

Response to Célérier and Tak (April 2021)

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Stein and Yannelis (2020) (SY) study the short-term impact of the Freedman's Savings Bank on human capital, labor market, and wealth outcomes. This short note is a response to Célérier and Tak (2021) (CT), which offers comments on SY, claiming to “empirically reject the assumptions of the study’s identification strategy” and arguing that “financial inclusion can be detrimental to minorities.” We show their claims are driven by a serious data error and by omitting data, and that their empirical tests do not evaluate the identification assumption in SY. After using an alternative matching strategy which throws out four-fifths of matches, CT present estimates with very large standard errors. We show that these estimates cannot statistically reject large effects, including many of the point estimates in SY.

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We are excited by the growing empirical literature in economics and finance using historical data to assess the short- and long-term effects of the rise and fall of the Freedman's Savings Bank, which includes Fu (2020), Fu (2021) and Traweek and Wardlaw (2021). In Stein and Yannelis (2020) (SY), we study the impact of the Freedman's Savings Bank on labor market, education, wealth, and entrepreneurial outcomes, and find significant effects. SY matches data from Bank records to the Census, and use complementary identification strategies, generating variation in opening accounts based on the distance from built bank branches relative to those that were planned, local electoral vote shares and the timing of account opening.

While the focus of SY was on assessing very short-term outcomes—which we found to be generally positive as of 1870, before the bank collapsed—there is other evidence of both short and long-term effects. For example, Fu (2020) matches Bank records to the 1880 and 1900 Census, finding that there was some persistence of positive effects over the following ten to thirty years. Fu notes that “using a different identification strategy, my study confirms the schooling and literacy finding of Stein and Yannelis” for 1880 and 1900 (p. 7), with “the positive literacy effect explained by an increase in school attendance and literacy for the depositors’ children prior to the bank failure” (p. 1).¹

In a new working paper, Célérier and Tak (2021) (CT) offer comments on SY, claiming to “empirically reject the assumptions of the study’s identification strategy” and “find[ing] no evidence of any positive effects” (CT p. 1) of the Freedman's Savings Bank, concluding that “financial inclusion can be detrimental to minorities” (CT p. 43).² In this short note, we document several issues with the implementation and interpretation of the CT critique.

CT replicate analysis from SY, which finds that the Bank had effects on human capital, labor market, and wealth outcomes, offering three main critiques. First, they argue that the exclusion restriction in one of the strategies used in SY is violated. Second, they argue that the matching procedure in SY generates many false matches and present an alternative matching procedure which is similar to Fu (2020). Third, they present a strategy employing the timing of when an individual opened an account to study outcomes.³ In this note we show that these claims

¹Identification in Fu (2020) is based on cross-county variation in the take-up rate of FSB accounts. It is described in Section 4.1 of the paper, which also documents that this take-up rate is uncorrelated with a number of county-level socioeconomic and demographic characteristics.

²References to CT refer to a version dated April 12, 2021, posted on SSRN, and now archived at https://lukestein.github.io/ctsy_response/Celerier_Tak_April2021.pdf.

³CT present this as a new and alternative approach relative to SY, although the strategy is quite similar in spirit to that presented in Table 6 of SY and in Table 3 of Fu (2020). The results in SY and Fu (2020) are also similar, even though SY uses 1870 outcomes and Fu uses 1880 outcomes, and Fu looks specifically at outcomes for children of account holders. Fu finds coefficients for literacy and schooling of 0.085 and 0.038, respectively, which are comparable to the values of 0.05 and 0.017 found by SY.

are driven by a serious data error, throwing out data, and using a strategy and sample entirely different from that in SY.

The first claim made by CT—that an exclusion restriction violation invalidates the distance-based identification strategy—is due to a misunderstanding of the strategy used in SY, and a data error which materially changes results. The primary challenge that CT argue is that the presence of Bank branches is correlated with the number of Black troops, a point discussed extensively in SY (Section 1.1.4). However, CT make a serious data error and reach the conclusion that there were more Black troops in the 37 branch counties post-war than at the peak of the U.S. Civil War. This mistake was likely due to a coding error. When the data error is corrected the significant correlation disappears, and is very close to zero when outcomes are measured. CT also inexplicably throw out school data from nine states, and mistakenly refer to private schools, the availability of which may have been impacted by access to financial services, as if run by a government agency, the Freedman's Bureau. Finally, CT claim that one of the instruments in SY is correlated with 1860 outcomes for Whites. They make this claim by (i) using a different sample, which includes Northern cities like New York and Philadelphia, (ii) focusing on Whites and (iii) running an alternative empirical strategy which does not assess SY identification assumptions. When a strategy similar to that in SY is employed, comparing branches built pre-1870 to branches built or planned to be built post-1870, the relationship largely disappears.

In their second claim, CT are of course correct that the matching procedure in SY is likely to generate some false matches, a point discussed extensively in SY Section 2.1, and which helps motivate the use of an IV strategy. CT propose an alternative matching strategy which (i) matches on characteristics which are frequently missing from Bank and Census records, (ii) matches on characteristics whose availability is correlated with key outcome variables, and (iii) does not attempt to match the family members of account-holders. The first issue leads them to incorrectly claim that SY has high rates of “false positive” matches. The second issue creates selection issues in their sample that invalidate their falsification tests. And the third issue causes CT to throw out 70–80% of the sample, leaving them under-powered to empirically reject effects of the magnitudes estimated in SY.

Finally, CT use an alternative strategy, comparing individuals who opened accounts before and after 1870 (when SY's outcomes are measured). CT claim that this alternative strategy leads to opposite conclusions from SY, in particular Table 6 which presents a similar approach. Our interpretation of their results is very different. While their estimates are generally statistically insignificant—the interpretation they emphasize—the point estimates and standard errors are large, and in many cases the estimates are larger than point estimates in SY. The large standard errors are consistent with their matching procedure throwing out approximately four-fifths of

individuals. For outcomes such as income and wealth, their point estimates are much larger than the SY estimates (Tables 9(A) and 9(B) of CT compared to Table 6 of ST), with 95% confidence intervals on the CT estimates unable rule out effects that are respectively 47 and over 100 times as large as in SY.

In the remainder of this note we further elaborate these points.

