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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. Using the random assignment of judges to bankruptcy cases as a natural experiment that forces some firms into liquidation, we find that 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 and in areas with low access to finance. These findings suggest that when search frictions are large, liquidation can lead to inefficient allocation of assets in bankruptcy.

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 economic shocks (Eisfeldt and Rampini (2006), Hsieh and Klenow (2009), Bartelsman, Haltiwanger, and Scarpetta (2013)). Since economies rely on courts to resolve insolvency, bankruptcy institutions play an important role in allocating the assets of distressed firms. Broadly speaking, bankruptcy is resolved through two approaches: liquidation and reorganization (Hart (2000), Strömberg (2000), Djankov et al. (2008)). While liquidation involves winding down the firm and putting all firm 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. For instance, how does the bankruptcy regime affect asset

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allocation and utilization? Are assets in liquidation utilized similarly to assets in reorganization? And, if not, what frictions lead to different effects under the two resolution approaches?

Theoretically, in frictionless markets, both bankruptcy approaches should lead to similar outcomes, as both regimes should allocate assets to their best use. This null hypothesis may not hold, however, in the presence of frictions. For example, conflicts of interests among claimholders, information asymmetry, and coordination costs in reorganization may lead to inefficient continuation and in turn to inefficient asset allocation (Baird (1986), Gertner and Scharfstein (1991), Aghion, Hart, and Moore (1992), Ivashina, Iverson, and Smith (2015)). In liquidation, assets may not be reallocated 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 address these questions, one must tackle two important issues. First, little information is available with respect to how assets are reallocated between firms and how assets are 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. Any comparison between two insolvent firms that experience different bankruptcy regimes may therefore be biased due to unobserved differences in firm prospects and other characteristics. This is a common limitation to papers that explore the implications of different bankruptcy codes.

In this paper, we focus on the U.S. bankruptcy system and compare the effects of liquidation (under Chapter 7 of the bankruptcy code) with those of reorganization (under Chapter 11 of the bankruptcy code) on asset allocation and utilization. To do so, we focus on the real estate assets of bankrupt firms and construct a novel data set 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. Furthermore, location benefits in terms of access to customers and suppliers, local labor markets, and knowledge spillovers vary across firms (Ellison, Glaeser, and Kerr (2010)).

We combine the U.S. Census Bureau's Longitudinal Business Database (LBD) with bankruptcy filings from LexisNexis Law to obtain a data set with rich information on 129,000 establishments belonging to 28,000 bankrupt firms that employ close to 4.7 million workers at the time of bankruptcy. The comprehensive nature of these data allow us to examine the population of bankrupt firms in the United States, including small and private businesses. An important methodological contribution of this work is the creation of geographic

[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.

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 when plants shut down and the real estate is vacant or when it is used for a different purpose from the original plant.[2]

To explore long-run (i.e., five-year) allocation and utilization of these assets, we rely on several measures. We first examine the length of time a location continues to be operated by the bankrupt firm and, if it does not continue, whether it is occupied by a new firm or falls vacant. We further study the average number of employees at a given location over time. While the former measure captures whether economic activity takes place in a given asset, the latter also captures 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 typically to a local firm, and to a firm in the same industry, consistent with a significant degree of asset specificity and search costs, as highlighted by Williamson (1988) and Ramey and Shapiro (2001). Third, we find that industry conditions, 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, as discussed by Shleifer and Vishny (1992) and Gavazza (2011).

In the main analysis, we address potential endogeneity of the bankruptcy regime by employing 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 Chapter 11 case to Chapter 7, there is significant variation in the interpretation of these criteria across judges.

Our empirical strategy compares bankrupt firms that are reorganized under 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 by the random judge assignment, which allows us to compare asset outcomes across the two regimes. Our empirical strategy follows a growing thread of literature that takes advantage of the random assignment of judges and variation in judge interpretation of the law (Kling (2006), Doyle (2007), Chang and Schoar (2013), Dobbie and Song (2015), Galasso and Schankerman (2015)).

[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, shuts down, and the building is then occupied by a shoe manufacturer, ShoesXYZ, linkages at the LBD would consider the death of AutoABC and the birth of ShoesXYZ as two separate incidents. Our linkages connect the two, by showing that ShoesXYZ replaced AutoABC at 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 as well as in the Internet Appendix, which may be found in the online version of this article.

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 than those in reorganization, and they shut down more quickly.

Interestingly, however, 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. In addition, the average number of employees at liquidated locations is significantly lower relative to reorganized locations. These findings illustrate that bankruptcy regime choice significantly affects 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 in limiting asset allocation. Search frictions can arise in thin markets with few potential users (Williamson (1988), Gavazza (2011)), and in markets where potential asset users are financially constrained (Shleifer and Vishny (1992)). Empirically, we rely on two sets of measures of search costs. First, we create a measure of market thickness that assesses the extent to which potential users of a bankrupt plant's real estate reside locally. Second, since assets typically reallocate to new and local businesses, we employ a measure that identifies markets with low access to small business finance.

We find that the drop in utilization is significantly larger in thin markets and areas with low access to capital. Five years following 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 across the two bankruptcy regimes in thick markets, but significantly lower employment when assets are liquidated in thin markets. We also find that local access to finance affects asset allocation in bankruptcy. In regions with high access to finance, we find similar levels of utilization for both liquidated and reorganized establishments. In markets with low access to finance, however, liquidated assets are less likely to be occupied and have significantly lower employment relative to plants in reorganization.[4]

[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 distinct from the ultimate outcome of the bankruptcy, as firms in a reorganization bankruptcy can be liquidated if that is the outcome of the bargaining process. The key difference is that liquidation is forced under a cash auction system like Chapter 7, while it is not under structured bargaining.

[4] The correlation between our measures of market thickness and access to finance is 0.10, suggesting that these channels capture different search frictions that contribute to the gap between liquidation and reallocation.

Are the above results driven by inefficient liquidation? Under this interpretation of our findings, assets are underutilized in liquidation since reallocation is impeded by high search costs or financial constraints (Williamson (1988), Shleifer and Vishny (1992), Gavazza (2011)). An alternative interpretation is that agency costs in the reorganization process lead to inefficient continuation of the firm, which leads the incumbent firm to maintain control over assets and thus prevents their deployment to other users (Franks and Torous (1989), Gertner and Scharfstein (1991), Hotchkiss (1995), Bolton and Scharfstein (1996)).[5]

If the results are driven by continuation bias in reorganization, we would expect to see greater utilization in randomly forced liquidations, since this allows assets to transfer to more productive users. This should be particularly true in markets with low search frictions in which reallocation can occur more easily. However, even in thick markets and markets with high access to capital, we do not find evidence that liquidation leads to higher asset utilization. Our empirical evidence is therefore inconsistent with strong continuation bias among the reorganized firms in our sample.

While the evidence does not support the interpretation of inefficient continuation in reorganization, it does not necessarily mean that liquidation is inefficient, since vacancies can be efficient. In particular, if a liquidated firm leaves a vacancy in one location but an identical store can easily open nearby, then the overall impact on economic efficiency is likely negligible. Following this logic, we argue that the costs of vacancy depend on the overall vacancy rate of the local market. In areas with low vacancy rates, the opportunity cost of leaving a location vacant should be high since there is relatively high demand for real estate. Accordingly, we use the vacancy rate of nonbankrupt plants in the local market as a proxy for the opportunity cost of vacancy.

Interestingly, we find that, even in markets with low local vacancy rates, liquidation reduces occupancy, with the magnitude of this effect very similar to that for the full sample. This evidence is consistent with the view that liquidation causes vacancy even when the opportunity cost of doing so is high. Of course, because we cannot directly measure asset efficiency, these results are not fully conclusive. Nevertheless, they suggest that liquidation leads to inefficient asset allocation in areas with high search frictions.

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 influencing the effects of liquidation and reorganization on asset allocation, and thus contributes to an extensive body of theoretical and empirical work that focuses on optimal design of the bankruptcy process.[6] In addition, this paper

[5] It is important to note that this paper only considers the efficiency of the asset allocation process in each bankruptcy regime. There are, of course, many other aspects of bankruptcy, such as legal fees, creditor recovery rates, and worker outcomes, that enter into the overall costs and benefits of liquidation and reorganization that we do not consider here.

[6] Some theoretical examples include Baird (1986, 1993), Gertner and Scharfstein (1991), Aghion, Hart, and Moore (1992), Shleifer and Vishny (1992), Hart (2000). Empirical studies include

adds to a large literature that explores the existence and implications of fire sales by relying on random variation that forces liquidation, which allows us to explore reallocation and utilization independent of the reasons that initially led to the forced sale.[7] Finally, this paper contributes to the literature that highlights the importance of labor and asset allocation for economic activity by studying frictions that may impede reallocation.[8]

The remainder of the paper is organized as follows. Section I summarizes the bankruptcy process. Section II discusses our data construction, Section III introduces the measurement of asset reallocation, and Section IV presents the empirical strategy. Section V presents the main results in the paper. Section VI discusses the efficiency implications of our results. Finally, Section VII 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 bankruptcy chapter, although under certain circumstances creditors can file for an involuntary bankruptcy. Firms can file for bankruptcy where they are incorporated, where they are headquartered, or where they do the bulk of their business (see 28 USC Section 1408), which gives large, multistate firms some leeway in the choice of bankruptcy venue. However, once a firm files for bankruptcy, it is randomly assigned to one of the bankruptcy judges in the divisional office in which it files. This random assignment is a key part of our identification strategy, which we outline below.

Firms that file for Chapter 7 bankruptcy expect to liquidate all assets of the firm and hence face a relatively straightforward process, although it can be lengthy (Bris, Welch, and Zhu (2006)). In particular, a trustee is assigned to oversee the liquidation of the firms' assets, with proceeds from the asset sales used to pay creditors according to their security and priority. According to U.S. court filing statistics, about 65% of all business bankruptcy filings in the United States are Chapter 7 filings.

A significant portion of firms that originally file for Chapter 11 bankruptcy 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

Hotchkiss (1995), Strömberg (2000), Davydenko and Franks (2008), Eckbo and Thorburn (2008), Benmelech and Bergman (2011), and Chang and Schoar (2013), among others.

[7] See, for example, Pulvino (1998, 1999), Ramey and Shapiro (2001), and Campbell, Giglio, and Pathak (2011). Shleifer and Vishny (2011) survey this literature.

