Identifying the Effects of Bank Failures from a Natural Experiment in Mississippi during the Great Depression[†]

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I examine the causal effect of bank failures during the Great Depression using the quasi-experimental setup of Richardson and Troost (2009). The experiment is based on Mississippi being divided into two Federal Reserve districts, which followed different policies for liquidity provision. This translated into variation in bank failures across the state. Employing a plant-level sample from the Census of Manufactures, I find that banking failures had a negative effect on revenue stemming from a fall in physical output. I find no effect on employment at the plant-level and a large decline at the county-level. (JEL E32, E44, G21, G33, N12, N22, N92)

Banking crises are often associated with declines in the aggregate economy, but how much do bank failures exacerbate these downturns? And at what cost can policy mitigate these deleterious effects? These questions are starkest in the context of the Great Depression where an unprecedented decline in output was paired with an unprecedented collapse in the banking sector and serious questions about policy choices. The problem in addressing this question during the Depression, or at any other point in time, is one of endogeneity. Did bank failures drive business failures or the reverse, with business failures leading to bank insolvencies? Furthermore, in order to formulate an appropriate policy response, if bank failures do lead to declines in economic activity, what is the mechanism?

While Keynes and other authors writing around the time of the Depression believed the arrow of causality ran from banks to firms, this was downplayed by future generations of economists who saw the banking crisis during the Depression as a sideshow. The seminal work by Friedman and Schwartz (1971) reinvigorated the old banking-led hypothesis and simultaneously indicted policymakers for their

role in the crash. They argued that the effects of bank failures were felt through a decline in the money stock from a loss of deposits and a decline in shareholder equity.[1] This decline in the money supply would then drive a decline in aggregate demand. Bernanke (1983) pointed to broader "nonmonetary" effects of the banking crisis besides solely changes in the money supply. Instead, in Bernanke's view, bank failures could lead to direct declines in investment and aggregate demand by raising the cost of credit intermediation, *holding fixed changes in the money supply*.

These works, while providing highly suggestive evidence for the role of the banking crisis, still left many questions to be answered. If bank failures were important during the Depression, why is there no correlation between state-level income growth and bank failures (Cole and Ohanian 2000)? Why was the Depression already "great" with aggregate output declining 15 percent before the major banking crisis started (Cole and Ohanian 2000)? If the nonmonetary effects are important for understanding output and, in particular, investment, why does investment fall by the amount predicted by a simple neoclassical growth model (Chari, Kehoe, and McGrattan 2002)? Finally, if the nonmonetary effects are important, why does the theory seem to fail in a cross section of industries (Temin 2000)?[2]

Much of the debate and discord in the literature, I submit, has been driven by a paucity of well-identified studies that address the reverse causality question. I attempt to fill in this hole by exploiting a quasi-experimental setup studied by Richardson and Troost (2009)—henceforth, RT—involving the state of Mississippi, which is divided into two Federal Reserve districts: Atlanta and St. Louis. These two regional banks followed very different policies with regard to discount lending and emergency liquidity provision. Atlanta followed Bagehot's rule and acted very aggressively to stem bank runs by extending discount loans to illiquid banks. St. Louis on the other hand acted in a pro-cyclical fashion, contracting and expanding credit with the cycle, and placed onerous terms on banks that attempted to procure discount loans. In 1930, the bank Caldwell and Company located in Tennessee, and having *no direct* connections to banks in Mississippi, failed, setting off a panic in its wake. The St. Louis Fed did nothing to contain the ensuing run on banks stemming from this failure. In the southern part of the state, the Atlanta Fed responded aggressively and was successful, at least in part, in stemming these runs. RT[3] (2009) show that this policy difference had major impacts on the fraction of banks that survived and trade credit extended in a response to this quasi exogenous shock to the Mississippi banking system.

I exploit the exogenous difference in bank failure rates by constructing a novel plant-level dataset from the Census of Manufactures taken in 1929, 1931, 1933, and 1935. The data provide a wealth of outcome measures with which to analyze this "treatment." There is information on both the number of workers employed and the number of hours they worked, as well as information on the output side, such as revenue and physical output. First, using a difference-in-differences framework, I find

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[1] They also noted that in a time before the FDIC, even bank suspensions could decrease the money supply by making assets that were once thought to be liquid, illiquid.

[2] See Mladjan (2010) for some cross-sectional industry evidence for the nonmonetary view.

[3] Earlier work by Wicker (1996) had noted the differences between the policy choices of regional reserve banks and their impact on regional bank failure rates.

a very large negative differential effect on revenue of around 21 percent (-0.24 log points) on plants in the St. Louis district (north) in 1931 relative to plants in Atlanta (south). This effect on revenue is also present at the county level of aggregation and is, in fact, slightly stronger. Moreover, this fall in revenue at the plant level appears to be driven by a fall in physical output of 31 percent (0.37 log points) rather than a decline in prices. In fact, the point estimates suggest that prices actually increased in the north, relatively speaking.

Maybe, surprisingly, given the point estimates for output, I find no differential effect on total workers employed at the plant level. The point estimate hovers around zero, although it is imprecisely estimated. Instead, the point estimate for hours per worker is a large negative value of 12 percent (-0.13 log points) in the preferred specification. This suggests that the majority of adjustment to the output decline at the plant level was occurring along the intensive margin of hours worked (Bernanke 1986). However, once I aggregate to the county level, there is a large negative effect of around 32 percent (0.39 log points) on the aggregate number of workers. The results at the county level are only significant at the 10 percent level. These results suggest that the extensive margin for labor in terms of wage earners was driven by the extensive margin of plant entry and exit. In an Appendix, I find that while plant exit rates do not appear to increase in the north, relative to the south, the entry rate falls initially.[4]

The natural experiment is also ideal for studying longer-term effects. In October 1931, the St. Louis Fed reversed course and began to extend discount loans aggressively to struggling banks. This policy change effectively "froze" in place the differences generated by the Caldwell collapse between the regions' banking sectors. In fact, there is no further substantial differential change in the percentage of failed banks in 1933 (even in the face of the national crisis) or in 1935. Hence, by comparing outcomes in 1933 and 1935, I can identify the persistent effect of the shock originating in 1930. At both the plant level and county level, I find no difference between the regions in terms of revenue, output, or labor inputs by 1933, although some of the estimates are imprecise. My long-run results suggest that the national banking crisis that happened in 1930 and 1931 does not explain why output was still so far below trend in 1935 or in 1937, for example. To be sure, I am not implying that the second national banking crisis of 1933 had no effect on the sluggish recovery in 1935. Instead, my results suggest that little of the subsequent decline in output from 1931 to 1933, or the slow recovery from the trough in 1933, should be attributed to the persistent effect of the first banking crisis of 1931. Compare this to the work of Reinhart and Rogoff (2009) in which the authors argue that financial crises can have very long lasting effects on the economy stretching over many years.