1 Exclusion Restriction in Stein and Yannelis (2020)

CT argue that the instrumental variables' exclusion restriction in SY is violated due to a correlation between the distance instrument and presence of soldiers, Freedman's schools and Freedman's bureau offices, as well as various pre-determined characteristics. While exclusion restrictions always rely on untestable assumptions, CT's argument is driven by serious data errors and by considering empirical specifications different from those in SY.

1.1 Black Troops

A significant element of CT's critique of the validity of our research design is related to the relationship between the presence of soldiers and branch locations. The relationship between the Freedman's Bank and the military is discussed extensively in Section 1.1.4 of SY. Out of concern about related issues, SY documents in Table A7 robustness of our results to the use of two alternate samples designed to minimize the ability of veterans to drive estimated effects of the bank: excluding households most likely to contain a veteran, and excluding states from which many Union Army Colored Troops were enrolled.

CT also state that they "compute the average monthly troop population in each county from May 1865 to December 1866" (p. 11–12), an exercise we attempted to repeat with the same data from Downs and Nesbit (2015) that they use. However, we reach a very different result. The numbers in Table 2 of CT seem to imply that the average numbers of troops in both branch and non-branch counties is implausibly high; indeed their analysis suggests that there were more Black soldiers stationed in then branch counties alone after the war than were in the Union Army during the entire war, including those killed in action. For example, Gladstone (1990) documents that 178,895 Union Army Colored Troops served in the Civil War; according to CT, in the 37 branch counties alone there were 214,600 Black troops in the year following the end of the Civil War.

A coding error may explain this discrepancy in the CT results. It appears that the CT analysis may have *added* together each month's troop counts rather than averaging across months,

counting each soldier numerous times depending how long they were stationed. Data from our replication with and without the erroneous addition is shown in Figure 1.⁴ County-level averages over this period were substantially lower, as the troop presence in most counties quickly drew down to zero after August 1865. When this error is corrected, the large and significant difference between counties in CT disappears.

SY discusses the relationship between the Freedman's Bank and the military and note that "Branch expansion was tightly linked to the presence of Black troops... In the first two years discharged soldiers' funds made up the majority of accounts; however, after this early period, non-veterans quickly opened accounts and former military personnel came to represent a relatively small share of depositors (Osthaus 1976)" (p. 5343). The data are consistent with the discussion in SY. Figure 2 shows the location of Black troops in 1865–66 and 1870, while Figure 3 shows point estimates and 95% confidence intervals from a regression of the number of Black soldiers on an indicator for the presence of a branch in the county, controlling for distance. Figure 2 shows that there are few Black troops in 1870, when outcomes are measured in SY. Figure 3 shows that there is a weak and marginally significant relationship between Black troops in 1865 and 1866, the period which CT sum over, and that this relationship disappears by the end of 1866.

1.2 Freedman's Schools

CT argue that the Bank created branches in many counties with or near locations with a high probability of containing schools for Freedmen. To assess the geographic distribution of schools for Freedman relative to branch locations, we follow CT in using the 1869–1870 Bureau school reports, which we access through the National Museum of African American History and Culture. To determine the locations of schools, we look at the standardized forms available on microfilm in the reports of "School Buildings owned by Bureau R., F., and A. L., and by other parties engaged in the Education of Freedmen."⁵ These reports each contain two parts: The first contains buildings owned directly by the Bureau of Refugees, Freedmen, and Abandoned Lands; the second buildings owned by other parties.⁶ CT restrict attention to a subset of states (citing a lack

⁴We were unable to perfectly replicate their sample. The authors did not respond to our request to share data, despite the fact that we shared data and code in March 2020. We typically would not point out coding errors in such a public fashion and would contact authors privately, but CT did not send us a draft of their working paper, which we initially found via Google Scholar.

⁵These are available at Section 5 of <https://sirismm.si.edu/EADpdfs/NMAAHC.FB.M803.pdf>. CT curiously refer to these schools as "Bureau schools" even though only a small minority of these schools were directly operated by the Freedmen's Bureau.

⁶For directly-owned buildings, the reports include fields for location by town and city, building construction material, building condition, the value of the building, who owns the land, and the estimated value of the land.

of data availability on p. 15, although some data appears available for all Southern and border states); in Figure 4 we show the identity of counties with at least one school for Freedman relative to the location of FSB branches built prior to 1870 or planned as of 1870. Panel (a) shows all states, while panel (b) is restricted to those considered by CT. We also include *all schools* for Freedman from the 1869–1870 school reports, not simply those owned directly by the Bureau.

CT’s concern is related to their suggestion that schools were more likely available closer to branch counties than further from them. It is important to recognize that their underlying comparison is fundamentally between central and outlying counties, and therefore fails to speak directly to any relevant endogeneity of branch locations. Indeed, Table 4 of CT shows that they find *no* statistically significant difference in school presence based on whether a potential branch was planned as of 1870, or whether it was ever built at all. The possibility that schools were more likely to be built in central counties than outlying ones—but were not more likely to be built in branch than planned branch locations—is entirely consistent with our identification strategy, and with the results in SY Tables 13 and 14 showing that even planned branch counties had somewhat higher school attendance. As shown in SY Table 14 and Figure A5, the association of proximity with school *attendance* is driven by proximity to built branches, not planned branches, consistent with a causal effect of access to the Bank.

An additional issue with interpreting any proposed differential availability of schools as a threat to identification is that schools were presumably built and expanded in response to demand for education—demand which was encouraged by financial inclusion. Access to the Bank is likely to have encouraged demand for education, and therefore perhaps school availability, for all the reasons discussed in SY. Furthermore, Fu (2020) documents that Freedman Bank cashiers “served as spokesmen for the promotion of education,” with important ties between the Bank and the American Missionary Association, which “after the Civil War... rose to be the strongest missionary association in the efforts to educate the freedman... [and] established schools throughout the South for African Americans” (p. 28). Given these mechanisms, it may be more natural to consider school availability as an outcome of access to the Bank.

1.3 Pre-1870 Characteristics

CT argue that the exclusion restriction in SY is violated, by running a different specification from that in SY on a different sample. The distance identification strategy in SY relies on comparing individuals who lived near branches that were built in 1870 (when outcomes are measured), which those who live near branches that were built after 1870 or which were planned but not

For buildings owned by other parties, the reports describe ownership and building dimensions.

built. SY instrument using indicators of whether individuals lived in counties with branches, and the distance from a built branch, including a rich set of controls and fixed effects. The SY strategy does not rely on unconditional comparisons of branch counties with other counties, which is what CT assess.