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

petitions are typically filed by either a creditor or the court itself (e.g., by a trustee), accompanied with a brief that outlines why liquidation will provide the highest recovery for the firm's creditors. As we discuss in Section V, the judge plays an important role in the decision to convert the case to Chapter 7.[9] 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 reorganization 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 firm's assets to be liquidated should that be the outcome of the bargaining process. The key difference from Chapter 7 is that liquidation under Chapter 11 is not forced. Assets that are owned by the firm can be sold via "Section 363 sales," according to which some or all of the firm's assets are auctioned off while the firm remains in bankruptcy.[11] Similarly, in Chapter 11 whether assets that are leased by the firm (as much commercial real estate is) should be retained or returned to their owners is determined in a negotiation. Firms in Chapter 11 can choose which leases to accept and which to reject, thereby terminating the contract. In Chapter 7, leases are automatically rejected, which forces the lessor to find a new tenant. Thus, regardless of whether an asset is owned or leased, Chapter 11 allows for negotiation surrounding which assets are kept in the firm, while a new buyer or user must be found for assets in Chapter 7.[12]

In this paper, we compare asset allocation and utilization across these two bankruptcy procedures. The key difference between the two regimes 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 the 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. court system. These data contain legal information about each filing, including the date the case was filed, the court in

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

[10] Specifically, the debtor firm creates a reorganization plan that outlines which assets will be retained or sold, how the firm will be restructured, and what recoveries creditors will receive. This plan is then distributed to creditors, who vote on the plan. The plan is approved if two-thirds of the 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 firm's assets can be liquidated through a formal reorganization plan. Creditors are allowed to vote on these plans.

[12] A full discussion of the treatment of leases in Chapter 11 can be found in Ayotte (2015).

which it was filed, the judge assigned to the case, an indicator for whether the filing was involuntary, and status updates on the case. From the status updates, we are able to identify cases that were converted to Chapter 7. The LexisNexis data set contains a few bankruptcies beginning as early as 1980, but coverage is not complete in these early years as courts were still transitioning to an electronic records system. We begin our sample in 1992, when LexisNexis's coverage jumped to over 2,000 bankruptcy filings per year (from 450 in 1991) across 70 different bankruptcy districts (of 91). By 1995, LexisNexis covers essentially 100% of all court cases across all bankruptcy districts.[13] 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.

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 LBD. The LBD includes all nonfarm tax-paying establishments in the United States that employ at least one 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 data sets. Importantly, each legal entity of a firm can have a separate EIN, and thus there can be multiple EINs (and multiple bankruptcy filings) for each firm. Furthermore, 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 that belong to 28,000 unique firms.[14]

Table I 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, with 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.[15] These differences are also apparent at the plant level,

[13] Iverson (2016) provides more details on the LexisNexis data.

[14] We provide extensive details on the matching process and sample selection in Internet Appendix Section III.

[15] Census disclosure rules prohibit reporting medians or percentiles, so in Table IV we report the number of firms in different size categories to shed light on the size distribution of our sample.

Table I

Sample Summary Statistics

Panel A of this table presents summary statistics for the plants and firms in our final sample, both overall and separately for firms that are reorganized in Chapter 11 and firms 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. *Market thickness* and *Share of small business loans* are defined in the text. *Share of small business loans* is only available beginning in 1996, leaving 99,000 plants for these summary stats. Panel B presents the industry distribution of our sample and the percent of firms liquidated in each industry. Panel C describes characteristics of the firms that replace 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 two- and three-digit NAICS industry.

Panel A: Summary Statistics

	All	Reorganized	Liquidated
Plant-level characteristics:			
Employment	35.9	38.0	26.9
Total plants	129,000	105,000	24,000
Firm-level characteristics:			
Number of 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
County-level characteristics:			
Market thickness	6.4%	6.4%	6.4%
Share of small business loans	43.8%	43.7%	43.9%

Panel B: Industry Distribution

	Total plants	Total firms	% Liquidated
Agriculture, Mining, and Construction	4,000	3,500	45
Manufacturing	7,000	4,500	38
Transportation, Utilities, and Warehousing	7,000	3,000	43
Wholesale and Retail Trade	62,000	6,500	44
Services	18,000	5,500	37
Accommodation, Food, and Entertainment	16,000	4,000	44
Other	15,000	3,500	32

Panel C: New Entrant Charactristics

	All		Reorganized		Liquidated	
Local versus nonlocal:						
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%
Nonlocal 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 three-digit NAICS	29,000	46.4%	24,000	49.0%	5,000	37.0%
In same two-digit NAICS	34,500	55.2%	28,500	58.2%	6,000	44.4%

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 vs. $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 two regimes.

Panel B of Table I summarizes the industry distribution of plants and firms in our sample. Firms are distributed across all industries, with wholesale and retail trade and services making up the largest portions of the sample.[16] We do not see large differences in the share of firms that are liquidated across sectors, as roughly 40% of firms are converted to Chapter 7 in all industries.

In Section V.B, we explore two measures of heterogeneity in local market characteristics that are related to search frictions: market thickness and access to capital. 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 a given industry (as we show below), we expect counties that contain many firms in an industry, or in other industries that rely on similar assets as the bankrupt plant, will be associated with lower search costs and in turn a higher probability of the vacated real estate finding a new user. We employ the full LBD to construct the market thickness of industry i in county c and year t according to

$$Thickness_{ict} = \sum_j \tau_{ij} s_{jct}, \tag{1}$$

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.[17] Note that τ_{ii}, the probability that a plant remains in a given industry, is substantially higher than any other τ_{ij} for all industries, which implies that it is often difficult to transition an asset to a new industry. Indeed, in weighting by τ_{ij}, this measure of thickness accounts at least in part for two types of asset specificity. First, in some industries the physical characteristics of an asset are not suited for other uses, making reallocation difficult. Second, a given location may be better suited to particular businesses due to its proximity to specific suppliers or customers, as in the case of tech firms in Silicon Valley or retail stores in shopping malls. It follows that $Thickness_{ict}$ is highest when a given county has a high concentration of plants in the same or related industries, where many potential buyers can use the asset for its intended purpose without having to overcome either form of asset specificity. Note that a same county can have both a high thickness measure for one type of asset and a low thickness measure for another, depending on

[16] The number of firms in Panel B sums to more than 28,000 because some firms have plants in multiple industries.

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

the local industrial composition. Interestingly, in Internet Appendix Table I.IX we show that market thickness is quite similar across industries, suggesting that the variation in this measure stems from geographic variation within an industry rather than across industries. In Panel A of Table I, we also show that levels of market thickness are similar for both reorganized and liquidated firms.

We next focus on access to finance as a determinant of asset reallocation, as in Shleifer and Vishny (1992). Because the majority of new occupants of bankrupt assets are local or new firms (as we discuss below), we expect small business loans to 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 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.[18] Specifically, we capture access to capital by using the share of small business loan originations going to small businesses, which we define as firms with less than \$1 million in annual gross revenue.[19] In Panel A, 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.

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, and thus capture whether real estate is occupied (by either a bankrupt firm or a different occupier), and if so, how intensively it is utilized, as indicated by the asset's total employment. 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, and thus reports the users of real estate assets. This allows us to carefully examine asset reallocation over the evolution

[18] 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, Mas, and Nguyen (2014) estimate that CRA-eligible banks accounted for approximately 86% of all loans under \$1 million.

[19] Following Greenstone, Mas, and Nguyen (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 are 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 to small businesses.

of asset occupiers, as well as asset usage, regardless of whether the property is owned or leased.[20]

To track real estate occupancy and employment outcomes over time, we create a careful address matching algorithm to link addresses over time. First, we clean all addresses and address abbreviations using the U.S. Postal Service formal algorithm.[21] Then, for each plant that is shut down, we attempt to match its address with subsequent LBD years (up to five years following the bankruptcy filing), to identify and track the next occupier of the real estate location.[22] We also match to previous years of the LBD up to three years prior to the bankruptcy to verify that the pretrend in employment is unrelated to the judge instrument, as we discussed in Section IV. Our address-matching algorithm forces a perfect match on both zip code and street numbers for each location, and then allows for (almost perfect) fuzzy matching on street name and city name. Details on the address-matching algorithm are provided in Internet Appendix IV.

With these geographical linkages, we categorize each plant outcome as follows. 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 if it is used by a different firm from the original bankrupt occupier, we classify it as "reallocated."[23] 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 shut down and no active plant currently occupies the real estate location.

We also link addresses of nonbankrupt plants in the same county as the bankrupt plants to create a benchmark of plant occupancy and utilization. To do so, we create a 5% random sample of all plants in the LBD in the same county and year as each bankrupt plant, and we track the asset allocation and utilization of this set of over 4 million establishments in exactly the same man-

[20] 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 or the extent to which the assets are used.

[21] Details on the postal addressing standards used are available at the following link (valid as of March 2018): http://pe.usps.gov/text/pub28/.

[22] 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 that 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%.

[23] A real estate location can be sold back to its original owner under a new legal entity, in which case it would be classified as "reallocated" in our data even though it has not truly been reallocated, as we lack the ability to determine if the new legal entity is related to the original owner. Strömberg (2000) shows that these "sale-backs" are relatively common in Sweden. However, in examining 100 randomly selected bankruptcy cases in our sample, we did not find any instances of asset sale-backs in the United States.

ner as the bankrupt establishments. This provides not only a local benchmark against which to compare our results, but also an estimate of local area vacancy rates that can proxy for the opportunity costs of vacancy, as we discuss in detail in Section VI.[24]

B. 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 guide our main analysis in Section V.

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 3. While it is expected that liquidated plants will not continue, noncontinuation is also prevalent in reorganization. Specifically, 70% of Chapter 11 plants continue after one year, 39% by year 3, and 26% by year 5. In comparison, nonbankrupt local plants that are located in the same county have a survival rate of 71% after three years and 59% by year 5.

Panel B of Figure 1 provides evidence on the importance of reallocation in bankruptcy. The figure compares the probability that a location is occupied by a firm across the two bankruptcy regimes. Comparison of this panel with Panel A illustrates the extent to which assets are reallocated. Five years after bankruptcy, only 26% of reorganized plants continue with the original bankrupt firm, but 69% are occupied, which implies that 43% of these locations are reallocated to new users. Meanwhile, the occupancy rate among liquidated plants for both bankruptcy regimes is 55% by year 5, with occupancy of these plants due entirely to reallocation.[25] However, the occupancy rate of both bankruptcy regimes remains below that of the nonbankrupt benchmark for all years. A

[24] Because address matching is inherently imperfect, we conduct a variety of checks, including manually examining matches and comparing to external data, to ensure that our match quality is high and that our results are not dependent on matching issues. These checks are discussed in Internet Appendix Section IV.D. Furthermore, Internet Appendix Table IA.VIII shows that our results are robust to excluding plants for which matching is less precise, such as office buildings or shopping malls with many establishments.

[25] Even after accounting for reallocation, in year 5 vacancy rates are still over 30% among bankrupt firms, and 20% among the local benchmark of nonbankrupt firms. 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 (see http://www.realtor.org/reports/commercial-real-estate-outlook; link valid as of January 2016). Bankrupt firms are more likely to reside in poorly performing regions, and assets may be more likely to be neglected, thus explaining the higher vacancy rates. 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 determined predominately by local factors. Moreover, he finds a significant persistence in vacancy rates in commercial real estate.

