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Bank Executive Experience in a Financial Crisis

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Abstract

This paper evaluates whether bank executive experience can influence bank outcomes during financial crises. Some bank presidents in New York City possessed experience as a bank president at the same bank in New York during the previous banking crisis. The evidence from four crises between 1884 and 1907 suggests that individual bank deposit losses at the same institution appear uncorrelated across adjacent crises, so bank performance does not persist across crises. Further, the retention of an experienced bank president does not mitigate deposit losses in a subsequent financial crisis.

Keywords: financial crisis, executive, learning

1. Introduction

1.1 Overview

Does bank performance persist across financial crises: if a bank performs poorly in one crisis, will the bank be more likely to perform poorly in the next crisis? A bank with a similar loan portfolio, investment strategy, and deposit base might suffer similar consequences across financial crises. If bank outcomes are correlated across panics, perhaps the experience of senior executives can improve bank performance during financial crises. Executives who have already served in a leadership position in a financial crisis might be better able to manage a subsequent crisis.

Patterns in bank performance during financial crises could suggest regulatory remedies to improve panic outcomes. If bank performance persists across crises, then perhaps regulators should monitor problem banks more carefully to prevent a poor repeat performance in the next crisis. Government assistance programs during one crisis could carry additional regulatory scrutiny through the next financial crisis when the bank would be expected to perform poorly. Alternately, if experience helps executives manage crises, then perhaps central banks should train financial executives how to navigate financial panics.

This paper investigates whether bank executive experience influences financial outcomes for a bank by focusing on historical financial crises. The United States experienced a series of financial crises in quick succession before the founding of the Federal Reserve, including important financial disturbances in 1884, 1890, 1893, and 1907. The historical data for this paper consist of individual bank balance sheets of 209 bank-panic observations of New York City commercial banks that can be matched with similar data from a previous crisis. The data also include the identity of the senior bank executive, the bank president, and a measure of bank performance, the percentage change in bank deposits during the crisis. A dynamic panel regression model evaluates whether financial outcomes of the bank in the previous crisis can influence panic outcomes in the subsequent crisis. More specifically, the paper tests whether the presence of the same president who served at the same bank during the earlier crisis improves bank performance.

The results suggest that executive experience does not explain panic outcomes. First, in contrast to the previous literature using the stock prices of modern financial firms, the percentage change in deposits during a crisis remains uncorrelated with deposit losses in the previous crisis. Even when crises occur only a few years apart, bank performance is not correlated across crises of the nineteenth century. There is some evidence for correlation between the panic of 1907 and the panic of 1893 only, but as a whole the results exhibit limited evidence for correlation. Further, deposit losses were not mitigated by an experienced bank president serving in the same capacity in the previous crisis. Most estimates of the effect of experience are statistically insignificant. That is, the past historical experience of the firm in the last financial crisis does not influence its deposit losses in a

subsequent financial crisis.

1.2 Literature Review

Fahlenbrach, Prilmeier, and Stulz (2012) suggest that executive experience is not an important driver of financial firm performance during crises. The authors compare bank performance across two crises, the Long Term Capital Management (LTCM) crisis in 1998 and the Great Recession of 2008. The authors show a positive correlation in bank stock price losses at the same institution across the two crises and attribute this correlation to established firm culture or business model persistence. Further, when the authors control for the presence of the same CEO leading the bank during both crises using both an indicator variable and an interaction term, these two variables lacked statistical explanatory power. Bank CEOs serving the same bank in both crises did not improve bank performance and could not explain the correlation in bank performance across crises.

In contrast, other previous literature suggests that senior executive experience can influence bank performance, especially during crises. Ahmed, Christensen, Olson, and Yust (2019) show that executive and director experience at banks in the savings and loan crisis of the late 1980s and early 1990s lead to higher returns on assets and lower failure rates during the Great Recession of 2008. Berglund and Makinen (2019) argue that the faster recovery of Nordic banks from the 2008 banking crisis was due to their previous experience with a financial crisis in the 1990s. Even if executive experience does matter, the lessons of experience may fade over time, and Berger and Udell (2004) suggest that the decay of loan officer experience or the replacement of experienced officers could contribute to the procyclicality of loan performance. Of course, previous crisis experience may not be positive, as bank executives who helped to initiate an earlier crisis could simply be repeating their behavior in the next crisis. For example, Minton, Taillard, and Williamson (2014) show that a greater proportion of independent directors with financial expertise led firms to increase risk before the financial crisis in 2008. Other authors focus on characteristics or personal history of the executives rather than employment experience (Malmendier, Tate, & Yan, 2011; Ho, Huang, Lin, & Yen, 2016; Schoar & Zuo, 2017). Beyond banks, Yao, Deng, and Wang (2017) identify a similar phenomenon in the insurance industry when the loss ratio across two large insurance losses 20 years apart remains correlated for individual firms. Calomiris and Carlson (2016) show that traditional corporate governance incentives functioned well for senior executives of national banks during this period.

