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Summaries of Doctoral Dissertations

The Dissertations of Sarah Quincy, Cory Smith, and Jessica LaVoice 2020 Allan Nevins Prize Competition of the Economic History Association

In September 2020, the members of the Economic History Association convened across different time zones and latitudes, for the first (and, one hopes, the final) virtual dissertation session. The last time I had moderated the panel for the Nevins prize was in 2008 when there were a financial crisis and a great recession. This time, there is a financial crisis, a great recession, AND a pandemic. However, I feel secure in the conviction that cliometricians will recognize that correlation does not imply causation.

The pandemic of 2020 has disrupted key institutions, overwritten central clauses of the social contract, and heightened concerns about economic inequality and social justice. As Allan Nevins (2018) pointed out, at such times as these, when humans are "harried and perplexed by the sweep of events, [they] peer earnestly into history for some illumination of their predicament and prospects." The dissertations under consideration as finalists for the Nevins prize all demonstrate that economic history is more important than ever for understanding vital problems of the present and for its ability to offer insights into more effective ways to attain a just society.

SARAH QUINCY completed her thesis as an affiliate of the unparalleled All-UC group in economic history, and this shows. For one, she thanks her advisors at Davis for instruction in the arcane art of (and I quote) "footnote surfing." Of course, the EHA does not yet give out prizes for surfing of any kind, but one cannot fail to be impressed by Quincy's deep-diving into the institutional context and intrepid attempts to get the details right. She crafts the project with artisanal care, making sure that the archival data, statistical tools, and level of aggregation match smoothly without the need for ad hoc econometric patches. The skeptical reader—yes, you will no doubt be surprised to learn that this is my default setting—finds her misgivings assuaged in the very next footnote or table.

Society does not seem to follow an evolutionary path of persistent upward progress, but rather one of the fully circular peregrinations. Google searches about the Great Depression have recently soared because the concerns of the 1920s have re-emerged in 2020. Quincy boldly goes where many economists have gone before: her captivating thesis is set in Depression-era California, and she manages to add new facets to this well-burnished topic, in itself a high achievement. Her project explores how credit-market frictions can constrain household spending, which has implications for resolving the longstanding puzzle in macroeconomics of how changes in nominal variables might have persistent real consequences. In one sentence: she essentially argues that what was good for Bank of America was good for California.[1]

[1] A.P. Giannini (1870–1949) founded in 1904 the institution that would become the Bank of America. As an aside, it seems appropriate that a dissertation about his bank's contributions should be written at Davis. In 1928, Bank of America set up an endowment of $1.5 million to establish the Giannini Foundation of Agricultural Economics at the University of California, whose activities largely involve the Davis, Berkeley, and Riverside campuses.

Quincy exploits the branching activities of the Bank of America as a proxy for enhanced access to credit. The bank's overall loan-deposit ratio was stable during this period, unlike those of many other competitors. As a result, areas with a Bank of America branch suffered less and were more resilient to the adverse economic shocks. Within a decade, cities with Bank of America branches experienced 25 percent growth, compared to the stagnation of communities without access to one of these branches. In California, labor and education markets provided a conduit through which the variation in financial liquidity affected real adjustments. Further evidence regarding the role of credit constraints at the individual level includes the relative success of WWI veterans over their counterparts, arguably owing to the unexpected cash bonus offered to those who had served in the war.

Hugh Rockoff once told me with his usual tendency for wry understatement: *it's hard to identify causal factors during the Great Depression*. Quincy's econometric methods are fastidious and thorough. However, the methodology at times is overly focused on nonrejection of a preferred claim rather than attempting to evaluate all potential sources of heterogeneity. (Aspiring economists are further advised to avoid reasoning that takes the form: "the answer is either x or y; I have shown it is not x, therefore it must be y.") Effective identification of an exogenous credit supply shock requires a randomized allocation of Bank of America branches. A battery of tests is deployed to decimate "threats to identification" (in capital letters) related to this assumption. Nevertheless, quantitative analysis must always be consistent with economic rationality, and it seems implausible that any large profit-maximizing corporation would essentially roll the dice in choosing its branch locations. I am actually a stockholder in Bank of America, and I would promptly sell off all my shares if their expansion strategy were not driven by systematic factors like expected profitability. As such, the argument would be strengthened by making adjustments for endogeneity and confirming that the results persist.

