocation of assets to new users.

Panel A of Table 5 shows both OLS and 2SLS estimates of the impact of liquidation on these plant outcomes (reduced-form regression can be found in Appendix Table A.2). These regressions include the full set of 129,000 plants, and contain all controls in Column 3 of Table 3, including industry and division-by-year fixed effects. For brevity, only the coefficient measuring the effect of liquidation on plant outcomes is presented. Regular OLS results, which do not account for selection, show that liquidation is associated with a 30% decrease in the likelihood of continuation five years after the bankruptcy filing. The 2SLS estimates in column 2, which incorporate the IV analysis, show that converting a firm to liquidation reduces the probability of continuation, with a magnitue of 32.4%. Columns 3 and 4 show that liquidated plants are significantly less likely to be occupied five years after bankruptcy. 2SLS estimates show that liquidation reduces occupancy rates by 17.4%, an effect that is both statistically and economically significant. This estimate is roughly half the size of the 32.4% decline in plant continuation, demonstrating that reallocation to new users closes some of the gap, but not entirely.[23]

The magnitude of the decline is even larger when measuring by employment or wages, estimated at 34% and 60.2%, respectively, in columns 6 and 8.[24] This suggests that not only does liquidation reduce occupancy rates on average (the extensive margin), but it also reduces employment and payrolls conditional on being occupied (the intensive margin).[25] Taken together, the results show that frictions to asset reallocation prevent liquidated plants from being utilized as fully as similar plants that are reorganized.

However, reallocation plays an important role in bankruptcy, and particularly in liquidation. Panel B of Table 5 shows similar regressions to those in Panel A, but here we re-define the utilization measures to be zero unless a plant has been reallocated to a new user. This allows us to estimate the extent to which asset reallocation increases utilization at liquidated plants, relative to reorganized plants. We find that reallocation increases utilization of liquidated plants substantially. For example, reallocation increases the occupancy rate by 13.4% among liquidated plants, relative to reorganized plants. As before, the magnitudes are even higher when measuring by employment or wages.

Panel C of Table 5 shows how liquidation affects occupancy rates in years 1, 3, and 5 after bankruptcy. The purpose of this table is to show how the gap in utilization between liquidated and reorganized plants slowly closes over time. In the first year after bankruptcy, occupancy at liquidated plants is 23.7% lower, and this difference declines by 6.3% by year 5. Thus, in a 5-year period about one quarter of the initial decline in occupancy is erased. This is a significant amount, again highlighting the importance of reallocation, but even so

[23] It is also interesting to discuss the gap between the OLS and IV estimates, which capture the selection into treatment. While there is clearly selection into Chapter 7, how this selection might bias OLS estimates is ex ante unclear. On one hand, it is likely that poorly-performing firms will be more likely to be converted, and their assets are less likely to be reallocated, which would bias OLS coefficients downwards. On the other hand, firms with assets that will be easily redeployed may be more likely to move to liquidation, which would bias OLS coefficients upwards. Results in Table 5 suggest that to a large extent these two effect balance each other out, so that OLS estimates are similar to 2SLS.

[24] Since these are log-linear models with the independent variable of interest, $Liquidation_{p,i}$, being a dummy variable, the estimated impact of moving from reorganization to liquidation is $100\,[\exp(\beta) - 1]$.

[25] We cannot estimate the intensive margin on its own, as we would have to condition the sample on plants that are occupied to do so. This would invalidate our instrument by created a selected subsample.

a gap of 17.4% remains in year 5.[26]

B. Heterogeneity Analysis

The results presented so far show that liquidation results in significantly lower occupancy, employment, and total payroll even five years after the bankruptcy filing. In this section, we explore why utilization is lower when bankruptcies are resolved via cash auction rather than structured bargaining. The principle concern with cash auctions is that in illiquid markets would-be buyers may be unable to purchase the assets of the firm due to asset specificity, financial constraints, or other search frictions, thereby leading to the inefficient allocation of the assets (Williamson (1988); Shleifer and Vishny (1992)).

In Section II above we describe three measures of local market characteristics that theory predicts are related to asset reallocation. In Table 6 we divide our sample using these measures to test whether the effects of liquidation are concentrated in markets where theory predicts reallocation will be difficult. Panel A focuses on a market thickness measure, $Thickness_{ict}$, which measures the market share of plants in the same or similar industries to the bankrupt firm in the same county. Due to asset specificity, new occupants tend to come from similar industries and so we expect that reallocation will most easily occur in thick asset markets. To test this, we define "thick" industry-county pairs as those having above-median $Thickness_{ict}$, and then run our specifications separately for plants in thick and thin markets.[27]

The differences between thick and thin markets are stark. In the first two columns of Panel A, we show that in both thick and thin markets liquidation reduces the probability that a plant will continue with the orginial bankrupt firm by a similar amount. However, column 3 shows that asset reallocation in thick markets completely erases this effect, such

[26] Appendix Table A.4 contains dynamic results for other utilization measures, and shows a similar pattern. In addition, this table also presents regression results where the dependent variable is $\ln(employment)$ or $\ln(wages)$ in each year after filing, rather than the log of the average of these variables.

[27] Note that we do not claim that plants are exogenously distributed across thick or thin markets, as firms in thick markets are likely different on many dimensions from firms in thin markets. However, this does not invalidate the instrument. By running the regressions on separate sub-samples, we compare thick-market firms that are randomly assigned to "liquidating judges" to those that are assigned to "reorganizing judges," and similarly we compare thin-market firms that are randomly liquidated to those that are not. Thus within each regression the estimates can still be interpreted as causal, and the comparison across regressions sheds light on which markets are driving the overall effects.

that occupancy is similar for liquidated and reorganized plants. Meanwhile, column 4 shows that occupancy rates for liquidated plants are 32.4% lower in thin asset markets, relative to plants that are reorganized in thin markets. Similarly, in comparing columns 5 and 6 we find that in thick markets liquidation does not have a significant effect on average employment (indeed, the coefficient estimate is even positive), but in thin markets liquidation reduces employment by 54.6%.[28] Clearly, the effect of liquidation on asset utilization is completely concentrated in thin markets, while realloction in thick markets results in liquidation having no impact on utilization.

Panel B turns to the role of access to finance in preventing fire sales. We proxy for access to finance by measuring for each county the share of loans given to small businesses, defined as firms with $1 million or less in annual gross revenue.[29] Similar to results for market thickness, we find that liquidation leads to substantial declines in utilization in counties with low access to finance, but insignificant differences in counties with high access to finance. This supports theories that access to capital, and in particular small business lending, is a key determinant in the ability to reallocate assets.

Our third market characteristic is economic growth, defined as the cumulative three-year growth in total employment in the county. If financial distress is correlated with poor economic conditions, we expect that liquidation will result in reduced utilization rates (Shleifer and Vishny (1992)). Accordingly, columns 3 and 4 in Panel C show that occupancy rates at liquidated plants are 21.1% lower in counties with below-median employment growth, while in counties with above-median growth the effect is negative but insignificant. Note that we find this difference despite the fact that the continuation probability declines significantly more in high-growth areas (shown in columns 1 and 2), meaning that more reallocation is required for liquidated plants to close this gap. Similarly, we find substantially larger declines in average employment when plants are liquidated in low-growth counties, and insignificant differences in high-growth areas.[30]

[28]For brevity, we do not report results for ln(*average total wages*), but the results show a similar pattern.

[29]Loan data comes from the Community Reinvestment Act disclosure data and is only available beginning in 1996. This removes about 30,000 plants from our sample that filed for bankruptcy prior to 1996.

[30]Appendix Table A.7 presents results using alternative heterogeneity measures to split the sample. Specifically, splitting the sample by liquidity in the real estate market, the presence of small banks in the county, and 3-year employment growth in the industry-county yields similar results to those outlined above.

We find strong support for all three hypothesized mechanisms that limit asset reallocation for liquidated establishments, with the results for market thickness being particularly strong. Importantly, all three measures are uncorrelated, as shown in Appendix Table A.6, suggesting that each channel is separate from the others and exerts a significant effect individually. Further, when using alternative measures of market thickness, access to capital, and economic growth, we find similar results, as described in Appendix Section C. Specifically, splitting the sample by liquidity in the real estate market, the presence of small banks in the county, and three-year employment growth in the industry-county yields similar results to those outlined above. We have also performed several additional robustness tests in unreported results. For example, one concern with $Thickness_{ict}$ is that, because it uses the market share of similar-industry firms, it might be affected by rural counties with few potential buyers but high market shares. However, dropping all counties with less than 20,000 employees (the 10th pecentile in our sample) does not affect the results. Further, splitting the sample by a measure of industry agglomeration developed by Ellison and Glaeser (1997), which is similar to our $Thickness_{ict}$ measure but explicitly adjusts for county size, shows similar results. In addition, scaling access to capital on a per capita basis, rather than market share, does not affect the results. We also find that liquidation causes lower utilization in areas with low house price growth, high levels of plant closings, and low levels of entrepreneurship. The robustness of these results suggests that our findings are not due to specifics of the main heterogeneity measures we focus on here.

Our final analysis shows that the interaction of market thickness with the other measures results in even larger disparities in utilization and reallocation. To perform this analysis, we first split the sample into thick and thin markets, as explained above. We then divide plants in thick markets into those in high-growth and low-growth areas, using median three-year employment growth. We similarly divide thin-market plants into high-growth and low-growth samples. This gives four quartiles, with plants in the top quartile being in counties with both thick markets and high employment growth, while plants in the bottom quartile are in thin markets with low employment growth. The effects of liquidation on utilization in these extreme quartiles are presented in Panel A of Table 7. As expected, we see large declines in utilization by all three measures when plants in bottom quartile counties are liquidated, shown in columns 2, 4, and 6. Perhaps more interesting is that in the top quartile (columns 1, 3, and 5) we find that the point estimate of liquidation's effect on utilization is positive and economically large by all three measures, although not statistically significant due to reduced sample sizes. This suggests that in some very particular markets liquidation actually *increases* overall utilization relative to reorganization by forcing reallocation to potentially better uses.