Historical data provide several additional advantages to study how executives might learn from financial crises. First, the frequency and severity of financial crises in the historical period are greater than during the modern period. When crises are often ten or twenty years apart, few executives may have a chance to accumulate experience in office. Further, historical financial panics often coincided with macroeconomic upheaval and large fluctuations in asset prices, making historical panics broader and more severe in comparison to modern crises such as the 1998 Long Term Capital Management crisis or the savings and loan crisis in the late 1980s. Thus, less pervasive modern crisis episodes would be easier to manage without experience. Second, historical banks were smaller organizations with a single branch that could be more easily influenced by the actions of one individual, so executive experience might play a larger role in historical bank outcomes. Finally, the historical period lacked both a true central bank backed by the full spending power of the federal government and federal government deposit insurance schemes to bail out financial institutions. Government assistance in modern data could reduce the importance of executive experience when government actions rescue marginal firms.

1.3 History

The nineteenth century United States is a fertile period to study banking crises because the large number of financial crises across a relatively short periods affords additional opportunities to estimate a correlation across crises. We focus on crises occurring in 1884, 1890, 1893, and 1907. Calomiris and Gorton (1991) and Hanes and Rhode (2013) include all four of these crises in their list major crises of the National Bank Era and include no crises between these years. Gorton (1985) observes that while clearinghouse loan certificates were authorized in 1896, none were issued in New York, and so the episode typically remains classified as a nonpanic year. The short average time between panics may allow more executives to remain in office and will permit them to be more familiar with what actions to take during a financial crisis based on their recent experience.

Previous work in financial history recounts the events of the panic of 1907. Odell and Weidenmier (2004) describe how the Bank of England sought to prevent gold flows to the United States after the San Francisco earthquake of 1906, creating a monetary stringency. Rodgers and Payne (2018) highlight how fluctuations in the copper price helped to ignite the crisis. Wicker (2000) recounts how upheaval in the copper market led to solvency concerns about a group of New York banks associated with speculation in copper stocks. Sprague (1910) and Bruner and Carr (2007) describe the defense of the market by the large national banks. Moen and

Tallman (1992) observe that major deposit losses fell on the trust companies, less regulated entities with some banking powers, rather than the national or state banking systems. Hansen (2014) argues the location and clientele of the trusts was an important determinant of which trusts endured runs. Frydman, Hilt, and Zhou (2015) discuss economic losses imposed on corporations affiliated with financial trusts that suffered large losses during the 1907 crisis.

Historical work on the origins and the transmission of these two crises suggests that experience in the panic of 1893 may not have translated directly to improved results in 1907 because the causes of the crises were different. First, investors expressed concern that the federal government would maintain the fixed exchange rate of the dollar under the gold standard in 1893 (Freidman & Schwartz, 1963; Miller, 1996). Adherence to the gold standard does not appear to be openly questioned in 1907. Second, the transmission of the crisis in 1893 involved more pressure from the interior regions of the country due to stressed agricultural markets. Carlson (2005), Dupont (2009), and Hoag (2005) study bank closure, depositor withdrawal, and interbank withdrawal patterns by banks in the agricultural interior of the country during the crisis of 1893.

In 1890, Sprague (1910) identifies the regular autumnal monetary stringency combined with a shock from the London market as the key explanation for the crisis. London firms issued and speculated in securities from emerging markets, particularly Argentina, culminating in the failure of the House of Baring. The international financial disturbance prompted sales of American securities and gold exports from New York. Mitchener and Weidenmier (2008) show that the crisis widened yield spreads not just from Argentina but on Latin American debt generally. In the U.S., much of the upheaval was concentrated in money centers rather than infecting the broader real economy. Wicker (2000) praises the New York Clearing House leadership for allowing loan certificates to be issued to several needy banks. In addition, the U.S. Treasury responded vigorously by offering to buy bonds and the prepay interest on outstanding bonds

Although the crisis of 1884 did involve a recession and stringency in the New York money market, it also did not necessarily result in a nationwide financial panic. Sprague (1910) describes how declines in the value of railroad securities led to failures of two prestigious brokerage firms and two New York City banks. The Metropolitan National Bank, a systemically important firm with a large correspondent network, threatened to export the liquidity crunch nationwide when country banks were not able to withdraw from their New York correspondent. Bluedorn and Park Anderson (2017) trace the effect of the distress of the Metropolitan on Pennsylvania state correspondent banks. The New York Clearing House Association allowed the bank to issue a substantial amount of clearinghouse loan certificates, approximately a quarter of all such certificates issued during the crisis, preventing a major financial meltdown. Hoag (2023) evaluates high frequency data on loan certificate borrowing by New York Clearing House member banks. Wicker (2000) categorizes events in 1884 as an “incipient” crisis that could have been much more damaging without the swift action of the New York Clearing House association.