CT look at the variables mentioned in the previous subsections, as well as 1860 outcomes for Whites. Importantly, they do not differentiate between pre-1870 and post-1870 and planned branches, and further include northern branches in cities like New York and Philadelphia and look at outcomes for Whites.⁷ Some of the variables that they use might reasonably be considered outcome variables—such as the presence of schools, as discussed in the previous subsection—or included as controls in our model, such as the metro area. Still, after running a specification similar to that in SY, the outcomes chosen by CT are generally not correlated with the SY instruments, conditional on observables.

As discussed extensively in SY, for example on p. 5349–5550, our empirical strategy relies on comparing branches that were built at different times, or planned and not built, noting for example that “*we do not need to assume that Blacks living near bank branches were similar to other Blacks in the 1870s South, but rather that Blacks living near branches were similar to Blacks who lived near branches that were built or scheduled to be built after 1870.*”

The analysis in CT does not evaluate the empirical strategy in SY as is claimed, and instead evaluates a different strategy that is not used in the paper. We revisit the analysis in CT by running a specification near what is actually run in SY. Table 2 shows specifications similar to Table 14 in SY. More precisely, we regress the CT outcomes on proximity to the nearest branch or planned branch, allowing these effects to differ based on whether a branch or planned branch is closer. More precisely, we estimate

$$y_i = \alpha_t + \alpha_c + \zeta_1 BP_i + \zeta_2 NB_i + \zeta_3 M_i^{BP} + \zeta_4 NB_i \times M_i^{BP} + \nu_i, \quad (1)$$

where BP_i is an indicator for whether an individual is located in a county with a branch or planned branch, NB_i is an indicator for whether the individual lives nearer to a branch than a planned branch, and M_i^{BP} is the distance from the nearest branch or planned branch.

Table 4 shows estimates of ζ_1 and ζ_2 , showing the coefficients on a county having a pre-1870 branch and the distance to a pre-1870 branch. The results are generally not statistically significant at conventional levels. Out of the 30 regressions, two are marginally significant at the 10% level. We would expect that three coefficients would be significant by pure chance, so

⁷It is important to note that the vast majority of Black Americans are not included in the 1860 CT sample, as they were enslaved during this time period and therefore not enumerated in the 1860 decennial census’ main forms. The SY sample is restricted to Blacks in the South.

the fact that two coefficients are marginally significant is consistent with our empirical strategy being uncorrelated with 1860 outcomes for Whites and the other variables picked by CT.

2 Matched Census and Freedman’s Savings Bank Account Records

CT’s analysis of matched FSB and Census data relies on a methodology different from SY in a number of respects. We acknowledged extensively in SY that its matching is imperfect, which serves as an important motivation for the use of an IV strategy. However, CT’s procedure effectively throws out 70–80% of all listed individuals, by not counting individuals in a household where someone else opened a Freedman’s Bank account. For example, children may have been impacted by their parents’ having accounts, as found in both SY and Fu (2020). CT then use a strategy similar to that in Table 6 of SY, but find much less precise estimates.

2.1 Matching Procedure

While SY relied on (Ruggles et al., 2015) sample data, CT—like Fu (2020)—use methodologies based on Abramitzky et al. (2012, 2014, 2019) to match Freedman Savings Bank records to the full-count 1870 census. In addition to the use of the full-count data, Fu (2020) and CT differ from our approach in several key ways.⁸

The first critical distinction is that in SY, we attempt to match all family members of the principal account holder. To the degree that the effects of financial inclusion can operate at the household and family levels, it seems reasonable to consider account holders’ listed family members as potentially treated. For example, a parent having an account may affect their child’s schooling, and married couples may share an account.⁹ Without taking advantage of the opportunity to match to these individuals in the the Banks’ listings (approximately 70—80% of all listed individuals), even the full-count census data appears to leave CT under-powered to empirically reject effects of the magnitude estimated in SY, and without the precision to conclude that “financial inclusion can be detrimental to minorities” (CT p. 43).

The second key distinction is related to the use of additional matching variables by CT; while SY attempted to match Black individuals living within 50-miles of a pre-1870 branch by name, CT also assess matches using age, place of birth, and parents’ initials. However, these variables

⁸CT’s matching strategy and use of the full-count census data are extremely similar to those used by Fu (2020), who notes that “using a different identification strategy, my study confirms the schooling and literacy finding of Stein and Yannelis” for 1880 and 1900.

⁹Female labor force participation was common among Black Americans during this time period (Oubre, 1978).

are frequently missing, both from the Census and Bank records. In particular, the Bank only collected a fuller set of information on principal account holders, not their family members. We should therefore *expect* a limited ability to match on these additional variables, especially for family members, but CT incorrectly label as a “false positive” whenever our approach fails to match any of these (usually-missing) variables.

The use of additional matching variables can improve match quality, but can also exacerbate selection issues to the degree that these additional variables are available or reliable for only a subset of records. Not only does the CT approach match on variables that are frequently missing, but these variables are missing non-randomly. Given the population studied and the historical record, we would expect—and find—that enslaved individuals or individuals held in harsher conditions likely had more missing data and more adverse outcomes; the match failures that CT incorrectly identify as “false positives” in SY *should* be more common there. Any correlations between outcomes and match failures is invalid as a falsification test of SY, and if anything suggests flaws in the use of these additional variables for matching.

CT approximately replicate our matching procedure, finding a similar rate of account-holding (15.5% vs 14.4%, p. 25). They compare this with the “much higher ratio than the one we [CT] obtain using our matching algorithm on the same dataset (2.37%)” (p. 25), which we should of course *expect* to be approximately one-fifth the size since the latter approach identifies *individual* account holders, FSB account records list on average 4.5 people, and households in our sample average 5.2 members. Of the replicated matches, CT note that only “10% have an age, place of birth, status, and father’s and mother’s initials that match,” identifying the remaining “close to 90%” as “false positives” (p. 25).¹⁰

However, these variables should only be available in the Bank records for *at most* the approximately one-fifth of listed individuals who are primary account holders, not their relatives. Even when these variables are available in the Bank records, they are often unavailable in the Census, as shown in Table 3. We should therefore expect that few records match across the two data sources on all these variables—a failure that does not indicate a “false positive.”

