Asset Allocation in Bankruptcy*

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ABSTRACT

This paper investigates the consequences of liquidation and reorganization on the allocation and subsequent utilization of assets in bankruptcy. We identify 129,000 bankrupt establishments and construct a novel dataset that tracks the occupancy, employment and wages paid at real estate assets over time. Using the random assignment of judges to bankruptcy cases as a natural experiment that forces some firms into liquidation, we find that even after accounting for reallocation, the long-run utilization of assets of liquidated firms is lower relative to assets of reorganized firms. 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. The results highlight that different bankruptcy approaches affect asset allocation and utilization particularly when search frictions and financial frictions are present.

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Declining industries, insolvency and distressed firms are unavoidable consequences of an evolving economy. The ability of an economy to subsequently direct assets to better uses has important implications for productivity and the speed of recovery following adverse shocks (Eisfeldt and Rampini (2006); Hsieh and Klenow (2009); Bartelsman et al. (2013)). Since economies rely on courts to resolve insolvency, bankruptcy institutions play an important role in allocating the assets of distressed firms. Two approaches characterize bankruptcy institutions: liquidation and reorganization (Hart (2000); Strömberg (2000); Djankov et al. (2008)). While the liquidation procedure winds down the firm and puts all assets back on the market, reorganization aims at rehabilitating the company whenever possible.

Despite the importance of the bankruptcy system, empirical evidence on key questions is scarce: How do bankruptcy regimes affect asset allocation and utilization? Are assets in liquidation utilized similarly to assets in reorganization? If not, what frictions lead to different consequences of the two bankruptcy approaches?

Theoretically, with frictionless markets, the outcomes of both bankruptcy approaches should be similar, as both regimes should effectively allocate assets to their best use. This null hypothesis may no longer hold, however, when frictions are present. For example, asset allocation in a reorganization regime may be affected by conflicts of interests between claimholders, information asymmetry, and coordination costs (Baird (1986); Gertner and Scharfstein (1991); Aghion et al. (1992); Ivashina et al. (2015)). In liquidation, assets may not reallocate to best uses if they are specific to the firm, and markets are thin with few potential users (Williamson (1988); Gavazza (2011)). Misallocation may be further exacerbated if potential users of the assets are financially constrained (Shleifer and Vishny (1992)).

To answer these questions, it is necessary to tackle two important issues. First, there is little information on how assets are reallocated between firms and subsequently utilized, particularly in bankruptcy, when plants are shut down and firms are dissolved. Second, distressed firms that go through liquidation may be fundamentally different from firms that are reorganized. This is a common limitation to papers that explore the implications of different bankruptcy codes. Any comparison between two insolvent firms that experience different bankruptcy regimes may be biased due to unobserved differences in firm prospects and other characteristics.

In this paper we focus on the U.S. bankruptcy system and compare the consequences of liquidation (under Chapter 7 of the bankruptcy code) with reorganization (under Chapter 11 of the bankruptcy code) on asset allocation and utilization. To do so, we focus on the real estate assets used by bankrupt firms, and construct a novel dataset that tracks the allocation and utilization of these assets over time. Real estate assets represent a significant portion of firms' total capital.[1] Moreover, these assets are likely to be highly specific, as the optimal user varies significantly with building features and location characteristics. For example, an industrial warehouse is unlikely to be suitable for a retail store, and a restaurant is unlikely to be replaced with a hotel. Further, 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 at the bankruptcy filing year. The comprehensive nature of these data allows us to examine the population of bankrupt firms in the U.S., including small and private businesses. An important methodological contribution of this work is the creation of geographic linkages that track occupier identities and economic activity at real estate assets over time. This allows us to capture the allocation and utilization of assets even when plants shut down and the real estate is vacant, or when it is used for a different purpose than the original plant.[2]

To explore long-run (five-year) allocation 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.

[1]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.

[2]These circumstances are not fully captured by the standard LBD linkages that link plants over time. For example, if an auto parts manufacturer, AutoABC, is shut down, and the building is then occupied by a shoes manufacturer, ShoesXYZ, linkages at the LBD will consider the death of AutoABC and the birth of ShoesXYZ as two separate incidents. Our linkages will connect the two, showing 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.

While the former measure captures only whether economic activity takes place in a given asset, the latter measures also capture the intensity of such economic activity.

Tracking assets 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 remains within the same industry, suggesting a significant degree of asset specificity, 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 and economic conditions for asset redeployment (Shleifer and Vishny (1992); Gavazza (2011)).

In the main analysis, 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 Chapter 7 liquidation due to the assignment of the judge. In effect, otherwise identical filers are randomly placed in either reorganization or liquidation, thereby allowing us to compare asset 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 (Kling (2006); Doyle Jr (2007); Chang and Schoar (2013); Dobbie and Song (2015); Galasso and Schankerman (2015)).

This 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?[3] We first show that, as expected, bankrupt plants in liquidation are more likely to be shut down,

[3] 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.

relative to reorganization. But interestingly, even after accounting for the subsequent reallocation of real estate to new users, liquidated plants are 17.4% less likely to be occupied 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 average payrolls in these assets are significantly lower in liquidation relative to reorganization. These findings illustrate that bankruptcy regimes have important effects on long-run asset allocation and utilization.

To better understand which frictions lead to the gap in utilization between reorganization and liquidation, we explore the role of search frictions, which arise in thin markets (Williamson (1988); Gavazza (2011)), and financial frictions, in which potential asset users are financially constrained (Shleifer and Vishny (1992)). To do so, we rely on three measures. 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, 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 low-growth regions experience 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 asset reallocation to new users in liquidation. In sharp contrast, liquidated plants in thin markets are over 30% less likely to be occupied than otherwise-identical assets in reorganized firms. Similarly, we find no long-term differences in employment and wages across the two bankruptcy regimes in thick markets, while employment and wages are significantly lower in assets of liquidated firms in thin markets.

We also find that local access to small business financing affects asset allocation 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 assets are less likely to be occupied and have significantly lower employment and payroll relative to plants in reorganization. Similarly, in high economic growth regions, both bankruptcy regimes generate similar levels of asset utilization. However, in low-growth markets, we again find that liquidated establishments experience significantly lower long-run utilization.[4]

Overall, the concentration of lower utilization of liquidated assets in thin markets, markets with low access to finance, and low employment growth illustrates the importance of bankruptcy regimes on asset allocation and utilization. That is, we are able to reject the null hypothesis in markets with high search frictions and high financial frictions. The magnitude of these effects illustrates their economic importance. On the other hand, it is important to note that liquidation and reorganization lead to similar levels of asset utilization in areas with high market thickness, access to finance, or employment growth. Thus, failing to reject the null hypothesis in these markets suggests that the frictions that drive a gap between the two bankruptcy regimes are not ubiquitous.