Historical discussion often emphasizes the differences between financial crises, while other work suggests similarities in the causes if not the direct transmission mechanism of historical crises. Common origins and transmission across crises might make correlation in performance or the ability to learn from crises more likely. Calomiris and Gorton (1991) emphasize the declines in the stock market and the asymmetric information problem of the revaluation of bank assets as a consistent feature of financial crises. Hanes and Rhode (2013) argue that weaker cotton crop harvests in the year before financial crises, causing smaller gold inflows and monetary stringency, explain the financial crises of the National Bank Era (1863-1912). These panic origin stories might generate an expectation of considerable correlation in bank performance across crises, especially since the bank regulatory framework remained relatively stable over this period.

2. Method

This paper tests two hypotheses about bank performance during financial crises. First, is bank performance correlated across financial crises? If firm culture or a persistent investment strategy in terms of its loan portfolio leads to consistent results across financial crises, we might expect a positive correlation. Second, after controlling for correlation in bank outcomes across crises, does bank executive experience influence bank performance during crises? Bank executive experience might improve or weaken performance directly, or experience might alter the correlation observed between crises for banks with experienced executives.

2.1 Design

Following Fahlenbrach et al. (2012), this paper focuses on the most senior bank officer, the bank president. In nineteenth century banking, the bank president was a member of the board of directors and often a large shareholder but held few formal powers. Often presidents served as chair of the board of directors. Gibbons (1858) describes the authority of the bank president as based more on persuasion and personality rather than raw

authority, as boards would more frequently operate by consensus rather than strict majority vote. Presidents would also represent the bank at industry events, including important meetings of bank presidents at the clearinghouse to respond to financial upheaval. The president would initiate lawsuits and possessed the authority to sign bills or notes of the bank. As banks became larger and more complicated, additional levels of executives held significant managerial authority (Langston, 1921).

The paper defines executive experience as the bank president having served as a bank president at the same bank during a previous crisis. Note the distinction between executive experience and financial crisis experience. That is, we distinguish between witnessing a financial crisis and serving as a senior executive at the bank during a crisis. Presumably, all bank presidents in New York in a later crisis were alive during the previous crisis, and most presidents in a later crisis were probably involved in financial markets in some capacity in the earlier crisis. Bank presidents in a later crisis probably had some recollection of the previous crisis even if they were not employed as executives or were not employed at the bank where they would eventually serve as president. However, the types of decisions that bank presidents would eventually be responsible for would be somewhat different than the skills developed by employment in other industries or by employment in a lesser capacity at the bank. Further, Ahmed et al. (2019) show that bank executives with experience at the same bank or having more directors with experience had even higher returns on assets in a subsequent crisis than simply possessing experienced executives alone. Note that we have not ruled out the possibility that New York bank presidents had experience as bank president in some city other than New York.

The endogeneity of executive employment could lead to other explanations for the benefits of experience in the event that any are observed. For example, suppose that weaker executives with poor bank performance become separated from the bank after financial crises. In that case, the survival of bank presidents to a subsequent crisis could mean that experienced executives have above average ability or have a more conservative philosophy that positions the bank to take less risk, and therefore surviving executives will perform better in the crisis even if there is no direct effect of experience, illustrating the effect of selection bias. Alternately, senior executives may tend to have more life experience or higher asset accumulations. Additional life experience might lead to cognitive changes in the evaluation of risk, or higher asset accumulations may lead experienced executives alter their risk tolerance and hence the financial position of the bank. But because the results do not find an effect of executive experience on panic performance consistently across crises, we do not attempt to unravel these possible explanations, and for the most part the previous literature does not provide solutions to these issues.

2.2 Data

The data include a sample of banks during a financial crisis in New York that can be matched with data from the same bank in a previous financial crisis (1884, 1890, 1893, 1907). Of course, banks that exist in one panic but close before the height of the next panic are not included in the sample, and naturally banks that open after 1884 may be included in subsequent crises, leading to an unbalanced panel. The data include both national and state banks as well as all New York Clearing House member banks but do not necessarily include all banks in the city, as only some nonmember banks are included. Further, we lack data about the trusts, institutions with some banking powers, which were less influential during earlier crises.

The data account for bank mergers. Mergers require careful tabulation because when two firms merge, which of the two merged firms should be considered the previous firm? The matching procedure for this paper matches banks with the same name. For banks that merged in between two adjacent crises, we match previous data for the merged bank based on the name retained by the merged bank. If the bank name is different from both the acquirer and the target firm (usually by combining the name of the two banks), we match the merged bank with data from the previous panic for the bank with the same clearinghouse number as the merged firm.