Panel B of Table 7 performs a similar analysis by interacting market thickness with access to finance. In counties with thick markets and a high share of small business lending (columns 1, 3, and 5) we find positive point estimates of the effect of liquidation for two of the three utilization measures, although the magnitudes are not as large as top-quatile plants in Panel A. In addition, columns 2, 4, and 6 show that liquidation has sharply negative effects on utilization in counties with both thin markets and low access to finance. This highlights the potential impact of liquidation when market conditions severely limit the number of potential buyers. In both Panels A and B, the size of the wedge between the top and bottom quartiles shows how multiple market frictions can interact to either ease or hinder reallocation when plants are liquidated.

C. Discussion

Our results show that liquidation causes reduced utilization of assets, particularly in markets where reallocation is difficult and fire sales are likely. However, the overall welfare implications of each bankruptcy regime are less clear. Our results could be interpreted as showing that liquidation reduces utilization below an optimal level. On the other hand, it could also be that reorganization leads to inefficient continuation (Hotchkiss (1995)), and that the higher utilization seen in reorganization is actually less efficient. For example, it could be that liquidated locations are converted into useful assets that have no employees (such as parking lots, residential housing, or public parks) which are not included in the LBD, or it could also be that there is no price at which these locations can be profitably used and therefore it is optimal to leave them vacant. We cannot fully rule out this possibility and hence we do not take a strong stand on the efficiency of either bankruptcy regime. However, several pieces of evidence cast doubt on the possibility that the reduced utilization of liquidated plants is optimal.

First, data from CoreLogic shows that less than 1.5% of commercial real estate transactions involve the conversion of commercial real estate to a non-employing usage such as residential housing or parking lots. Thus, it is unlikely that a large portion of the vacant locations in our data are being put to alternative uses not captured by the LBD.

Second, as long as capital and labor are complements, a more productive firm will employ more, not fewer individuals. We find that our effects increase in magnitude when measuring utilization using total employees as opposed to occupancy. Further, wages should be highest at more productive firms as long as labor markets are competitive. Given that the size of our effects are even larger when measuring utilization by total wages, this casts doubt on the possibility that liquidation allocates some plants to more productive uses even while leaving some locations vacant.

Third, a strict definition of inefficient continuation is that negative net present value (NPV) projects are allowed to continue due to frictions in the reorganization process. However, negative NPV projects cannot continue indefinitely; eventually the firm will fail when it runs out of capital. Given that our effects persist for at least five years this possibility seems unlikely.

Finally, theory predicts that inefficiencies in the reorganization process will occur when coordination problems or information asymmetries are largest (Franks and Torous (1989); Gertner and Scharfstein (1991); Bolton and Scharfstein (1996)). However, we find that the largest differences between reorganization and liquidation occur in counties with thin markets, low access to finance, and low economic growth, all of which tie directly to difficulties in reallocating assets. There is no reason to predict that inefficiencies in the bargaining process are largest in these same markets.

Despite these pieces of evidence, it is important to keep in mind a few caveats. First, while most of prior literature has focused on publicly traded firms, our sample includes private and smaller firms where there are potentially fewer frictions to bargaining. It is likely that complexity costs and incentive issues are greatest for large, public firms, and thus our results should not be interepreted as showing that bargaining frictions are unimportant for the latter subgroup. Second, the analysis does not consider potential spillovers to other firms. Further, our results deal only with ex post outcomes, but ex post bankruptcy costs could impact ex ante incentives and contracts in important ways, such as by disciplining management to avoid financial distress, and affecting the cost of capital. Thus, we make no conclusions about the ex-ante implications of forced liquidation and reorganization or its effect on social welfare, and leave this analysis to future work.

VI. Conclusion

How do institutions affect the reallocation of assets in the economy? In this work we explore the role of the bankruptcy system in affecting the reallocation of commercial real estate, an important form of capital used by firms. In particular, we explore how the two common bankruptcy approaches in the US - Chapter 7 liquidation and Chapter 11 reorganization - affect the reallocation and subsequent utilization of the real estate assets occupied by bankrupt firms.

We exploit the random assignment of judges to bankruptcy cases and variations in judges interpretation of the law, to instrument for the endogenous conversion of Chapter 11 filers into Chapter 7 liquidation cases. We rely on the Census LBD database to create geographcial linkages that allow us to track changes in real estate occupancy over time. We explore several measures of asset utilization such as whether real estate is occupied, and if so, how many workers are employed in a given location, and what is their total wage bill.

We find that liquidation leads to reduced occupancy and lower utilization of real estate assets, and this effect persists at least five years after the bankruptcy filing. These effects are concentrated in thin asset markets where there are few potential buyers for bankrupt assets, in areas with low access to capital, and in counties with low economic growth. Overall, the results highlight the importance of local asset markets on the reallocation of assets in bankruptcy.

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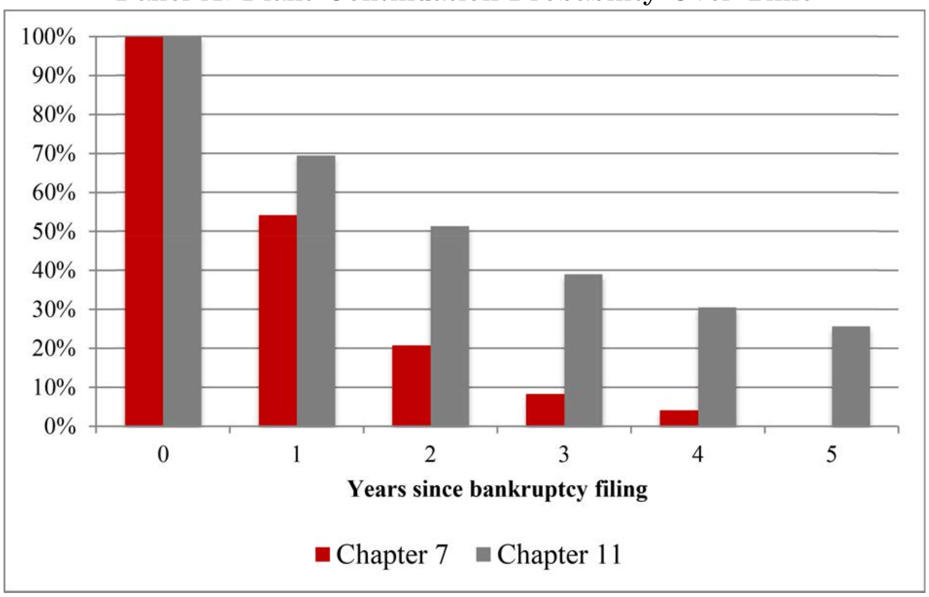
Figure 1
Stylized Facts About Bankruptcy Reallocation

These figures illustrate summary statistics about the reallocation process for bankrupt establishments. Panel A shows the percentage of plants that continue to be operated by a bankrupt firm in the 5 years following the firm's bankruptcy filing for reorganized and liquidated firms. Panels B and C show the role that reallocation plays in affecting utilization rates. Panel B plots the share of bankrupt plant locations that are occupied over a 5-year window after bankruptcy, distinguishing between occupancy rates due only to the original bankrupt plant and those that take into account reallocation to other firms. Panel C is similar to Panel B, but focuses on total employment levels. In this figure, the left-hand axis shows total employment in thousands, while the right-hand axis shows percentage of employment in year 0. Panel D plots the percentage of plants that are reallocated in each year following the death of the bankrupt plant, conditional on reallocation taking place.

Panel A: Plant Continuation Probability Over Time



Panel B: Role of Reallocation - Occupancy Rates

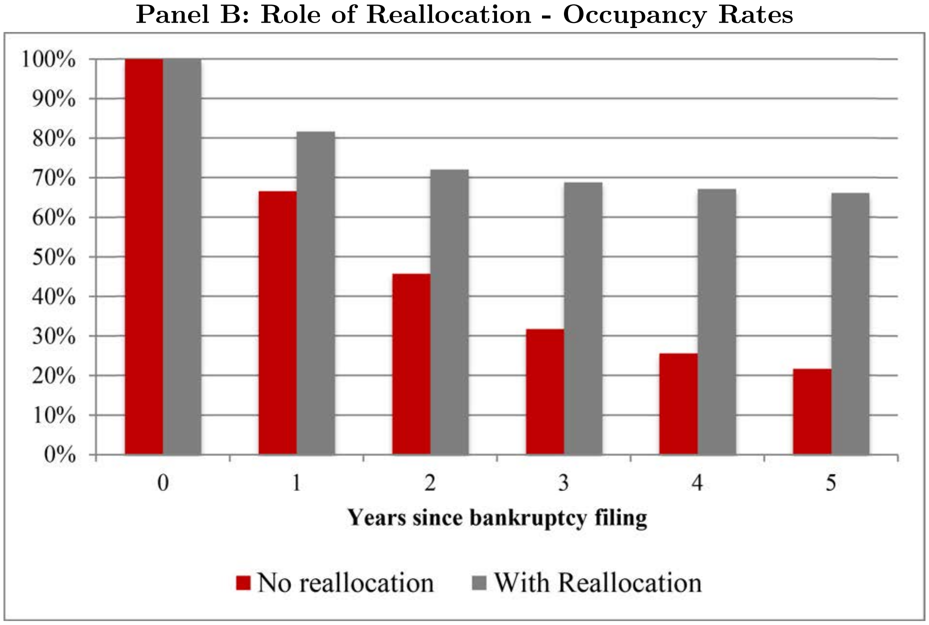
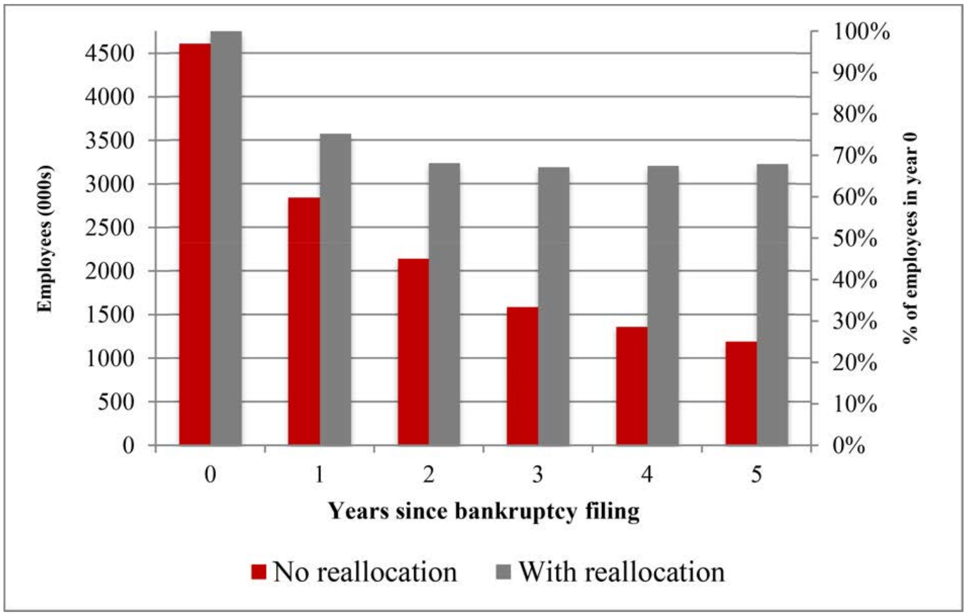


Figure 1
Stylized Facts About Bankruptcy Reallocation (cont.)