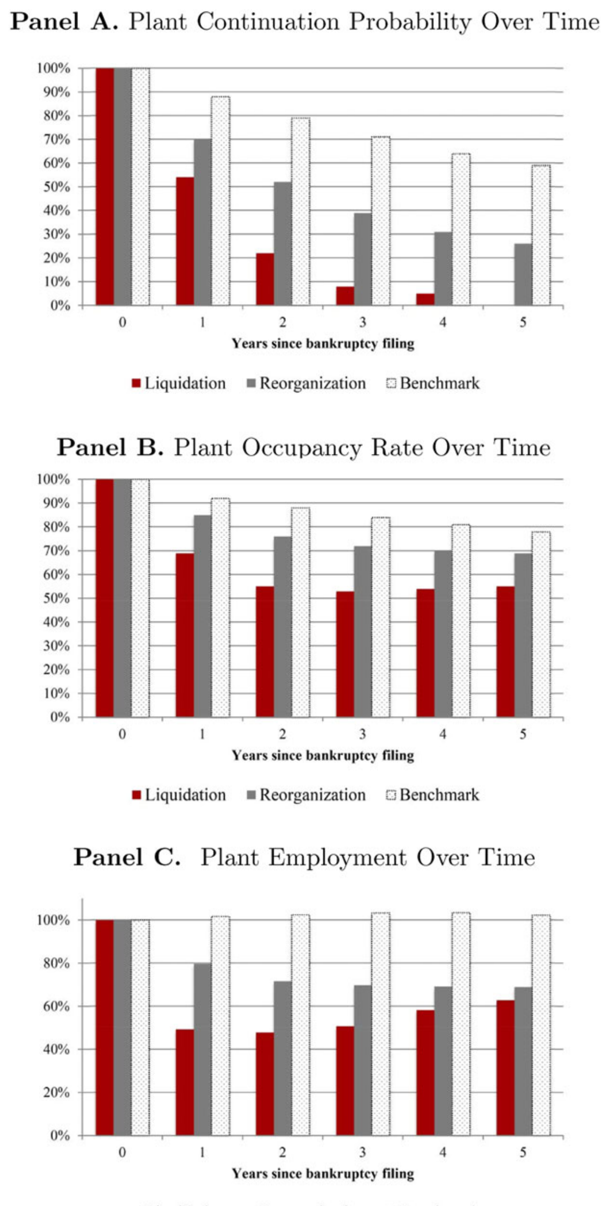

Figure 1. Stylized facts about bankruptcy reallocation. This figure depicts summary statistics on the reallocation process for liquidated, reorganized, and benchmark establishments. The benchmark is created from a random 5% sample of all plants in the same county as the bankrupt establishment. Panel A shows the percentage of plants that continue to be operated by the original firm in the five years following the bankruptcy filing. Panel B shows the share of establishments that are occupied regardless of the owner, and thereby accounts for reallocation to new users. Panel C is similar to Panel B but instead shows total employment as a percentage of employment in year 0. (Color figure can be viewed at wileyonlinelibrary.com)

similar picture emerges when exploring utilization in terms of total employment, as illustrated in Panel C of Figure 1. After accounting for reallocation to new users, employment at reorganized firms drops to about 70% of its prebankruptcy level by year 3 and remains close to that level thereafter. Meanwhile, employment drops quickly at liquidated plants and then recovers to just over 60% of prebankruptcy employment by five years after bankruptcy. These results are strikingly different from those for benchmark plants, where employment grows modestly over time.[26] Of the 3.25 million workers employed at bankrupt locations by year 5, more than two million workers are employed at locations that have been reallocated to new users. Thus, in terms of either occupancy or employment, asset reallocation plays an important role in the utilization of these bankrupt firms' assets.[27]

Stylized Fact 2: Search Costs and Asset Specificity Matter for Reallocation

We find that search costs and asset specificity are important features of the reallocation process in bankruptcy. In Panel C of Table I, we explore 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 one plant in the same county (34.4%). Nonlocal entrants account for only a small fraction (13.6%) of total reallocations. This is especially true for liquidated plants, where new entrants account for 70.4% of all reallocations, and nonlocal entrants make up only 7.4%. We also find a high degree of reallocation within industries, as the probability that reallocated assets remain in the same three-digit NAICS industry is 46.4%. Note that if assets were to randomly transition between industries, the probability of reallocating within the same three-digit industry would be about 2.2%, given the size distribution of industries in the LBD. Interestingly, Panel C also shows that liquidated plants are about 11 percentage points less likely to remain in the same three-digit NAICS than reorganized plants. These results are consistent with the literature documenting the importance of asset specificity and search costs in asset reallocation (Ramey and Shapiro (2001), Eisfeldt and Rampini (2006), Gavazza (2011)), as discussed above.

Stylized Fact 3: Industry and Local Economic Conditions Affect Reallocation

Finally, we find that industry and local economic conditions are important in determining the degree of asset reallocation. Table II reports regression results in which we focus on plants that do not continue with the bankrupt firm

[26] The occupancy rate of benchmark plants falls over time since by definition all benchmark plants are occupied in year 0. However, overall employment increases at benchmark plants as nonvacant establishments grow by more than the decrease in employment at vacant locations.

[27] Relatedly, we find that reallocation, when it takes place, occurs almost immediately in both bankruptcy regimes. Conditional on transitioning to a new user, approximately 65% of plants are reallocated in the same year they are shut down. The probability that real estate is redeployed falls drastically in subsequent years.

Table II

Reallocation Determinants

This table reports results from a regression of a dummy for whether a plant is replaced within five years from bankruptcy filing (conditional on death of original plant) on a set of county and industry (two-digit NAICS) characteristics computed at the year of filing. All county- and industry-level controls are dummy variables equal to one if the county is above-median in the given category. Plant- and firm-level controls identical to those in Table III are also included, but are not reported for brevity. In addition, with the exception of column (4), 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 five years after filing). The sample includes all establishments that died within five 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.

	Plant Reallocation Dummy					
Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)
Local economic conditions:						
No. plants above median	0.029***			0.034***	0.029***	0.028***
	(0.005)			(0.007)	(0.005)	(0.005)
Three-year employment growth above median	0.019***			0.027***	0.019***	0.018***
	(0.004)			(0.005)	(0.004)	(0.004)
Payroll per employee above median	0.032***			0.004	0.032***	0.029***
	(0.006)			(0.009)	(0.007)	(0.006)
Industry economic conditions:						
No. plants above median		−0.012	−0.017*	−0.035*	−0.016*	0.025
		(0.008)	(0.009)	(0.019)	(0.009)	(0.021)
Three-year employment growth above median		0.026***	0.005	0.026*	0.006	−0.004
		(0.007)	(0.009)	(0.015)	(0.009)	(0.009)
Payroll per employee above median		0.006	0.011	−0.011	0.011	−0.000
		(0.009)	(0.010)	(0.019)	(0.009)	(0.014)

(*Continued*)

Table II—*Continued*

	Plant Reallocation Dummy					
Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)
Industry fixed effects						
(Omitted: Agriculture, Mining, and Construction)						
Manufacturing	0.037***		0.025*	0.020	0.028*	
	(0.013)		(0.015)	(0.024)	(0.015)	
Transportation, Utilities, and Warehousing	0.005		−0.006	−0.051*	−0.008	
	(0.018)		(0.020)	(0.029)	(0.019)	
Wholesale and Retail Trade	0.080***		0.087***	0.042**	0.082***	
	(0.013)		(0.014)	(0.021)	(0.014)	
Services	0.091***		0.096***	0.083***	0.086***	
	(0.014)		(0.014)	(0.022)	(0.014)	
Accommodation, Food, and Entertainment	0.087***		0.087***	0.060*	0.082***	
	(0.015)		(0.019)	(0.035)	(0.018)	
Other	0.146***		0.144***	0.143***	0.138***	
	(0.016)		(0.018)	(0.022)	(0.017)	
Plant and firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Two-digit NAICS FE	No	No	No	No	No	Yes
Filing year FE	Yes	Yes	Yes	No	Yes	Yes
Number of years searched FE	Yes	Yes	Yes	No	Yes	Yes
Observations	101,000	101,000	101,000	101000	101,000	101,000
Adj. R^2	0.0952	0.0872	0.0921	0.0295	0.0955	0.0984

and examine what factors affect 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 large total number of plants, high economic growth (as measured by three-year employment growth in a county), and high payroll per employee are significantly correlated with a higher probability that a discontinued plant will be reallocated.

In column (2), we find that industry-level conditions matter as well, as the results show that real estate in high-growth industries is more likely to be reallocated. In column (3), we use industry dummies to illustrate the degree of heterogeneity across industries in reallocation likelihood. We find, for example, that real estate in accommodation, food, and entertainment is much more likely to be reallocated (conditional on plant closure) relative 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), (5), and (6) of Table II, we control simultaneously for county-level and industry characteristics, and we vary the set of fixed effects that we include in the estimation. All county-level characteristics remain highly significant in these regressions as well as the industry fixed effects, but the effect of industry growth rates falls to zero. Overall, the results highlight cross-industry variations in reallocation propensities and the importance of local economic conditions. This motivates our focus on local market conditions, and in particular on the presence of local firms in similar industries, as important determinants of reallocation in bankruptcy.[28]

IV. Identification Strategy

A. Empirical Design

Identifying the effect of Chapter 7 liquidation on asset allocation relative to Chapter 11 reorganization is challenging given the inherent selection into bankruptcy regime. Firms filing directly for Chapter 7 may have worse prospects, which would be reflected in the way their assets are allocated and subsequently utilized. To mitigate selection bias, we focus on firms that filed for Chapter 11 reorganization and exploit the fact that a significant fraction (40%) of these firms are subsequently converted to Chapter 7 liquidation. Hence, the baseline specification of interest is

$$Y_{p,i,t+k} = \alpha + \beta \cdot Liquidation_{p,i,t} + \gamma X_{p,i,t} + \epsilon_{p,i,t+k}, \tag{2}$$

[28] The fact that reallocation is strongly related to local market and industry conditions also supports the validity of our address matching procedure. If the matching were noisy, such strong patterns would not emerge in the data.

where p indexes an individual real estate asset used by firm i, t is the year of the bankruptcy filing, and k indexes the number of years after bankruptcy (ranging from one to five). The dependent variable $Y_{p,i,t+k}$ is a measure of the postbankruptcy plant outcome and real estate asset utilization such as the total number of workers employed at plant p in year $t+k$. We are interested in estimating β, which captures the impact of conversion to liquidation on $Y_{p,i,t+k}$, after controlling for a set of firm- and plant-level variables, X_{pit}, such as prebankruptcy filing employment and plant age. We index $Liquidation_{p,i,t}$ as occurring in year t, since the decision of whether the case is converted to Chapter 7 liquidation or remains in Chapter 11 reorganization is typically taken in the bankruptcy filing year.[29] Under the null hypothesis that liquidation has a 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 I illustrates this point, as firms converted into Chapter 7 liquidation tend to be slightly younger, have a smaller number of plants, and employ fewer workers. Therefore, to identify the causal effect of liquidation on plant outcomes and asset allocation, we use judge heterogeneity in the propensity to convert Chapter 11 filings to Chapter 7 as an instrumental variable.[30] 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, Welch, and Zhu (2006), Chang and Schoar (2013)).