The paper measures bank outcomes during the crisis as well as bank characteristics using balance sheet information. For each bank, we obtain two balance sheets for each panic: one balance sheet from a little less than a year before the crisis and one balance sheet during the crisis. Because of the long financial turbulence before the actual crisis, we usually obtain the balance sheet about a year before the crisis began. Under the dual banking system, different types of banks reported to different regulators: national banks reported balance sheets to the Comptroller of the Currency, while state banks reported to the New York Superintendent of Banks. Further, to reduce bank window dressing, each regulator would call for reports on unpredictable dates, so reports from national and state regulators can fall on different dates (Hoag, 2015). Table 1 lists the date of observation of each balance sheet. For most sets of national and state banks, we obtain reports that fall within a week or two of each other. Generally, panic observations of balance sheets occur somewhat after the height of the crisis, so some banks may have already started recovering from the crisis by the time that data are recorded on the panic balance

sheet.

Table 1. Dates of reports of condition to bank regulators

	National	State
1884 Pre-panic	12/31/83	12/22/83
1884 Panic	06/20/84	06/14/84
1890 Pre-panic	12/11/89	01/11/90
1890 Panic	12/19/90	12/20/90
1893 Pre-panic	12/09/92	12/15/92
1893 Panic	10/03/93	09/19/93
1907 Pre-panic	11/12/06	11/14/06
1907 Panic	12/03/07	12/19/07

Historical data limitations require the consideration of different variables. Unlike Fahlenbrach et al. (2012), bank stock prices are highly illiquid during this period, so we cannot measure the health of the bank by the stock price. Instead, the paper measures the bank outcome during the crisis by the percent change in deposits. Deposit withdrawals due to bank runs or due to or an overall contraction in lending could lead to lower levels of deposits for the bank. Banks that suffered large decreases in deposits risked failure or at least a suspension of currency payments on demand. The calculation of bank deposits includes both individual deposits and deposits from other banks. We measure the status of the bank during the crisis by the percentage change in bank deposits from the pre-panic balance sheet to the panic balance sheet of the same crisis. Thus, the lagged percentage change in deposits in a previous crisis (LAG%ΔDEP) explains the percentage change in deposits in the current crisis (%ΔDEP).

Two variables capture executive experience. First, we include an indicator variable PRESSAME that equals one if the bank president was in office at the same bank during the previous financial crisis. Both New York state and national regulators recorded the identity of the president during this period, and we use the annual report published by regulators based on data observed before the panic to determine the identity of the executive (with one exception for state banks in 1893 which uses the 1895 report based on data from 1894). Second, when it is feasible in some versions of the model we interact the percent change in deposits from the previous crisis with the PRESSAME variable (LAG%ΔDEP * PRESSAME). We might expect banks with the same president across crises to exhibit a greater correlation in outcomes than banks that experience executive turnover.

Matching banks across call reports results in a panel of 209 bank-panic observations for the years 1907, 1893, and 1890, where each bank observation in the sample can be matched to the performance of a predecessor bank in the prior panic. But the PRESSAME variable is missing for banks in 1884, because that would require knowing whether the executive were the same as in the previous crisis of 1873, which is currently outside the sample. Thus, we essentially have an unbalanced panel where we have observations on $t = 3, 2, 1$, and mostly 0 (except for PRESSAME). Unfortunately, not every bank observed in a crisis that can be matched with an observation in a previous crisis is observed in every time period in the sample, as banks enter or exit the sample for various reasons, and at this point we do not attempt to correct for the selection of bank observations into the panel. For estimators that require a bank to have three observations, we have at 121 individual bank-panic observations, with some banks observed twice. For estimators that require a balanced panel where every bank in the sample is observed at all four time periods, we have only 48 banks.

The data provide a sense of the persistence of executive employment. In the sample of 208 bank-panic observations that can be matched with a previous panic, sixty percent of bank presidents in the sample who experienced a financial crisis served in the same capacity at the same bank in the next financial crisis. Across the first two sets of crises, from 1884 to 1890 and 1890 to 1893, 70% and 71% of the presidents in the sample remained in office. Only 35% of presidents employed in 1907 in the sample served in the same office during the panic of 1893, as of course the longer period between the crises would lead to greater executive turnover.