Panel C: Role of Reallocation - Employment



Panel D: Share of Plants Reallocated Over Time

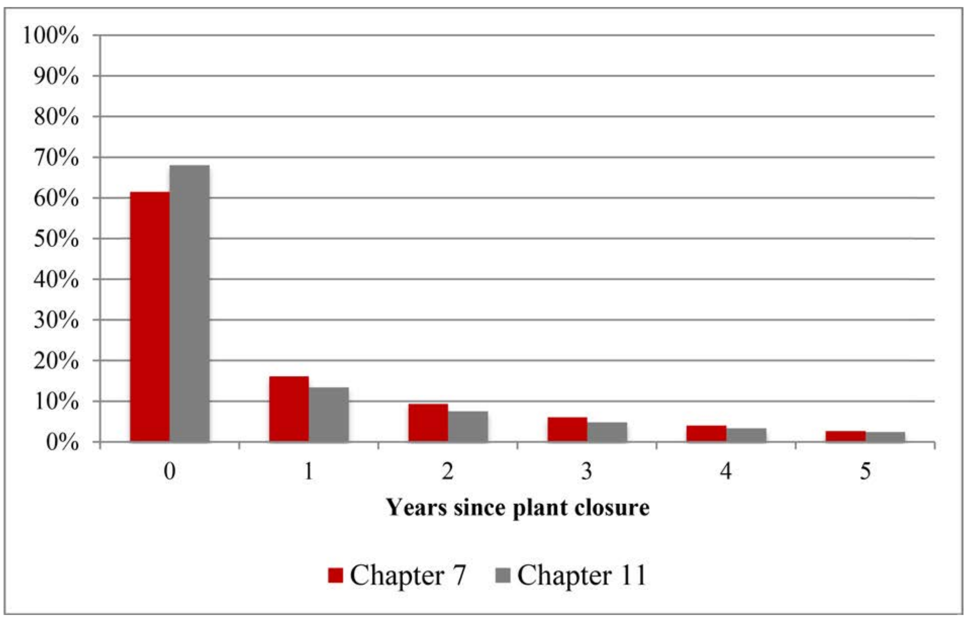


Figure 2

Non-Parametric First Stage

This figure plots the relationship between the probability of case conversion and our preferred instrument, the share of all other Chapter 11 cases that a judge has converted to Chapter 7, using a non-parametric kernel regression. For disclosure reasons, we truncate the 5% tails of the distribution.

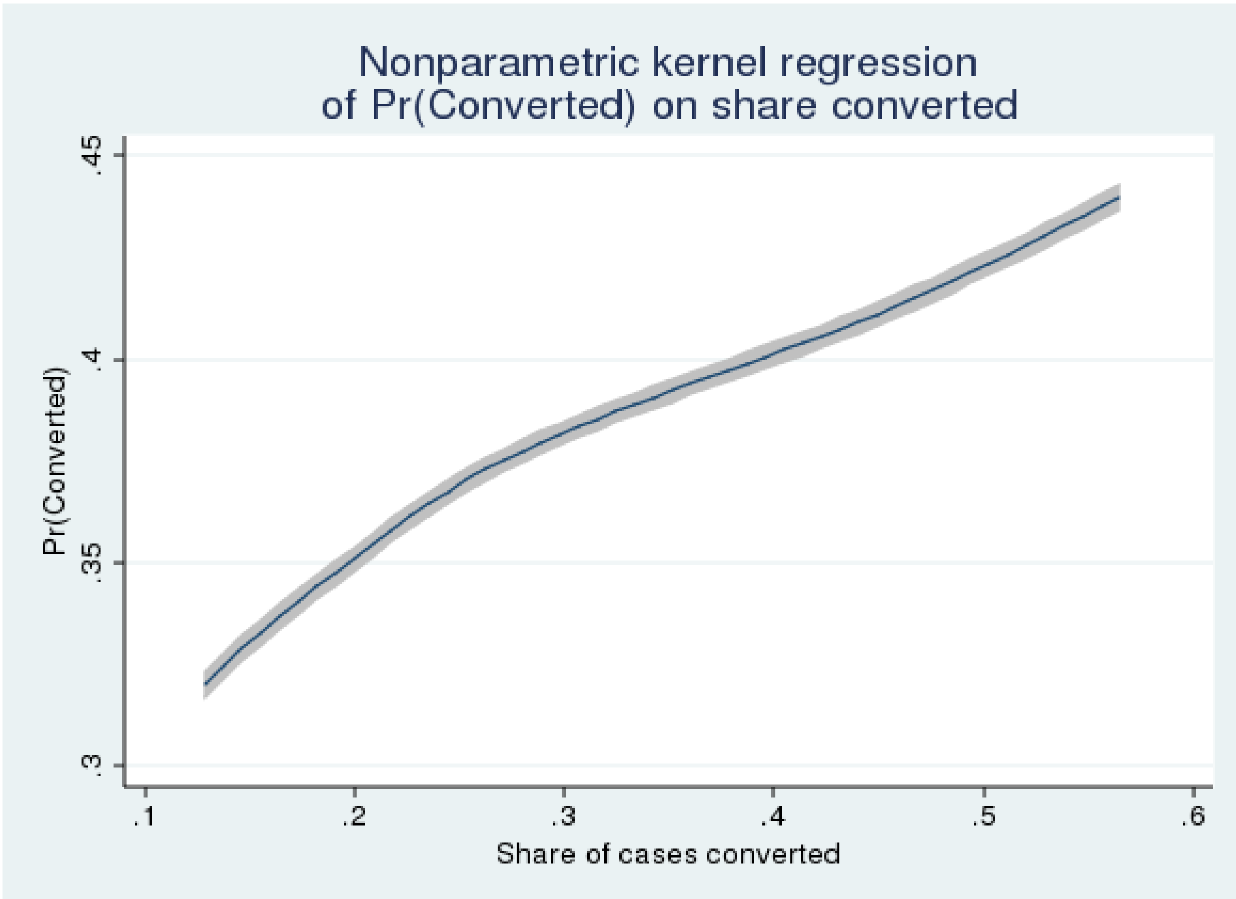


Table 1
Sample Summary Statistics

Panel A of this table presents summary statistics on the plants and firms in our final sample, both overall and split by firms that are reorganized in Chapter 11 and those that are liquidated in Chapter 7. Observation counts are rounded to the nearest thousand due to disclosure requirements of the U.S. Census. All numbers shown are averages, except for observation counts. Payroll and payroll per employee are in thousands of nominal U.S. dollars. Panel B gives average measures of three county-level characteristics, defined in the text, which we use to split the sample to test for heterogeneous effects of liquidation. *Share of small business loans* is only available beginning in 1996, leaving a total of 99,000 plants for these summary stats. Panel C presents characteristics of new entrants that move into locations vacated by bankrupt firms, showing the share of reallocation that is accounted for by new firms, existing local firms (firms that already had an establishment in the same county), and existing non-local firms. This panel also displays the share of new entrants that come from the same industry as the bankrupt firm. For this panel, the sample is restricted to plants that are closed within 5 years of the bankruptcy filing and that are replaced within the same time frame.

Panel A: Average Plant- and Firm-level Characteristics

	All	Reorganized	Liquidated
Plant-level characteristics			
Employment	35.9	38.0	26.9
Total plants	129,000	105,000	24,000
Firm-level characteristics			
No. Plants	4.7	6.5	2.2
Employment	169.0	245.4	57.9
Payroll (000s)	4,507.7	6,819.0	1,146.3
Payroll/Employee (000s)	23.7	26.0	20.2
Age	9.9	10.7	8.9
Number of firms	28,000	17,000	11,000

Panel B: Average County-level Characteristics

	All	Reorganized	Liquidated
Market thickness	6.4%	6.4%	6.4%
Share of small business loans	43.8%	43.7%	43.9%
Cumulative employment growth (3 years)	5.2%	5.4%	4.6%

Panel C: New Entrant Characteristics

	All		Reorganized		Liquidated	
Local vs. non-local						
New entrant	32,500	52.0%	23,500	48.0%	9,500	70.4%
Local entrant, existing	21,500	34.4%	18,000	36.7%	3,000	22.2%
Non-local entrant, existing	8,500	13.6%	7,500	15.3%	1,000	7.4%
Total	62,500	100.0%	49,000	100.0%	13,500	100.0%
Industry transitions						
In same 3-digit NAICS	29,000	46.4%	24,000	49.0%	5,000	37.0%
In same 2-digit NAICS	34,500	55.2%	28,500	58.2%	6,000	44.4%