It is important to emphasize the magnitude of these effects. They imply an elasticity of around -1.6 at the plant level between the percentage of banks that fail and plant revenue. For physical output, the elasticity is closer to -2. They become even larger when weighed against the costs of the Atlanta Fed incurred, which were none. The Atlanta Fed did not lose any money on the discount loans it extended

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[4] There is a caveat to these results related to the existence of a pretreatment year.

(RT 2009). So not only could St. Louis have mitigated a large fall in output, it could have done it for next to nothing.[5]

The one other paper that directly addresses the endogeneity issue in the context of the Great Depression is a paper by Calomiris and Mason (2003). Using an instrumental variables technique with both state- and county-level data, the authors identify exogenous changes in loan supply using lagged values of bank variables, such as assets and real estate loans, among others. They show that even after instrumenting for changes in demand for and supply of credit by these variables, there is a positive effect of loan supply on income growth and building permits, another measure of economic activity. Overall, the paper of Calomiris and Mason (2003) confirms Bernanke's view that bank failures had an effect independent of the direct impact on the money supply. However, there are reasons to doubt the validity of the instruments based on lags of banking variables. In particular, if there is any serial correlation in the endogenous variables or the error term, then these instruments will be invalid (Angrist and Krueger 2001). Given no strong theoretical justification for the identifying assumption, this instrumental variable strategy is rather tenuous. My paper on the other hand employs a quasi-experimental setup to identify an effect that is free of relying on lagged variables as instruments. To be sure, the paper by 2009 RT considers some broader effects of this differential banking crisis on firms engaged in wholesale trade. They find that the number of firms engaged in wholesale trade in the north is drastically reduced relative to the southern part of the state. Relative to both of these papers, I go far beyond considering a wide variety of outcome variables.

There are some studies using more modern settings that provide convincing solutions to the endogeneity problem. Ashcraft (2005) uses FDIC-induced closures of healthy banks to study similar causal effects. He finds a strong negative effect on county income from these "exogenous" bank closures. Using variation induced by the Japanese banking collapse in the early 1990s, Peek and Rosengren (2000) identify a positive relationship between loan supply and construction spending at the local level. They exploit the fact that affiliates of Japanese banks are negatively affected by the collapse of land prices in Japan, which is plausibly uncorrelated with local economic conditions in the United States. In a similar vein, Imai and Takarabe (2011) examine how local shocks to land prices are propagated throughout Japan through nationwide banking networks. My work goes beyond these studies as well, by studying a much richer set of outcome measures to include not only output but also input use and the extensive margin.

I. Data

The data used for this paper come from the Census of Manufactures (CoM) collected by the Census Bureau in one form or another since 1810. Unlike many of the other manufacturing censuses around this time which were destroyed, the schedules for 1929, 1931, 1933, and 1935, the first half of the Great Depression, were kept

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[5] In an Appendix, I attempt to make this accounting exercise more precise. I find that the point estimates for, say, output imply that 60 percent of the decline in industrial production between 1929 and 1931 can be explained by the banking crisis.

and are housed at the National Archives. Over these four censuses, there are close to 750,000 schedules with a wide variety of questions on the schedules. These include revenue broken down by price (unit value) and quantity, total wage bill, cost of intermediate goods, and number of wage earners employed at a monthly frequency. There is other information about whether the plant is incorporated (if unincorporated, who the owner is) and whether the plant has moved or changed names in the last year.

A limitation of that data is that there are no specific questions about assets or liabilities. The modern census and even its nineteenth century counterpart have a question about the value of capital invested. However, for these censuses, there are only questions related to the specific type of capital employed in a given industry. For example, bakery plants are asked the number of bread slicing machines and ovens.[6] An additional limitation is that the census did not attach unique identifiers to each plant as the modern census does. Hence, the entry and exit variables I consider only reflect whether I am able to match a plant in the previous or subsequent census, respectively.[7] This implies that the indicators should be considered with care, as any misplaced schedules on the part of the census will translate into errors in these variables.

There is one issue with the data for Mississippi (and the country as a whole). The behavior of the lumber industry appears to be anomalous with a massive amount of exit between 1929 and 1931.[8] Of course, one would expect a fair amount of exit over the first years of the Depression. But from the published reports for the whole country, the number of lumber establishments jumps from around 20,000 in 1927 to 26,000 in 1929, before falling to 16,000 in 1931. For the four censuses before 1927, the number had hovered around 21,000. However, there is no correspondent jump from 1927 to 1929 in value of products, wages, or wage earners. While there was a huge construction boom in the 1920s, Field (1992) points out that the cyclical peak comes in 1926, well before this large jump in the number of plants. This suggests that a vast number of these excess plants were canvassed despite the fact that they were very small and were not meant to be covered. This goes well beyond the difference in the cutoff values for inclusion in the census between 1929 and 1931. Even after dropping the plants in 1929 that would not have been counted in 1931 because of their size, there are many more plants than there should be in 1929. Because of this issue, I ignore the lumber industry in the estimates reported in the body of the paper. At the same time, an Appendix shows that the results are unchanged when this group of plants is included.

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[6] An important question when working with this data is coverage. Raff (1998) thinks the coverage in his industries of interest is quite good. In my experience, I have discovered some missing schedules most likely related to misplacement by the Archives. In other work (Chicu, Vickers, and Ziebarth 2012), I have compared the census information to other sources for the cement industry. In that work, we cross-checked not only the numbers from the schedules to other independent sources but to the census itself. The tabulations from the schedules lined up remarkably well. First, it appears that at least for the cement industry, all of the schedules that the census tabulated in creating the published tables are at the Archives. Second, in comparison to independent sources on revenue and physical output, the census values differ by only plus or minus a few percent in any given year.