The analysis controls for other variables involving the status of the bank from the prepanic balance sheets of the later crisis. We include the total capital to asset ratio, CAP/ASSET, as well as the reserve ratio, RESRAT, from the prepanic bank balance sheet. The capital to asset ratio is the sum of capital and surplus divided by total assets. We measure the reserve ratio, RESRAT, as the sum of specie plus legal tender notes over total deposits, where total deposits are the sum of individual deposits and deposits due to other banks. The variable also includes reserves due from other banks for state banks in 1907 but not in other years. Note that this ratio is slightly

different than the reserve ratio that would have been calculated by regulators. The size of the bank is represented by the logarithm of total assets, or LNTOT. Sprague (1910) describes how certain large banks, known as "interest-paying" banks because they paid interest on demand deposits made by correspondent banks located on the agricultural interior of the country, might be riskier than other banks and expect greater deposit outflows. We include BANKBAL, which measures deposits due to banks as a fraction of the sum of deposits due to individuals and deposits due to banks. Because we are interested in capturing unobserved characteristics of the banks through fixed effects, we omit variables that are mostly time-invariant, such as bank charter type (whether the bank was chartered by the federal government or by the state government) or clearinghouse membership. Though the data suffer some multicollinarity, the condition number of 12 falls below a standard cutoff of 30 for extreme multicollinearity. Table 2 presents summary statistics of the relevant variables.

Table 2. Summary statistics

Variable	Obs.	Mean	Std. Dev.	Minimum	Maxiumum
%ΔDEP	209	-0.1003625	0.1783359	-0.718168	0.4388894
LAG%ΔDEP	209	-0.0753088	0.1898095	-0.6407515	0.6818807
PRESSAME	209	0.6028708	0.490478	0	1
LAG%ΔDEP * PRESSAME	209	-0.0351557	0.1475084	-0.6407515	0.6441679
CAP/ASSET	209	0.1895005	0.0678982	0.0680886	0.3875339
RESRAT	209	0.2021066	0.0576775	0.0404199	0.3232242
LNTOT	209	15.76432	1.100785	13.16312	19.26982
BANKBAL	209	0.2025761	0.2145795	0	0.750198
YEAR1890	209	0.3205742	0.4678178	0	1
YEAR1893	209	0.3923445	0.4894451	0	1
YEAR1907	209	0.2870813	0.4534861	0	1

2.3 Empirical Method

This paper employs a dynamic panel data model to capture potential correlation in bank performance across panic years. The results will focus on the effect of the performance of the bank in the previous crisis through the coefficient on LAG%ΔDEP as well as the coefficients on both the indicator variable PRESSAME and an interaction term for the effect of executive experience through the coefficient on LAG%ΔDEP * PRESSAME after controlling for other prepanic bank characteristics. Thus, the regression equation is:

$$\%\Delta DEP = \beta_0 + \beta_1 LAG\%\Delta DEP + \beta_2 PRESSAME + \beta_3 (LAG\%\Delta DEP * PRESSAME) + \beta_4 (CAP/ASSET) + \beta_5 RESRAT + \beta_6 LNTOT + \beta_7 BANKBAL + \varepsilon \quad (1)$$

where we drop the bank *i* subscript and the time subscript *t* for convenience. The model includes time indicator variables when appropriate, but since the model includes a constant, with at most four time periods we will have at most two time indicator variables, and potentially less if additional lagged variables are involved.

This paper employs dynamic panel data models to avoid potential inconsistency due to the lagged dependent variable (Nickell, 1981). Although the lagged dependent variable is not strictly exogenous, it might be sequentially exogenous conditional on the unobserved effect (Wooldrige, 2002, p. 299). Anderson and Hsaio (1982) use the lagged difference of the dependent variable as an instrument for the lagged dependent variable, and we can instrument for the interaction term as well. Because we have four panic years, instrumental variables estimation where the instrument is the twice lagged level of the dependent variable reduces the number of time periods to at most two time periods.

The instrumental variables estimator presumes a condition called dynamic completeness conditional on the unobserved effects (Wooldridge, 2002, p. 300), in that one lag of the dependent variable sufficiently captures its dynamics. Note that dynamic completeness does not imply that previous crises do not provide information, only that previous crises provide no more information than that contained in the latest crisis. That is, all of the lessons learned in older crises were applied in the most recent crisis. This assumption is less implausible since the panics of 1893 and 1907 were most likely more chaotic and more damaging to the macroeconomy of the United States than the crises of 1884 and 1890.

But since the instrumental variable estimation is not necessarily efficient, we consider one step estimation following Arellano and Bond (1991). In addition to recent lagged differences of the dependent variable, Arellano and Bond (1991) use additional instruments from further past lagged differences for later observations in the data

set, increasing the number of orthogonality conditions in a GMM procedure. Since essentially $T = 3$, with one additional zeroth observation, the number of additional orthogonality conditions is not explosively large, reducing concerns about the tradeoff between bias and efficiency of the estimates. Arellano and Bond (1991) provide a diagnostic test of first order serial correlation in the errors, and the procedure requires a rejection of first order autocorrelation of the residuals. Unfortunately, with a short panel in the time dimension, the diagnostic test that evaluates second order autocorrelation in the error terms cannot be computed.