This dissertation makes important contributions to the literature but would arguably be even more compelling after a few adjustments. The author constructs too grandiose an edifice on the elegant but slight foundation of what is, after all, a simple binary variable about the presence of a Bank of America branch. There is a marked lack of systematic evidence to back up crucial assertions such as the credit-smoothing operations across Bank of America branches relative to other institutions, especially during this major downturn when risks were more likely to be correlated. Such data may not be available for Bank of America itself during the Depression, but it would be worthwhile to acquire them for other time periods or other financial institutions. Moreover, the central claims about Bank of America's superior credit injections are based almost exclusively on the aggregate loan-deposit ratio. However, banking liabilities consist of deposits plus capital, so it is important to more fully address variation in bank capitalization. It would likewise bolster the argument to control for other related measures such as dividend payouts.

A more convincing analysis would further address alternative forms of financing outside the commercial banking sector, which could potentially account for the observed patterns. Along these lines, it would be relevant to assess the elasticity of substitution across these different sources of funds. This excursion beyond banking institutions is especially necessary to gauge the effect of credit constraints on housing markets. In

particular, around 40 percent of home buyers obtained loans from Building and Loan associations, which at that time introduced important and persistent innovations that benefited borrowers (Rose and Snowden 2013).

California was the destination for the largest inflow of migrants in the country, many from the lower end of the income distribution, during this period. Bank of America was self-celebrated as a bank for immigrants and the "little fellow," as we tangentially observe in the chapter on education. Moreover, Bank of America was a pioneer in women's banking, with an independent Women's Banking Department where female customers could manage their finances on their own account (literally and figuratively) and obtain investment advice and education. Future work could usefully investigate the distributional effects of funding for such relatively disadvantaged groups.

California was "the great exception," according to Quincy, because Bank of America was the great exception. I am certainly willing to buy this. But she further insists that Bank of America affected outcomes "solely through its willingness to lend." This is overselling. It makes me wonder about the multi-faceted world beyond these regressions. After all, like any financial institution, Bank of America's impact was not merely from doling out cash but rather through a multitude of channels and relationships such as the close monitoring of borrowers, the attenuation of risk, and provision of advice and information.[2] There was widespread ownership in the bank itself, even among the "little people," who benefited from expected gains and distributions related to ownership of its shares (it is worth noting that a great deal of speculation in Bank of America shares was occurring over this period). According to Carlson and Mitchener (2009), branch banking generated positive externalities by compelling greater efficiency among competing financial institutions in a manner that increased the likelihood that they would survive the Great Depression.

Nevins thought that good history requires a narrative, and this dissertation complies with that directive, offering a coherent thesis rather than three disjoint essays. In short, this is a fruitful study that in the future can branch out in many different directions. (Yes, I agree that puns are dreadful, but this is a small tribute to Sarah Quincy, who is on record as declaring puns to be the highest form of humor.)

Now we're going to *Zoom* from the University of California on the West Coast to MIT on the East Coast. (And I solemnly promise on my well-thumbed copy of the *Wealth of Nations* that this is the last pun.) CORY SMITH's dissertation offers thought-provoking perspectives on race, inequality, and climate change over place and time. In the modern knowledge economy, land has seemingly been designated as the fourth factor of production; Smith's research reminds us of its important role in economic development.

As every schoolchild and reader of the *Journal of Economic History* should know, federal and state governments subsidized the construction of canals and railroad transportation networks, through conveyances of adjacent real estate tracts. Scholars such as Robert Fogel and Lloyd Mercer held a generally favorable view about land-grant

[2] Bank of America's "chief contribution was not in money, however, but in counsel and leadership in devising and putting through plans for refinancing," according to James and James (1954, p. 406). Part of the success of branch banking was in the resemblance to universal banking, such as the ability of the central institution to facilitate upstream and downstream alliances with greater efficiency, owing to its access to information about all parties.

policies, which they regarded as rational and appropriate in their form and size, whereas Stanley Engerman was more ambivalent about the need for transfers and redistribution. Several other economic historians held that the net outcome in overall social welfare was negative, and populists similarly decried these handouts as a form of feudal capitalism or capitalist feudalism. Land conveyances to a railroad corporation did not occur with a single flourish of the pen, but through a process that typically lasted decades, so it is perhaps inevitable that quantitative evaluations of these grants would remain contentious.