Bankruptcy judges work in 276 divisional offices across the United States, each of which pertains to one of 94 U.S. bankruptcy districts. A firm filing for bankruptcy may choose to file where it is (1) headquartered, (2) incorporated, or (3) does most of its business, which gives the largest firms some leeway in the choice of bankruptcy venue. However, once a filing is made in a particular division, judge assignment is random.[31] We can thus rely on this random assignment as a source of exogenous variation in the probability that a given case is converted, since judges vary in their propensity to convert filings. To

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

[30] This approach was pioneered by Kling (2006), and has been applied in a variety of settings (Doyle (2007, 2008), Chang and Schoar (2013), Di Tella and Schargrodsky (2013), Maestas, Mullen, and Strand (2013), Dahl, Kostøl, and Mogstad (2014), Dobbie and Song (2015), Galasso and Schankerman (2015)).

[31] As an example, consider the bankruptcy district of New Jersey, which is divided into three divisions: Camden, Newark, and Trenton. The Local Rules of the New Jersey Bankruptcy Court lay out exactly which counties pertain to each division, and firms must file in the division "in which the debtor has its principal place of business." Once a case is filed in a particular division, the Local Rules state that "case assignments shall be made by the random draw method used by the Court."

implement the instrumental variables approach, we estimate the following first-stage regression:

$$Liquidation_{p,i,t} = \rho + \pi \cdot \phi_j + \lambda X_{p,i,t} + \delta_{d,t} + \mu_k + \epsilon_{p,i,t}, \tag{3}$$

where $Liquidation_{p,i,t}$ 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, $\delta_{d,t}$, to ensure that we exploit judge random variation within a division-year. We also include plant-level controls, $X_{p,i,t}$, 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 estimate ϕ_j as the share of Chapter 11 cases that judge j converted to Chapter 7, excluding the current case. This standard leave-one-out measure addresses the mechanical relationship that would otherwise exist between the instrument and the conversion decision for a given case and follows previous literature that uses the random assignment of judges as an instrument (see, for example, Doyle (2007), Galasso and Schankerman (2015), among others).[32] The second-stage equation estimates the effect of liquidation on plant outcomes:

$$Y_{p,i,t+k} = \alpha + \beta \cdot \widehat{Liquidation}_{p,i,t} + \gamma X_{p,i,t} + \delta_{d,t} + \mu_k + \epsilon_{p,i,t+k}, \tag{4}$$

where $\widehat{Liquidation}_{p,i,t}$ gives 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 that switch bankruptcy regime because they were randomly assigned a judge that commonly converts cases (Imbens and Angrist (1994)). Clearly, some firms will stay in Chapter 11 no matter the judge and other firms will convert to Chapter 7 regardless of the judge. Thus, the instrumental variable estimates only capture the local average treatment effect on the sensitive firms, and should be interpreted as such. We discuss the set of sensitive firms in our sample below.

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 is illustrated in Figure 2, which plots

[32] In Table IA.II in the Internet Appendix, we show that the results are unchanged if we define the instrument as the share of cases that judge j converted to Chapter 7 in the five years prior to the current case. We also show that judge leniency is highly consistent over time, as judge decisions in the first half of their tenure strongly predict their decisions in the second half of their tenure with a coefficient close to one, as shown in Panel C of Internet Appendix Table IA.II. This further motivates the use of the standard leave-one-out measure of judge leniency in our main analysis.

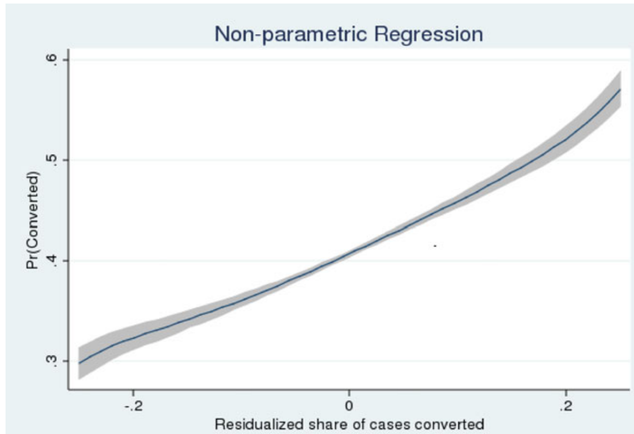

Figure 2. Nonparametric first stage. This figure plots the relationship between the probability of case conversion and our preferred instrument, the share of all other Chapter 11 cases that a judge has converted to Chapter 7, using a nonparametric kernel regression. To be consistent with the regression analysis in the paper, we first residualize the probability of case conversion to all control variables in Table III, including division-year fixed effects. The gray shaded area denotes the 95% confidence interval. For disclosure reasons, we truncate the 5% tails of the distribution. (Color figure can be viewed at wileyonlinelibrary.com)

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 converted, after purging both variables of division-year fixed effects, industry fixed effects, and all control variables in $X_{p,i,t}$. We confirm this evidence in our first-stage regression, presented in Table III, 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 III, 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%, an 18.37% increase from the unconditional propensity of 40.74%.

In the remaining columns of Table III, and in fact in the entire analysis below, observations are 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 to 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 those in column (1), where 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

Table III
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 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.

	Converted to Liquidation		
Dependent Variable:	(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(total employees at firm)	−0.023***	−0.022***	−0.033***
	(0.003)	(0.002)	(0.004)
Ln(number 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
Two-digit NAICS Fixed Effects	Yes	Yes	Yes
Division-year Fixed Effects	Yes	Yes	Yes
Observations	28,000	129,000	129,000
Adj. R^2	0.102	0.165	0.170
F-stat for instrument	107.2	114.9	113.5

the year of the bankruptcy filing.[33] The results remain unchanged. In Table IA.II of the Internet Appendix, we show that the results are robust to alternative instrumental variable specifications. In all specifications, the F-statistic is above 100, well above the required threshold of $F = 10$ to alleviate concerns about weak instruments (Staiger and Stock (1997)).

[33] Surprisingly, $\ln(employeesatplant)$ is positively related to the likelihood of liquidation, while $\ln(totalemployeesatfirm)$ is negatively related to this likelihood. This is because these two covariates are the same for all single-establishment firms in our sample, making them somewhat multicollinear. If $\ln(totalemployeesatfirm)$ is omitted from the regression, the coefficient on $\ln(employeesatplant)$ becomes negative and significant, as expected. However, the instrument is orthogonal to both of these variables, so it is unaffected by either of these controls, and hence we include both to control for plant-level and firm-level characteristics in the second stage.

Another identifying assumption is monotonicity, which requires that the assignment of a judge have 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 that are affected must be affected in the same way. The assumption would be violated if we were to observe that the likelihood of conversion increases for some firms, but decreases for other firms after being assigned to a given judge. This implies that the first-stage estimates should be nonnegative for all subsamples. We estimate the first stage separately for multistate (multicounty) firms and single-state (single-county) firms, since firms in multiple counties or states might be able to "forum shop" for a different bankruptcy venue. As we can see from Table IA.III in the Internet Appendix, the estimates are positive and sizable in all subsamples, in line with the monotonicity assumption. When we test the first stage on further subsamples as we discuss in the next section, we continue to find that the coefficient is positive in all sample splits.

C. Characterizing Marginal Firms in the Bankruptcy System

In this section, we characterize the marginal firms in the bankruptcy system by running the first-stage regression on various subsets of the data. This analysis accomplishes two goals. First, it helps us understand the scope of the local average treatment effects of our analysis, by shedding light on which firms are likely to be sensitive to the instrument. Second, from a policy perspective, it indicates which firms are most affected by a policy change in bankruptcy.

As pointed out by Maestas, Mullen, and Strand (2013), when the treatment variable is binary and the instrument varies between zero to one, the size of the population that is on the margin and sensitive to the instrument is equal to the first-stage coefficient. In our case, the instrument ranges between values of zero and one, where the strictest judges converted 100% of all other cases (excluding the existing case) to liquidation while the most lenient judges did not convert any other cases. Thus, a coefficient of 0.581 (Table III) indicates that 58.1% of all firms would shift from reorganization to liquidation if they were assigned to the strictest judge, relative to the most lenient. Put differently, 58.1% of firms may change their behavior if they encounter a sufficiently strict judge, and thus in our sample 58.1% are sensitive to judge assignment.[34] Building on this insight, by running the first-stage regression on different subsamples we can determine the share of firms in each category that are "compliers," that is, that are sensitive to the judge assigned to the case. Table IV summarizes our findings.

We first split the sample by firm size, where we look at firms with 0 to 5 employees, 6 to 25 employees, 26 to 100 employees, 101 to 1,000 employees, and

[34] Of course, moving from the most lenient to the most strict judge is an extreme shift and is only a hypothetical exercise that does not reflect the actual sample distribution. Because most judges are near the center of the distribution, randomly assigning firms to judges would result in far fewer firms moving between bankruptcy regimes.

Table IV

First-Stage Heterogeneity

This table reports first-stage results for subsamples of the data by size and market characteristics. The first column shows the number of establishments in the group, and the second column shows the number of firms. Column (3) shows the percentage of firms in the subsample that are converted to Chapter 7. Column (4) shows the coefficient on the instrument *share converted* when the first stage is run only on that subsample, with *** denoting statistical significance at the 1% level. These regressions include all control variables and fixed effects as in column (3) of Table III. As discussed in the text, these coefficients can also be interpreted as the share of firms that are sensitive to the instrument. In column (5), we display the F-statistic for the first stage. Finally, column (6) shows the estimated percentage of firms in the group that would always be converted regardless of the judge.

	Observations (1)	No. of Firms (2)	Percent Liquidated (3)	Coefficient on *Share Converted* (4)	F-Stat. (5)	Fraction of Always Takers (6)
Full Sample	129,000	28,000	41	0.581***	114.10	18
Employees in firm:						
0 to 5	8,000	7,000	44	0.492***	19.51	25
6 to 25	11,000	10,500	44	0.559***	33.59	21
26 to 100	11,000	6,500	41	0.880***	66.87	6
101 to 1,000	22,000	3,000	29	0.521***	8.74	9
>1,000	77,000	1,000	13	0.262	1.34	5
Market characteristics:						
Thick markets	64,000	12,500	41	0.557***	41.93	19
Thin markets	65,000	17,000	40	0.603***	77.11	16
High access to capital	50,000	12,000	40	0.664***	56.65	14
Low access to capital	49,000	9,500	43	0.484***	24.23	23

1,000+ employees. Among these groups, the raw share of firms converted to liquidation, as reported in column (3), is 44%, 44%, 41%, 29%, and 13%, respectively, which shows that, even among larger firms, a substantial number are liquidated. In column (4), we report the coefficient on *share converted* from the first-stage regression. With the exception of firms over 1,000 employees, the coefficient is large and highly significant for each subgroup, which demonstrates that a high proportion of firms with fewer than 1,000 employees is sensitive to judge biases. Interestingly, the share of firms that are marginal is nonmonotonic in size. Just under 50% of firms with 0 to 5 employees are sensitive to judge assignment, and this proportion grows to 88% of firms with 26 to 100 employees, before decreasing to 52% of firms with 101 to 1,000 employees and only 26% of firms with over 1,000 employees. It is perhaps unsurprising that few of the largest firms are compliers, as only 13% of these firms are converted to Chapter 7 overall and presumably the stakes are large enough in these cases that judicial preferences are of less consequence.[35] However, we note that our sample contains only 1,000 firms with more than 1,000 employees, and thus there is not much statistical power to identify these effects after the inclusion of division-by-year fixed effects.