[7] I am unaware of a register that the census used to canvas plants. I have considered using local business directories from the time to compare the coverage.

[8] I have found no mention of this peculiarity in primary literature from the Census Bureau itself nor in the secondary literature that used tabulations from the census.

The plant-level data have been used previously in limited contexts. Bresnahan and Raff (1991); Bertin, Bresnahan, and Raff (1996); Vickers and Ziebarth (2011); and Chicu, Vickers, and Ziebarth (2012) each study different industries from automobiles to cement. Ziebarth (2011) studies three industries from a macro perspective to understand aggregate productivity dynamics. To be sure, the published volumes have been employed in a number of works. For example, Rosenbloom and Sundstrom (1999) used information at the state industry level to study the incidence of the Depression across regions and industries.

II. The Quasi-Experimental Setup

A. *Mississippi and the Federal Reserve Districts*

This discussion follows RT (2009) closely. See that work for a much richer discussion of the differences between these two regional Feds. The basis of the natural experiment relies on the differences across the two Federal Reserve districts that covered the state of Mississippi. The northern part of the state is in the St. Louis district (8th district) while the southern part is in the Atlanta district (6th district).[9] From 1913 until the mid-1930s, the Atlanta Fed hewed quite closely to Bagehot's rule that central banks need to operate as lenders of last resort in times of panic. The St. Louis Fed was a strong proponent of the "Real Bills" doctrine that suggested credit should expand and contract with the cycle. RT (2009) suggest that this was partly related to the fact that St. Louis served as a major agricultural trading post and, hence, the St. Louis Fed was mainly focused on accommodating the natural credit cycle of agriculture. They also argue that part of the reason for the difference was historical contingency in who led the bank during this time. Eugene Black, the governor of the Atlanta Fed, was a staunch proponent of easing through open market operations as well as discount lending. So much so that he was appointed by Roosevelt to be chairman of the Federal Reserve Board in 1933. That being said, there is no reason to believe that the policies of the different Feds were somehow tailored to the different regions of Mississippi. The state was too small to carry much weight in shaping regional banking policy.

Part of the reason for this was that Mississippi was surely no bellwether for the American economy at this time. The state then and still today is much more rural and impoverished than the rest of the country. In 1929, over 80 percent of people living in Mississippi were living in a rural area, while for the United States as a whole, the percentage was around 45 percent. The manufacturing sector was also relatively underdeveloped and concentrated in relatively less capital-intensive industries and consumer nondurables. It is important to keep these differences in mind when interpreting the estimates presented here and in extrapolating to the broader economy. The fact that so many of the plants were small and underdeveloped suggests that the estimated effect might be biased upward in absolute value (more negative). At the same time, the fact that plants were concentrated in relatively less capital intensive and consumer

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[9] While there is some discussion of why regional banks were located in particular cities (Willis 1923), I am aware of no discussion as to why some states were divided between different banks. To be sure, Mississippi is not the only state to be cleaved. More than ten states fall into two regions.

TABLE 1—COMPARISON OF ST. LOUIS REGION OF MISSISSIPPI VERSUS THE ATLANTA REGION IN 1929

	Revenue (1)	Quantity (2)	Price (3)	Wage earners (4)	Hourly wage (5)	Hours per worker (6)
St. Louis	10.78	8.83	1.54	4.69	−1.24	4.00
Atlanta	10.88	8.63	1.72	4.54	−1.40	4.07
Significance level	0.37	0.39	0.36	0.11	0.00	0.00

Notes: The numbers reported are means of the log of the respective variable. The last line reports the significance level of a two-sided hypothesis test for the equality of means. As discussed in the paper, the comparison for price and output is restricted to the set of plants that produce only a single product. All variables are in logs.

nondurable industries would suggest that the estimate could be a lower bound in absolute value (closer to zero) for the effect on the broader economy.

Comparing the region of the state in the St. Louis district to that in the Atlanta district, RT (2009) argue that there were only minor differences in terms of manufacturing, agriculture, and urbanization rates. One obvious difference is that the Atlanta region has a coast with the associated industries of canning and fishing. Another less obvious geographical feature is the Mississippi Delta, a very rich agricultural region predominantly in the St. Louis Fed district. I will return to these two geographic differences in the robustness checks section. More directly, Table 1 offers the results of comparison of means tests for the two regions in 1929 for all of the dependent variables I study. In terms of revenue, quantity, and price, though there are some differences in means, none of them are statistically significant. The difference in means for wage earners is only marginally so ($p = 0.11$), though there is a sharp difference in hours worked that translates into a difference for the derived variable of average wage. I think there is only limited evidence for major differences across the regions of Mississippi. Furthermore, it is important to note that the framework will control for any persistent differences across the regions.[10]

I have noted that there does not appear to be major observable differences between the plants in the Atlanta region relative to the St. Louis region. But what about selection on unobservables on the part of plants? RT (2009) argue forcefully that there is no evidence that banks differentially sorted because of these policies. So first of all, if banks do not systematically differ across regions as RT (2009) argued, then there is not much reason for plants to differentially sort across regions either. The worry could still be that plants perceive that even though banks are the same across regions, in tough times, the Atlanta Fed will support the banks and, thereby, the plants themselves. This would push riskier plants to open in the south knowing full well that they too have a backstop against liquidity shortfalls. This selection bias would then lead me to overstate (more negative) the impact of bank failures. However, as RT (2009) point out in their discussion of sorting by banks, there was (and still is) a major debate about the efficacy of discount policy. RT (2009) state that many leading academics, and the leadership of the Federal Reserve system at the time, believed that discount policy would have deleterious effects. Therefore, it

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[10] It will not control for differences in response based on those persistent differences. This is why an initial homogeneity across regions is preferred.

is hard to imagine that plants would sort themselves into regions based on a potential effect that leading analysts believed evanescent. Finally, because I focus on one particular state, there are no other major policy differences across these two regions that would lead plants to locate in one region or another.