Smith's essay revitalizes this hoary debate about federal land grants that was most vociferous around the 1960s. He examines links between railroads, ownership concentration, and efficiency in land markets. Aligning himself squarely within the camp of the pessimists, he contends that the negative consequences of this policy persist to the present. Today, American agriculture is certainly highly concentrated, but it should be emphasized that many other forms of land use, such as timberland, have become even more concentrated than farms. Even in the 1940s, the Department of Agriculture estimated that just 3 percent of owners held over 40 percent of farmlands. Most theorists and empirical researchers concur that economies of scale are in part responsible for the high observed productivity in land use among large farms and the corresponding decline in the viability of family farms (Key 2019).

Smith finds that railroad grants were associated with greater land concentration, but he dismisses the conventional arguments about the benefits of economies of scale and large size in accounting for these patterns. Instead, he contends that the federal land grant policies, by enabling greater concentration in landholdings, resulted in efficiency losses that have persisted to the present time. These adverse effects from land grants are much larger than a paltry few dollar bills left on the ground. Rather, losses from the policies of the distant past amount to fully 23 percent of investment and a population penalty of 8 percent in the present. He concludes that concentration in land ownership reduced prospects for economic development, and "despite the passage of roughly 150 years since the policy's enactment, these basic effects remain in place."

Like the other two panelists, this dissertation exhibits expertise with an impressive range of statistical techniques. We are offered binary quadratic optimization using state-of-the-art algorithms, intriguing discussions about quantile effects on the inverse hyperbolic sine of land values, and ruminations on bandwidth robustness. At the same time, excellence in applied econometrics is necessarily bounded by the quality and appropriateness of the data, and these seem less than persuasive. (And, call me reactionary, but I would willingly sacrifice insights regarding bandwidth robustness in exchange for some plain-vanilla summary statistics.)

Smith has been exceedingly diligent, but the sources of data for this project still comprise a greater checkerboard than the land grant patterns themselves. The modern outcome variables are from six not entirely representative states: Florida, Kansas, Montana, Nebraska, Oregon, and Wyoming. The historical records incorporate a vertiginous range of locations, sources, periods, and inconsistent types, such as 1912 tax data from Morrill County, Nebraska; property assessments from Lincoln County in 1965; farms in 40 Nebraska and Kansas counties in 1940. Along with a detailed study of the southern part of Banner County, Nebraska. The optimal size of a holding will depend on the type of crop, soil fertility, aridity, and temperature, and the analysis creatively includes such controls.

The chapter's central claim is that proprietors of large railroad grants were unable to manage the land themselves and therefore inefficiently subcontracted to tenants and sharecroppers. However, the key variable of tenancy is inferred rather than observed. The names of landowners were matched in the 1900 census and, if no match was found within the same county, it was assumed that the farm was worked by tenants. This empirical strategy is obviously problematic. I would also recommend that the author direct some attention to heterogeneity in tenancy. Ownership status was neither binary nor fixed and typically varied with age. This was especially true for families: a quarter of landlords rented to their sons, who often started as a laborer and then progressed to tenant, and finally owner. In addition, if any one plot size was suboptimal, tenants could and did rent land from more than one person to attain a larger scale.

True believers in Coasean market efficiency are unlikely to be persuaded that real estate markets were land-locked and suboptimal for more than 150 years. What were the transaction costs that could have prevented such large value-increasing trades? The proposed answers turn out to be the least satisfactory part of this chapter. Smith suggests (oddly) that it would have been too costly for farmers to move from their houses. He also speculates that prospective buyers were likely to be paralyzed by prohibitively high information costs in land markets. At the same time, the presence of excessive information costs seems to conflict with his proposed optimal policy; that the federal administrators should have given out lower-quality land to the railroads and turned over the higher-quality lots to small homesteaders.