We can also estimate the share of firms that would always be converted to liquidation regardless of the judge. We estimate this share of "always takers" by using the first-stage regression coefficients to predict the probability of liquidation if $\phi_j = 0$, meaning that the firm was assigned to the most lenient judge in the sample. We then average the predicted probability of liquidation under the most lenient judge to estimate the share of always takers in each subgroup.[36] The results are both intuitive and interesting. In column (6), we find that the smallest firms have the highest share of always takers, consistent with the smallest firms being the least likely to need Chapter 11 protection. Meanwhile, the fraction of always takers declines substantially for all firms with more than 25 employees.

Combining this with the analysis of the share of compliers in column (4), we can characterize the different size categories as follows. Among firms with fewer than 25 employees, about one quarter are converted to liquidation regardless of the judge and about half are marginal. Nearly all of the middle-sized firms are compliers, with only about 6% being always takers and an additional 6% being "never takers"—firms that would not be converted even under the strictest judge. Meanwhile, hardly any of the largest firms are always takers and a relatively small portion are compliers, leaving a large fraction of never takers.

Table IV also displays first-stage coefficients and the percentage of always takers for the sample splits of market thickness and access to finance. We find little difference in the first stage when we split by market thickness, with a

[35] When firms of this size liquidate, it is more common for them to do so within Chapter 11, rather than converting to Chapter 7.

[36] The proportion of firms that would not be converted even under the strictest judge ("never takers") can be easily calculated as 1 – (% always takers) – (% compliers). For example, in the full sample, we estimate that 18% are always takers, 58% are compliers, and 24% are never takers.

55.7% (19%) share of compliers (always takers) in thick markets compared to 60.3% (16%) in thin markets. We see slightly larger differences when we split the sample by access to finance, with 66.4% of firms sensitive to the judge assignment in markets with high access to finance (14% always takers) compared to 48.4% in low access to finance areas (23% always takers).

D. 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, satisfy the exclusion restriction condition. Specifically, it is required that judge assignment only affect the outcomes of interest (e.g., whether a plant location is occupied five years after bankruptcy filing) via its effect on the probability that a case is converted to liquidation. An important threat to our empirical strategy is the possibility that less lenient judges are nonrandomly assigned to bankruptcy cases in which the firm has bleak prospects. As evidence in support of our identification assumption, Table V reports results of 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 V 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 both between divisions and over time. In the next column, we explore whether within a division-year such variation is correlated with bankruptcy case characteristics by adding controls for plant size and age, firm size, an indicator for multiple associated bankruptcy filings, and industry fixed effects. None of these variables are statistically significant and the R^2 is unaffected by their addition. In column (3), we include pretrends in employment at the bankrupt plant for three years prior to the bankruptcy filing. These employment figures are calculated using the address matching algorithm discussed in Section III.A to calculate total employment at a location prior to bankruptcy even if it was not owned by the bankrupt firm. As can be seen, we find no evidence that judge leniency is correlated with these pretrends. We provide further evidence for the lack of pretrends in Figure 3, which displays the reduced-form results in the years around the bankruptcy filing. In all cases, ϕ_j is uncorrelated with utilization in the years prior to the bankruptcy filing.

The next columns in Table V explore whether local market conditions are correlated with the instrument. In columns (4), (5), and (6), we separately add dummy variables indicating whether a plant was in a county with above-median market thickness (as defined in Section II), share of small business loans (also defined in Section II), or three-year cumulative employment growth prior to the bankruptcy. In column (7), we add all three measures together. In none of these specifications are any of these measures statistically significant. In column (8), we also add variables that capture local economic activity and

Table V

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 regressions are at 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) to (8). Plant-level controls include employment at the location in the three years prior to bankruptcy, in addition to the standard controls in Table III. 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.

	Share Converted							
Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Plant- and firm-level controls:								
Ln(employees at plant)$_0$		0.0002	−0.0001	−0.0001	−0.0001	−0.0001	−0.0001	−0.0001
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Ln(employees at plant)$_{-1}$			0.0004	0.0004	0.0004	0.0004	0.0004	0.0004
			(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ln(employees at plant)$_{-2}$			−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(employees at plant)$_{-3}$			0.0003	0.0003	0.0003	0.0003	0.0003	0.0003
			(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Plant age (years)		−0.0000	−0.0001	−0.0001	−0.0001	−0.0001	−0.0001	−0.0001
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ln(total employees at firm)		0.0009	0.0010	0.0010	0.0010	0.0009	0.0009	0.0010
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Ln(number of plants at firm)		−0.0012	−0.0012	−0.0012	−0.0012	−0.0012	−0.0012	−0.0012
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)

(Continued)

Table V—*Continued*

	Share Converted							
Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Part of a group filing		0.0014	0.0014	0.0014	0.0014	0.0014	0.0014	0.0014
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Dummy =1 if above median:								
Heterogeneity measures:								
Market Thickness				0.0001			0.0000	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)
Three-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)
Number of plants in industry								0.0061
								(0.004)
Payroll per employee in industry								0.0006
								(0.002)
Three-year employment growth in industry								−0.0015
								(0.001)
Two-digit NAICS Fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Division-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-stat for joint significance of industry FE		0.791	0.786	0.791	0.791	0.785	0.794	0.766
Observations	129,000	129,000	129,000	129,000	129,000	129,000	129,000	129,000
Adj. R^2	0.777	0.777	0.777	0.777	0.777	0.778	0.778	0.778

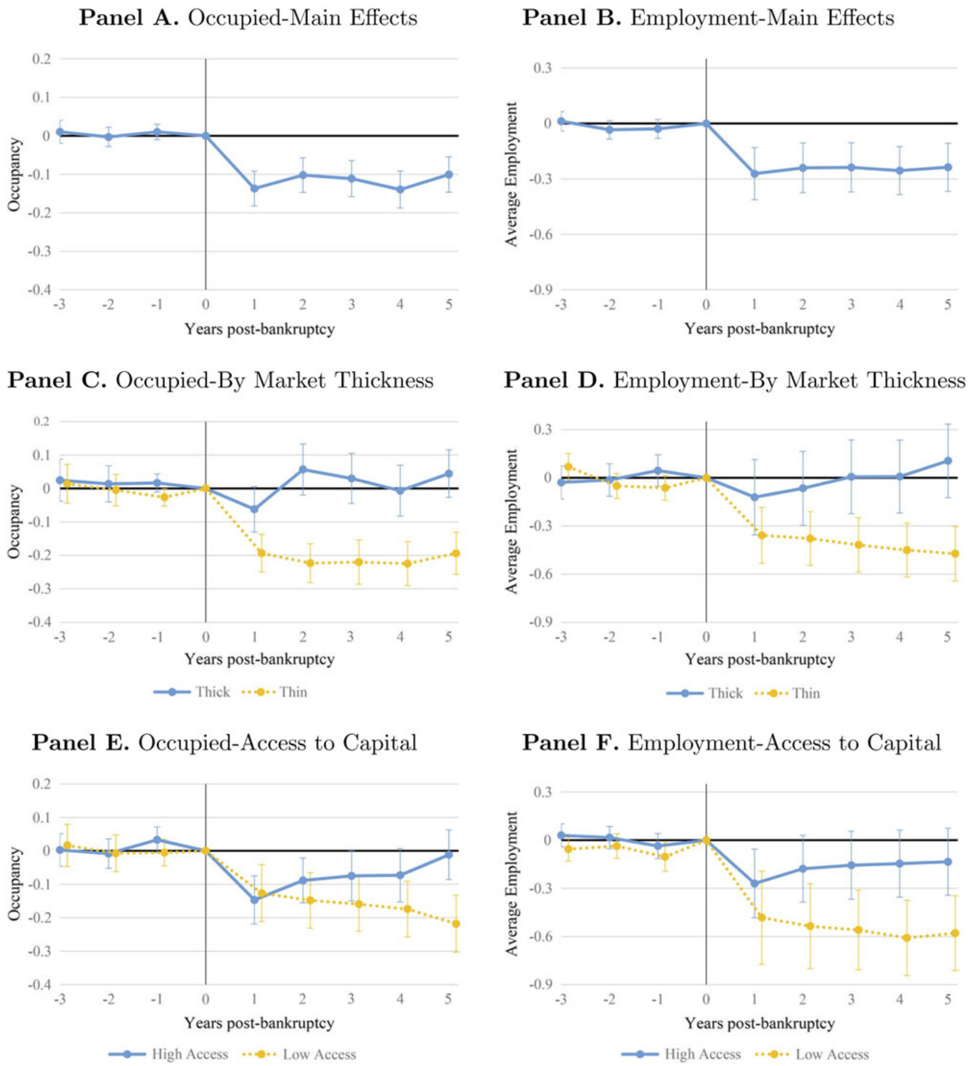

Figure 3. Reduced-form dynamics. This figure plots coefficient estimates of the effect of judge instrument on occupancy rates (left column) and average employment (right column) both before and after bankrutpcy. In Panels A and B, coefficient estimates β are from the reduced-form regressions $Y_{p,i,t+k} = \alpha + \beta \cdot \phi_j + \gamma X_{p,i,t} + \delta_{d,t} + \mu_k + \epsilon_{p,i,t+k}$, where ϕ_j is the judge leniency instrument, and specifications contain the full set of control variables in column (3) of Table III, including division-by-year fixed effects. In the remaining panels, coefficient estimates displayed are β_L and β_H, which are derived from the reduced-form regressions $Y_{p,i,t+k} = \alpha + \beta_L \cdot \phi_j \cdot Low_{p,t} + \beta_H \cdot \phi_j \cdot High_{p,t} + \gamma X_{p,i,t} + \delta_{d,t} + \mu_k + \epsilon_{p,i,t+k}$, where the instrument ϕ_j is interacted with dummy variables $Low_{p,t}$ and $High_{p,t}$, which indicate whether a plant resides in a county with low or high market thickness (Panels C and D) or access to finance (Panels E and F). All control variables and fixed effects are also interacted with $Low_{p,t}$ to allow for flexible estimates across market types. Standard-error bars, based on clustering at the division-year level, are also displayed. (Color figure can be viewed at wileyonlinelibrary.com)

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. Taken together, the evidence in Table V suggests that there is indeed random assignment of judges to bankruptcy filings within court divisions, which alleviates 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. 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 is systematically 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, which implies that we cannot separate the law from the way it is implemented. In this case, the liquidation treatment should be viewed not as just the motion to convert to Chapter 7, but rather more broadly 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 this broader interpretation is warranted.