B. *The Initial Shock*

In November of 1930, the important regional bank Caldwell and Company headquartered in Nashville, Tennessee collapsed in a cloud of controversy over misappropriation of funds. It is important to note for the empirical strategy that Caldwell's correspondent network did not extend into Mississippi.[11] One would be worried about unobserved trends if the bank that set off the whole banking panic was located in a particular region of the state. After the collapse, the banking system remained stable in Mississippi for the next six weeks. However, over this period of time, bad press continued to leak out about the scandal surrounding Caldwell's failure and the closing of the Bank of the United States. In conjunction with the growing reports of a collapse in industrial output, the negative news took a toll and eventually panic struck Mississippi in the middle of December in 1930. Over the next two months, the level of deposits for the whole state fell by 55 percent. Loans and deposits did not begin to recover until 1934.

In response, Atlanta extended credit and rushed cash to the banks as fast as it possibly could. St. Louis made no such effort and may have actually slowed lending by more carefully examining the assets of banks looking for short-term loans. When the St. Louis Fed did rediscount eligible paper, it imposed onerous collateral requirements that in essence required banks to turn over $2 worth of their most liquid assets for $1 of cash. A few months after the collapse of Caldwell, while the Atlanta portion of Mississippi had 80 percent of its original banks in operation, only 65 percent of banks were in operation in the St. Louis region of the state.[12] To be sure, the situation was not ideal in the southern portion of the state as it too faced a deterioration in the banking sector, just on a different scale, and that difference in scale will be the variation I exploit. Similar differences are present if instead of looking at number of banks surviving, total deposits or loans are considered (RT 2009).

III. Empirical Framework

To estimate the short-run causal effect of this "exogenous" banking crisis in the northern part of the state, I use a difference-in-differences (DD) framework. This method removes both a common time trend and persistent differences between the control (Atlanta) and treatment (St. Louis) group. The basic specification is given by

$$(1) \qquad y_{it} = \alpha_0 + \alpha_1 StLouis_i + \alpha_2 1931_t + \alpha_3 StLouis_i \times 1931_t + \varepsilon_{it},$$

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[11] Banks at this time were linked through correspondent networks, which were a key source of short-term funding. These networks were akin to today's "repo" markets, and one failure in a network could take down all the other banks in that network (Richardson 2008).

[12] Banks in operation were those whose doors were open to the public, while those in business were those simply not bankrupt. The difference between the two was the set of banks that temporarily suspended redemptions.

where y_{it} is some outcome measure for plant i at time t; 1931_t is an indicator for 1931; and $StLouis_i$ is an indicator for being in that district. The DD estimate is given by $\hat{\alpha}_3$, which measures the relative change between the two regions over these two years.[13] For all of the specifications using plant-level data, I include industry-specific time trends. I now discuss different methods for controlling for selection at the plant level and estimates at the county level of aggregation.

A. *Plant-Level Estimates*

Estimates at the plant level may be subject to selection bias due to attrition. In this case, a reasonable assumption is that unobserved heterogeneity in selection is correlated with unobserved heterogeneity in the outcome variable. How this correlation biases the estimated effects depends on the variable in question. Plants that want to exit (by definition) also want to severely reduce their output, in fact, to zero. So for output, presumably not controlling for selection leads to an attenuation in the effect. For other variables, like price or wage, the bias is not as obvious. I offer three different plant-level specifications that attempt to deal with this worry to different degrees: within, balance, and unbalanced.

The first, the within estimator, differences out plant-level fixed effects on a balanced panel of plants by demeaning each plant's outcome measure by its time-series average. The identifying assumption then states that all unobserved heterogeneity comes from plant-specific fixed effects and not correlation in the unobservables driving both exit and final outcomes. When unobservables driving selection are independent of observables driving outcomes *after conditioning on all the explanatory variables including plant fixed effects*, then the within-estimator i.e., a fixed effects regression on the balanced panel, is a consistent estimator for the DD parameter (Cameron and Trivedi 2005). The question is whether this is a reasonable identifying assumption. For example, assume that the unobservable driving exit decisions is plant productivity. Now productivity is highly persistent over time (Foster, Haltiwanger, and Syverson 2008). So, in fact, after conditioning on plant fixed effects, there may be only little correlation between the unobservables left, and the selection bias may be small. That being said, any idiosyncratic shocks to plant-level productivity that drive both exit and final outcomes invalidate the identifying assumption.

The second and third estimators do not attempt to control for persistent fixed differences across plants. The balanced panel estimate attempts to, at least, consider a consistent group of plants across both years, those that survive. The unbalanced panel makes no such attempt and simply uses all the data from each year. In neither of these specifications do I include plant-level fixed effects, but I do estimate a full set of industry specific time effects, as well as fixed differences, between the regions, $StLouis_i$. I report robust standard errors, i.e., clustered at the plant-level. I do

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[13] Rather than treating $StLouis_i$ as a binary treatment, a different approach would have been to estimate an instrumental variables specification where the St. Louis indicator served as an instrument for the percentage of banks that failed. This, in some ways, gives a more direct measure of the effect of bank failures on plant outcomes. However, because there is no variation in the bank failure rate across a particular region, the instrumental variable specification is equivalent to the DD specification employed here. If plant-level variation in some kind of bank failure variable were available, then an instrumental variables approach would be preferred.

not correct the standard errors for within-treatment group (Fed region) correlation in the residuals because the intraclass correlation coefficient is basically zero. In other words, the vast majority of variation comes from within groups not between groups. Combining this intraclass correlation with an average group size of about 600, the Moulton factor is approximately 1.01 suggesting limited bias in the standard errors reported here (Angrist and Pischke 2009).[14]

B. *County-Level Estimates*

Estimates at the county level include both the effects at continuing plants and effects through the changing composition of plants. The drawback of aggregating to the county level is that I cannot consider a similar range of outcomes as I can at the plant-level. In particular, because I do not possess a price deflator with which to aggregate the outputs of different plants, I cannot study the effects on price and quantity at the county level. Furthermore, I simply do not have the information available to construct such a deflator myself. Also, it is more difficult to study how the composition of plants changes at the county level. Therefore, the county-level estimates will be restricted to variables that are measured in the same units across industries including revenue and total wage earners.[15] I include county-level fixed effects to soak up persistent differences across counties. The county-level observations will be weighted by the number of plants in a given county, and the standard errors will be clustered at the county level.[16]

IV. Results

A. *Effects on Revenue, Output, and Price*

The initial regression examines the effects of the differential banking crisis on the log of total revenue.[17] Results at the plant level are reported in the first three columns of Table 2. There is a very large negative effect on revenue from this "random" financial crisis from 15 percent (-0.16 log points) for the within model to 16 percent (-0.18 log points) for the balanced panel, and 12 percent (-0.13 log points) for the unbalanced. These effects do not seem to be driven by outliers. Repeating the analysis after trimming the 1 percent tails yields no major differences. The total change in revenue for plants in the St. Louis Fed district over these two years of around 45 percent (-0.60 log points) is very similar to the decline in US manufacturing revenue as a whole of 41 percent (-0.53 log points). The Atlanta district suffers only about two-thirds of that fall, showing the efficacy of liquidity provision

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[14] It is relevant to note that any errors in identifying plants over time will effect the standard errors since they are clustered on this variable. I have also estimated basic Eicher White heteroskedasticity robust standard errors with little difference.