Account holders of course vary systematically from their relatives, and in the Census we should expect that individuals for whom these variables are present vary systematically from those missing them, mechanically driving CT’s observed correlations. Indeed, Table 4 shows systematic outcome differences for Black individuals’ whose parents’ names could not be identified in the Census.

¹⁰We rely on the numbers in the text, although Figure 5 in CT appears to show their claimed “share of false positives” as approximately 10–20%.

2.2 Estimates

After matching a selected sample, which throws out approximately four-fifths of matches, CT employ an empirical strategy similar in spirit to that used in Table 6 of SY. The strategy compares account holders who opened accounts prior to 1870 to those who opened accounts later. CT claim that their estimates indicate no effects of account holding on outcomes. While their estimates are generally not statistically significant, they often are larger than point estimates in SY. However, their standard errors are substantially larger than those in SY, perhaps due to their procedure eliminating a large portion of individuals in households with a branch.

The comparison between the estimates in SY and CT can be seen clearly in Figure 5. This figure shows point estimates and 95% confidence intervals from several estimates in CT and SY. The first two bars are based on Table 9(B) of CT. The third bars are based on Table 6 of SY. Note that confidence intervals are plotted for all the SY estimates, they are simply significantly smaller and not visible for some outcomes. For two of the outcomes, income and wealth, the point estimates are much larger than the SY estimates. In fact, 95% confidence intervals using the CT estimates cannot rule out estimates of income and wealth that are respectively 47 and over 100 times as large as the SY estimates. The CT estimates for working are quite similar to the SY estimates: 0.02 for CT and 0.0255 for SY, and the CT estimates appear marginally significant given the point estimates and standard errors provided ($.02/.01=2$, however CT do not indicate significance stars in the table.) The school attendance estimates are smaller than the SY estimates, as well as those in Fu (2020) which are quite close to the SY estimates. This may be due to the fact that the CT procedure does not include children in households with an account, who were likely most affected by the bank. Curiously, CT do not include other outcomes in SY such as literacy, for which Fu (2020) has independently shown similar effects to SY using a different identification strategy. Overall, the CT estimates should be treated as imprecise, and generally not inconsistent with those in SY and Fu (2020).

3 Conclusion

We are excited by the growing number of papers using historical data to analyze the effects of the Bank. The use of full-count Census data as in Fu (2020) and CT can allow rich investigation of many important questions. That depositors in aggregate suffered significant losses when the bank collapsed is well known, but detailed microdata on claims for partial reimbursement—used by Fu (2020) and Traweek and Wardlaw (2021)—help us understand more about who lost more and with what consequences.

There is also evidence that the collapse of the Bank had adverse long-term effects on African Americans' trust in financial institutions. There is of course no conflict between the existence of positive short-term and negative long-term effects; if anything, long-lasting shocks to trust are consistent with the Bank's importance, *supporting* the possibility that access to the Bank could have affected its customers quickly and significantly. We document in SY (p. 5374) that African Americans in 2015 who live in counties that once had a FSB branch are more likely to report mistrust of financial institutions as a reason for being unbanked, an association not present for Whites.¹¹ Fu (2021) also investigates the relationship between exposure to knowledge of the Bank's collapse and current African-American underbanking.

Trust in the financial system also played a critical role in determining who made deposits with the Freedman's Savings Bank (especially in its final, fragile months), who successfully withdrew deposits before the Bank's collapse, and who filed for (partial) reimbursement of collapse-induced losses. Traweek and Wardlaw (2021) document a number of individual and geographic factors associated with these loss determinants, relying not only on account opening records, but also on microdata with reimbursement requests and payments. Since the Bank largely served Black depositors and losses were only partially reimbursed, Black depositors clearly suffered from the collapse (consistent with the evidence on long-term trust in SY and Fu, 2021); Traweek and Wardlaw (2021) show for which Black depositors these losses may have been particularly severe, and Fu (2020) also uses microdata to show that families with larger wealth losses from the Bank's failure were less likely to send children to school and more likely to work in the decade's after the collapse.

Of course, documentation of longer-term—likely adverse effects—of the Bank's collapse is critical to understanding and fully characterizing its effects. Although CT characterize the “negative effects of the large fraud and abuse of trust” as standing “in contrast” (p. 1, 42) to the positive short-run effects *before* the collapse documented in SY, they are in fact entirely consistent with and complement results suggesting the Bank is associated with present-day mistrust of the financial system (see SY p. 5374 and Table A9, and Fu, 2021).

CT argue that the instrumental variables strategy used in SY is invalidated by non-random

¹¹Long-term analysis was not the focus of SY, which we explicitly suggested as a fruitful avenue for future research: “The new data in this paper may be used to explore many other questions in the future. The experience of Freedman's Savings Bank *may* have had other important effects on the development of African Americans in the United States. In particular, after 1870 the collapse of the bank and loss of deposits may have had adverse effects on African Americans, and potentially important intergenerational effects” (SY p. 5374; emphasis added by CT). Indeed, we characterize our (limited) evidence on long-term trust as consistent with such adverse effects. Somewhat surprisingly, CT emphasize our use of the word “may” as implying that we are “suggesting that the fraud, abuse of trust and depositor losses alone might not be enough to durably alter the trust of African American in financial institutions” (CT p. 7).

branch location. These arguments, however, are largely based on comparison of branch with non-branch locations (perhaps picking up general differences between more central and more outlying counties), and their results on the presence of Black troops seem driven by a coding error. Schools for Freedman were not statistically more likely to be near built (pre-1870) branches than planned (as of 1870) ones, but even if there were differences in school availability, this could be an outcome of demand for education driven in part by financial inclusion. Similarly, it does not appear that built and planned branch locations differ on key pre-1870 characteristics.

Full-count Census data obviously has advantages over the sample data we used in SY, where we were clear about the important role that measurement error could play in our analysis. However, without matching account-holders' household members, CT consider a sample that seems underpowered. Where their results are comparable to ours, they focus on their null results, but in fact their estimates do not allow rejection of significant effects as large—and in some cases much larger—than we find.