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

$$Y_{p,i,t+k} = \alpha + \beta \cdot \phi_j + \gamma X_{p,i,t} + \delta_{d,t} + \mu_k + \epsilon_{p,i,t+k}. \tag{5}$$

These regressions, reported in Table IA.IV in the Internet Appendix, show that there is a strong relationship between the instrument, ϕ_j, and $Y_{p,i,t+k}$ 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 outside of the bankruptcy regime, then ϕ_j should also affect $Y_{p,i,t+k}$ when limiting the sample to firms that remain in reorganization, or to firms that are liquidated. As reported in Table IA.V in the Internet Appendix, however, when we run reduced-form regressions on these two subsets of firms, we find no significant relationship between the instrument and plant outcomes. In column (7) of Table IA.V, we also find that, within Chapter 11 reorganization, ϕ_j is uncorrelated with bankruptcy refiling rates, a proxy for bankruptcy resolution success that may depend on judge skill. This suggests that other judge characteristics or tendencies that may be correlated with ϕ_j do not affect plant outcomes outside of the bankruptcy regime.

To shed further light on this result, we examine whether judges have a large effect on bankruptcy cases before making a decision on whether to convert a case. Based on a random sample of 200 cases, we find that the median time between the bankruptcy filing and the selection of the bankruptcy regime (either liquidation or reorganization) is only four months. In addition, we find that typically no significant motions are passed in the case prior to a ruling on a motion to convert the case. Last, we also note that Chang and Schoar (2013), who use detailed data on court motions to perform principal component analysis on the most important rulings of a bankruptcy judge in an effort to identify pro-debtor judges, find that the motion to convert a case receives by far the lowest weight in the first principal component. This suggests that the decision to convert may be at most weakly 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 asset allocation and utilization.

V. Results

A. Main Results

We begin by focusing on how liquidation affects reallocation and utilization in the full sample by testing its impact on four main outcome variables. First, *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. This variable captures the extent to which liquidation forces more discontinuation than reorganization. Related to *Continues* is *Holding Time*, which we define as the number of years until a plant is no longer occupied by the original bankrupt firm (either reallocated to a new user or becomes vacant).[37] This variable captures the extent to which liquidation accelerates the discontinuation of a plant versus reorganization. Finally, we examine two measures of real estate asset utilization, regardless of who the occupant is: *Occupied* is an indicator equal to one if the asset is occupied five years after the bankruptcy filing and ln*(average employment)* is the average employment at a specific location over the five years after the bankruptcy filing. Because vacant establishments by definition have zero employment and payroll, this employment measure accounts for any interim years in which a plant is not occupied, even if it is occupied in year 5. Furthermore, it has the advantage of accounting for the intensive margin of employment as well as the extensive margin, since it includes plants that are reallocated but have fewer employees. For both measures of utilization, the geographical linkages discussed in Section III.A allow us to account for reallocation of assets to new users.

Table VI shows both OLS and 2SLS estimates of the impact of liquidation on these plant outcomes.[38] These regressions include all controls in column (3) of Table III, 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

[37] We cap *Holding Time* at five years to match the horizon in other variables. However, the results are unchanged if we set it to 7 or 10 years for these plants.

[38] 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.

Table VI

Liquidation and Plant Outcomes

This table reports regression results showing the effect of liquidation on four plant outcomes five years after the bankruptcy filing. *Continues* is an indicator equal to one if the plant has at least one employee and is still owned by the original bankrupt firm five years after the bankruptcy filing. *Holding Time* is the number of years after bankruptcy (from 0 to 5) until a plant is not occupied by the original bankrupt firm. *Occupied* is an indicator equal to one if the plant has at least one employee regardless of the occupant. *Average employment* is the mean number of employees at the plant over the five years after the bankruptcy filing (similar results for payrolls are presented in the Internet Appendix). For all four dependent variables, we display regular OLS and 2SLS estimates. All specifications contain the full set of control variables in column (3) of Table III, 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.

	Continues		Holding Time		Occupied		Ln(avg. employment)	
Dependent Variable:	OLS	IV-2SLS	OLS	IV-2SLS	OLS	IV-2SLS	OLS	IV-2SLS
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Liquidation	−0.300***	−0.324***	−1.773***	−1.657***	−0.156***	−0.174**	−0.565***	−0.416*
	(0.005)	(0.061)	(0.024)	(0.297)	(0.007)	(0.079)	(0.019)	(0.217)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Div × 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^2	0.230	0.152	0.288	0.211	0.130	0.039	0.295	0.214

bankruptcy filing. The 2SLS estimates in column (2) show that exogenously converting a firm to liquidation reduces the probability of continuation, with a magnitude of 32.4%. In addition, columns (3) and (4) show that asset holding time is on average 1.7 years shorter in liquidation. These results are somewhat mechanical, since liquidation forces discontinuation while reorganization does not, and thus it is not surprising that it causes more discontinuation and speeds up the time until the asset is no longer used by the bankrupt firm. However, these findings are useful for three reasons. First, they confirm that liquidation does indeed cause an increase in shutdowns. Second, they demonstrate that reorganization leads to a significant amount of discontinuation as well. Indeed, given that liquidation forces discontinuation, it is somewhat surprising that only 32.4% more firms are discontinued in liquidation than reorganization. Third, the holding time result shows that forced sales do accelerate discontinuation, as expected.

The main findings of the paper relate to the utilization of the location regardless of the owner, as captured by occupied and ln*(average employment)* in columns (5) to (8) of Table VI. Across all specifications, we find that liquidation leads to a significant decline in asset utilization five years after the bankruptcy filing. 2SLS estimates show that liquidation reduces occupancy rates by 17.4%, an effect that is both statistically and economically significant.[39] This estimate is roughly half the size of the 32.4% decline in plant continuation, demonstrating that reallocation to new users closes some of the gap between liquidation and reorganization, but not entirely.

The magnitude of the decline is even larger when measuring by employment, which is estimated at 34% in column (8).[40] This suggests that liquidation not only reduces occupancy rates on average (the extensive margin), but also employment, which proxies for the extent to which an occupied asset is used.[41]

While Table VI focuses on outcomes in year 5 after the bankruptcy, the effect of liquidation on occupancy and employment is apparent quite quickly after bankruptcy. This can be seen in Figure 3, which plots coefficients from reduced-form regressions over a time horizon from three years prior to five years after the bankruptcy filing. As shown in Panels A and B, liquidation leads to a decline in both occupancy and employment in the first year after

[39] 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 affect OLS estimates is ex ante unclear. On the one hand, poorly performing firms are more likely to be converted, and their assets are less likely to be reallocated, which would bias OLS coefficients downward. On the other hand, firms with assets that are easily redeployed may be more likely to move to liquidation, which would bias OLS coefficients upward. Results in Table VI suggest that these two effects largely balance each other out, so that the OLS estimates are similar to the 2SLS estimates.

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

[41] In fact, focusing on manufacturing firms only, we find that liquidation reduces productivity as reported in Table IA.XII in the Internet Appendix. However, this analysis is fairly suggestive due to data limitations, as we discuss in Section I in the Internet Appendix.

bankruptcy, with this decrease quite stable over time. However, these results mask considerable heterogeneity across markets, which we discuss in the next section.[42]

Taken together, the results show that the bankruptcy regime significantly affects asset allocation and subsequent utilization. In liquidation, plants are more likely to be discontinued, as expected, but these assets are not fully reallocated, such that assets in liquidation exhibit lower utilization relative to reorganization, as measured by either occupancy or employment.

B. The Role of Market Thickness and Access to Finance

The results presented so far show that liquidation causes significantly faster discontinuation and lower asset utilization 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 two local market characteristics (described in Section II) that theory predicts to affect asset reallocation: market thickness and access to finance.

In Table VII, we split the sample based on the market thickness, when we define "thick" industry-county-year triplets as those having above-median $Thickness_{ict}$, and we run our IV specifications separately for plants in thick and thin markets. We also report results from tests in which we interact *liquidation* with this *thick market* dummy, where we interact *share converted* with *thick market* in the first stage so that we have two endogenous variables and two instruments. To allow full flexibility, we interact all other covariates with *thick market* as well. Under this setup, the coefficient on *liquidation*thick* market tests whether the effect of liquidation is significantly different in thick markets versus thin markets.[43]

The first three columns of Panel A show that, in both thick and thin markets, liquidation reduces the probability that a plant will continue with the original bankrupt firm by a similar amount. However, columns (4) to (6) show that this discontinuation is faster with liquidation in thin asset markets. In thick markets, reorganized plants are held by the original owner for 1.4 more years than liquidated plants, while in thin markets the gap is 29% larger at 1.8 years.[44]

Panel B of Table VII shows that the differences in asset utilization between thick and thin markets are stark. In column (1), we find that asset reallocation

[42] Internet Appendix Table IA.VII, which reports dynamic 2SLS coefficient estimates instead of reduced-form coefficients, also shows that liquidation leads to lower overall wages at the plant level.

[43] Note that we do not claim that plants are exogenously distributed across thick or thin markets. By running the regressions on separate subsamples (or fully interacted), 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 drive the overall effects.

[44] While the difference in holding times is economically important, the gap is not statistically significant.

Table VII

Heterogeneous Effects of Utilization: Market Thickness

This table shows how the effects of liquidation vary with the thickness of the local asset market. Using our measure of market *thickness* (defined in the text), we divide the sample into plants in thick counties (above median) or thin counties (below median). We then present, in columns (1), (2), (4), and (5), regression results similar to those in Table VI for each subsample. In columns (3) and (6), we interact *liquidation* and the indicator for *above-median market thickness* and run the regression on the full sample to determine if liquidation's impact is significantly different in thick markets. For these interacted regressions, in the first stage we interact the instrument *share converted* and the *above-median* indicator to generate two instruments for the two endogenous variables. Furthermore, all control variables and fixed effects are interacted with *above-median market thickness*. Panel A focuses on the two dependent variables that have to do with plant continuation: *continues* and *holding time*. Panel B displays the results for the two measures of utilization: *occupied* and *ln(average employment)*. All dependent variables are measured five years after bankruptcy and are defined as in Table VI. All regressions are estimated using 2SLS and contain the full set of control variables in column (3) of Table III, 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 Continuation

Dependent Variable:	Continues			Holding Time		
	Above	Below	Interacted	Above	Below	Interacted
Above or Below Median:	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation	−0.337***	−0.321***	−0.321***	−1.449***	−1.836***	−1.836***
	(0.101)	(0.076)	(0.076)	(0.492)	(0.360)	(0.360)
Liquidation* Above median		−0.016				0.387
		(0.127)				(0.609)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Div × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	64,000	65,000	129,000	64,000	65,000	129,000

(Continued)

Table VII—*Continued*

Panel B: Utilization						
	Occupied			Ln(Avg. Employment)		
Dependent Variable:	Above	Below	Interacted	Above	Below	Interacted
Above or Below Median:	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation	0.080	−0.324***	−0.324***	0.190	−0.790***	−0.790***
	(0.129)	(0.109)	(0.109)	(0.413)	(0.278)	(0.278)
Liquidation* Above median		0.404**			0.980**	
		(0.169)				(0.498)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Div × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	64,000	65,000	129,000	64,000	65,000	129,000

in thick markets allows the occupancy rate of liquidated plants to be similar to that of reorganized plants, despite the high level of discontinuation in liquidation shown in Panel A. 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 (2) 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.[45] Column (3) shows that the difference between the two markets is statistically significant. Similarly, in comparing columns (4) and (5) we find that in thick markets liquidation does not have a significant effect on average employment, but in thin markets liquidation reduces employment significantly. Again, this difference between the two markets is statistically significant. Overall, the effect of liquidation on asset utilization is concentrated entirely in thin markets, while reallocation in thick markets results in liquidation having no impact on utilization.[46]

Similar to market thickness, access to finance can affect asset allocation and utilization in bankruptcy regimes by limiting the set of potential users (Shleifer and Vishny (1992)). In Table VIII, we present regression results similar to Table VII, except here we split our sample based on access to capital in the local market. 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.[47] In all cases, we find similar results to those for market thickness. Specifically, in markets with low access to finance, liquidation reduces holding times by two years, while in areas with high access to finance, the gap is 1.4 years. Furthermore, we see no decline in occupancy or employment in markets with high access to finance, but 45% lower occupancy and 69% lower employment in areas with low access to capital. This supports

[45] 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.