[15] This variable is a total of the wage earner counts over each month of the calendar year summed over every plant.

[16] The fact that inference is roughly similar here provides further evidence that the method for calculating standard errors above is a reasonable one. Again, see Angrist and Pischke (2009) for a discussion of estimating treatment effects at the level of the treatment.

[17] These regressions exclude any plant reporting less than $5,000 in revenue since this is the larger cutoff for canvassing by the census for 1929 and 1931.

TABLE 2—EFFECTS ON OUTPUT VARIABLES

	Revenue				Physical output		
	Within (1)	Balanced (2)	Unbalanced (3)	County (4)	Within (5)	Balanced (6)	Unbalanced (7)
St. Louis Fed 1931	−0.24*** (0.06)	−0.21*** (0.08)	−0.18** (0.09)	−0.28* (0.16)	−0.37*** (0.11)	−0.53** (0.19)	−0.43** (0.17)
St. Louis Fed	—	−0.12 (0.11)	−0.15* (0.08)	—	—	−0.18 (0.20)	0.20 (0.28)
Observations	1,226	635	1,224	148	479	282	479
Adjusted R^2	0.57	0.61	0.56	0.94	0.64	0.81	0.79

Notes: All dependent variables are in logs. The within specification includes plant fixed effects. All the regressions include industry-specific time trends though the coefficients are excluded for clarity. The price and quantity effects are only for plants producing one good. Plant-clustered standard errors are reported in parentheses. County-level regressions include full set of county fixed effects with standard errors clustered at the county level and observations weighted by number of plants in a given county. Note there is no St. Louis Fed coefficient for the county estimates because I estimate a full set of county fixed effects.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

by the Atlanta Fed. In column 4 of Table 2, the same specification is run at the county level. I choose to include county-level fixed effects to soak up any permanent differences between counties instead of a simple St. Louis indicator. The results at this level of aggregation are similar with a differential fall in revenue larger than the estimate at the plant level, with a decline of 24 percent (0.28 log points), suggesting the importance of changes on the extensive margin.

Given this fall in revenue, the question is whether price or physical output falls. I use direct measures of physical output and back out unit prices from revenue divided by quantity. For simplicity, I restrict the estimates to plants that produce only one product. This allows me to avoid the knotty issue of assigning a price to a plant that produces a variety of products. This comes at the cost of restricting attention to a group of plants that is potentially different from the population. In the Appendix, I report similar effects on revenue for this group of plants to the broader population. Columns 5–7 of Table 2 report the results for output, and columns 1–3 of Table 3 report the results for prices.[18] What is quite clear is that the fall in revenue is driven by a fall in physical output rather than a fall in price. Output drops by up to 39 percent (0.5 log points) in some specifications. And recall that this is for the plants *that actually continue to produce* and does not even count the lost production from those exiting. Somewhat surprisingly, the point estimates for prices, if anything, suggest a relative increase hinting at the relative shifts in supply and demand stemming from this shock.[19] I do hasten to point out that the trends are estimated rather imprecisely.

[18] For these regressions, I trim the 1 percent tails of the price distribution by year and industry. The Appendix considers different degrees of tail trimming and reports similar results.

[19] The results may also suggest the importance of selection. If selection is driven mainly by demand *shocks*, not the *level* of demand, then the plants that remain after this shock, on average, charge higher prices with higher demand. Note that this is a slightly more subtle form of selection than that focusing on the level of demand (Foster,

TABLE 3—EFFECTS ON PRICES AND WAGES

	Price			Average wage		
	Within	Balanced	Unbalanced	Within	Balanced	Unbalanced
St. Louis Fed 1931	0.16**	0.12	0.15	0.15*	0.11	0.08
	(0.08)	(0.09)	(0.09)	(0.08)	(0.08)	(0.06)
St. Louis Fed	—	0.05	0.04	—	−0.10**	−0.09***
		(0.06)	(0.11)		(0.05)	(0.04)
Observations	459	274	459	1,113	593	1,112
Adjusted R^2	0.86	0.94	0.89	0.11	0.28	0.37

Notes: All dependent variables are in logs. Both variables are derived by dividing total revenue or wage bill by total output or total hours. The within specification includes plant fixed effects. All the regressions include industry-specific time trends though the coefficients are excluded for clarity. Standard errors are clustered at the plant level and reported in parentheses. County-level regressions include county and year fixed effects and standard errors are clustered at the county level. County-level observations are weighted by number of plants in a given county. Note there is no St. Louis Fed coefficient for the county estimates because I estimate a full set of county fixed effects.

   ***Significant at the 1 percent level.
  **Significant at the 5 percent level.
   *Significant at the 10 percent level.

The results highlight the importance of geographic distance for credit relationships. Even though there was a relative abundance of banking just across the district border, plants north of that border were simply unable to access those resources in the southern part of the state.[20] This is not totally surprising at a time of sharp restrictions not only on branching of banks between states but even *within* states. So potentially "flush" banks located in the Atlanta region could not easily extend loans to plants suffering in the St. Louis region through a branch in the north.[21] This branching restriction is what in many ways makes the Great Depression the ideal time period to study the connection between banking and finance.