CT make a number of other non-scientific points, for example criticizing our introductory quote from Frederick Douglass and offer an alternative Frederick Douglass quotation. CT also use ellipses to characterize portions of our paper in ways that seem to imply the opposite of what is clear in context.¹² There are also a number of historical errors in the paper.¹³ For the sake of brevity, we keep this response document focused on the main empirical claims made in CT.

¹²For example, we write that (SY p. 5374) “Further work should disentangle whether this historical experience can at least partly explain persistent gaps in the utilization of financial services, but the possibility that the collapse of the Freedman's Savings Bank had measurable effects [on heightened mistrust of financial institutions] more than a century later is consistent with its having played a significant role in its customers' lives, and therefore with the large, positive short-run effects of financial inclusion that we estimate in this paper.” CT characterize this as “(...) the possibility that the collapse of the Freedman's Savings Bank had measurable effects (...) is consistent with (...) the large, positive short run effects of financial inclusion that we estimate in this paper” (CT p. 7), as if our estimated long-run effects were positive rather than negative.

¹³For example, CT claim that the Freedman's Savings Bank did not make loans. It is well documented that the bank made loans. Even in times when the bank was officially not allowed to make personal loans, loans were made which were referred to as “overdrafts” (Davis, 2003; Fleming, 2013; Oubre, 1978).

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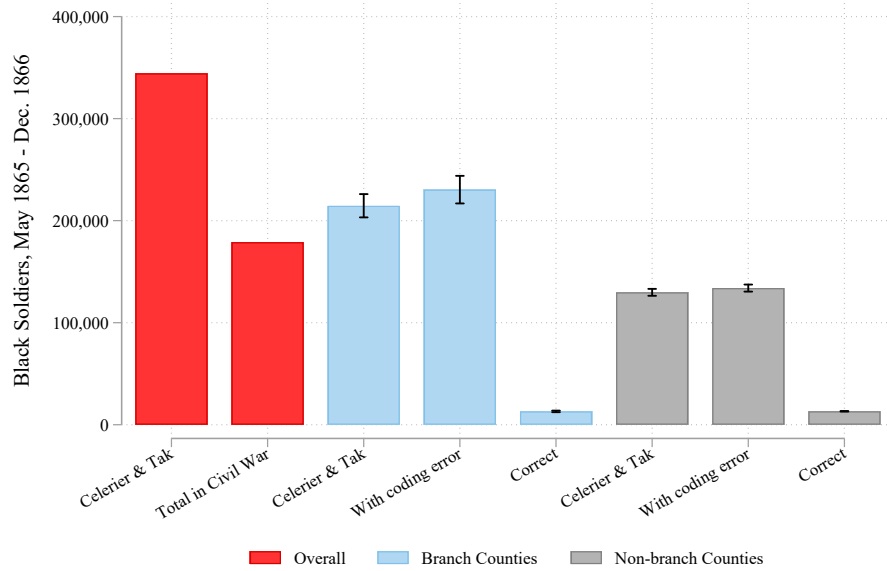
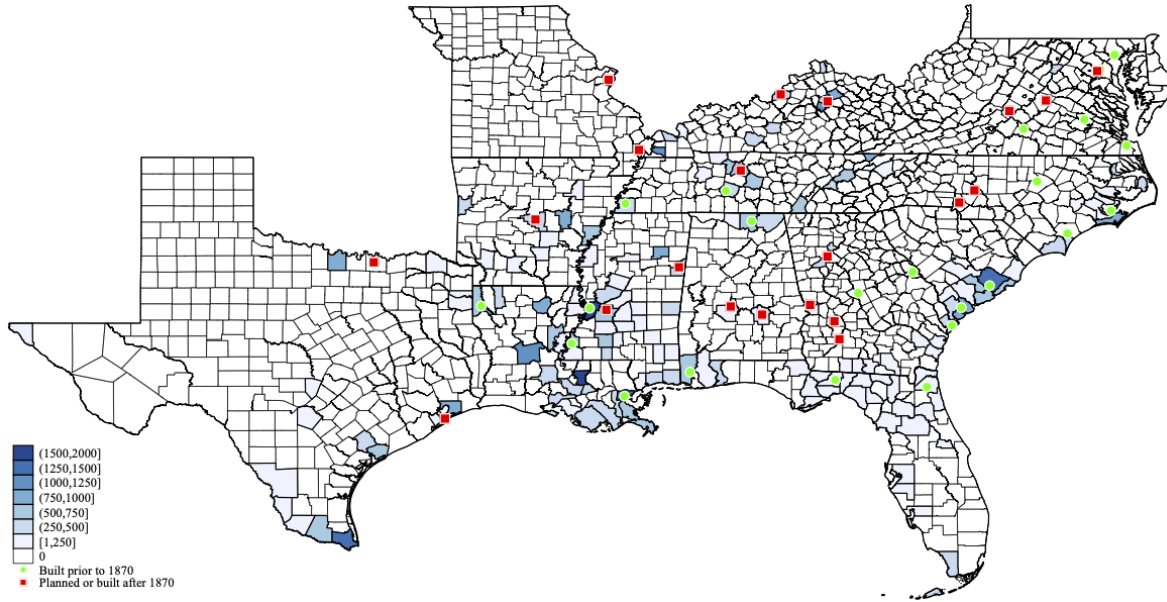
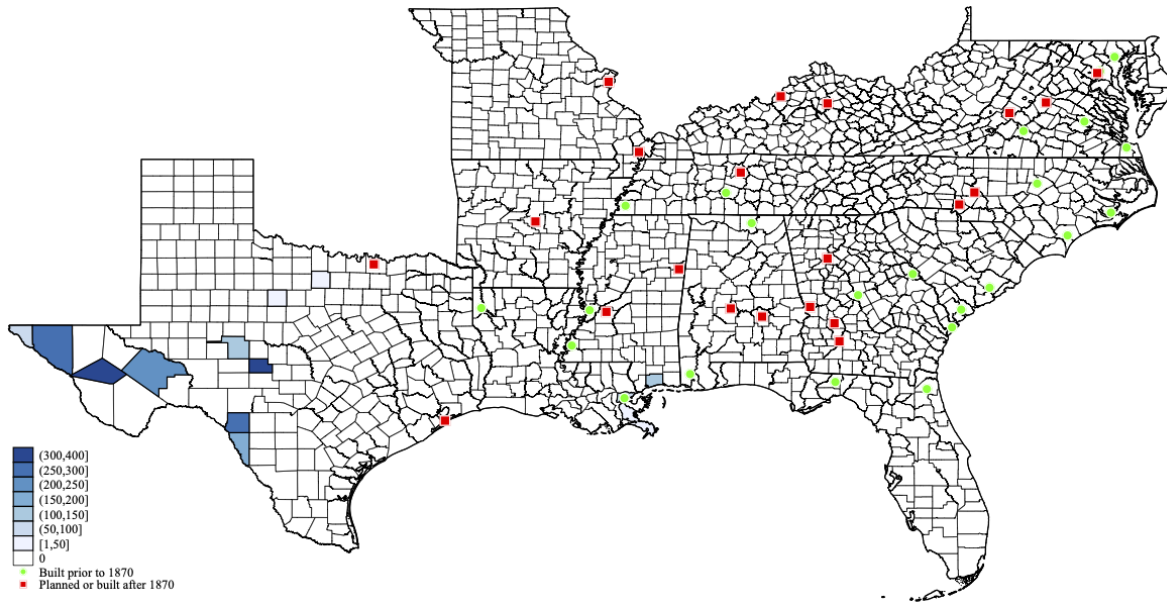