[46] These results are robust to using an alternative measure of market thickness, namely, 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 the CoreLogic data set by dividing the total number of real estate transactions in a county by the county population. The results are discussed in detail in the Internet Appendix, and reported in Panel A of Table IA.XI in the Internet Appendix.

[47] Loan data come from the CRA disclosure data and are only available beginning in 1996, which removes from our sample about 30,000 plants that filed for bankruptcy prior to 1996. Note that the CRA data are 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. However, 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.

Table VIII
Heterogeneous Effects of Utilization: Access to Capital

This table shows how the effects of liquidation vary with the access to small business finance in the local market. Counties with an above-median share of bank loans going to small firms (firms with less than $1 million in annual revenue) are defined as having high access to capital, while below-median counties have low access to capital. We then present, in columns (1), (2), (4), and (5), regression results similar to those in Table VI separately for plants in above- and below-median counties. In columns (3) and (6), we interact *liquidation* and the indicator for *above-median access to capital* and run the regression on the full sample to determine whether liquidation's impact is significantly different in markets with high access to capital. For these interacted regressions, in the first stage we interact the instrument *share converted* and the *above-median* indicator to generate two instruments for the two endogenous variables. Furthermore, all control variables and fixed effects are interacted with *above-median access to capital*. Panel A focuses on the two dependent variables that have to do with plant continuation: *continues* and *holding time*. Panel B displays the results for the two measures of utilization: *occupied* and *ln(average employment)*. All dependent variables are measured five years after bankruptcy and are defined as in Table VI. All regressions are estimated by 2SLS and contain the full set of control variables in column (3) of Table III, 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 Continuation						
	Continues			Holding Time		
Dependent Variable:	Above	Below	Interacted	Above	Below	Interacted
Above or Below Median:	(1)	(2)	(3)	(4)	(5)	(6)
Liquidation	−0.273***	−0.341***	−0.341***	−1.402***	−1.999***	−1.999***
	(0.083)	(0.126)	(0.126)	(0.413)	(0.569)	(0.569)
Liquidation* Above median		0.069				0.596
		(0.151)				(0.703)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Div × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	50,000	49,000	99,000	50,000	49,000	99,000

(Continued)

Table VIII—*Continued*

Panel B: Utilization						
	Occupied			Ln(avg. employment)		
Dependent Variable: Above or Below Median:	Above (1)	Below (2)	Interacted (3)	Above (4)	Below (5)	Interacted (6)
Liquidation	−0.018	−0.450**	−0.450**	−0.206	−1.197**	−1.200**
	(0.111)	(0.193)	(0.193)	(0.310)	(0.480)	(0.480)
Liquidation* Above median		0.432*				0.992*
		(0.223)				(0.571)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Div × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	50,000	49,000	99,000	50,000	49,000	99,000

the theory that search costs, and access to capital in particular, are key factors determining the ability to reallocate bankrupt assets.[48]

In Panels C to F of Figure 3, we examine how the effect of liquidation evolves dynamically in markets with high and low search costs. These panels plot the coefficients from reduced-form regressions where we interact the instrument with dummy variables for high and low search costs for market thickness and access to finance. Importantly, we find no pretrends in occupancy or employment for any of these sample splits. In the postbankruptcy period, Panels C and D show that liquidation in thick markets causes an initial drop in utilization in year 1, but this gap disappears by year 2, consistent with low search costs facilitating asset reallocation after the liquidated firm is shut down. However, the drop in utilization caused by liquidation slightly increases over time in thin markets, where search costs are high. A similar pattern emerges in Panels E and F, where we split by areas with high and low access to finance.[49]

Whether we proxy for search costs using market thickness or access to finance, we find support for the potential implications of search frictions on asset allocations. Importantly, market thickness is uncorrelated with access to finance, as shown in Internet Appendix Table IA.X, which suggests that each channel is separate and exerts a significant effect individually. Moreover, Table IA.X shows that both measures are uncorrelated with local employment growth, hence do not seem to reflect changes in local economic conditions. These results are robust to using alternative measures of market thickness and access to capital, as described in Internet Appendix Section I.

C. External Validity and Economic Importance

While our results show that liquidation has a large effect on the long-term utilization of bankrupt locations, the aggregate economic importance of these findings could be small if only the smallest firms are liquidated or if few firms are at the margin between the two bankruptcy procedures. In this section, we discuss the external validity of our results and their overall economic importance.

In Section IV.C, we show that over 40% of firms with fewer than 100 employees are converted to liquidation, and even among firms with 100 to 1,000 employees 29% are liquidated. Furthermore, we estimate that moving from the most lenient to the most strict judge would shift between 49% and 88% of all firms with fewer than 1,000 employees to liquidation, suggesting that the majority of firms with fewer than 1,000 employees are marginal in the sense

[48] We find similar results when using an alternative measure of local access to finance, namely, the share of bank deposits in a county held by 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 small local banks is likely to result in higher access to capital to small businesses. We discuss the construction of this measure in the Internet Appendix, and report the results in Panel C of Table IA.XI.

[49] For robustness, Internet Appendix Figure IA.1 displays similar results using a binary version of our instrument in reduced-form regressions with no other controls or fixed effects.

that whether they end up in reorganization or liquidation depends on judge assignment. This result supports Bris, Welch, and Zhu (2006, p. 1261), who find that there is considerable overlap in the size of Chapter 7 and Chapter 11 firms, and that "a good number of firms could have chosen either procedure."

Firms with fewer than 1,000 employees also constitute a significant portion of the U.S. economy. Using data from the full LBD, we find that firms with fewer than 1,000 employees comprise 99.8% of all firms between 1992 and 2005 (our sample period). These firms employ 55% of all workers and, more importantly for this study, occupy 85% of all business locations in the United States. Thus, a large portion of the assets in the economy are held by firms that could marginally be placed in liquidation or reorganization.

A second important point is that many firms in the United States file directly for Chapter 7. According to U.S. court filing statistics, direct-to-Chapter-7 filings account for 72% of all nonfarm business bankruptcies, while Chapter 11 filings make up the remaining 28%.[50] For identification purposes, our sample consists only of firms that file for Chapter 11 (of which 40% are subsequently converted to Chapter 7), but to the extent that all firms in Chapter 7 face search costs when liquidated, our results can be interpreted more broadly.

VI. Efficiency Discussion

While the empirical results in the paper demonstrate that liquidation leads to lower utilization in thin markets and in markets with low access to finance, the question remains whether this gap in utilization implies that liquidation or continuation of reorganized firms is inefficient. Liquidation may lead to underutilization if reallocation is impeded by high search costs due to thin markets (Williamson (1988), Gavazza (2011)) or financial constraints (Shleifer and Vishny (1992)). Meanwhile, continuation through reorganization is inefficient if agency costs induce the incumbent firm to maintain control over assets that could be better employed elsewhere (Franks and Torous (1989), Gertner and Scharfstein (1991), Hotchkiss (1995), Bolton and Scharfstein (1996)). In this section, we first examine whether our empirical results are consistent with inefficient continuation through reorganization. We then conduct additional tests that explore whether our results arise from inefficient liquidation.[51] Importantly, this discussion focuses on the efficiency of the ex post asset allocation of each bankruptcy procedure. Other costs and benefits of liquidation and reorganization, including legal fees, creditor recovery rates, and worker outcomes,

[50] In Internet Appendix Table IA.I, we present summary statistics for a sample of firms that filed directly for Chapter 7 that we match to the LBD. The average Chapter 7 firm employs 20.4 workers and occupies 1.4 locations, which is significantly smaller than the average Chapter 11 firm but not negligible in aggregate.

[51] Section II in the Internet Appendix contains a model of asset allocation between firms that includes both search costs, which can lead to inefficient liquidation, and agency costs, which can lead to inefficient continuation in reorganization. We use this model to theoretically derive the conditions under which liquidation or reorganization are inefficient. We informally discuss these conditions and their related empirical predictions in this section.

play an important role in the overall welfare implications of each bankruptcy regime but are not considered in this paper.

A. Inefficient Continuation

Reorganization may lead to inefficient continuation if assets in reorganization are prevented from being reallocated to better uses due to agency costs. For example, if managers of the bankrupt firm derive private benefits from continuing the firm, they may prevent the firm from reallocating the assets to new users even if this would result in higher asset utilization. Empirically, a strong continuation bias would suggest that the assets of a firm that is randomly forced to liquidate would see higher utilization subsequently. Our main results in Table VI are inconsistent with this prediction, as liquidation leads to lower utilization.

Importantly, agency costs and search costs exist simultaneously. If this were the case, it could be that search costs may prevent the reallocation of liquidated assets, which would mask the presence of a continuation bias in reorganization. That is, we expect asset utilization to increase after liquidation only if there is a strong continuation bias and liquidated assets can be easily reallocated to new uses. The predicted effect of liquidation on utilization rates therefore depends on the size of the continuation bias in reorganization relative to the magnitude of the search costs in liquidation.

To test for a continuation bias, one needs to identify markets in which search frictions are low and hence asset reallocation is relatively easy. This should be the case when asset markets are thick (Williamson (1988), Gavazza (2011)), and when potential users have better access to capital (Shleifer and Vishny (1992)). Panel B of Table VII shows that even in thick asset markets, where reallocation should be relatively easy, we fail to find an increase in asset utilization when firms are randomly liquidated. Similarly, Panel B of Table VIII shows that liquidation does not lead to significantly higher utilization in markets with high access to capital.[52] Thus, our empirical evidence is inconsistent with a strong continuation bias among the firms in our sample.[53]

It is important to note a few caveats that prevent us from drawing strong conclusions about the inefficiency of continuation. First, because we cannot measure efficiency directly, we rely on measures of utilization as proxies. Generally

[52] In Table IA.XIII of the Internet Appendix, we also explore the effects of liquidation on asset utilization in markets that are in the top quartile of the interaction of high market thickness and high access to capital. Even in these markets, where search frictions are least likely to occur, we fail to find that liquidation increases asset utilization.