B. *Effects on Labor*

I now turn to the effects on a variety of labor related variables. Results are reported in columns 1–3 of Table 4. Even though the effects on revenue and output are substantially negative, there does not appear to be much of an effect on the number of wage earners employed at the plant level. All of the DD estimates hover around zero with no greater degree of statistical uncertainty than the results for revenue. There is a pronounced downward trend overall, as would be expected given this is the Depression. Some adjustment does appear to be present at the county level. There is a major differential negative effect of 32 percent (0.39 log points) at the county

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Haltiwanger, and Syverson 2008). This potential driver of selection will be useful to keep in mind when I discuss the results for wages.

   [20] On the other hand, I will show, subsequently, that when the results are restricted to the border regions, there is no negative differential effect for plants in the northern region.

   [21] Carlson and Mitchener (2009) have argued that banks, which were allowed to open branches, were more stable during the Great Depression. On the other hand, the results of Imai and Takarabe (2011) for Japan actually emphasize the negative effects of branching as local shocks get propagated throughout the banking network.

TABLE 4—EFFECTS ON LABOR INPUTS

	Total wage earners				Hours per wage earner		
	Within (1)	Balanced (2)	Unbalanced (3)	County (4)	Within (5)	Balanced (6)	Unbalanced (7)
St. Louis Fed 1931	−0.02 (0.07)	0.03 (0.07)	−0.00 (0.08)	−0.39* (0.25)	−0.13** (0.07)	−0.11* (0.06)	−0.09** (0.05)
St. Louis Fed	—	−0.20** (0.10)	−0.22*** (0.07)	—	—	0.04** (0.02)	0.03** (0.02)
Observations	1,224	640	1,223	146	1,109	590	1,108
Adjusted R^2	0.32	0.49	0.53	0.89	0.15	0.16	0.18

Notes: All dependent variables are in logs. The within specification includes plant fixed effects. All the regressions include industry-specific time trends though the coefficients are excluded for clarity. Standard errors are clustered at the plant level and reported in parentheses. County-level regressions include county and year fixed effects and standard errors are clustered at the county level. County-level observations are weighted by number of plants in a given county. Note there is no St. Louis Fed coefficient for the county estimates because I estimate a full set of county fixed effects.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE 5—EFFECTS ON WAGE EARNERS BEFORE AND AFTER THE SHIFT IN THE POLICY OF THE ST. LOUIS FED

	Wage earners	
	Plant	County
Before St. Louis policy shift (March)	−0.12 (0.08)	−0.52** (0.22)
After St. Louis policy shift (October)	−0.07 (0.09)	−0.37 (0.20)

Notes: Results are from using the within estimator for plant-level data and county-level aggregates. The regression specifications are the same as above.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

level in column 4 of Table 4 highlighting the potentially important role played by the extensive margin of plant exit and entry.

The small effect for wage earners at the plant level is not coming from an averaging of a large negative effect before the St. Louis Fed changed policy and a catchup effect afterward. In Table 5, I take advantage of monthly counts of workers to address this question. I estimate the effect of the shock on employment counts before St. Louis reversed course and then after the policy reversal. St. Louis began to offer discount loans on more favorable terms in July, so, for a pre-policy shift month, I choose March. For a post-policy shift month, I take October.[22] What is evident from the results from the plant level (column 1) is that the null effect is present even before the St. Louis Fed changed policy, though the point estimate before is a bit smaller in absolute value in October. This suggests that there really is little effect

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[22] RT (2009) make no note of whether or not, this shift was expected in advance. Some foresight on the part of the banks and plants would surely impact the results.

on the number of workers from this shock, at least at the plant level. At the county level, the story regarding employment changes appears to be slightly different. In column 2, while the DD point estimate is sharply negative in October, it is no longer statistically significant and, more importantly, is substantially less negative than the estimate in March. This suggests that the change in St. Louis' policy may have had an immediate effect on the extensive margin of firms, either preventing more from exiting or allowing more to enter. This is interesting to keep in mind when I consider the long-term effects and the relationship of these results to some of those in RT (2009) for wholesale firms.

For the plants that survive, the question then is along what dimension do they make adjustments to this decline in revenue. One possibility is that adjustment is taking place along the intensive margin of hours worked, as suggested by Bernanke (1986). Results for this variable[23] are in columns 5–7 of Table 4. The point estimate is a quite large decline of 12 percent (0.13 log points) for the within estimator and statistically significant at the 5 percent level. Similar results hold for the other two plant-level specifications, with slightly smaller effects and roughly similar degrees of precision. Taken together, these results regarding hours and employment counts suggest some degree of labor hoarding on the extensive margin by plants that survive with the continually employed workers having their hours slashed. To be sure, all of these labor effects are for *wage earners*, which is a category distinct from total employees that also includes salaried workers.[24]

I report the results for average wages in columns 4–6 of Table 3.[25] Using the within estimator, it appears that the average wage increases by an economically meaningful amount of 16 percent (0.15 log points) in the St. Louis region relative to the Atlanta region. Similar point estimates are present for the other plant-level specifications, but none of them are statistically significant. These results seem to mirror those for price, which also seemed to rise. Like the explanation for the price effects, an explanation for the wage results based on shocks to productivity, rather than levels, may also apply. The plants that survive are ones that receive positive shocks to productivity. These positive productivity shocks after the differential selection push up the average wage observed. The productivity is not in terms of physical productivity, which, all else equal, should translate into lower prices. Instead in line with Foster, Haltiwanger, and Syverson (2008), selection here appears to be on demand. Finally, note that this composition bias is not the one often discussed as an explanation for why wages appear to be sticky (Solon, Barsky, and Parker 1994). Here selection is on plants rather than workers. Furthermore, the composition bias is not from the extensive margin in terms of workers being laid off. Rather it is on the intensive margin with the least productive workers having their *hours* slashed.

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[23] For these regressions, I exclude any plant reporting a value of greater than 84 for hours per week per wage earner.

[24] While the 1929 census has questions about salaried employees and their salaries, the 1931 census lacks this question. So I am unable to examine the response of this variable. It is possible to construct stories for why the effect on salaried workers would be larger or smaller than the effect on wage earners.

[25] I trim the 1 percent tails by year of the wage distribution before running this regression for the same reasons as I trim the tails of the price distribution.