While the results described thus far show that liquidation leads to lower utilization, the question that remains is whether this is inefficient. That is, are assets in liquidation under-utilized?[5] While we cannot conclusively answer this question, the concentration of lower utilization in markets with high search frictions and high financial frictions is consistent with theories that predict that in these circumstances assets will be under-utilized in liquidation. This evidence is also supported by an additional analysis of a subsample of manufacturing firms in which we find that productivity is lower in liquidation, relative to reorganization. But, this is not to say that liquidation is always inefficient, as we find no difference in utilization in areas with low search and financing frictions.[6]

This paper contributes to several strands of literature. It is most directly related to Maksimovic and Phillips (1998), who explore how industry conditions affect the reorganization of large manufacturing firms in Chapter 11. More broadly, this paper highlights the importance of local market characteristics in affecting the consequences of liquidation and reorganization on asset allocation, and thus contributes to an extensive body of theoretical and empirical literature that discusses the optimal design and frictions of the bankruptcy process.[7] Second,

[4]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.

[5]The alternative hypothesis is that the gap is due to inefficient continuation in reorganization, which may lead to over-utilization of assets. This may arise due to asymmetric information or conflicts of interest (Baird (1986); Gertner and Scharfstein (1991); Aghion et al. (1992)).

[6]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.

[7]Some theoretical examples include Baird (1986, 1993); Gertner and Scharfstein (1991); Aghion et al.

a large literature explores the existence and implications of fire sales.[8] 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 allocation for economic activity, by studying frictions that may impede reallocation.[9]

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: liquidation through a cash auction, and reorganization through a structured bargaining process (Hart (2000)). The U.S. Bankruptcy code contains both procedures, with liquidation falling under Chapter 7 and reorganization taking place 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.

(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); Chang and Schoar (2013) among others.

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

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

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

A significant portion of firms that originally file for Chapter 11 bankruptcy also end up in Chapter 7 through case conversion. Conversion to Chapter 7 occurs when the bankruptcy judge approves a petition to convert the case. Conversion petitions are typically filed either by a creditor or the court itself (e.g. by a trustee), accompanied with a brief which outlines why liquidation will provide the highest recovery for the creditors. 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 proceed with the reogranization through a structured bargaining process governed by specific rights and voting rules defined by the law.[10]

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

[10]Specifically, the debtor firm creates a plan of reorganization which outlines which assets will be retained or sold, how the firm will be restructured, and what recoveries credtiors will receive. This plan is then distributed to creditors who vote on the plan. The plan is approved if 2/3rds of creditors accept the plan. Because plans are typically negotiated with creditors prior to the vote, plan rejections are rare.

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

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 allocation and utilization across these two bankruptcy procedures. The key difference between the procedures for our purposes is that in Chapter 7 liquidation all assets are potentially reallocated, while in Chapter 11 reorganization 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 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.

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

B. Census Data and Measures of Local Market Characteristics

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 that employ at least a single worker. 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. This sample is then reduced 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 the importance of selection into bankruptcy regimes, and hence the need for identification in assessing the impact of the regimes.

In Section V.B, we explore three measures of heterogeneity of local market characteristics:

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

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.[14] $Thickness_{ict}$ 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 an asset 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. Therefore, the same county can have both a high thickness measure for one type of asset and a low thickness measure for another, depending on the local industrial composition. In Panel B of Table 1, we show that levels of market thickness are similar for both reorganized firms and firms converted to liquidation.

Second, we focus on access to finance as a determinant of asset reallocation. Because the majority of new occupants of bankrupt assets are local or new firms (as we discuss below), 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 million.[15] Specifically, we proxy for access to capital by measuring

[14] Results remain unchanged if we define s_{jct} as the share of plants in industry j rather than the share of employment.

[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

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 reorganized firms is similar to those in regions of firms that were converted to liquidation.

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. Reorganized firms reside in regions with slightly higher past economic growth than liquidated firms.

III. Asset Allocation 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. A significant benefit of the LBD is that it captures the location of tax-paying establishments, thereby reporting the *users* of real estate assets. This allows us to carefully explore asset reallocation through the evolution of asset occupiers, and asset usage, regardless of whether the property is owned or leased.[17]

To track real estate occupancy, employment, and payroll outcomes over time, we create a

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 firms, 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]An alternative approach would be to rely on real estate transactions, following changes in asset ownership. However, such an approach cannot identify whether assets are directed to different uses if reallocation occurs through leases. Moreover, this approach cannot identify when assets are vacant, and the extent to which the assets are being used.

careful address matching algorithm to link addresses over time. First, we clean all addresses and address abbreviations using the United States Postal Service formal algorithm.[18] Then, for each shut-down plant, we attempt to match its address with subsequent LBD years (up to five years following the bankruptcy filing), to track the next occupier of the real estate location.[19] 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 (i.e. has positive payroll) after the bankruptcy filing under its original ownership we classify the plant as "continued." Second, if a real estate location is occupied and active, and is used 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. In this section we discuss several issues regarding the measurement of asset allocation. 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

[18] See the following link (valid as of January 2016) for details of the postal addressing standards used: http://pe.usps.gov/text/pub28/

[19] 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. Importantly, in our sample the standard LBD linkages account for only about 25% of reallocation, while the geographical linkages we construct account for the remaining 75%.

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 firm, plant and local market characteristics can predict whether real estate is likely to be reallocated subsequent to plant closure, as illustrated in Table 3 and discussed in more detail in Section III.C. Moreover, 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 commercial real estate assets are converted into non-commercial types of real estate (residential, parks, etc.) in less than 1.5% of all transactions. This is not surprising, in light of the rigidity imposed by zoning regulations that restrict the nature of usage of real estate assets in commercial areas (Gyourko et al. (2008)).

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.6 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 Allocation in Bankruptcy

In this paper we construct measures of asset allocation and utilization of real estate assets. Given the novelty of the measures, in this section we describe three stylized facts that also 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 be operated by their initial users 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 liquidated plants will not continue, non-continuation is also prevalent in reorganization. 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.[20] 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, with more than two million workers added due to asset reallocation. Both figures illustrate that asset reallocation plays an important role in the utilization of these bankrupt 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.