Table 2
Reallocation Determinants

This table shows results from a regression of a dummy for whether a plant is replaced within 5 years from bankruptcy filing (conditional on death of original plant) on a set of county and industry (2-digit NAICS) characteristics computed at the year of filing. All county-level and indusry-level controls are dummy variables equal to 1 if the county is above-median in the given category. Plant- and firm-level controls identical to those in Table 3 are also included, but are not reported for brevity. In addition, we include fixed effects for the filing year, as well as for the number of years after plant death in which we looked for a replacement (up to 5 years after filing). The sample includes all establishments that died within 5 years of filing. Standard errors, clustered at the division by year level, are shown in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	Plant reallocation dummy				
	(1)	(2)	(3)	(4)	(5)
Local Economic Conditions					
No. plants above median	0.029***			0.029***	0.028***
	(0.005)			(0.005)	(0.005)
3-year employment growth above median	0.019***			0.019***	0.018***
	(0.004)			(0.004)	(0.004)
Payroll per employee above median	0.031***			0.031***	0.029***
	(0.006)			(0.006)	(0.006)
Industry Economic Conditions					
No. plants above median		-0.012	-0.010	-0.010	0.025
		(0.008)	(0.011)	(0.011)	(0.021)
3-year employment growth above median		0.026***	0.008	0.008	-0.004
		(0.007)	(0.009)	(0.008)	(0.009)
Payroll per employee above median		0.006	0.013	0.012	-0.000
		(0.009)	(0.010)	(0.010)	(0.014)
Industry Fixed Effects					
(Omitted: Agriculture, Mining, and Construction)					
Manufacturing	0.037***		0.031**	0.033**	
	(0.013)		(0.015)	(0.015)	
Transportation, Utilities & Warehousing	0.005		0.001	-0.002	
	(0.018)		(0.021)	(0.020)	
Wholesale & Retail Trade	0.082***		0.091***	0.086***	
	(0.013)		(0.014)	(0.014)	
Finance	0.171***		0.173***	0.162***	
	(0.020)		(0.027)	(0.024)	
Other Services	0.092***		0.101***	0.090***	
	(0.014)		(0.015)	(0.014)	
Accomodation, Food & Entertainment	0.088***		0.094***	0.088***	
	(0.015)		(0.020)	(0.019)	
Healthcare and Education	0.121***		0.119***	0.118***	
	(0.017)		(0.019)	(0.019)	
Plant and firm controls	Yes	Yes	Yes	Yes	Yes
2-digit NAICS FE	No	No	No	No	Yes
Filing year FE	Yes	Yes	Yes	Yes	Yes
# of years searched FE	Yes	Yes	Yes	Yes	Yes
Observations	101,000	101,000	101,000	101,000	101,000
Adj. R-squared	0.096	0.087	0.092	0.096	0.098

Table 3
First Stage

This table reports first stage results. The dependent variable is a dummy equal to one if a case is converted from Chapter 11 reorganization to Chapter 7 liquidation. Column 1 reports results at the level of the bankruptcy filing, while Columns 2 and 3 report results at the level of the plant. In this and all other regression tables, each observation is weighted by the inverse of the total number of plants belonging to the bankruptcy filing so as to give equal weight to each bankruptcy filing. The instrument we use is defined as the share of all other Chapter 11 cases that a judge converted to Chapter 7. The sample includes all firms that filed for Chapter 11 bankruptcy between 1992 and 2005. *Part of a group filing* is an indicator variable equal to one if other related firms (e.g. subsidiaries of the same firm) also filed for bankruptcy at the same time. Other controls are self-explanatory. All specifications contain 24 industry fixed effects and 2,361 bankruptcy-division-by-year fixed effects. Standard errors, clustered at the division-by-year level, are shown in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	Converted to Liquidation		
	(1)	(2)	(3)
Share of other cases converted	0.581***	0.581***	0.580***
	(0.056)	(0.054)	(0.054)
Ln(employees at plant)			0.016***
			(0.003)
Plant age (years)			-0.005***
			(0.000)
Ln(tot. employees at firm)	-0.023***	-0.022***	-0.033***
	(0.003)	(0.002)	(0.004)
Ln(no. of plants at firm)	-0.038***	-0.039***	-0.022***
	(0.006)	(0.005)	(0.006)
Part of a group filing	-0.086***	-0.085***	-0.086***
	(0.011)	(0.011)	(0.011)
Unit of Observation	Bankruptcy	Plant	Plant
2-digit NAICS Fixed Effects	Yes	Yes	Yes
Division-year Fixed Effects	Yes	Yes	Yes
Observations	28,000	129,000	129,000
Adj. R-squared	0.102	0.165	0.170
F-stat for instrument	107.2	114.9	113.5

Table 4
Random Judge Assignment

This table reports randomization tests to illustrate the random assignment of judges to bankruptcy filings within a division. The dependent variable is the share of Chapter 11 cases that a judge ever converted to Chapter 7, which we use as an instrumental variable. All the regressions are at the plant level. Column 1 contains only division-by-year fixed effects as controls and is included to demonstrate that the R^2 is not affected by the inclusion of any controls in Columns 2 - 7. Heterogeneity measures are as defined in the text, and other independent variables are self-explanatory. The sample includes all firms that filed for Chapter 11 bankruptcy between 1992 and 2005. Standard errors, clustered at the division-by-year level, are shown in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:				Share converted			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Plant- and firm-level controls:							
Ln(employees at plant)		0.0002	0.0002	0.0002	0.0002	0.0002	0.0002
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Plant age (years)		-0.0000	-0.0000	-0.0000	-0.0000	-0.0000	-0.0000
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ln(tot. Employees at firm)		0.0009	0.0009	0.0009	0.0009	0.0009	0.0009
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Ln(no. Plants at firm)		-0.0012	-0.0012	-0.0012	-0.0012	-0.0012	-0.0012
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Part of a group filing		0.0014	0.0014	0.0014	0.0014	0.0014	0.0014
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Dummy =1 if above median:							
Heterogeneity measures:							
Market Thickness			0.0001			0.0001	-0.0000
			(0.001)			(0.001)	(0.001)
Share of small business loans				0.0007		0.0006	0.0007
				(0.001)		(0.001)	(0.001)
3-year employment growth in county					0.0017	0.0017	0.0016
					(0.001)	(0.001)	(0.001)
Other economic conditions:							
No. of plants in county							-0.0006
							(0.001)
Payroll per employee in county							0.0012
							(0.001)
No. of plants in industry							0.0061
							(0.004)
Payroll per employee in industry							0.0008
							(0.002)
3-year employment growth in industry							-0.0016
							(0.001)
2-digit NAICS fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Division-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-stat for joint significance of industry FE		0.791	0.798	0.796	0.791	0.801	0.826
Observations	129,000	129,000	129,000	129,000	129,000	129,000	129,000
Adj. R-squared	0.777	0.777	0.777	0.777	0.778	0.778	0.778

Table 5
Liquidation and Plant Outcomes

This table reports regression results showing the effect of liquidation on four plant outcomes. Panel A focus on these outcomes 5 years after the bankruptcy filing. *Continues* is an indicator equal to 1 if the plant has at least one employee and is still owned by the original bankrupt firm 5 years after the bankruptcy filing. *Occupied* is an indicator equal to 1 if the plant has at least one employee regardless of the occupant. *Average employment* and *average total wages* is the mean number of employees or total payroll at the plant over the five years after the bankruptcy filing. For all four dependent variables we display regular OLS and 2SLS estimates. Panel B contains similar regressions, but focuses on the role of reallocation by setting the measure of utilization (*occupied*, *avg. employment*, or *avg. wages*) to zero unless the plant has been reallocated. In Panel C, we show 2SLS estimates for *occupied* 1, 3, and 5 years after bankruptcy (similar results for the other measures of utilization are presented in the appendix). All specifications contain the full set of control variables in Column 3 of Table 3, including division-by-year and industry fixed effects. Standard errors, clustered at the division-by-year level, are shown in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Plant Utilization in Year Five

Dependent variable:	Continues		Occupied		Ln(Avg. Employment)		Ln(Avg. Total Wages)	
Model:	OLS	IV-2SLS	OLS	IV-2SLS	OLS	IV-2SLS	OLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Liquidation	-0.300***	-0.324***	-0.156***	-0.174**	-0.565***	-0.416*	-0.986***	-0.921**
	(0.005)	(0.061)	(0.007)	(0.079)	(0.019)	(0.217)	(0.032)	(0.368)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	129,000	129,000	129,000	129,000	129,000	129,000	129,000	129,000
Adjusted R-squared	0.230	0.152	0.130	0.039	0.295	0.214	0.314	0.231

Panel B: Plant Utilization Due to Reallocation Only

Dependent variable:	Occupied		Ln(Avg. Employment)		Ln(Avg. Total Wages)	
Model:	OLS	IV-2SLS	OLS	IV-2SLS	OLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation	0.142***	0.134*	0.324***	0.412*	0.813***	0.908**
	(0.006)	(0.080)	(0.019)	(0.215)	(0.038)	(0.441)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	129,000	129,000	129,000	129,000	129,000	129,000
Adjusted R-squared	0.133	0.042	0.149	0.055	0.156	0.058

Panel C: Dynamics of Plant Occupancy

Dependent variable:		Occupied	
Model:		IV-2SLS	
Years post filing:	+1	+3	+5
	(1)	(2)	(3)
Liquidation	-0.237***	-0.192**	-0.174**
	(0.075)	(0.078)	(0.079)
Control Variables	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes
Observations	129,000	129,000	129,000
Adjusted R-squared	0.067	0.063	0.039

Table 6
Heterogeneous Effects on Utilization

This table shows how the effects of liquidation vary depending on the local market, which we define as the county where an establishment is located. In each panel we divide the sample in half around the median level of a given measure of market conditions, and then present regression results similar to those in Table 5 separately for each sub-sample. In Panel A, we use our measure of market *thickness* (defined in the text) to divide the sample into plants in thick (above-median) or thin (below-median) markets. Panel B splits the sample by the share of loans in a county that go to small businesses, defined as firms with less than $1 million in annual revenue. Loan data are only available beginning in 1996, so for these regressions the full sample is limited to 99,000 plants. In Panel C, we divide the sample by the employment growth rate in the county over the 3 years prior to bankruptcy. Dependent variables are measured 5 years after bankruptcy and are defined identically as Panel A of Table 5. For brevity, we omit regressions with ln(*avg. total wages*) as the dependent variable; results for this measure show a similar pattern. All regressions are estimated by 2SLS and contain the full set of control variables in Column 3 of Table 3, including division-by-year and industry fixed effects. Standard errors, clustered at the division-by-year level, are shown in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Market Thickness

Dependent variable:	Continues		Occupied		Ln(Avg. Employment)	
Above or below median:	Above	Below	Above	Below	Above	Below
	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation	-0.337***	-0.321***	0.080	-0.324***	0.190	-0.790***
	(0.101)	(0.076)	(0.129)	(0.109)	(0.413)	(0.278)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	64,000	65,000	64,000	65,000	64,000	65,000

Panel B: Share of Small Business Loans

Dependent variable:	Continues		Occupied		Ln(Avg. Employment)	
Above or below median:	Above	Below	Above	Below	Above	Below
	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation	-0.273***	-0.341***	-0.018	-0.450**	-0.206	-1.197**
	(0.083)	(0.126)	(0.111)	(0.193)	(0.310)	(0.480)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	50,000	49,000	50,000	49,000	50,000	49,000