C. *Long-Run Effects*

One of the biggest puzzles surrounding the Great Depression is that it lasted so long. It was not until World War II that the US economy shook off the ill effects of the Depression. As Cole and Ohanian (2000) have pointed out, this is especially puzzling because productivity growth was at unprecedented levels after 1933 (Field 2011). They argued that this sluggish recovery was due to the government sponsored cartelization of vast swaths of the American economy under the "Codes of Fair Conduct Promulgated" by the National Industrial Recovery Act of 1933. They suggested that this collusion persisted long after the law was struck down in 1935, and explains why, even with rapid productivity growth, output grew so slowly.[26] Still others have highlighted drags coming from fiscal policy in 1937 (Eggertsson and Pugsley 2006). A different explanation for the slow recovery flows from the work of Reinhart and Rogoff (2009). Using a comprehensive sample of financial crises over hundreds of years of history and numerous countries, they find that recoveries from banking collapses tend to be quite protracted stretching on for many years.

My setup allows me to test whether the short-run effects identified above continue to persist into 1933 and 1935. This exercise provides some insights into the possible role of the nationwide banking crises of 1931 and 1933 on the slow recovery from 1935 onward. Of course, the estimates cannot distinguish between the effects of bank failures induced illiquidity versus insolvency, and it may simply be the case that the localized banking crisis I study has no analog with a widespread banking crisis that envelops the nation in 1933 due to general equilibrium effects.

Even with these caveats in mind, the setup is ideal for studying long-range effects because of a policy change by the St. Louis Fed that "freezes" in place the differences in banking sectors arising in late 1930 and early 1931. In July 1931, the St. Louis Fed relented and began to rediscount loans and provide liquidity. However, the damage relative to the northern part of the state was already done. Furthermore, the differences setup by the initial shock, and the subsequent response, persisted through the rest of my sample. In particular, the gap between the percentage of banks in business across the two regions never closed. After this policy shift, RT (2009) find that the banking sectors of the two regions of Mississippi appear to respond similarly to subsequent shocks, such as the crisis of 1933. The policy change by the St. Louis Fed mitigated any further differential damage to banks in its region, but it did not induce a catchup in the number of banks operating or loan volumes in the St. Louis region relative to the Atlanta region of Mississippi. This is the sense in which the policy reversal of St. Louis "froze" in place the initial shock. Instead of having to worry about other intervening differential shocks in 1933 or 1935, I can examine cleanly the effect of the initial shock of the collapse of Caldwell on "long-run" outcomes in 1933 and 1935.

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[26] See Eggertsson (2012) for a different view of this law's effect.

TABLE 6—LONG-RUN EFFECTS OF THE SHOCK

	Revenue		Workers				
	Plant (1)	County (2)	Plant (3)	County (4)	Hours (5)	Quantity (6)	Price (7)
1931 St. Louis	−0.19*** (0.06)	−0.24* (0.14)	−0.03 (0.08)	−0.37** (0.19)	−0.09 (0.07)	−0.29*** (0.10)	0.24* (0.15)
1933 St. Louis	0.01 (0.09)	−0.28 (0.19)	0.05 (0.13)	−0.10 (0.24)	0.02 (0.09)	−0.14 (0.13)	0.30 (0.22)
1935 St. Louis	−0.10 (0.11)	0.07 (0.25)	−0.01 (0.13)	0.05 (0.31)	−0.05 (0.10)	−0.16 (0.13)	0.08 (0.21)

Notes: All plant-level regressions are using the within estimator and include industry-specific time trends. All variables are in logs. Standard errors are clustered at the plant level. Results for quantity and price are estimated using only plants that produce one good. The results for hours, quantity, and price are all at the plant level. County-level regressions include county and year fixed effects and standard errors are clustered at the county level. County-level observations are weighted by number of plants in a given county.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

In Table 6, I report the estimates for the subsequent years, pooling all the years together.[27] All of the variables show an attenuation in effect over time. In fact, at the plant level, none of the effects are statistically significant after 1931, though the point estimate on the quantity effect remains rather large and negative through 1935. In addition, there is very little persistence at the county level. The wage earners effect in 1933 is a quarter of the effect in 1931, and by 1935, there is basically no difference. Compare these results to RT (2009) who find continued differences in wholesale trade credit and firms. Those differences seem to not matter for the health of the manufacturing sector. The results mirror those in Calvo, Izquierdo, and Talvi (2006), who find that recoveries from large crashes in output stemming from sudden stops do not require similar recoveries in credit conditions. The results of these regressions offer tentative evidence that recovery in the formal banking sector was not necessary for a recovery in the overall economy during the Depression.

V. Robustness Checks and Extensions

A. *Three Checks*

I now conduct three robustness checks. The first robustness check involves restricting attention to counties and plants that are along the border of the Fed regions. One would expect that the differential crisis should not matter for these plants given the border is meaningless. Plants in the northern border counties can always go across the Federal Reserve district border to borrow from relatively unaffected banks. This should mitigate any negative effect stemming from bank failures in their particular region. These regressions then provide a falsification test of my view that banks matter. A large negative effect here would be suggestive that there

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[27] Remember that I still cannot estimate industry fixed effects because they are time invariant.

TABLE 7—THREE "ROBUSTNESS" CHECKS

	Only border counties		Exclude coastal counties		Exclude delta counties	
	Revenue (1)	Total wage earners (2)	Revenue (3)	Total wage earners (4)	Revenue (5)	Total wage earners (6)
Panel A. Plant-level						
St. Louis 1931	−0.08	0.20	−0.24***	−0.02	−0.20***	0.02
	(0.13)	(0.21)	(0.06)	(0.07)	(0.07)	(0.08)
Panel B. County-level						
	0.60	0.42	−0.28*	−0.39	−0.34	−0.50
	(0.92)	(0.84)	(0.16)	(0.24)	(0.23)	(0.31)

Notes: All variables are in logs. Regressions at the plant level use the within estimate. Other specification details are the same as in the baseline regressions. Standard errors are clustered at the plant level.

   ***Significant at the 1 percent level.
   **Significant at the 5 percent level.
   *Significant at the 10 percent level.

are unobserved trends that are driving the outcomes in these two regions that are unrelated to the crisis. In columns 1 and 2 of Table 7, I report the estimates restricting attention to these plants in these counties. The first row in the table displays the estimates from the within estimator at the plant level, while the second row repeats the exercise aggregating to the county level. As expected, there appears to be no differential effects between border counties in the Atlanta region versus border counties in the St. Louis region.