Figure 1: Black Troop Totals

This figure displays information on the number of Black soldiers in and near FSB branches after the Civil War. The first two bars display the number of Black troops in the sample counties implied by Table 2 in Célérrier and Tak (2021), as well as the total number of Black troops that served in the Union Army during the Civil War, per Gladstone (1990). The second triplet shows the number implied by Table 2 in branch counties, our replication using data from Downs and Nesbit (2015) with the coding error described in Section TK, and the corrected mean across May 1865 to December 1866. The third triplet shows analogous results for the “nonbranch” counties used in Célérrier and Tak (2021).



(a) May 1865–December 1866 Average



(b) January–December 1870 Average

Figure 2: Counties with Black Troops

These maps show counties in the South that played host to Black soldiers after the end of the Civil War, per Downs and Nesbit (2015). The numbers indicated are average number of Black soldiers that were stationed in that county during months between May 1865 and December 1866 (Panel a) and between January and December 1870 (Panel b). Many of these numbers are very low, as the troop presence in many counties drew down quickly and there are therefore many zeroes in the time series. Branches opened prior to 1870 and branches opened either in 1870 or only ever proposed (as defined in Stein and Yannelis, 2020) are also indicated.

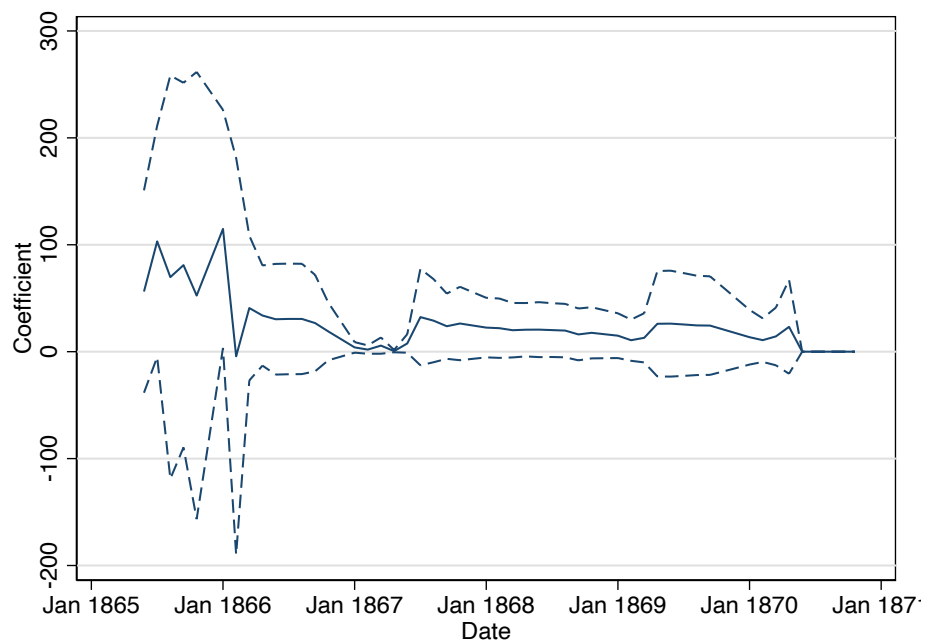
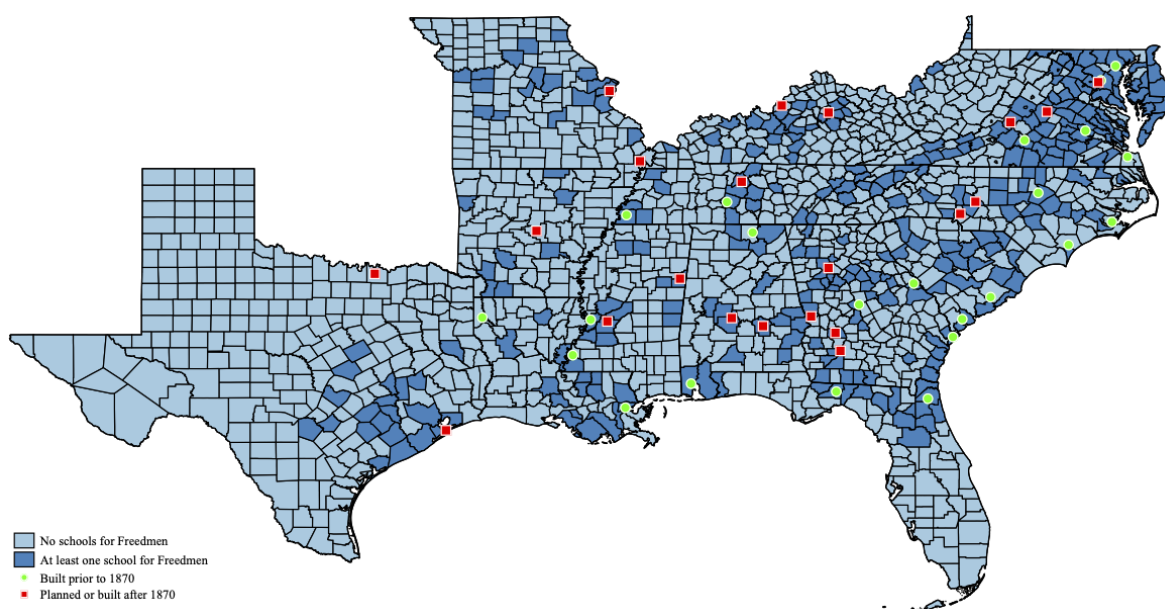
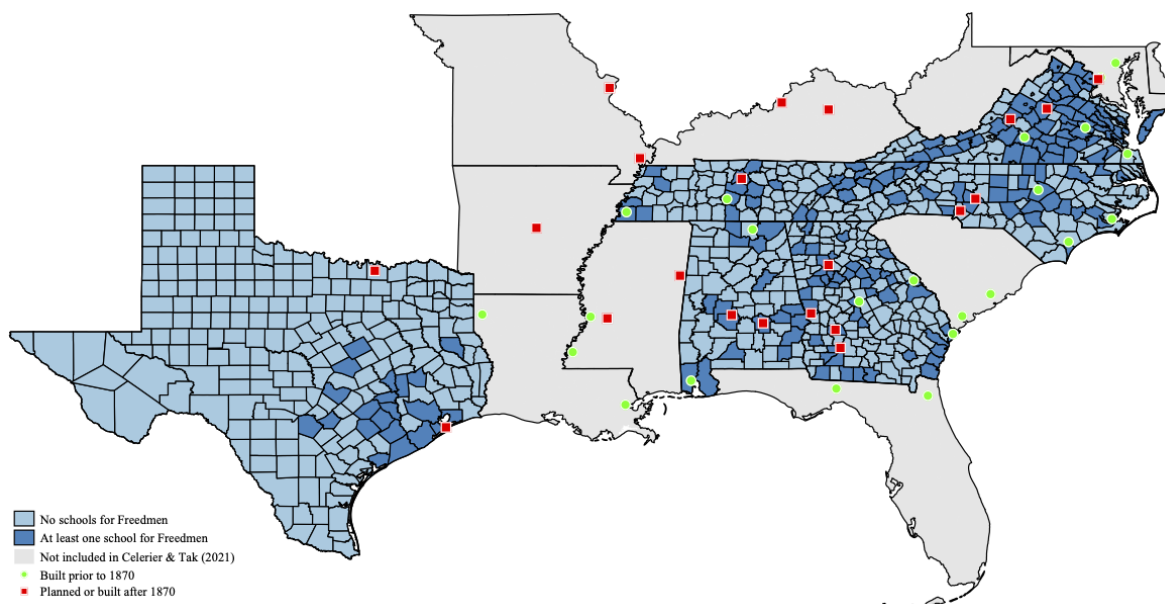