We also consider another estimator for the dynamic panel model. Moral-Benito (2013) describes subsystem LIML, or ssLIML, a maximum likelihood estimator of the simultaneous equations model. Although the estimator assumes normality, under sequential exogeneity conditional on the unobserved effects the estimator is consistent and asymptotically normally distributed. Moral-Benito, Allison, and Williams (2019) conduct a Monte Carlo investigation to show that the estimator has smaller bias and tighter parameter estimates than the traditional Arellano-Bond estimator, even when the data are missing up to 10% of observations or are generated by nonnormal data. The Arellano-Bond estimator does have an advantage for our data in that the estimator does not require the full range of the time observations for each data point in the regression (under dynamic completeness of one lag, two lags of the dependent variable for the instrument are sufficient), whereas the maximum likelihood estimator ideally employs all four time observations for each data point (Williams, Allison, and Moral-Benito 2018), which will significantly reduce the number of useable observations because not all banks survived for the entire sample.

3. Results

We first confirm the choice of fixed effects over random effects using a Hausman test (Wooldridge, 2002). The Hausman test rejects the null hypothesis that random effects estimation and fixed effects estimation are similar ($p = 0.049$), thus we proceed with fixed effects. We omit the lagged dependent variable from the estimation of the Hausman test, otherwise both estimation methods are inconsistent.

Table 3 presents estimates of Equation (1) that evaluate the determinants of the percentage change in deposits in a financial crisis based on panic outcomes in the previous financial crisis, executive experience with the previous financial crisis, and prepanic balance sheet data from the current financial crisis. We include individual fixed effects to account for unobserved characteristics in addition to as many time indicator variables as the estimation will permit. Column (1) begins with fixed effects estimation without the lagged dependent variable or any interaction term. Note that the coefficient of the PRESSAME variable is small in magnitude and statistically insignificant. Further, an F test that the fixed effects are all equal to zero is rejected ($p < 0.01$), so it is important to control for unobserved characteristics.

Table 3 also presents estimates of Equation (1) that account for the lagged dependent variable. We begin by setting the coefficient on the interaction term LAG%ΔDEP*PRESSAME to zero. Column (2) presents results based on a version of the instrumental variable strategy of Anderson and Hsaio (1982) where the instrument is the second lagged level of the dependent variable. We report a version of the estimates that includes correction for general heteroskedasticity and serial correlation. Column (2) shows that the percentage change in bank deposits across crises remains uncorrelated, as the coefficient on the lagged dependent variable LAG%ΔDEP is both small in magnitude and statistically insignificant ($p = 0.88$). Thus, we see no evidence of correlation of the loss in bank deposits at the same bank across crises. Further, the coefficient on the variable PRESSAME is moderate in size (-0.043) but is likewise statistically insignificant ($p = 0.31$). The experience of the bank executive in a past crisis does not appear to influence bank performance in a subsequent crisis. In general, the balance sheet control variables are statistically insignificant, though they tend to have the correct sign. In addition, the instrumental variable strategy seems to be reasonable, as the first stage regression has an F statistic of 12.62, slightly above ten, the cutoff suggested by Staiger and Stock (1997).

Column (3) of Table 3 presents results with the interaction term, where the lagged dependent variable LAG%ΔDEP is interacted with the PRESSAME variable. In this case, we use the second lag of the dependent variable and a second lag of the interaction term to serve as instruments for the regression. The coefficient on the lagged dependent variable and the interaction term are substantially larger in magnitude. The coefficient would indicate that the correlation with past deposit losses is ordinarily negative, but when a bank retains the same president the correlation is positive. However, the coefficient on both the lagged dependent variable ($p = 0.437$) and the interaction term ($p = 0.343$) are both statistically insignificant individually and are jointly statistically insignificant as well ($p = 0.64$). Deposit losses at banks with the same president do not show statistical evidence of being more correlated with past deposit losses. Due to the statistical insignificance of the interaction term, we would prefer the estimates from Column (2).