[20] 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 January 2016). 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 papers, 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.

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 (13.6%) of total reallocations. We also 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, 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.

In addition, in the case of reallocation, new entrant characteristics vary with the bankruptcy regime. OLS regressions in Table 2 focus only on cases in which assets are reallocated, and show that plants that are converted to liquidation are 7.5 percentage points more likely to transition to a different 3-digit NAICS industry. Liquidated plants are also more likely to be replaced by either a new firm (2.6 percentage points) or a local firm (1.9 percentage points). These regressions control for both firm- and plant-level characteristics including size and industry, as well as bankruptcy division-by-year fixed effects. However, they should not be interpreted as causal relationships. Rather, they are evidence that characteristics of new users vary significantly with the bankruptcy regime.

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 3 reports regression results in which we limit the sample to 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 as opposed to remaining vacant. 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 asset reallocation. In particular, we find that being located in a county with a high total number of plants, high economic growth (measured by three-year employment growth in a county), and high payroll per employee, 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 3, 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 in the way their assets are allocated and subsequently utilized. To mitigate the selection, 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 real estate 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 real estate p in year t. We are interested in estimating β, which captures the impact of conversion to 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 allocation, we rely on judge heterogeneity in their propensity to convert Chapter 11 filings to Chapter 7 as an instrumental variable.[21] This instrument does not rely on differences in actual bankruptcy laws, as the bankruptcy code is uniform at the federal level. Rather, the instrument makes use of the fact that bankruptcy judges' interpretation of the law varies significantly (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.[22] 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

[21]This approach was pioneered by Kling (2006), and has been applied in a variety of settings (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)).

[22]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."

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 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 including all dismissed cases in the denominator; (b) the share of cases that judge j converted to Chapter 7 in the five years prior to the current case; (c) 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 liquidation on plant outcomes:

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

where $Liqui\widehat{dation}_{pi}$ 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 allocation, relative to 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 4, which demonstrates that there is a strong and tightly estimated relationship between the instrument and the probability of conversion to liquidation, even after introducing a comprehensive set of controls.

In column 1 of Table 4 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 4, 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 alternative 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 instruments (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. The 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 5 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 5 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 5 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 judge leniency affects plant outcomes through channels other than the bankruptcy regime, outside the liquidation or reorganization treatments. At this point, it is important to clarify the definition of the liquidation treatment in our setting. It may be the case that in the economy, the motion of Chapter 7 conversion is systematically correlated with other motions, or may systematically be approved by judges with particular characteristics. If this is how firms are liquidated in the economy, then naturally, this is also the liquidation treatment in our setting. We cannot separate the law from the way it is implemented. In that case, the liquidation treatment should be viewed more broadly than just the motion to convert to Chapter 7, but rather as the package of motions and judge characteristics that typically lead to conversion, and the results should be interpreted accordingly. Below, we attempt to explore the extent to which such broader interpretation is warranted.

We first estimate reduced-form regressions which directly relate judge leniency, ϕ_j, to plant outcomes:

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

These regressions, reported in Table A.2 in the Appendix, illustrate a strong relationship between the instrument, ϕ_j, and y_{pit} for all of our outcome variables. Arguably, this is because judge leniency leads to liquidation, which subsequently affects asset reallocation. However, if ϕ_j is *systematically* correlated with judge skill or other judge attributes that affect asset allocation, then ϕ_j should affect y_{pit} also when limiting the sample only to firms that remain in reorganization, or only to firms that are liquidated. 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, however. In column 7 of Table A.3, we also find that within Chapter 11 reorganization, ϕ_j is uncorrelated with bankruptcy refiling rates, a proxy for bankruptcy resolution success which may depend on judge skill.

Similarly, if the instrument is correlated with other motions approved by the judge that affect asset allocation, then this should be apparent in Table A.3, 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. Hence, while we cannot fully reject the broader interpretation of the liquidation treatment, we find no evidence for its existence in affecting aseet allocation and utilization.

V. Results

A. Full-sample Results

We first focus on how liquidation affects reallocation and utilization in the 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 explore the extent to which bankruptcy regimes affect the probability of discontinuation. The other three variables are measures of utilization of real estate assets, regardless of who the occupant is. *Occupied* is an indicator equal to one if the asset is occupied 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 the five years after the bankruptcy filing. 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 6 shows both OLS and 2SLS estimates of the impact of liquidation on these plant outcomes (reduced-form regressions, for brevity, are reported in Appendix Table A.2).[23] These regressions include the full set of 129,000 plants, and contain all controls in column 3 of Table 4, including industry and division-by-year fixed effects. 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%. This result is somewhat mechanical, since liquidation forces discontinuation while reorganization does not, and thus it serves more as a sanity check and also to measure a baseline effect against which overall utilization rates can be compared. In columns 3 and 4 we find that liquidated plants are significantly less likely to be occupied by any user five years after bankruptcy. 2SLS estimates show that liquidation reduces occupancy rates by 17.4%, an effect that is both statistically and economically significant.[24] This estimate is roughly half the size of

[23]As noted previously, observations are weighted by the inverse of the number of establishments in the bankrupt firm to avoid overweighting a few large bankruptcy cases. However, we find essentially identical results in unweighted OLS regressions.

[24]It is also interesting to note the gap between the OLS and IV estimates, which capture the selection into treatment. While there is clearly selection into Chapter 7 liquidation, 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 6 suggest that to a large extent these two effect balance each other out, so that OLS estimates are similar to 2SLS.

the 32.4% decline in plant continuation, demonstrating that reallocation to new users closes some of the gap between liquidation and reorganization, but not entirely.[25]

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.[26] This suggests that not only does liquidation reduce occupancy rates on average (the extensive margin), but it also reduces employment and payrolls which proxy for the extent to which an asset is used.[27] Taken together, the results show that bankruptcy regimes importantly affect asset allocation and subsequent untilization. In liquidation, plants are more likely to be discontinued, as expected, but these assets are not fully reallocated, and asstets in liquidation exhibit lower utilization relative to reorganization, as measured by occupancy, employment and wages.