Panel C: 3-year Employment Growth

Dependent variable:	Continues		Occupied		Ln(Avg. Employment)	
Above or below median:	Above	Below	Above	Below	Above	Below
	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation	-0.412***	-0.262***	-0.126	-0.211**	-0.120	-0.644**
	(0.094)	(0.074)	(0.133)	(0.093)	(0.360)	(0.254)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	65,000	64,000	65,000	64,000	65,000	64,000

Table 7

Interaction of Market Conditions

This table shows how the interaction of market thickness with access to finance and economic growth creates sizable differences in the effect of liquidation on plant utilization. In Panel A, we split the sample into quartiles based on both market thickness and 3-year employment growth in the county. To do this, we first split the sample into thick and thin markets based on the median of county-level market *thickness* (defined in the text). Then, we take plants in thick and thin markets and divide by median employment growth to create quartiles. Plants in the 1st quartile are in thick markets with high economic growth, and plants in the 4th quartile are in thin markets with low growth. Panel B is constructed similarly by dividing first by market thickness and then by the share of loans that go to small businesses in each county. Plants in the 1st quartile are in thick markets with high access to finance, and plants in the 4th quartile are in thin markets with low access to finance. Small business loans from CRA data are available beginning in 1996, leaving 25,000 plants in each quartile. Each panel presents results only for the 1st and 4th quartiles. Dependent variables are measured 5 years after bankruptcy and are defined identically as Panel A of Table 5. All regressions are estimated by 2SLS and contain the full set of control variables in Column 3 of Table 3, including division-by-year and industry fixed effects. Standard errors, clustered at the division-by-year level, are shown in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Market Thickness Interacted with 3-year Employment Growth

Dependent variable:	Occupied		Ln(Avg. Employment)		Ln(Avg. Total Wages)	
Quartile:	1st	4th	1st	4th	1st	4th
	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation	0.103	-0.300**	0.954	-0.755***	1.803*	-1.298**
	(0.211)	(0.125)	(0.699)	(0.290)	(1.058)	(0.521)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,000	32,000	32,000	32,000	32,000	32,000

Panel B: Market Thickness Interacted with Share of Small Business Loans

Dependent variable:	Occupied		Ln(Avg. Employment)		Ln(Avg. Total Wages)	
Quartile:	1st	4th	1st	4th	1st	4th
	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation	-0.017	-0.680***	0.146	-1.567***	0.272	-2.580**
	(0.173)	(0.232)	(0.594)	(0.590)	(0.873)	(1.108)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25,000	25,000	25,000	25,000	25,000	25,000

Appendix

A. Matching Bankruptcy Filings to Census Data and Sample Selection

A first step in our analysis is to match bankruptcy filings data from LexisNexis to Longitudinal Business Database (LBD) maintained by the Census Bureau. In this appendix we describe this matching process.

The data from LexisNexis contains individual Chapter 11 bankruptcy filings obtained directly from the U.S. Court system. When a firm files for bankruptcy, each individual legal entity that is seeking bankruptcy protection must create its own individual bankruptcy filing. Thus, it is common for firms to have multiple associated filings, which are all assigned to the same bankruptcy judge and are typically jointly administered. Importantly, the LexisNexis data contains information on both the bankruptcy judge and whether the case remained in Chapter 11, was converted to Chapter 7, or was dismissed from court entirely. For the purposes of our analysis, we remove all dismissed cases (roughly one third of all filings) from the sample to focus only on firms that were treated with a bankruptcy regime. In total, our sample contains 67,810 unique bankruptcy filings.

We use the employer identification number (EIN), contained in both the bankruptcy filing and the LBD, to match the two datasets. Firm can have multiple EINs if they have separate subsidiaries for tax purposes. Further, multiple establishments in the BR can pertain to the same EIN. Thus, an EIN is an identifier somewhere between the level of the firm and the establishment. We use the set of all EINs associated with bankruptcy filings in the LexisNexis data and identify all plants in the LBD with the same EIN that were active in the year of the bankruptcy filing. This is the initial set of plants in our sample. In total, we match about 45,000 bankruptcy filings to over 141,000 unique establishments in the LBD, with a match rate of 65%.

Since the LBD covers the entire non-farm private sector of the U.S., it may seem odd that our match rate is not higher. One reason for this is that only businesses that have at least one employee are included in the LBD, while every unique legal enitty of a firm (each with a separate EIN) must create a separate bankruptcy filing. Thus, in the list of bankruptcy cases, we appear to have a substantial number of EINs that have no associated employees and are thus not in the LBD. Since there is no economic activity associated with these EINs, omitting them from our sample should not bias our estimates. Our match rate of 65% is similar to that in other studies that have used the LBD, such as Davis et al. (2014). Further, to ensure that the matching process is as comprehensive as possible, we also attempt to use the business names in the bankruptcy filings to match to the BR. However, this approach did not improve the match rate relative to the EIN matching, and therefore we focus exclusively on the latter.

From the initial set of 141,000 matched plants, we remove plants that only have P.O. box addresses or have missing addresses altogether, since we need a complete address to link establishments over time. Further, since occasionally firms may use accounting firms to report their information, we remove all accounting firms and any plant whose address matches that of an accounting firm from the sample. These restrictions leave us with a final sample of 129,000 plants, belonging to 28,000 unique firms.[31]

B. Address Matching Algorithm

A principal goal in this paper is to track the economic activity at specific locations over time and across ownership changes. While the LBD links establishments over time, when an establishment changes ownership it is typically recorded as a "death" and a "birth," so that the economic activity at the location is not linked between the old and the new plant. In this appendix, we describe in detail the algorithm used to link geographic locations over time using the LBD.

[31]In a small number of cases, firms with multiple EINs only partially matched to the LexisNexis data. This can happen if, for example, one subsidiary of a firm files for bankruptcy while other subsidiaries do not. In these cases, we only include establishments belonging to the bankrupt EIN in our sample, since it is unclear how the other establishments belonging to the firm are affected by the bankruptcy.

A. *Address Cleaning and Sample Selection*

Prior to matching any addresses, we first define the sample of plants we are interesting in linking and clean their addresses. We begin with an initial set of 141,000 bankrupt establishments that matched to the set of Chapter 11 bankruptcy filings. From this group, we set aside those that survive (or are sold but continue to be linked) for at least five years after their bankruptcy filing, as there is no need to attempt to match these plants. This leaves us with just over 100,000 total establishments that we will attempt to match to future establishment births. For ease in exposition, we will refer to this dataset as the "DBP," for "dead bankrupt plants."

We next collect addresses from the Business Register (BR) for the entire LBD from 1992 - 2010. The BR contains both a physical address and a mailing address for each plant. The matching algorithm uses the physical address whenever possible, as this reflects the actual geographic location of the plant, but also attempts to match using the mailing address in cases where the physical address is not provided, on the assumption that in such cases the mailing address is likely the same as the physical address.

For each LBD plant we also bring in addresses reported in the Economic Censuses, which occur in 1992, 1997, 2002, and 2007 during our sample period. During these years, the Census Bureau itself collects detailed information on each establishment, rather than relying on tax data.[32] Thus, we would expect addresses reported in these years to be the most accurate. For each plant in the LBD in a given year, we merge addresses from the census before and after, and attempt to merge using those addresses as well. Hence, for a given plant there can be up to six different addresses:

1. Physical address
2. Mailing address
3. Physical address from prior census
4. Mailing address from prior census

[32]In non-census years, the LBD is based on information obtained from IRS tax records, rather than information collected directly by the Census Bureau.

5. Physical address from next census

6. Mailing address from next census

However, it is extremely rare for a plant to actually have six different addresses associated with it. In the vast majority of cases the physical and mailing addresses are the same, as are those from census years. Further, many plants do not survive across two censuses, and hence they will not have addresses from both the prior and next censuses.

Before matching, we use a combination of address cleaning algorithms from the NBER Patent Project, Wasi and Flaaen (2014)[33], and our own code to prepare the addresses for matching. In this process, we carefully abbreviate all common words and separate street numbers and unit numbers from the name of the street using the United States Postal Service (USPS) formal algorithm. For example, an address of "123 South Main Street Suite 444" would be separated into three pieces: the street number "123," the street name "S MAIN ST," and the unit number "444." We also clean city names and abbreviate all states to standard USPS abbreviations, although this matters little as the zip code is a better identifier for matching because it is nested within cities (usually) and states (always).

One potential problem with matching by address is that firms may use accountants to report their taxes to the IRS.If accountants report the accounting firm's address on the form, then we will have the incorrect address for a given firm. To account for this, we identify all addresses associated with accounting firms in each year in the LBD, and erase all addresses of all LBD plants in that year that are in this set. In addition, we remove all accounting firms from the DBP so that they play no part in our analysis.

In addition to dropping accounting firms, we also remove all plants that do not have a street address or that only report a PO Box address. These plants cannot be matched by address and we therefore exclude them from all analysis. Combined, the set of plants belonging to accounting firms or that have incomplete addresses comprise 11,000 plants, resulting in a final set of 89,000 DBP plants to match.

To speed up the computational process, we create from the final DBP the unique set of all street numbers and zip codes that are in our sample, and limit the LBD to only

[33]Wasi, Nada and Aaron Flaaen (2014), "Record Linkage using STATA: Pre-processing, Linking and Reviewing Utilities," Working paper, University of Michigan.

establishments with relevant street numbers in relevant zip codes. For example, suppose there are two dead bankrupt plats in zip code 45678: one has a street number of 123 and the other has a street number of 55. We then exclude all plants in the LBD in zip code "45678" that do NOT have a street number of 123 or 55 from our set of potential matches. This dramatically reduces the set of potential matches and speeds up the matching process considerably, while reducing the number of incorrectly matched plants. To make the explanation of the algorithm easier, we will refer to this set of Potential Matches in the LBD as the "LBD-PM."

B. *Identifying Non-Unique Locations*

Another major issue in linking geographic locations is dealing with non-unique addresses. Non-unique addresses occur anytime multiple plants are co-located, such as when a single firm has several establishments that share an address, or in office buildings or shopping centers with multiple businesses located in the same building. While in some of these cases we could in principle identify individual establishments by their unit number, in practice the reporting of unit or suite numbers is not always consistent over time, especially across ownership changes. Further, office numbers can be easily changed and offices can be combined or split as locations are repurposed to new uses.