Table 3. Deposit drains in New York during four financial crises, 1884-1907

Dependent variable: %ΔDEP (standard errors in parentheses)					
Variable	Panel OLS Column (1)	Panel IV Column (2)	Panel IV Column (3)	Arellano-Bond Column (4)	Panel IV for PRESSAME Column(5)
LAG%ΔDEP	-	-0.062	-0.578	-0.031	-0.338
		(0.411)	(0.744)	(0.098)	(0.445)
PRESSAME	-0.017	-0.043	0.040	-0.033	-0.163*
	(0.026)	(0.043)	(0.095)	(0.023)	(0.099)
LAG%ΔDEP * PRESSAME	-	-	0.886	-	-
			(0.934)		
CAP/ASSET	0.682**	0.532	0.272	0.673	0.464
	(0.319)	(0.717)	(0.849)	(0.422)	(0.810)
RESRAT	0.638*	0.749	0.934	0.609**	0.989
	(0.331)	(0.598)	(0.635)	(0.279)	(0.605)
LNTOT	-0.088**	-0.080	-0.107	-0.107**	-0.050
	(0.041)	(0.059)	(0.070)	(0.049)	(0.062)
BANKBAL	-0.116	-0.258	-0.493	-0.284	-0.164
	(0.203)	(0.364)	(0.477)	(0.301)	(0.458)
CONSTANT	1.062	1.109	1.565	1.399*	0.570
	(0.652)	(0.968)	(1.144)	(0.770)	(1.029)
Overall R-squared	0.023	0.023	0.004	-	0.004
Number of obs.	209	121	121	121	121
Time indicators	2	1	1	2	1
Robust errors	N	Y	Y	Y	Y

* = significant at the 10% level, two-tailed test.

** = significant at the 5% level, two-tailed test.

Column (4) of Table 3 presents results from the Arellano-Bond estimator. Once again, the coefficient on LAG%ΔDEP remains small and statistically insignificant, and the coefficient on PRESSAME likewise remains small and statistically insignificant. Unfortunately, this estimator does not satisfy a diagnostic test. The Arellano-Bond estimator requires the first differenced errors to reject the null of no autocorrelation, and the data do not reject the null (p = 0.28), so it might be best not to rely on this estimation technique.

One might worry that the PRESSAME variable fails the assumption of strict exogeneity and is predetermined. For example, suppose that a bank suffered unexpectedly large decrease in deposits (thus the error ε_{it} was negative), and as a consequence the bank president was more likely to be replaced (thus an independent variable $x_{i,t+1}$, in this case PRESSAME, is more likely to be zero). So we might imagine that $COV(\varepsilon_{it}, x_{i,t+1}) > 0$, violating the assumption of strict exogeneity. But in this case we might still presume that the variable PRESSAME satisfies sequential exogeneity, thus one potential correction would involve instrumenting the PRESSAME variable with a lagged variable also. Table 3, Column (5) presents instrumental variable results which instrument for PRESSAME as well. The coefficient on LAG%ΔDEP is again negative but statistically insignificant. The PRESSAME variable has a more negative coefficient (-16.3%) but is statistically insignificant at the five percent level (p = 0.099). Unfortunately, in this case the instrument for PRESSAME may be weak, as the F statistic for the first stage of the regression equation PRESSAME is only 5.72, less than ten.

Table 4 presents results from the Moral-Benito (2013) estimator ssLIML. Since the maximum likelihood estimator requires a balanced panel, we are limited to 48 banks observed for all four periods. Column (1) presents the results of Equation (1) with no interaction term. In this case, the coefficient on the lagged dependent variable is small, negative, and marginally statistically insignificant. Surprisingly, the experience variable is both negative (-5.2%) and statistically significant. Three of the four control variables have the appropriate sign, and two are statistically significant at the 10% level. The maximum likelihood technique allows for treating some independent variables as predetermined, again by lagging the independent variable. Column (2) presents maximum likelihood results if we allow for the PRESSAME variable to be predetermined. Note that in this case the coefficient on the lagged dependent variable is still small, negative, and statistically insignificant. Also, the coefficient on PRESSAME is still negative but is smaller (-4.6%) and is statistically insignificant. Thus, we see limited evidence that bank deposit losses appear to be correlated across crises. Further, we can definitely

conclude that executive experience does not improve bank performance, as there is at best weak evidence that the continued presence of a bank president leads to negative performance across crises.

Table 4. Deposit drains in New York during four financial crises, 1884-1907

maximum likelihood estimation Dependent variable: %ΔDEP (standard errors in parentheses)		
	ssLIML	ssLIML with predetermined PRESSAME
Variable	Column (1)	Column (2)
LAG%ΔDEP	-0.161*	-0.127
	(0.088)	(0.085)
PRESSAME	-0.052**	-0.046
	(0.025)	(0.029)
CAP/ASSET	0.709**	0.587
	(0.345)	(0.405)
RESRAT	0.828*	0.765*
	(0.460)	(0.416)
LNTOT	-0.023	-0.103*
	(0.029)	(0.055)
BANKBAL	-0.226	-0.025
	(0.338)	(0.231)
Log Likelihood	710.09	717.60
Number of Observations	48	48

* = significant at the 10% level, two-tailed test.