Panel B of Table 6 shows how liquidation affects occupancy rates in years 1, 3, and 5 after bankruptcy. The purpose of this panel 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, highlighting the importance of reallocation, but even so a gap of 17.4% remains in year 5.[28]

B. *Heterogeneity Analysis*

The results presented so far show that liquidation causes significantly lower occupancy, employment, and total payroll five years after the bankruptcy filing. In this section, we explore how the gap between liquidation and reorganization is related to the market in which bankruptcy occurs. In particular, we focus on three local market characteristics (described in Section II above) that theory predicts affect asset reallocation: market thickness, access

[25] In the appendix (Table A.4) we estimate the role that reallocation plays in increasing the utilization of liquidated plants explicitly.

[26] 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 $100\,[\exp(\beta) - 1]$.

[27] 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 creating an ex-post selected subsample.

[28] Appendix Table A.5 contains dynamic results for other utilization measures, and shows a similar pattern. In addition, Table A.5 in the Appendix 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.

to finance, and economic growth. This allows us to better understand what frictions drive the utilization gap between reorganized and liquidated assets.

In Panel A of Table 7, we split the sample based on the market thickness measure, $Thickness_{ict}$, which measures the market share of potential users of the asset in the same county. Due to asset specificity and the local nature of reallocation, new users tend to come from similar industries and reside locally. Thus, 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 IV specifications separately for plants in thick and thin markets.[29]

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 that occupancy is similar for liquidated and reorganized plants. Thus, the null hypothesis of no difference between the two bankruptcy regimes is not rejected, as the market fully absorbs the increased numbers of discontinued plants in liquidation. 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. Hence, in contrast to thick markets, liquidated plants do not seem to reallocate to new uses at higher rates than reorganized plants.[30] 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%.[31] Overall, the effect of liquidation on asset utilization is entirely concentrated in thin markets, while

[29] 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.

[30] Indeed, the coefficient estimate of liquidation's impact on continuation in thin markets (Column 2, -32.1%) is almost identical to its impact on occupancy (Column 4, -32.4%). This does not mean, however, that there is no reallocation of liquidated plants in thin markets. Rather, it shows that the reallocation of assets increases the occupancy of both reorganized and liquidated plants at similar rates in thin markets.

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

realloction in thick markets results in liquidation having no impact on utilization. These results are consistent with theories that highlight the implications of search frictions and thin markets on asset reallocation (Williamson (1988); Gavazza (2011)).[32]

In Panel B of Table 7 we turn to the role of local access to finance in affecting asset allocation and utilization in bankruptcy regimes. 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.[33] 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 highlight the importance of access to capital as a key determinant in the ability to reallocate assets (Shleifer and Vishny (1992)).[34]

Our third market characteristic is economic growth, defined as the cumulative three-year growth in total employment in the county. [35] 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 of Table 7 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 re-

[32]These results are robust to using an alternative measure of market thickness, that is, local commercial real estate transactions per capita. This measure aims to capture the liquidity of the local commercial real estate market. We construct this measure using CoreLogic dataset by dividing the total number of real estate transactions in a county by the county population. The results are discussed in detail in the Appendix, and reported in Panel A of Table A.8 in the Appendix.

[33]Loan data comes from the Community Reinvestment Act (CRA) disclosure data and is only available beginning in 1996, which removes about 30,000 plants from our sample that filed for bankruptcy prior to 1996. Note that the CRA data is based on the location of the loan recipient rather than the location of the bank, and thus the bank is not necessarily located in the same county. Further, the 2003 Survey of Small Business Finances shows that bank loan markets tend to be quite local, as over 70% of firms borrow from banks located less than 20 miles away.

[34]We find similar results when using an alternative measure of local access to finance, that is, the share of bank deposits in a county held small banks. This variable stems from the idea that small, local banks are the principle providers of capital for small firms (Petersen and Rajan (1994)). Hence, a higher concentration of deposits in local small banks is likely to provide higher access to capital to small businesses. We discuss the construction of the measure in the Appendix, and report the results in Panel C of Table A.8 in the Appendix.

[35]Appendix Table A.8 reports results using 3-year employment growth in the industry-county. The findings remain unchanged.

allocation 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.

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, the three measures are uncorrelated, as shown in Appendix Table A.7, suggesting that each channel is separate from the others and exerts a significant effect individually. Indeed, the separate frictions can interact in important ways to create even larger differences in utilization. Specifically, we show in Table A.9 in the appendix that the utilization of liquidated plants is especially low in counties that combine low employment growth and low $Thickness_{ict}$. Perhaps more striking, our estimates suggest that liquidation can lead to *higher* utilization in areas with both high employment growth and high $Thickness_{ict}$. Further, when using alternative measures of market thickness, access to capital, and economic growth, we find similar results, as described in Appendix Section C. 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.

C. Discussion

The results show that liquidation causes lower utilization of assets relative to reorganization in thin markets, markets with low access to finance, and low employment growth. The question that remains is whether this is inefficient. That is, are liquidated assets under-utilized in these markets? This may not be the case if reorganization leads to inefficient continuation (Hotchkiss (1995)) and the higher utilization seen in reorganization is actually not efficient. We cannot answer this question conclusively. However, in this section, we discuss several pieces of evidence that are more consistent with the interpretation that liquidated assets are under-utilized.

First, we find that lower utilization of liquidated assets is concentrated in markets in which theories predict asset reallocation should be less efficient in liquidation — where search costs are high such as in thin markets (Williamson (1988); Gavazza (2011)), or when potential users of the assets are financially constrained (Shleifer and Vishny (1992)). Meanwhile, theories that predict inefficient asset allocation in reorganization point to coordination problems or information assymetries (Franks and Torous (1989); Gertner and Scharfstein (1991); Bolton and Scharfstein (1996)). There is no reason to predict that these frictions are largest in these same markets.

Second, we note that a large portion of the utilization gap between liquidation and reorganization is driven by long-term plant vacancy. Vacancy may be the efficient outcome if reorganizing the firm is a strictly negative net present value (NPV) project, i.e. if operating costs in these regions are higher than potential revenues. If this is the case, then reorganziation in these areas is less efficient. But this interpretation is hard to reconcile with the persistence of the effects over a five-year period post bankruptcy, as eventually negative NPV projects will run out of cash and fail.

Finally, because our measures of utilization are not direct measures of efficiency, we perform a similar analysis that examines the total factor productivity (TFP) of the subset of manufacturing plants in our sample in Table A.10 in the appendix. We find that liquidation has a significant and persistent negative impact on TFP that is similar in magnitude to our main results.[36]

Despite these pieces of evidence, it is important to keep in mind a few caveats. First, while most of the prior literature has focused on publicly traded firms, our sample is representative of the universe of Chapter 11 firms and thus includes many 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.