The two co-authored papers in the dissertation are rather more optimistic about market forces. As the song says, "a boll weevil is an insect. And he's found mostly where cotton grows." For impartial economists, the boll weevil is not a heinous pest whom the bigoted farmer should wish to consign to ****, but rather a literally natural experiment to fuel their difference-in-difference engines. Consequently, a long list of articles has investigated the nature and consequences of boll weevil infestations, and many come down decidedly on the side of the boll weevil. They argue that its spread exerted a benign influence manifested in improvements ranging from education to health, labor market conditions, and nutrition. In fact, the naïve observer might even wonder whether policymakers would not be better advised to reintroduce the boll weevil as a cost-effective means of promoting social progress.

According to Smith et al., the boll weevil once again features as a force for good. The exogenous negative economic shock of the infestation created incentives for a decline in racial violence. They measure systemic violence in terms of lynchings and the building of confederate memorials. The major result is that lynchings at the time fell by 0.04 and statues by 0.003 per year in the average county. These coefficients might pass the t-test, but they offer a very faint signal indeed, especially in per capita terms. And of course, it is possible that counties with boll weevil infestations were less likely to invest in confederate memorials simply because they were short of funding. The analysis overlooks other forms of violence, such as murders by shooting and property damage that might have replaced lynchings, as well as nonviolent methods of racial suppression. Moreover, one would like to know whether crimes against blacks increased in the locales to which they migrated outside the South.

The authors find long-run correlations between infestation in 1892–1922 to outcome variables in the 1960s. In these sorts of analyses, a major question is always: What is the mechanism that continuously connects time period A to time period B? They answer that

out-migration triggered fundamental reforms in systemic racism and its manifestations. By contrast, other studies have found negative social effects from economic shocks like the boll weevil, including higher rates of black incarceration and political repression. One, therefore, wonders why the migration mechanism selectively improved outcomes in terms of lynchings and monument installations, but not in terms of the other facets of institutionalized racism which have endured to the present day.

In all three of the essays in this dissertation, there is a delicate question about *size*—or the magnitude of the effects in question. The third chapter considers "the long run in the future," and shows how international trade can moderate the negative effects of climate change. Smith and his co-authors analyze secondary data from 2009, as well as predictions that others have made regarding agricultural output, land use, and prices. The scope of the data set is comprehensive, covering the 10 major crops in over a million fields in 50 countries. (As an aside, I was surprised to learn that tomatoes accounted for a larger share of world output than potatoes.) They find that global GDP would fall by around 0.34 percent and, if markets adjust, by 0.26 percent instead. The paper does not consider shifts in comparative advantage toward the nonfarming sector, which would likely shrink the adverse effects even further. If we believe these results, the predicted effect of climate change on overall economic welfare appears to be rather small potatoes/tomatoes. Food for thought! (And now you know what my pun-related promises are worth.)

JESSICA LAVOICE completed her dissertation at the University of Pittsburgh, and it is a shame that it was not possible for us all to be celebrating there, instead of sheltering in our own sitting rooms.

Her trio of essays presents different perspectives on the economic history of race and inequality. The Keystone project in the thesis (the pun here is more subtle, hence the parenthesis) considers the consequences of urban renewal projects in terms of racial disparities. These measures are designed to improve the quality of a neighborhood, but a corollary is that the supply of affordable housing declines, likely creating a disproportionately negative impact on socially disadvantaged groups and lower-income residents. Prior research on these programs found a significant positive overall effect on cities and concluded that observed outcomes were unrelated to race. But it was possible that the level of aggregation in the studies might elide relevant underlying heterogeneity, so LaVoice approaches the analysis at the more appropriate neighborhood level.

The data set covers the largest U.S. cities, incorporating the locations and land use of 200 urban renewal projects authorized by the Housing Act of 1949. Fourteen percent of majority-black areas were redeveloped, and these locales were more likely than equivalent white neighborhoods to be targeted, holding other factors constant. Interestingly, expansions in streets and highways were the second-largest form of urban renewal. As a result, we observe a process of gentrification where the quality of the neighborhood improves and more non-residential spaces are developed, but the supply of affordable housing correspondingly decreases.