The second robustness check is to eliminate counties and plants that are on the Gulf Coast. There is a worry that this area is different from the other regions of Mississippi since its economy is driven mainly by fishing and canning. If the effect disappeared after dropping these plants and counties, this would suggest that potentially shocks to the fishing industry are what is actually driving the differential effect and not the differential bank crisis. The results for revenue in columns 3 and 4 of Table 7 show no change. If anything, the results appear to be stronger when I exclude this group. Similar results hold for the other variables considered, such as output and labor inputs. This makes it unlikely that the overall results are being driven by an unobserved trend in the fishing industry that differentially impacts the two regions.

The third robustness check considers the possible effects of another unique region of the state, the Mississippi Delta. This is a region in the northwest part of the state with some of the richest soil in the world and the heart of the cotton growing region of the state. While not completely enclosed in the St. Louis portion of the state, 75 percent of the plants in counties with some part in the Delta are in the St. Louis district. The worry is that there was a severe drought that particularly affected the cotton crop in 1930 (Hamilton 1985). The worry is that this shock may lead to differential trends in the two regions unrelated to the banking crisis I consider. So columns 5 and 6 of Table 7 report the estimates dropping plants in counties with any part in the Delta. Point estimates for revenue and employment at both the plant and county-level are basically unchanged relative to my preferred specification. The

TABLE 8—ESTIMATE OF EFFECT ON REVENUE ACROSS INITIAL SIZE QUARTILES

	1st quartile	2nd quartile	3rd quartile	4th quartile
St. Louis 1931	−0.61**	0.02	−0.22*	−0.23**
	(0.16)	(0.11)	(0.13)	(0.09)
Observations	267	252	279	253
Adjusted R^2	0.67	0.72	0.64	0.79

Notes: Size quartiles are defined by industry as measured by total revenue in 1929. Estimates are from within-estimator and standard errors are clustered at the plant level. These regressions again include industry-specific time trends estimated separately for each size quartile.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

only difference is larger standard errors for the county-level estimates. I conclude that the drought of 1930 is not driving differences across the two regions.

B. *An Extension: Effects across Size Distribution*

The final extension involves estimating the effect across different plant sizes as measured by revenue. Uniformly, studies such as Khwaja and Mian (2008) or Petersen and Rajan (1994) have found that the smallest plants or firms are most affected by shocks to the financial sector. In Table 8, I display the effects of the crisis on revenue for different quartiles of the size distribution. I place plants into quartiles based on their revenue in 1929, relative to the rest of plants in their industry. Across three out of four quartiles, the results show large negative effects with the largest effect for the smallest plants of 46 percent (0.61 log points). Interestingly, even the largest plants are also negatively effected, with the top quartile showing a decline of over 20 percent (0.23 log points). Note that this effect cannot be explained by a regression to the mean effect given that I stratify on *initial* size. This regression to the mean effect would be common across all large plants and, hence, would be captured in the aggregate time trend, not the DD coefficient.

These results cast doubt on a different interpretation of the results. That is, the effect of the banking collapse is not through the actual failures of the banks *per se*, but the uncertainty and fear that it generated. This increase in uncertainty would cause people to delay purchases and plants to hold off on hiring. This would still implicate Fed policy in not preventing this panic, but the effects would not be through the usual credit channel. The difficulty with this story is that it offers no reason why the effects should be heterogeneous across plants of different sizes. On the other hand, the interpretation that emphasizes the direct effects of bank failures on credit availability can easily explain the differences. It is interesting to note that even the largest plants appear to be affected.

VI. Conclusion

What is the role of banking crises in economic crises generally, and, in particular, what about for the Great Depression? This has proven to be a difficult question to address because of concerns regarding identification and endogeneity. To address

this question, I have built a new dataset consisting of all manufacturing plants for Mississippi in 1929, 1931, 1933, and 1935. Along with variation in Fed policy and an external shock identified in RT (2009), this has allowed me to identify the causal effect of bank failures on plant outcomes. I find negative effects on revenue due to a fall in physical output. There is no effect on employment as measured by the number of workers at the plant level, while there is a large negative effect on hours worked. Once I aggregate to the county level, the number of wage earners falls sharply highlighting the important role of changes in the extensive margin of plant entry and exit. The effects do not seem to persist after 1931.

These results show the serious costs of particular policy choices. The losses are all too clear, but what is striking is the fact that this decline could have been averted by extending discount loans that ended up being more or less free. RT (2009) highlight that the Atlanta Fed actually *profited* from its liquidity injection because the loans were paid back in full. Furthermore, creditors and depositors of failed banks in the Atlanta district eventually recovered nearly 99 percent of their assets, while those in the St. Louis district only ended up receiving 80 percent. The St. Louis Fed could have averted this decline in output for basically no direct cost. Maybe it is not altogether surprising that Chairmen Bernanke, then Governor Bernanke, apologized for the actions of the Federal Reserve during the Great Depression at Milton Friedman's 90th birthday.

The question, in the end, is where this places the banking crises of 1931 and 1933 in the macroeconomic history of the Great Depression. While I have highlighted some direct effects on a variety of economic outcomes in this work, there are other seemingly central puzzles that at first pass seem as if they should be unrelated to the banking collapse. For example, from a neoclassical point of view, Chari, Kehoe, and McGrattan (2002) argue that the central puzzle of the Depression is why the marginal rate of substitution between consumption and labor became so detached from the real wage. They argue that it is difficult for models of financial frictions to explain this labor market fact. In addition, the behavior of productivity during the Depression is rather puzzling with a large decline of around 18 percent, much too large to be accounted for through factor hoarding. (Ohanian 2001). But why should the decline in productivity be driven by bank failures? With this decline in productivity, the neoclassical model can, in fact, match the observed fall in investment during the Depression. So what room is left for the nonfinancial effects of bank failures? These are important questions to be addressed, and recent theoretical and empirical work is beginning to do just that. Theoretically, Buera and Moll (2012) have shown that financial frictions can affect both the labor wedge and productivity. Some of my own work (Ziebarth 2011) has offered some initial empirical evidence for the role of the collapse of financial markets in the decline in productivity. Whether this and future work reasserts the centrality of the banking crisis to the macro history of the Depression remains to be seen. This paper has attempted to provide one piece of evidence towards that final goal.

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