[36] This analysis requires making assumptions about the TFP of vacant plants and locations that tranistion out of manufacturing. Section C in the appendix discusses this and shows that the results are robust to various TFP assumptions for these locations.

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 more broadly, and leave this analysis to future work.

VI. Conclusion

How do institutions affect the allocation of assets in the economy? In this work we explore the role of the bankruptcy system in affecting the allocation of commercial real estate, an important form of capital used by firms. In particular, we explore how liquidation and reorganization affect the allocation 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 create unique geographical linkages from the Census LBD database that allow us to track 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 significantly reduced utilization of real estate assets on average, and this effect persists at least five years after the bankruptcy filing. These effects are fully concentrated in thin asset markets where there are few potential users for bankrupt assets, in areas with low access to capital, and in counties with low economic growth. In these areas, the economic magnitude of our findings is meaningful. In contrast, in markets with low search frictions and financing frictions we find no differential effect of bankruptcy institutions on asset utilization. Overall, the results highlight that local asset market frictions play an important role in determining the consequences of bankruptcy approaches on asset allocation and utilization in the economy.

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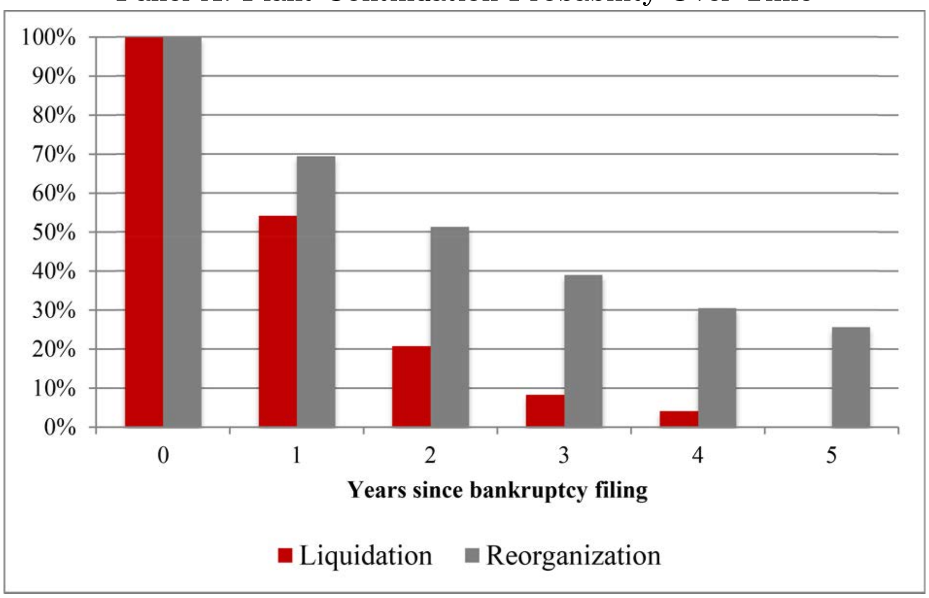
Figure 1
Stylized Facts About Bankruptcy Reallocation

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

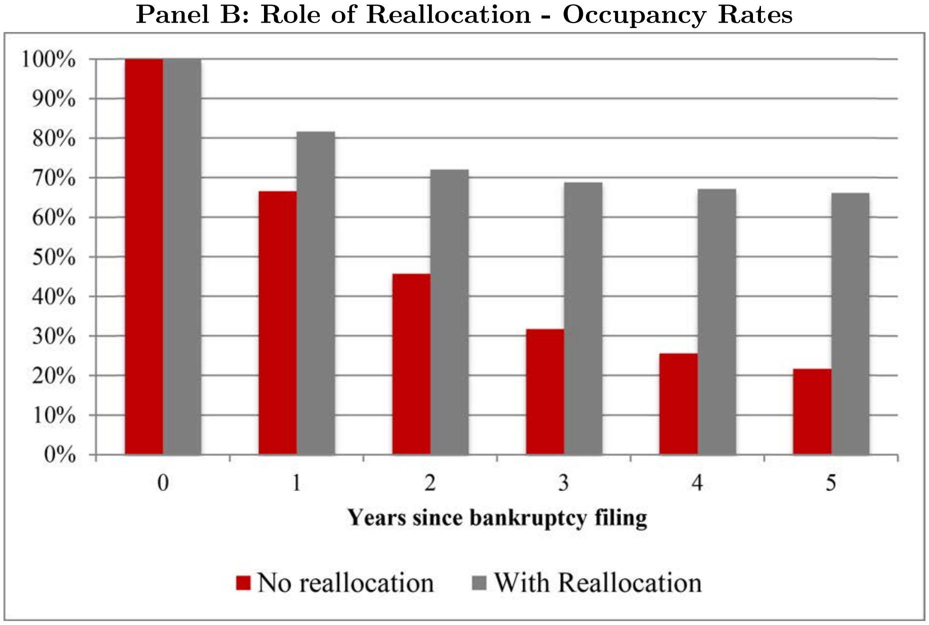
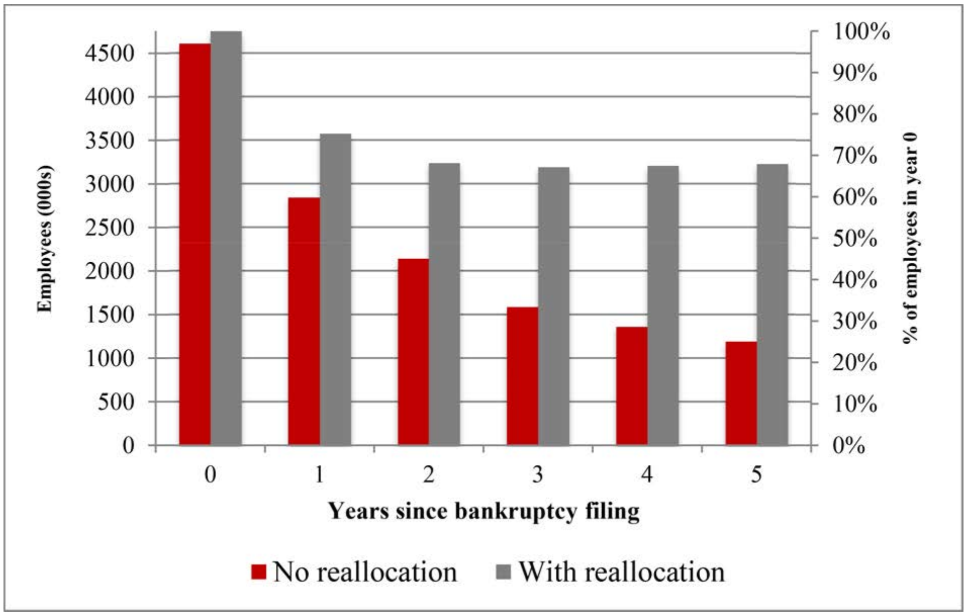


Figure 1
Stylized Facts About Bankruptcy Reallocation (cont.)