** = significant at the 5% level, two-tailed test.

For completeness, we also examine the cross-section results for each individual pair of adjacent crises. By examining individual pairs of crises, we are able to include more bank observations because individual banks need not be observed for the entire panel. The data include all 209 bank-panic observations where the banks have been matched to the previous crisis. Table 5 displays regression results based on Equation (1) for three pairs of panics: 1884 to 1890, 1890 to 1893, and 1893 to 1907. In general, the coefficients on LAG%DEP which measures the correlation of bank performance between two crises are small and statistically insignificant, and most often there is no premium to experience as a bank president. These results appear similar to the panel data results with two exceptions. First, in column (2), the presence of the same bank president from 1890 led to a statistically significant decrease in bank deposits in 1893 of about six percent. No other individual panic year experienced a negative premium to presidential experience. Second, column (4) in Table 5 suggests that when we omit the interaction term, bank performance in 1907 is marginally statistically significantly correlated with bank performance in 1893 (p = 0.056). Observing a correlation in 1907 only is somewhat surprising due to the longer time period between crises when compared to crises observed in other years. Further, the origins of the two crises appeared very different, as the crisis of 1907 involved concern about speculation in copper markets and the solvency of trusts as opposed to maintaining the gold standard and agricultural issues in 1893. However, because testing three crises separately generates a higher probability of identifying at least one statistically significant coefficient, we would not necessarily conclude that the coefficient represents a statistically significant effect, and in this case we would suspect that there is not a strong correlation for the percentage change in deposits across most financial crises. In these cross-sectional regressions, the other independent variables such as capital/asset ratio (CAP/ASSET), bankers' balances (BANKBAL), and bank size (LNTOT) often have the expected sign but are not necessarily statistically significant.

Table 5. Deposit drains in New York in individual crises of 1890, 1893, 1907

Dependent variable: %ΔDEP (standard errors in parentheses)				
	1890	1893	1907	1907
Variable	Column (1)	Column (2)	Column (3)	Column (4)
LAG%ΔDEP	0.130	0.114	0.380	0.426*
	(0.191)	(0.097)	(0.269)	(0.218)
PRESSAME	0.022	-0.062**	0.046	0.030
	(0.044)	(0.027)	(0.085)	(0.063)
LAG%ΔDEP* PRESSAME	-0.268	-0.124	0.126	-
	(0.205)	(0.138)	(0.426)	
CAP/ASSET	0.431	1.060	0.307	0.293
	(0.271)	(0.201)	(0.676)	(0.668)
RESRAT	-0.204	0.423	1.116	1.151
	(0.354)	(0.260)	(0.788)	(0.773)
LNTOT	0.117**	0.046**	0.011	0.010
	(0.034)	(0.020)	(0.041)	(0.040)
BANKBAL	-0.346**	-0.127	-0.189	-0.182
	(0.121)	(0.079)	(0.224)	(0.221)
CONSTANT	-1.860**	-1.114**	-0.412	-0.404
	(0.462)	(0.280)	(0.673)	(0.667)
R-squared	0.297	0.444	0.146	0.144
Adjusted R-squared	0.214	0.391	0.031	0.047
Number of Observations	67	82	60	60

* = significant at the 10% level, two-tailed test.

** = significant at the 5% level, two-tailed test.

4. Conclusion

This paper considers four financial crises in the United States from 1884 to 1907 to investigate the influence of previous panic outcomes on outcomes in the next financial crisis. In contrast to the previous literature, the results find little correlation between bank performance in adjacent crises. At least in terms of bank deposits, the previous financial crisis does not contribute to an explanation of what will happen to the bank during the subsequent crisis. The shadow of the past financial crisis does not influence the destiny of the bank during the next crisis. One exception might be in 1907, when deposit losses in the crisis of 1907 were perhaps mildly correlated with deposit losses from the crisis of 1893. Further, executive experience at the firm does not appear to improve firm performance during financial crises. There is even some evidence suggesting that the continued presence of the bank president reduces bank deposits during crises. Even though banks were smaller and might be more easily guided by a single executive, historical forces do not appear to operate through the presence or absence of the primary executive who led the bank in the previous crisis. Despite a potential common origin of panics, the consequences of panics propagate across financial institutions in a different manner in each crisis.

Even if executive strategy or firm culture do not necessarily lead to persistence in bank outcomes across historical crises, firm decisions about surviving a banking panic might potentially be related across crises. Bank executives might pursue similar tactics across crises based on their previous crisis experience. One example of a decision that a bank executive could pursue would be to borrow from a lender of last resort. A companion paper (Hoag, 2019b) evaluates the correlation in executive decisions across crises to borrow clearinghouse loan certificates, which represents a quasi-lender of last resort. Executives might decide to borrow more or less from a lender of last resort based on their borrowing during the previous crisis, reflecting a level of familiarity with the instrument. Even if we do not observe a correlation in bank outcomes based on previous experience, perhaps we might observe a correlation in bank executive decisions.

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