For these reasons, we ignore unit/office/suite numbers in our matching process completely. Instead, we first identify non-unique plant locations, and then take account of this information when we allocate employment and wages at the plant, as described below. In this section we describe the process for identifying these non-unique locations.

First, for each plant in DBP, we identify a single address that we will use to track economic activity at that location. We do this according to the following hierarchy:[34]

1. Use the physical address in the year of death (available for approx. 90% of plants)

2. Use the physical address from the census prior to death if physical address at death is not available (used for approx. 2% of plants)

3. Use the mailing address in the year of death if no physical address is available (used for approx. 7% of plants)

[34]Note that, because plants in the DBP die, none of them have addresses available in the next census.

4. Use the mailing address from the census prior to death if no other address is available (used for approx. 1% of plants)

This selected address is the key unique address at which we wish to follow economic activity for five years after the bankruptcy filing.

We next take all of the addresses of plants that die in year t, and match them to the LBD-PM in year *t-1*, the year before death. To link the addresses in this and future matches, we use the Stata module *reclink2*, developed by Wasi and Flaaen (2014). *reclink2* allows for fuzzy matching, and further allows us to place different weights on the importance of different components of the address. In our matching, we require both the zip code and the street number to match exactly, but allow the street name and city name to differ slightly.[35] As stated previously, we do not match on unit and suite numbers at all in this process, as the goal is to identify all plants associated with a given address in the year before death.[36]

While this matching process allows for street names to differ slightly (e.g. "S MAIN ST" will match to "S MIAN ST"), we take care to remove matches where streets are numbered and the street numbers do not match exactly. For example, we do not wish to match a plant located at 123 14th ST to one located at 123 15th ST, even though these addresses differ by only a single character.

We match the DBP addresses to both physical addresses in the LBD-PM first, and then to mailing addresses of LBD-PM plants that do not have a physical address. As before, the vast majority of plants have a physical address, and we only use the mailing address where necessary. This matching process identifies all establishments associated with a specific address in the LBD in the year prior to the bankrupt establishment's death.

With this set of matches in hand, we count the total number of active plants at each DBP address in the year prior to death. Addresses with only a single match (the dead bankrupt plant itself), are unique locations where there was a single active establishment prior to bankruptcy. Meanwhile, addresses that have multiple establishments are deemed "non-unique," and care must be taken to allocate future employment at these locations.

[35] It is difficult to tell which number is a house number and which is a street number when the raw reported address begins with two numbers, such as "123 444 MAIN ST." This issue affects 2.1% of the plants in our sample. By convention, after manually checking several of these cases, we take a leading number in the address as the house number.

[36] Readers may contact the authors for the exact reclink2 weights used for the match.

To aid in calculating employment and payroll allocated to a bankrupt plant after a plant's death, we also calculate the "number of vacancies" at each address in each year after the bankruptcy filing. This is defined as the number of establishments that have died in that location between the bankruptcy filing and given year, and annotated $v_{p,t}$, where p indexes plants and t indexes years. For unique locations, the number of vacancies will be zero before the bankrupt plant's death, and 1 after it dies. However, for non-unique locations the number of vacancies depends on the death dates of non-bankrupt plants as well. For example, suppose there are 5 plants active in a location in 1998, one of which goes bankrupt and dies in 1999. If the other 4 plants are still alive in 1999, then $v_{p,1999}$=1. If 2 more plants die in 2000, then $v_{p,2000}$=3. If the other 2 plants survive past 2003 (5 years after the bankruptcy filing), then $v_{p,2000} = v_{p,2001} = v_{p,2002} = v_{p,2003} = 3$. We use this number of vacancies to divide employment at newly born plants at the address of plant p across the number of vacant units at the location, as described in Section B.E below.

C. *Address Matching After Bankruptcy*

We next take the plants in DBP and match them to LBD-PM plants that are born subsequent to their death. We do this by looping over all years from 1992 to 2010 and searching the LBD-PM in each year for plants that are born that match addresses of dead plants in the DBP. Specifically, in year t of the loop the algorithm follows the following process:

1. Identify all plants in the DBP that died in or prior to year t, but whose bankruptcy filing date was after year $t - 5$ (since we only follow plants for 5 years after their bankruptcy filing). This is the set of plants we will attempt to match in this year of the loop.

2. Open the LBD-PM for year t, which contains all plants that were active in year t and that have an address that matches a house number-zip code combination of the DBP. Remove from this list all plants that were born in invalid years. Specifically, the birth year must be:

    (a) After the census before the minimum filing year of the set of DBP plants identified in step 1 AND

   (b) Before the census after the maximum filing year of the set of DBP identified in step 1.[37]

3. Match the DBP plants from step 1 with the LBD-PM plants identified in step 2 using *reclink2*, as described above.
4. Filter out bad matches by eliminating matches where:
   (a) A DBP plant matched to itself
   (b) The LBD-PM plant was born before the death of the DBP plant, and hence could not have replaced the DBP plant.
   (c) The address match was incorrect due to numbered streets matching, as described above.
5. Repeat steps 3 and 4 for each of the following addresses in the LBD-PM:[38]
   (a) Physical address
   (b) Mailing address
   (c) Physical address from prior census
   (d) Mailing address from prior census
   (e) Physical address from next census
   (f) Mailing address from next census
6. Save the full set of matches.

We repeat this process for each year in our sample period, leaving us with a set of all new births at the same addresses of dead bankrupt plants. In section B.E below we describe how we aggregate cases with multiple new births. First, we note two important aspects of the matching algorithm.

[37]We focus on births between census years rather than filing years to account for inexact birth and death years, as described later in this appendix.

[38]Recall that for each DBP we only use a single address.

Between censuses, the LBD obtains information on plant births and deaths (and employment and payrolls) through IRS tax records as well as surveys conducted by the U.S. Census Bureau. Importantly, the Census Bureau surveys cover all firms with more than 250 employees, and so information on plant births and deaths belonging to these firms is accurate in all years. Further, exact birth and death years of plants belonging to single-establishment firms are known simply by when the firm enters or exits the IRS tax data. However, birth and death years for plants belonging to multi-establishment firms with less than 250 employees cannot be known exactly, since taxes are only reported at the firm level and information on plants is only obtained every 5 years via census. The birth and/or death years for these plants is not known exactly, although it is known that it occurred between two given census years. For example, a small firm may have 2 establishments in the 1997 census and then it has 3 plants in 2002. We then know then the 3rd plant was born between 1997 and 2002, but we do not know the exact year. A similar situation can arise with death years. When this occurs, we allow plants to match as long as it is possible that the birth could have been after the death of the bankrupt plant. This affects less than 2% of our matches and does not appear to bias our estimates in any way.

The second aspect of the linkage algorithm that is important to point out is that once a bankrupt plant has matched to a newly opened establishment we do not remove the bankrupt plant from the set of addresses we wish to match. For example, suppose that Plant A, located at 123 Main St., goes bankrupt and dies in year t, and that we subsequently find that Plant B was born at 123 Main St. in year $t+2$. Even though we have already found a match, we continue to search for plants that open at 123 Main St. in years $t+3$, $t+4$, and $t+5$. We continue to match in this fashion to account for the fact that there can be multiple establishments at the same address, even if the original plant was uniquely located. That is, even if Plant A was the only establishment located at 123 Main St. in year t, it is possible for Plant B and Plant C to share that space later on, in which case we should allocate both the employment of Plant B and that of Plant C to 123 Main St. Further, if Plant A was not uniquely located (e.g. if 123 Main St. was shopping mall), we cannot be sure that Plant B filled Plant A's spot, and therefore we wish to find all possible matches for this location even after Plant B has been identified as a possible match.

D. Verifying match quality

Because a high percentage of the plants in our sample close after filing for bankruptcy, it is vital that the linking algorithm be accurate in finding new economic activity occurring at each address. In particular, if the algorithm is too strict, we will miss some matches that should be made, thereby biasing downwards the estimates of economic activity at closed plants – which disproportionately come from cases that were converted to Chapter 7.

To address these concerns, we took the full sample of plants that closed but did not match to a new plant within 5 years of the bankruptcy filing (34,000 plants), and matched them to the LBD-PM 5 years after their bankruptcy filing again, but this time merging on only zip code and street number (not street name, city, or state). This allows for complete flexibility in street names, which are the item that tends to vary the most across addresses. In this matching process, we find that 86% of these plants do not match to any plant in the LBD-PM. That is, there was no plant in the entire LBD that was born after the original establishment closed that had the same zip code and street number for 86% of the cases. Further, we then took a random subsample of 500 of the cases which did have a match on street number and zip code (out of about 5,000 total, so this is a 10% subsample), and manually checked if the street names were similar but did not match using the fuzzy matching algorithm outlined above. We find that only 22% (112 of the 500) were potentially on the same street.[39] Assuming our subsample is representative, this would mean that only 22% of the 14% of firms that did have a match were actually good matches that were missed by our algorithm. Multiplying these percentages together (22%*14%=3%), we estimate that 97% of the plants that were not matched have no possible match in the LBD. We thus feel confident that we are not missing many matches that should be made.

The flip side of this problem is also important: we must be sure that we are not incorrectly matching plants that were not at the same address. The reclink2 algorithm generates a match score, scaled from 0-1, that measures how closely the addresses match. By default, reclink2 uses a threshold of 0.6 as the minimum score for a match, but we opt for a stricter 0.9.

[39] We tried to be as generous as possible in determining whether two plants are a good match. For example, a match of a street name of "Herald Court Mall" to "Herald" or "Mall" would be counted as a match, even though there are potentially other streets in the same zip code with the word "Herald" or "Mall."

In our data, 95% of all matches have a score higher than 0.987, with 58% being perfectly matched. The 1st percentile of our match scores is 0.909. Even among this set with lower match scores, we manually verify that the majority are correctly paired.

A final potential problem is that zip codes may be altered over time, thereby preventing us from making a match because we require zip codes to match exactly. The United States Postal Service lists zip code changes in their Postal Bulletins, available online at www.about.usps.com. From 2013-2015, on average only 8 zip codes were altered per year, out of a total of over 43,000 zip codes. Based on this, it does not appear that zip code changes will affect a large number of our addresses.