[53] We should note that if the variables capturing market thickness or access to capital are positively correlated with local economic growth, this should bias our analysis toward finding higher utilization after forced liquidation in such markets. The fact that we do not find such evidence further argues against continuation bias. Table IA.X in the Internet Appendix shows that while the correlation of both measures with local employment growth is positive, it is very small and close to zero. Hence, these measures do not seem to reflect changes in local economic conditions.

speaking, as long as productivity and utilization are complements, utilization is a good proxy for efficiency. However, there can be cases in which a decrease in employment at a location may be efficient, for example, when the best use of a location is as a storage facility or a server farm with few employees, or when the current owners have a bias toward overutilization of an asset (i.e., a preference to employ more workers than the optimal). To the extent that these cases are the norm rather than the exception, our results would be evidence in favor of rather than against a continuation bias. It is important to keep in mind, however, that our results hold when looking at the extensive margin of occupancy versus vacancy, and thus there would need to be many instances in which vacancy is more efficient than occupancy to draw this conclusion. In addition, Table IA.XII in the Internet Appendix shows that liquidation leads to a reduction in productivity at manufacturing establishments.

A related issue is that our proxy for market thickness is an imperfect indicator of markets in which search frictions are low. It is possible that there are no markets in which search costs are low enough to identify a continuation bias, or that the measures of market thickness and access to finance are too imprecise to identify such areas, which limits our ability to rule out inefficient continuation. In addition, we note that the standard errors in column (4), Table VII, Panel B are quite large, and thus it is possible that a continuation bias exists but the data are too noisy to distinguish it. The estimates with respect to occupancy are more accurate.

B. Inefficient Liquidation

While the evidence above suggests that continuation bias is unlikely to be large, this does not necessarily mean that liquidation is inefficient, even if it leads to lower utilization. From an empirical standpoint, determining whether an asset is efficiently vacant or underutilized is challenging because of the difficulty in measuring its opportunity cost. For example, when a firm is randomly liquidated and its location is left vacant, if an identical store could open in a nearby location, then the opportunity cost of vacancy would be low. Thus, if liquidation causes lower utilization only in areas where the opportunity cost is low, then the impact on overall economic efficiency is likely negligible.

While measuring the opportunity cost of vacancy directly is difficult, the example above suggests a natural proxy: the utilization of nonbankrupt local establishments. We posit that the opportunity cost of leaving a location vacant depends on the overall vacancy rate of the local area.[54] In areas with low vacancy rates, the opportunity cost of leaving a location vacant should be high since there is relatively high demand for real estate. Meanwhile, areas with high vacancy should have low opportunity costs of vacancy since firms may have many alternative locations to move into. According to this argument, evidence

[54] Grenadier (1995, 1996) finds that commercial real estate equilibrium vacancy rates are determined predominately by local factors. Moreover, he documents a significant persistence in local vacancy rates.

that liquidation causes a drop in utilization even in areas with low vacancy (high opportunity cost) would provide evidence consistent with the inefficiency of liquidation.

We measure local area occupancy rates by randomly selecting a 5% sample of all establishments in the LBD in the same county as the bankrupt establishment and using the same address matching procedure described in Section III to track their utilization in the five years after the bankruptcy filing.[55] Importantly, we note that this measure of local asset utilization is distinct from the measure of market thickness defined in Section II. While market thickness measures the presence of other likely users of a given asset in the county, the measure of local occupancy rates simply captures the extent to which existing nonbankrupt commercial real estate assets are utilized. Indeed, the correlation between market thickness and benchmark utilization is only 0.05.

In Figure 1, we compare the utilization of bankrupt locations relative to the average local utilization of nonbankrupt plants, which proxies for the opportunity cost of vacancy. As can be seen, bankrupt assets are less likely to be occupied and utilized relative to average local plants in all five years after the bankruptcy. More importantly, liquidation is associated with significantly lower utilization relative to reorganization. This evidence points indirectly toward the inefficient use of the firm's assets relative to the local characteristics.[56]

In Table IX, we directly test whether liquidation leads to vacancy only when opportunity costs are low. First, in column (1) of Panel A, we show that our main 2SLS specification (in Table VI) remains unaffected when we control for an indicator equal to one if the bankrupt establishment is in a county with high (above-median) local occupancy rates. As expected, high local occupancy rates strongly predict the utilization of the bankrupt firm's asset. However, the coefficient on liquidation remains unchanged, demonstrating that the liquidation treatment is similar even when holding opportunity cost constant.

Even more telling is column (2), where we limit the sample to counties with above-median local utilization and thereby focus on areas with high opportunity costs of vacancy. We find that liquidation reduces occupancy even in these markets, with the effect again very similar to that of the full sample. This suggests that, even in areas where vacancy is likely inefficient due to high local utilization, liquidation leads to a significant drop in the utilization rates of the bankrupt asset and hence is consistent with inefficient liquidation.

Columns (3) and (4) show that our results are driven by areas with low market thickness and low access to capital, as before, even when we allow for

[55] We use a 5% sample due to the computational complexities associated with the address matching procedure for a large set of plants. This 5% benchmark sample contains asset utilization information for approximately four million additional establishments.

[56] We do not have an instrument for entering bankruptcy, and so this comparison with benchmark plants is only suggestive as it does not account for selection effects. For example, bankrupt buildings could remain vacant at higher rates because they are worse in some way. To the extent that these locations enter bankruptcy because the *firms* (and not the *buildings*) are bad, this is less of a concern. That is, as long as a building can be reallocated to a more productive user, we should see utilization of these locations approach that of the overall economy.

Table IX

Controlling for Benchmark Utilization

This table shows that liquidation affects plant utilization even when controlling for local utilization rates. Benchmark utilization rates come from a 5% random sample of establishments in the same county as each bankrupt plant, with utilization measured five years after bankruptcy in an identical manner to the bankrupt plant. Panel A focuses on liquidation's impact on establishment occupancy, similar to column (6) of Table VI, with the dependent variable being the *occupied* dummy for the bankrupt plant. *High occupancy county* is a dummy indicating that a bankrupt plant is in a county with above-median benchmark occupancy, and *liquidation*high occupancy* is the interaction of this dummy with the liquidation indicator. *Liquidation*thick market* and *liquidation*high access to capital* are interactions between the liquidation indicator and local market conditions, as in Tables VII and VIII. For all interaction terms, we also include the interaction between the instrument and that variable in the first-stage regression. Panel B is similar to Panel A, but instead shows the effect on *ln(avg. employment)* column (2) of both panels limits the sample to *high occupancy counties*, while the remaining columns contain the full sample. All regressions are estimated using 2SLS and contain the full set of control variables in column (3) of Table III, 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: Occupied

	Occupied			
Dependent Variable:	Full	High Occ.	Full	Full
Sample	(1)	(2)	(3)	(4)
Liquidation	−0.173**	−0.198**	−0.295**	−0.445**
	(0.079)	(0.090)	(0.118)	(0.196)
High occupancy county	0.045***		0.074***	0.047
	(0.008)		(0.026)	(0.031)
Liquidation* Thick market			0.402**	
			(0.169)	
Liquiation * High access to capital				0.433*
				(0.223)
Liquidation* High occupancy			−0.044	−0.006
			(0.062)	(0.069)
Control variables	Yes	Yes	Yes	Yes
Div × Year FE	Yes	Yes	Yes	Yes
Observations	129,000	64,000	129,000	99,000

Panel B: Ln(avg. employment)

	Ln(avg. employment)			
Dependent Variable:	Full	High Occ.	Full	Full
Sample	(1)	(2)	(3)	(4)
Liquidation	−0.408*	−0.381	−0.733**	−1.116**
	(0.217)	(0.237)	(0.306)	(0.494)
High occupancy county	0.088***		0.135*	0.133
	(0.020)		(0.072)	(0.089)
Liquidation* Thick market			0.977**	
			(0.497)	
Liquiation * High access to capital				0.994*
				(0.572)
Liquidation* High occupancy			−0.089	−0.131
			(0.165)	(0.193)
Control variables	Yes	Yes	Yes	Yes
Div × Year FE	Yes	Yes	Yes	Yes
Observations	129,000	64,000	129,000	99,000

differential effects in areas with high opportunity costs of vacancy. In these columns, we include an interaction between the liquidation dummy and the high local occupancy indicator.[57] While the direct effect of high local occupancy is statistically significant and predicts whether the bankrupt plant is utilized, we find that the coefficient on this interaction term is insignificant and near zero. This means that liquidation does not have a differential effect in areas with high local occupancy, when the opportunity cost of vacancy is arguably low. These columns also support the view that the high local occupancy indicator captures separate variation from market thickness or access to capital.[58] In Panel B, we find similar results with respect to employment utilization.[59]

An additional possibility is that liquidation increases efficiency by accelerating what was already inevitable, that is, a decline in utilization. If randomized liquidation only decreases utilization of firms that were already shrinking, then it could be efficient to speed up this process. In Internet Appendix Table IA.XIV, we interact the liquidation variable with the establishment's three-year prebankruptcy employment growth rate to test whether our results are driven by low-growth rate plants. We find that the results of reduced utilization in liquidation are independent of prebankruptcy growth rates, which is inconsistent with the hypothesis that liquidation simply accelerates an inescapable decline. This result is again more consistent with the interpretation that liquidation is inefficient.

Taken together, these results are inconsistent with the notion that liquidation leads to low utilization only in high-vacancy areas where the opportunity cost of leaving a location vacant is likely low. Instead, the evidence is consistent with the interpretation that liquidation leads to vacancy even when the opportunity cost of doing so is high. The findings are also inconsistent with the idea that liquidation is efficiently accelerating the inevitable decline of failing firms. Of course, because we cannot directly measure asset efficiency, these results are not fully conclusive. Nonetheless, they point toward the interpretation that liquidation leads to inefficient asset allocation in areas with high search costs.

VII. Conclusion

How do institutions affect the allocation of assets in the economy? In this paper, we explore the role of the bankruptcy regime in the allocation of distressed firms' assets. In particular, we explore how liquidation and reorgani-

[57] As before, when interaction terms are included in this second-stage regression, we include the interaction between the instrument and that variable in the first stage.

[58] The results are also similar if we use as the local occupancy measure those plants in the same county and three-digit NAICS as the bankrupt plant. Furthermore, the results are unchanged if we use the continuous benchmark occupancy rate rather than the above-median indicator as a control.

[59] The one exception is that when we limit the sample only to areas with above-median local occupancy in column (2), the coefficient on liquidation is no longer statistically significant due to the reduced sample size, although the point estimate is almost identical to the effect in the full sample.

zation affect the allocation and subsequent utilization of the real estate assets used by bankrupt firms. To do so, we exploit the random assignment of judges to bankruptcy cases and variation in judges' interpretation of the law to instrument for the endogenous conversion of Chapter 11 filers into Chapter 7 liquidation. We further create unique geographical linkages from the Census LBD database that allow us to track real estate utilization over time.

We find that liquidation leads to a significant reduction in the utilization of real estate assets on average, with this effect persisting in the five years after the bankruptcy filing. These effects concentrate in areas with high search costs, such as thin asset markets where there are few potential users for bankrupt assets, and in areas with low access to finance. In contrast, in markets with low search frictions we find no differential effect of bankruptcy regime on asset utilization. Taken together, the evidence is most consistent with the interpretation that liquidation leads to inefficient asset allocation in areas with high search costs and thus highlights the importance of local market frictions in the allocation of assets of firms in bankruptcy.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.
Replication code.