Panel C: Role of Reallocation - Employment



Panel D: Share of Plants Reallocated Over Time

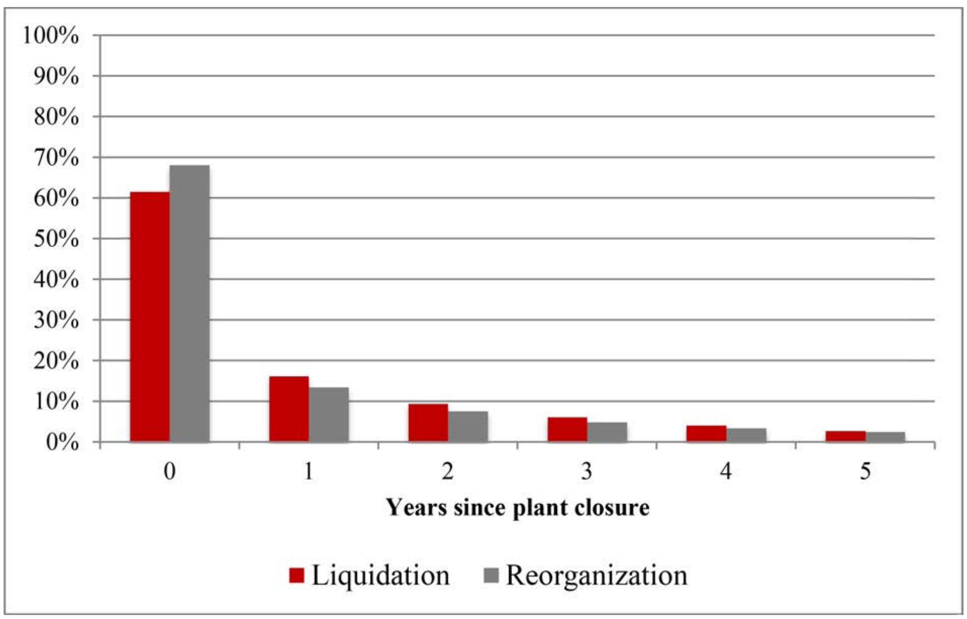


Figure 2
Non-Parametric First Stage

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.

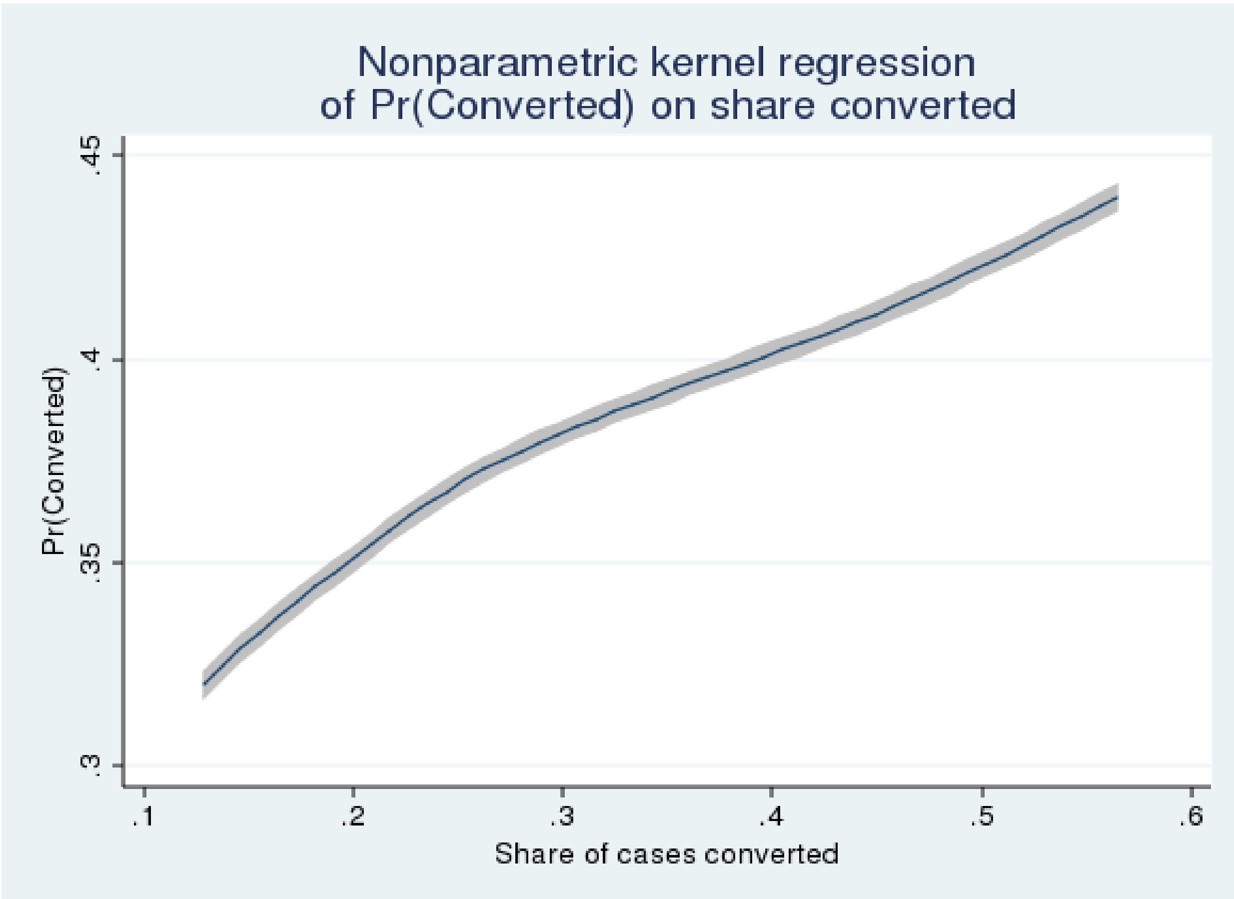


Table 1
Sample Summary Statistics

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 describes the characteristics of the firms replacing the dead bankrupt plants, distinguishing between new firms, existing firms that already had an establishment in the same county, and other existing firms. In Panel C we also report the percentages of reallocations to the same 2- and 3-digit NAICS industry.