E. *Consolidating matches*

At the end of the matching process described above, we potentially have multiple matches for each dead bankrupt plant. This is by design, as it allows us to account for the fact that multiple establishments may be located at the same address. The end goal of this process is to estimate the economic activity (in terms of total employment and total payroll) occurring at a location over time. This section describes how we consolidate employment and payroll at all matched plants to get this measure.

A key component of this calculation is the number of vacant units at a given address in year t, denoted $v_{p,t}$ and described in Section B.B above. Using this variable, we calculate total employment for a location pertaining to a bankrupt plant p in year t as

$$TotalEmp_{p,t} = \sum_{j} \frac{emp_{j,p,t}}{v_{p,k}}$$

where j indexes newly born plants that matched to dead bankrupt plant p in year k, with $k \leq t$. In words, this formula allocates an equal share of employment at newly born establishments across all vacancies in that location. For plants that are uniquely located, $v_{p,k} = 1$ and thus we simply sum employment across any new plant born at the location. Similarly, if a plant is not uniquely located but no other establishments at the same address die within five years of the bankruptcy, $v_{p,k} = 1$ for all k. However, if other plants besides the bankrupt plant close in the same location, we allocate an even portion of employment to each vacancy at the location. For example, if 3 establishments (one of which was bankrupt) have closed in a given location when a single new plant is born in the location, we allocate 1/3rd of the employment of the new plant to the bankrupt plant. Note, however, that if in the next year $v_{p,t}$ increases to 4, we continue to allocate 1/3rd of employment to the bankrupt plant, since the new plant could not have taken the spot of this new vacancy. We allocate payroll using exactly the same method.

We allocate employment and wages in this way because when a new plant is born and there are multiple vacancies at its location we cannot determine if the new plant is using the location vacated by the bankrupt plant or that of one of the other co-located plants. There are two main underlying assumptions to the formula. First, that when there are multiple vacancies in a location there is an equal probability that a new plant will occupy any of the vacant units. Hence, when there are 3 vacancies we allocate 1/3rd of the employment to the bankrupt plant on the assumption that there is a one in three chance that the new plant filled the bankrupt establishment's slot.

The second assumption is that $v_{p,k}$ captures all vacancies at an address. Recall that we measure $v_{p,k}$ based on plants appearing in the LBD in the year prior to a bankrupt plant's death. If there are no vacant units at a location prior to the bankrupt plant's death, then $v_{p,k}$ should accurately reflect the total number of plants that have closed at that location in a given year. However, it is likely some locations had vacancies in the year before the death of the bankrupt plant; these vacancies go undetected in our algorithm, and hence $v_{p,k}$ is too low for these cases. This will tend to bias $TotalEmp_{p,t}$ upwards. However, this will only bias our regression estimates if $TotalEmp_{p,t}$ is biased upwards specifically for Ch. 11 or Ch. 7 cases, which seems unlikely. To confirm this, we construct an alternative measure as a simple average of employment across all matches:

$$TotalEmpAlt_{p,t} = \frac{\sum_j emp_{j,p,t}}{n_{p,t}}$$

where $n_{p,t}$ is the total number of new plants that have matched to bankrupt plant p in year t. This alternative formula biases $TotalEmpAlt_{p,t}$ downwards by implicitly assuming that only one plant can fill each vacancy. Results using this alternative specification are essentially identical to our main specification, and so we conclude that the potential bias in $TotalEmp_{p,t}$ does not affect our conclusions.

C. Robustness and Additional Results

This section describes a set of additional tables mainly aimed at testing the robustness of our results to alternative measures and different samples. Further results are also discussed.

In Table A.1, we report first and second stage results using alternative instruments. One might be concerned that a judge's preferences change over time and therefore using the share of all cases converted might not accurately represent his current views. Accordingly, Panel A shows that our first stage results are robust to using the share of cases converted in the previous 5 years as the instrument. Further, column 2 of this panel shows that a comprehensive set of judge fixed effects are highly significant, and that including these fixed effects does not appreciably change the coefficient estimates of other control variables. In Panel B, we report the 2SLS results using the share of cases converted in the previous 5 years as the instrument, focusing on the four main dependent variables discussed in the main results. Reassuringly, the results are nearly identical in sign and magnitude, with liquidated plants being associated with lower continuation rates, higher vacancies, and lower utilization of real estate assets compared to plants in reorganization.[40]

Table A.2 shows reduced form results, where we regress our main dependent variables of interest directly on the preferred instrument, namely the share of all other Chapter 11 cases that a judge converted to Chapter 7. Consistent with our story, and similarly to what we show in Table 5, Panel A, we find that a higher *share of other cases converted* is associated with lower asset utilization, both on the extensive (Column 1 and 2) and intensive (Column 3 and 4) margin.

While we cannot test the exclusion restriction directly, indirect tests support the identifying assumption, as also discussed in the main text. We report the results of these tests in Table A.3. We run a set of reduced-form regressions which directly relate our preferred

[40]We observe similar results when using the set of judge fixed effects as instrumental variable; for brevity, these results are omitted.

instrument, *share of other cases converted*, to plant outcomes. In particular, we do so by limiting the samples to either firms that stay in Chapter 11, or to firms that are converted to Chapter 7. Since we find a strong relationship between the instrument and the plant outcomes on the full sample, we should expect to find similar results separately on the Chapter 11 and Chapter 7 sub-samples *if* judge attributes are such that the exclusion restriction condition is violated (i.e. if our instrument affects plant outcomes in other ways that are different from the conversion of a case to Chapter 7). Reassuringly, this is not the case, as it is clear from the statistically insignificant coefficients of Columns 1-6. Further, we also find that within Chapter 11 reorganization, the instrument is uncorrelated with bankruptcy refiling rates, a proxy for bankruptcy resolution success.

Table A.4 illustrates how utilization is affected by liquidation over time. Panel A shows 2SLS estimates of the effect of liquidation on utilization 1, 3, and 5 years after the bankruptcy filing. These regressions are similar to those reported in Panel C of Table 5. In Panel B, we present alternative measures of the effect on employment and wages. In Panel A (and in the main text), we measure employment and wages as the average over the full post-bankruptcy period. In these regressions, we instead use the log of employment or wages in years 1, 3, or 5, ignoring any effect of liquidation on prior years. Overall, these results display how the gap in utilization between liquidated and reorganized plants slowly closes over time.

Given the inherent imprecision of address matching, in Table A.5 we report our main results when limiting the sample to plants for which we are more confident of the address match and, hence, utilization measurement. The goal is to show that the results are not affected by the co-location of establishments. Panel A limits the sample only to establishments with unique addresses in the year prior to the bankruptcy. In Panel B, we remove from the sample any plant that matched to multiple new establishments after closing. Panel C removes locations that are likely to be shopping centers or office buildings by dropping all locations that have >5 establishments. All the results are essentially unchanged and, if anything, larger in magnitude.

Our main analysis emphasizes the role played by local market characteristics in determining the impact of different bankruptcy regimes on plant outcomes. In order to test the robustness of our heterogeneity results, we therefore construct and adopt a set of alternative measures as well. As an alternative measure to market thickness, we rely on the Core Logic dataset to create *real estate transactions per capita*. This measure is computed as the total number of real estate transactions reported in CoreLogic in a county in the year of the bankruptcy filing, scaled by the total population of the county, and is meant to capture the number of potential buyers of real estate assets. We then move to our main measure of access to finance, namely *share of small business loans*, and complement it with two additional measures. The first is the value-weighted version of *share of small business loans*. The second, *small bank market share*, is computed using the FDIC's Summary of Deposits data and is defined as the share of bank deposits in a county held by commercial banks below the 95th percentile in the overall bank size distribution in the bankruptcy filing year. This variable stems from the idea that small, local banks are the principle providers of capital for small firms (Petersen and Rajan (1994)). Banks below the 95th percentile hold about 25% of all deposits and constitute about 40% of all branches in the U.S. during our sample period. Further, small banks tend to have deposits concentrated in local markets. Large banks have branches in over 15 different counties on average, while small banks are present in only 1.7 counties. Finally, on top of our main measure of the growth rate of employment *in the entire county* over 3 years prior to the bankruptcy filing, we also look at the growth rate of employment *in the county in the same 2-digit NAICS* as the bankrupt firm over 3 years prior to bankruptcy. Overall, we therefore work with 7 measures of market conditions. Table A.6 reports the pairwise correlation matrix of these 7 measures. As expected, there is positive correlation within categories (e.g. measures of access to finance are correlated with each other). However, most of the correlations tend to be low, displaying a substantial variation that each measure is independently responsible for when looking at local market conditions. To conclude, in Table A.7 we report results identical to Table 6 but using the four alterntaive measures of market charactertistics to split the sample. The results show a similar pattern to what we find using the main heterogeneity measures, thus providing further convincing evidence for the results discussed in the main text.