Figure 3: Branch Status and Black Troops Over Time

This figure shows point estimates and 95% confidence intervals from a regression of the number of Black soldiers on an indicator of a branch county, weighted by population. Standard errors are clustered at the branch level.



(a) Southern and Border States



(b) Southern and Border States Considered by Célérier and Tak (2021)

Figure 4: Counties with Schools for Freedmen

These maps indicate counties with at least one school for Freedmen after the Civil War, according to the 1869 School Reports (of African American History and Culture, of African American History and Culture as used by Célérier and Tak, 2021). Panel (a) includes Southern and border states; Panel (b) restricts the schools data to the subset of these states considered by Célérier and Tak (2021). Branches opened prior to 1870 and branches opened either in 1870 or only ever proposed (as defined in Stein and Yannelis, 2020) are also indicated.

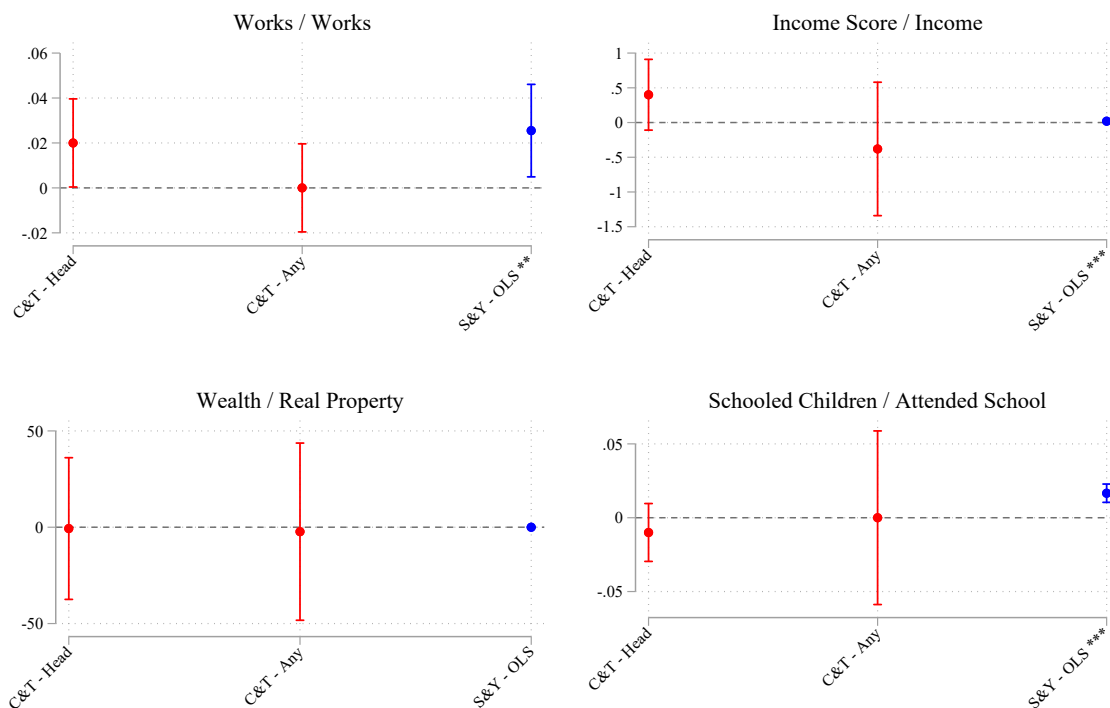


Figure 5: Comparison of Selected Estimates in Célérier and Tak (2021) and Stein and Yannelis (2020)

This figures illustrate the point estimates and 95% confidence intervals from several estimates in Célérier and Tak (2021) and Stein and Yannelis (2020). The first two bars in each panel are based on Table 9(B) of CT. The third bars are based on Table 6 of SY. Confidence intervals are plotted for all nine bars, but some are difficult to distinguish visually given the relative magnitude of the CT and SY standard errors.