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
New Entrants Characteristics and Bankruptcy Regimes

This table examines characteristics of new entrants that move into locations vacated by bankrupt firms. Each column is a separate OLS regression on the sample of plants that are closed within 5 years of the bankruptcy filing and that are replaced within the same time frame. In the first column, the dependent variable is a dummy that indicates if the new entrant is in a different 3-digit NAICS industry than the bankrupt firm. The second column is similar but uses the broader 2-digit NAICS classification. In column 3 the dependent variable is a dummy equal to 1 if the new entrant is a new firm that did not exist in the previous year. The dependent variable in column 4 indicates if the entrant is a local firm, defined as an existing firm that already had an establishment in the same county. Plant- and firm-level controls identical to those in Table 4 are also included, but are not reported for brevity. In addition, we include 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:	In different 3-digit NAICS (1)	In different 2-digit NAICS (2)	New entrant (3)	Local entrant (4)
Liquidated	0.075*** (0.008)	0.071*** (0.008)	0.026*** (0.006)	0.019*** (0.004)
Control variables	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes
Observations	62,500	62,500	62,500	62,500
R-squared	0.125	0.112	0.084	0.043

Table 3
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 4 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 4
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 5
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 6
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. In Panel B, 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 4, 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: 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 7
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 6 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 6. 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 4, 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

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 the 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 focus only on firms that were treated with either Chapter 7 or Chapter 11. 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 these EINs are less likely to have commercial real estate assets, 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.[37]

B. Address Matching Algorithm

A principal goal in this paper is to track the economic activity at specific locations over time and across occupancy changes. The LBD links establishments over time only when the user of a location keeps the same name. Thus, only in cases where the establishment maintains the same name asset sales are tracked in the LBD. In the majority of cases, however, the new occupant has a different name and thus the transaction will be 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.

[37] 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 129,000 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 in the LBD) for at least five years after their bankruptcy filing, as there is no need to track these plants. This leaves us with 89,000 total establishments that are shut down at some point, which we attempt to match to future establishment births located in the same addresses. 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.[38] 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

[38] 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),[39] 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).

B. *Identifying Non-Unique Locations*

Another important issue in linking geographic locations is dealing with non-unique addresses, which occur when multiple businesses are located in the same building, such as in office buildings or shopping centers. 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 take this information into account when allocating employment and wages to reallocated plants, as described below in

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

Section B.E. More importantly, as shown in Appendix Table A.6, the results hold for various subsamples of the data that exclude addresses that have mutiple establishments within the same location. 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:[40]

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)
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, and must therefore check if the address is unique for the bankrupt firm. We match each of these addresses to the LBD in year *t-1*, the year before the plant shutdown. 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. 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.

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

[40] Note that, because plants in the DBP shut down, none of them have addresses available in the next census.

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 first, and then to mailing addresses of LBD 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.

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 plants that are born subsequent to their death. We do this by looping over all years from 1992 to 2010 and searching the LBD 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. Identify all potentially matching plants in the LBD. These are plants that were active in year t and that have an address that matches a house number-zip code combination of the DBP. In addition, plants must have valid birth 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.[41]

3. Match the DBP plants from step 1 with the LBD 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 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:[42]

   (a) Physical address

[41]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.

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

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 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 grow to 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 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. 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.[43] 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. 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 vast 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.

[43] 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."

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 that test the robustness of our results to alternative measures and different samples. We also present auxiliary results that support the main analysis in the paper.

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. In addition, we also report results where the instrument is the share of all cases converted including dismissed cases. That is, in the paper the instrument used is the share of cases converted *excluding* dismissed cases, i.e. # liquidated / (# liquidated + # reorganized). This instrument is preferable since it is orthogonal to a judge's propensity to dismiss cases (the correlation is -0.05 and insignificant) and thus we do not get selection into the non-dismissed sample. However, here, we show results that include dismissed cases in the denominator. Reassuringly, the results are nearly identical in sign and magnitude for both alternative instruments, with liquidated plants being associated with lower continuation rates, higher vacancies, and lower utilization of real estate assets compared to plants in reorganization.[44]

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 6, Panel A, we find that a higher *share of other cases converted* is associated with lower asset utilization for all three measures.

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 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.

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

In Table A.4 we demonstrate the role that liquidation plays in forcing the reallocation of plants to new users. These regressions are similar to those in Panel A of Table 6, but in this case we set each utilization measure to zero unless the plant has been reallocated. Thus, coefficient estimates should be interpreted as showing the extent to which asset reallocation increases utilization at liquidated plants relative to reorganized plants. For example, reallocation increases the occupancy rate by 13.4 percentage points among liquidated plants, relative to reorganized plants. These results demonstrate how reallocation serves to close the utilization gap between liquidated and reorganized establishments.

Table A.5 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 B of Table 6. In Panel B of Table A.5, 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.6 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 commercial 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.7 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.8 we report results identical to Table 7 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 evidence for the results discussed in the main text.

We also show that the interaction of these frictions can create even larger gaps in asset utilization. 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 Table A.9. As expected, we see large declines in utilization when plants in bottom quartile counties are liquidated, shown in columns 2 and 4. Perhaps more interesting is that in the top quartile (columns 1 and 3) we find that the point estimate of liquidation's effect on utilization is positive and economically large, although not statistically significant due to reduced sample sizes. This suggests that in some very particular markets liquidation may actually *increase* overall utilization relative to reorganization by forcing reallocation to potentially better uses.

Our main results focus on measures of utilization as outcome variables. However, since utilization is not a direct measure of efficiency it is difficult to make a definitive judgment on the effiiciency of either bankruptcy regime. To shed some light on this issue, in Table A.10 we focus on liquidation's impact on total factor productivity (TFP) of manufacturing plants. This analysis is limited to 2,500 manufacturing plants which are in the Annual Survey of Manufacturers (ASM) or Census of Manufacturers (CMF) in the year prior to bankruptcy, allowing us to measure TFP.[45][46] We then track the TFP at a given location in the five years after bankruptcy, using the same 2SLS strategy as in the main analysis.[47]

Tracking TFP is not possible in two situtations. First, TFP is not measured at locations that transition to manufacturing firms not included in the ASM/CMF sample or out of manufacturing altogether. In Table A.10 we assume this set of plants has the median TFP from the full ASM/CMF sample in a given year. However, assuming any other level of TFP or dropping these plants from the sample completely does not impact the size or significance of the results.