Table A.1
Alternative Instruments

This table reports results using alternative instruments. Panel A shows first stage regression results identical to Column 3 of Table 3. In the first column, we use the share of cases assigned to the judge in the past 5 years that have been converted to Chapter 7 as the instrument. In the second column, we include a comprehensive set of 559 individual judge fixed effects. In Panel B, we present the main 2SLS results using the share of cases converted in the past 5 years as the instrument, rather than the share of all cases. Dependent variables are defined identically to Panel A of Table 5, and included control variables are identical as well. Standard errors, clustered at the division-by-year level, are shown in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: First Stage with Alternative Instruments

Dependent variable:	Converted to Chapter 7	
	(1)	(2)
Share of cases in past 5 years converted	0.304***	–
	(0.028)	
Judge fixed effects	–	Yes***
Ln(employees at plant)	0.016***	0.009***
	(0.003)	(0.003)
Plant age (years)	-0.005***	-0.002***
	(0.000)	(0.000)
Ln(tot. employees at firm)	-0.033***	-0.037***
	(0.004)	(0.007)
Ln(no. of plants at firm)	-0.022***	-0.011
	(0.006)	(0.011)
Part of a group filing	-0.087***	-0.061*
	(0.011)	(0.037)
2-digit NAICS Fixed Effects	Yes	Yes
Division-year Fixed Effects	Yes	Yes
Observations	129,000	129,000
Adj. R-squared	0.172	0.465
F-stat for instrument	116.6	

Panel B: Second Stage using Share of Past Cases as Instrument

Dependent variable:	Continues	Occupied	Ln(Avg. Employment)	Ln(Avg. Total Wages)
	(1)	(2)	(3)	(4)
Liquidation	-0.368***	-0.135*	-0.555***	-0.819**
	(0.051)	(0.071)	(0.212)	(0.369)
Control Variables	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes
Observations	129,000	129,000	129,000	129,000
Adjusted R-squared	0.146	0.039	0.217	0.231

Table A.2
Reduced Form Regressions

This table reports reduced-form regressions in which the instrument, *share converted*, is entered directly as an independent variable, rather than the 2SLS procedure used in the main text. Dependent variables and control variables are identical to those in Panel A of Table 5. Standard errors, clustered at the division-by-year level, are shown in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	Continues (1)	Occupied (2)	Ln(Avg. Employment) (3)	Ln(Avg. Total Wages) (4)
Share converted	-0.188***	-0.101**	-0.241*	-0.544**
	(0.039)	(0.046)	(0.130)	(0.227)
Control Variables	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes
Observations	129,000	129,000	129,000	129,000
Adjusted R-squared	0.114	0.110	0.264	0.284

Table A.3
Exclusion Restriction Tests

This table reports tests of the exclusion restriction condition. Reduced-form regression results are presented where the instrument, *share converted*, is entered directly as an independent variable. We run these regressions separately on the sub-sample of firms that remain in Chapter 11 reorganization and on the sub-sample that is converted to Chapter 7 liquidation. Dependent variables and control variables are identical to those in Panel A of Table 5, excluding $Ln.Avg.TotalWages$ for brevity (for results are similar). In Column 7, we also show that the instrument is unrelated to the propensity for reorganized firms to re-file for bankruptcy. Standard errors, clustered at the division-by-year level, are shown in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	Continues		Occupied		Ln(Avg. Employment)		Re-filing
Sample:	Reorganized (1)	Liquidated (2)	Reorganized (3)	Liquidated (4)	Reorganized (5)	Liquidated (6)	Reorganized (7)
Share converted	-0.050	-0.016	-0.063	-0.001	-0.113	0.296	-0.001
	(0.062)	(0.019)	(0.061)	(0.082)	(0.168)	(0.214)	(0.042)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	105,000	24,000	105,000	24,000	105,000	24,000	105,000
Adjusted R-squared	0.186	0.190	0.151	0.208	0.373	0.259	0.1509

Table A.4
Dynamics of Utilization

This table shows how utilization is affected by liquidation over time. Panel A shows 2SLS estimates of the effect of liquidation on utilization 1, 3, and 5 years after the bankruptcy filing. These regressions are similar to those reported in Panel C of Table 5, and dependent variables are defined as in Panel A of that table. In Panel B, we present alternative measures of the effect on employment and wages. In Panel A (and in the main text), we measure employment and wages as the average over the full post-bankruptcy period. In these regressions, we instead use the log of employment or wages in years 1, 3, or 5, ignoring any effect of liquidation on prior years. Thus, year 1 in Panel B is identical to year 1 in Panel A, but years 3 and 5 differ because in Panel B the dependent variable is employment or wages in that year only. Control variables are identical to those in Panel A of Table 5. Standard errors, clustered at the division-by-year level, are shown in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Dynamics of Plant Utilization

Dependent variable:		Continue			Ln(Avg. Employment)			Ln(Avg. Total Wages)	
Years post filing:	+1	+3	+5	+1	+3	+5	+1	+3	+5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Liquidation	-0.271***	-0.368***	-0.324***	-0.479**	-0.419*	-0.416*	-1.448***	-1.031***	-0.921**
	(0.086)	(0.074)	(0.061)	(0.236)	(0.222)	(0.217)	(0.395)	(0.365)	(0.368)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	129,000	129,000	129,000	129,000	129,000	129,000	129,000	129,000	129,000

Panel B: Point Estimates of Effect on Employment and Wages

Dependent variable:		Ln(Employment)			Ln(Total Wages)	
Years post filing:	+1	+3	+5	+1	+3	+5
	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation	-0.479**	-0.499*	-0.314	-1.448***	-1.160**	-0.885*
	(0.236)	(0.264)	(0.261)	(0.394)	(0.467)	(0.478)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	129,000	129,000	129,000	129,000	129,000	129,000

Table A.5
Robustness of Results to Matching Algorithm

This table repeats the main analysis from Panel A of Table 5 on three sub-samples of plants to demonstrate that the results are not affected by the co-location of establishments. Panel A limits the sample only to establishments with unique addresses in the year prior to the bankruptcy. In Panel B, we remove from the sample any plant that matched to multiple new establishments after closing. Panel C removes locations that are likely to be shopping centers or office buildings by dropping all locations that have >5 establishments. Dependent variables and controls are identical to those in Panel A of Table 5. Standard errors, clustered at the division-by-year level, are shown in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Only Single-unit Locations

Dependent variable:	Occupied (1)	Ln(Avg. Employment) (2)	Ln(Avg. Total Wages) (3)
Liquidation	-0.309*** (0.112)	-1.045*** (0.282)	-2.044*** (0.524)
Control Variables	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes
Observations	68,000	68,000	68,000

Panel B: No Multiple-matched Locations

Dependent variable:	Occupied (1)	Ln(Avg. Employment) (2)	Ln(Avg. Total Wages) (3)
Liquidation	-0.373*** (0.093)	-1.062*** (0.237)	-2.059*** (0.427)
Control Variables	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes
Observations	97,000	97,000	97,000

Panel C: No Shopping Centers or Office Buildings

Dependent variable:	Occupied (1)	Ln(Avg. Employment) (2)	Ln(Avg. Total Wages) (3)
Liquidation	-0.282*** (0.089)	-0.770*** (0.210)	-1.415*** (0.383)
Control Variables	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes
Observations	107,000	107,000	107,000

Table A.6
Heterogeneity Measure Correlation Matrix

This table reports pairwise correlations between 7 measures of market conditions used to test for heterogeneity in the main results. There are 2 measures of the number of potential buyers in the county. *Market thickness* is a measure of the market share of firms in the same or similar industries in the county, and is defined in the text. *Real estate transactions per capita* is the total number of real estate transactions reported in CoreLogic in a county in the year of the bankruptcy filing, scaled by the total population of the county. There are 3 measures of access to finance. *Share of small business loans* is the percentage of loans in the county that are given to small businesses. We present this metric both on a number- and value-weighted basis. *Small bank market share* is the share of bank deposits in a county held by commercial banks below the 95th percentile in the overall size distribution in the bankruptcy filing year. Finally, there are 2 measures of 3-year employment growth. Our main measure is the growth rate of employment in the entire county over 3 years prior to the bankruptcy filing. The second is the growth rate of employment in the county in the same 2-digit NAICS as the bankrupt firm over 3 years prior to bankruptcy. Correlations are measured over the full sample of 129,000 plants, except for measures (3) and (4) for which data is available only beginning in 1996.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Number of potential buyers							
(1) Market thickness	1.00						
(2) Real estate transactions per capita	0.07	1.00					
Access to finance							
(3) Share of small business loans (number-weighted)	0.08	-0.11	1.00				
(4) Share of small business loans (value-weighted)	0.10	-0.03	0.53	1.00			
(5) Small bank market share	0.09	-0.13	0.27	0.47	1.00		
Employment growth							
(6) 3-year employment growth rate for county	0.04	0.14	0.11	0.00	0.03	1.00	
(7) 3-year employment growth rate for industry-county	0.01	0.02	0.02	0.02	0.02	0.09	1.00

Table A.7

Alternative Heterogeneity Measures

This table reports results identical to Table 6 but using alternative measures of market charactertistics to split the sample. Panel A uses *real estate transactions per capita*, defined as the number of real estate transactions reported in CoreLogic in a county in the year of the bankruptcy filing, scaled by population. Panel B uses the value-weighted (instead of number-weighted) share of small business loans in the county. In Panel C, the sample is split by s*mall bank market share,* defined as the share of bank deposits in a county held by commercial banks below the 95th percentile in the overall size distribution in the bankruptcy filing year. Panel D splits the sample by the 3-year growth of employment in the county in the same 2-digit NAICS as the bankrupt firm over 3 years prior to bankruptcy. Dependent variables are measured 5 years after bankruptcy and are defined identically as Panel A of Table 5. For brevity, we omit regressions with ln(*avg. total wages*) as the dependent variable; results for this measure show a similar pattern. All regressions are estimated by 2SLS and contain the full set of control variables in Column 3 of Table 3, including division-by-year and industry fixed effects. Standard errors, clustered by division-year company, are shown in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Real Estate Transactions Per Capita

Dependent variable:	Continues		Occupied		Ln(Avg. Employment)	
Above or below median:	Above	Below	Above	Below	Above	Below
	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation	-0.390***	-0.276***	-0.110	-0.196**	-0.080	-0.538**
	(0.112)	(0.069)	(0.150)	(0.090)	(0.428)	(0.222)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	64,000	65,000	64,000	65,000	64,000	65,000

Panel B: Value-weighted Share of Small Business Loans

Dependent variable:	Continues		Occupied		Ln(Avg. Employment)	
Above or below median:	Above	Below	Above	Below	Above	Below
	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation	-0.317**	-0.300***	0.002	-0.261**	-0.462	-0.718**
	(0.152)	(0.080)	(0.192)	(0.111)	(0.474)	(0.291)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	50,000	49,000	50,000	49,000	50,000	49,000

Panel C: Market Share of Small Banks

Dependent variable:	Continues		Occupied		Ln(Avg. Employment)	
Above or below median:	Above	Below	Above	Below	Above	Below
	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation	-0.363***	-0.293***	-0.211	-0.131	0.085	-0.460**
	(0.128)	(0.064)	(0.170)	(0.088)	(0.472)	(0.233)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	64,000	65,000	64,000	65,000	64,000	65,000

Table A.7
Alternative Heterogeneity Measures (cont.)

Panel D: 3-year Employment Growth in Industry-County

Dependent variable:	Continues		Occupied		Ln(Avg. Employment)	
Above or below median:	Above	Below	Above	Below	Above	Below
	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation	-0.267***	-0.408***	-0.118	-0.218**	-0.316	-0.497*
	(0.090)	(0.082)	(0.119)	(0.099)	(0.340)	(0.263)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	64,000	65,000	64,000	65,000	64,000	65,000