Table 1: Counties with Largest Number of Black Troops

This table reports the ten Southern counties with the largest number of Black troops in August of each year from 1865–1870 (using data from Downs and Nesbit 2015).

1865			1866			1867		
County	State	Number	County	State	Number	County	State	Number
Jessamine	KY	4,807	Cameron	TX	2,522	Cameron	TX	468
Carteret	NC	3,827	Orleans	LA	988	Pecos	TX	302
New Hanover	NC	3,187	Hidalgo	TX	735	Montague	TX	289
Rapides	LA	3,100	Chambers	TX	723	Jeff Davis	TX	272
Chatham	GA	2,834	Charleston	SC	686	Orleans	LA	259
Jefferson	KY	2,764	East Baton Rouge	LA	658	Wayne	NC	236
Beaufort	SC	2,613	Caddo	LA	488	Charleston	SC	186
Greene	TN	2,451	Montague	TX	483	Harrison	MS	177
East Baton Rouge	LA	2,318	Pulaski	AR	465	Plaquemines	LA	137
Hamilton	TN	2,294	Plaquemines	LA	453	Carteret	NC	102

1865			1866			1867		
County	State	Number	County	State	Number	County	State	Number
Wayne	NC	698	Orleans	LA	400	Jeff Davis	TX	432
Pecos	TX	453	Harrison	TX	319	Kinney	TX	370
Jeff Davis	TX	345	Jeff Davis	TX	299	Menard	TX	335
Harrison	TX	299	Menard	TX	284	Pecos	TX	229
Kinney	TX	294	Pecos	TX	273	Hudspeth	TX	222
Maverick	TX	219	Jack	TX	224	Maverick	TX	219
Plaquemines	LA	210	Hudspeth	TX	204	El Paso	TX	51
Orleans	LA	205	Plaquemines	LA	194			
Hudspeth	TX	195	Shackelford	TX	177			
Uvalde	TX	64	Tom Green	TX	173			

Table 2: Distance from Branches and 1860 Outcomes

This table shows estimates analogous to Table 14 in Stein and Yannelis (2020). We regress 1860 outcomes on indicators of whether a county has a pre-1870 branch, whether a county has a post-1870 or planned branch, the minimum distance from a pre-1870 built branch and the minimum distance from a post-1870 or planned branch. Estimates are weighted by total population. Standard errors are clustered at the branch level.

	(1) Troops	(2) Black Troops	(3) School	(4) Office	(5) Pop.	(6) Metro	(7) Urban	(8) White Lit.
Branch in County	259.1 (378.3)	173.6 (157.2)	-0.0300 (0.233)	-0.00220 (0.325)	66035.6 (218576.5)	0.00607 (0.177)	0.0907 (0.0963)	-0.00451 (0.00530)
Branch Distance	-0.473 (0.451)	-0.0134 (0.122)	-0.000729 (0.000608)	-0.000832 (0.000553)	-271.5 (188.2)	-0.0000203 (0.000347)	0.000324 (0.000467)	0.0000423 (0.0000310)
Observations	501	501	501	501	501	501	501	459

	(9) Farm Value	(10) Man.Output	(11) Man.Wages	(12) Real Estate Wealth	(13) Total Wealth	(14) Water	(15) Rail
Branch in County	2648.4 (1574.0)	-49.37 (47.38)	-11.55 (9.464)	-53.30 (54.75)	-178.2 (131.1)	0.0231 (0.0642)	-0.00397 (0.0459)
Branch Distance	-0.318* (0.183)	-0.0168 (0.0868)	-0.0134 (0.0182)	0.287 (0.203)	-0.595* (0.313)	-0.00000407 (0.000413)	-0.00000308 (0.000332)
Observations	498	460	460	500	500	500	500

Table 3: Missing Parents' Names in 1870 Census Sample

This table shows, by race, the percentage of individuals for whom it is *not* possible to identify parents' names in the 1870 Census "1% Sample with [approximately 2%] Black Oversample" sample (Ruggles et al., 2015).

	White	Black
Missing mother's name	51.8%	56.7%
Missing father's name	55.1%	62.5%
Missing both parents' names	50.0%	54.7%
Missing either parent's name	35.5%	64.5%

Table 4: OLS Effect of Missing Parents' Names on Outcomes

This table displays the main coefficients from OLS regressions of various outcomes on an indicator variable for the absence of name data for an individual's mother and/slash or father; each coefficient is estimated using a separate regression. The five outcomes are an indicator for whether a person works or not, log Income, log Real Property, literacy, and an indicator for school attendance, as in Stein and Yannelis (2020). Estimates are conducted for Blacks living in southern and border states using the 1870 Census "1% Sample with [approximately 2%] Black Oversample" sample (Ruggles et al., 2015); for outcomes other than school attendance, the sample is restricted to individuals aged 16 and older. Observations are weighted using IPUMS sample weights (PERWT). Standard errors clustered by state and county are reported in parentheses; significance levels are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

	Works	Income	Real Property	Literate	Attended School
Missing Mother's Name	-0.0754*** (0.0203)	-0.145** (0.0494)	0.0818*** (0.0115)	-0.0454*** (0.00926)	-0.0389*** (0.00571)
Missing Father's Name	-0.0189 (0.0212)	0.000617 (0.0535)	0.117*** (0.0133)	-0.0402*** (0.00866)	-0.0356*** (0.00575)
Missing Both Parents' Names	-0.0593*** (0.0192)	-0.105** (0.0465)	0.0832*** (0.0115)	-0.0446*** (0.00980)	-0.0391*** (0.00565)
Missing Either Parent's Name	-0.0373 (0.0235)	-0.0425 (0.0594)	0.121*** (0.0134)	-0.0414*** (0.00705)	-0.0359*** (0.00595)
<i>N</i>	48,191	48,191	48,191	48,191	89,253