Second, TFP is not defined for vacant locations. Because liquidation leads to higher

[45]We use the ln(TFP) measure contained in the auxiliary Census ASM files, which is computed following the standard TFP estimation procedures outlined in Foster, Haltiwanger, and Krizan (2000): "Aggregate productivity growth. Lessons from microeconomic evidence." New developments in productivity analysis. University of Chicago Press, 2001. 303-372.

[46]Further, since the ASM and CMF do not cover the universe of manufacturing plants, we follow the previous literature and weight regressions by the inverse of the sampling weight. However, this weighting does not affect the results.

[47]Despite the much smaller sample size, the first stage remains strong with an F-stat of 13.

instances of vacancy, the results are dependent on how the implied productivity of vacant locations is interpreted. Accordingly, in Table A.10 we present results where the TFP of vacant locations is set at various levels. In columns 1 and 2, we assume that vacant locations are unproductive by setting ln(TFP)=0 whenever a plant is vacant. Under this assumption, liquidation results in a sharp decline in TFP of 41.6% (based on the 2SLS estimates). In the remaining columns, we set TFP of vacant establishments at various percentiles of the full distribution of TFP from the ASM/CMF. Columns 3-6 show that even if vacant-plant TFP is set at the 10th or 20th percentile liquidation leads to a significant reduction in productivity. Meanwhile, liquidation has an insignificant effect on productivity if we assume the TFP of vacant plants is between the 30th and 70th percentiles. Only if TFP is set to the 80th percentile or above do we find that liquidation has a positive and significant effect on productivity overall. Put differently, for liquidation to lead to a more efficient usage of establishments on average, it would need to be the case that vacant locations are put to uses with social value equivalent to the 80th percentile of the productivity distribution or higher. Meanwhile, if the "productivity" of vacant establishments is below the 20th percentile, liquidation results in a significant decline in efficiency for manufacturing plants.

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 4. 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. We also show second stage results where the instrument is the share of all cases including dismissed cases in the denomiator. Dependent variables are defined identically to Panel A of Table 6, 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 with Alternative Instruments

Dependent variable:	Continues		Occupied		Ln(Avg. Employment)		Ln(Avg. Total Wages)	
Instrument:	Past cases	Incl. dismissed	Past cases	Incl. dismissed	Past cases	Incl. dismissed	Past cases	Incl. dismissed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Liquidation	-0.368***	-0.334***	-0.135*	-0.214***	-0.555***	-0.377**	-0.819**	-0.942***
	(0.051)	(0.050)	(0.071)	(0.071)	(0.212)	(0.183)	(0.369)	(0.320)
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	0.146	0.151	0.039	0.036	0.217	0.213	0.231	0.232

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 6. 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 6, 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
Liquidation and Reallocation

This table focuses on the role of reallocation in liquidation. These regressions are similar to those reported in Panel A of Table 6, but here the dependent variables are set to zero unless the plant has been reallocated. Thus, these regressions test the extent to which liquidation causes higher reallocation of real estate to new users. Control variables are identical to those in Panel A of Table 6. 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:	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

Table A.5
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 B of Table 6, 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 6. 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.6

Robustness of Results to Matching Algorithm

This table repeats the main analysis from Panel A of Table 6 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 6. 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	Ln(Avg. Employment)	Ln(Avg. Total Wages)
	(1)	(2)	(3)
Liquidation	-0.309***	-1.045***	-2.044***
	(0.112)	(0.282)	(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	Ln(Avg. Employment)	Ln(Avg. Total Wages)
	(1)	(2)	(3)
Liquidation	-0.373***	-1.062***	-2.059***
	(0.093)	(0.237)	(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	Ln(Avg. Employment)	Ln(Avg. Total Wages)
	(1)	(2)	(3)
Liquidation	-0.282***	-0.770***	-1.415***
	(0.089)	(0.210)	(0.383)
Control Variables	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes
Observations	107,000	107,000	107,000

Table A.7

Heterogeneity Measures 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 commercial 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.8

Alternative Heterogeneity Measures

This table reports results identical to Table 7 but using alternative measures of market charactertistics to split the sample. Panel A uses *real estate transactions per capita*, defined as the number of commercial 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 6. 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 4, 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.8
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

Table A.9
Interaction of Market Conditions

This table shows how the interaction of market thickness with economic growth creates sizable differences in the effect of liquidation on plant utilization. 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. We present 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 6. All regressions are estimated by 2SLS and contain the full set of control variables in Column 3 of Table 4, 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.

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

Table A.10
Liquidation and Productivity

This table shows the effect of liquidation on total factor productivity (TFP) for manufacturing plants. The dependent variable is the log of average TFP over 5 years after bankruptcy at a given location, regardless of the plant occupant. TFP is not measured for two sets of plants: those that transition out of manufacturing and those that are vacant. We assume that plants that transition out of manufacturing have the median TFP in that year, but results are not sensitive to this assumption. Meanwhile, each set of columns shows a different assumption for the TFP of vacant locations. In the first two columns, we assume that vacant locations have *ln(TFP)*=0, and in the remaining columns we set TFP to the 10th, 20th, 50th, and 80th percentiles of the full TFP distribution. Both OLS and instrumented (2SLS) estimates are shown. All regressions contain the full set of control variables in Column 3 of Table 4, 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.

Dependent variable:	Ln(Average TFP)									
Vacant-plant TFP set to:	Zero		10th percentile		20th percentile		50th percentile		80th percentile	
Model:	OLS	IV-2SLS	OLS	IV-2SLS	OLS	IV-2SLS	OLS	IV-2SLS	OLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Liquidation	-0.388***	-0.538**	-0.181***	-0.220*	-0.132***	-0.145	-0.023	0.027	0.106***	0.233**
	(0.048)	(0.227)	(0.026)	(0.128)	(0.022)	(0.111)	(0.016)	(0.088)	(0.020)	(0.105)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Div x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,500	2,500	2,500	2,500	2,500	2,500	2,500	2,500	2,500	2,500
R-squared	0.416	0.153	0.392	0.160	0.379	0.153	0.350	0.100	0.373	0.050
1st stage F-stat		13.08		13.08		13.08		13.08		13.08