LaVoice concludes that low-income black households were made worse off by urban renewal policies. She presents a spatial equilibrium model of locational choice, which incorporates the assumption that low-income households initially revealed a preference for a low-quality neighborhood with low housing prices. Thus, for nonmovers, "it must be the case" that they are worse off when both price and quality go up after urban renewal. One wonders instead whether these groups might have confronted

constraints that prevented them from attaining higher utility from a different price-quality combination.

Silicon Valley types like to say that the last mile matters most, and this is also true of cliometrics. The last mile would include more institutional context. The last half-mile requires greater attention to the measurement of housing quality and urban blight, unobserved variation in neighborhood crime, the role of ethnicity, and social capital in communities that were cleared. For instance, the share of houses needing major repairs was not statistically significant, which suggests to me that the variable was mismeasured, rather than that decrepit buildings did not influence the likelihood of slum clearance. A more complete analysis would attempt the valuation of benefits from access to improved amenities like transportation and parks and perhaps improved employment prospects. We also need to know what was happening just beyond the boundaries of these urban projects. It would be especially valuable to try to acquire information on the status of displaced residents after removal. Had some of them, like TV's Jefferson family, "moved on up to the east side, to a deluxe apartment in the sky"?

This dissertation, to a far greater extent than any of the other submissions, offers a master class in applied econometrics using historical data. LaVoice revels in dealing with kernel densities, SHapley Additive exPlanations of gradient boosted trees estimation and the like. This project is especially to be commended for its creative solution to selection problems by painstakingly constructing synthetic control groups matched on race, population and housing density, median rents, and income. As a specialist in law and economics, I would encourage the author to apply these methods to evaluate eminent domain policies in economically-disadvantaged areas like Poletown in Detroit.

Popular discussions about racial discrimination and recent calls for reparations to blacks have pointed to redlining as a source of economic harm. The second chapter in this thesis convincingly refutes such claims. Hillier (2003) had used archival material to show that redlined maps were not commonly used and were not associated with racially-motivated variation in lending. LaVoice et al. likewise find that redlining was not associated with racial differences in housing outcomes. Indeed, although whites were lower-risk borrowers, they made up the vast majority of the redlined population. The analysis and exposition are comprehensive and compelling and can be pointed to as a model for excellence in economic history. Here I would just offer two minor comments for consideration. First, the data are restricted to the largest cities in the United States in 1930 and 1940, but it would be interesting to assess variation in city size. Second, the authors conclude by proposing that racial bias in private markets caused blacks to cluster in low-income neighborhoods; and the federal government engaged in "malignant neglect" because it ought to have taken active measures to deal with this alleged private market discrimination. These assertions exceed the evidence and thus appear to be overly normative, if only by contrast with the meticulous empirical work disproving charges that redlining enabled invidious racial discrimination in housing.

The final essay deals with race and debt collection, again using neighborhood-level information. A judgment is the last step in a routine debt collection process, and an outcome in favor of the plaintiff who is pursuing a debtor allows for more effective enforcement of the outstanding claim. The data are limited to court decisions on debt collection cases in Missouri from 2004 to 2013, which are alleged to be representative

of other jurisdictions. Race is inferred from names rather than observed, and zip codes are used to link the cases to credit reports and other location-specific variables. LaVoice and her co-author find that debt collection cases are more likely to occur in predominantly black neighborhoods. Decisions tend to favor plaintiffs in these areas, even after accounting for differences in income, credit scores, and other neighborhood characteristics. The quantitative results are quite modest: zip codes with a majority of black residents experience 0.6 more judgments per 100 individuals.

This project is still a work in progress. First of all, legal judgments are obviously brought against individuals, not against neighborhoods. Averages across zip codes cannot capture the most relevant sources of heterogeneity, such as debtor recidivism and default risk, nor identify the specific mechanisms affecting racial differences in debt collection judgments. It is unclear why we should be concerned that neighborhoods experience different outcomes in debt cases, especially since the authors acknowledge that the results provide no evidence of unfair discrimination against blacks. Second, American debt collection processes are routine and straightforward and perhaps overly-biased against *creditors*. There are rules to protect debtors against "bad actors"; for instance, if creditors act outside the law, they could be required to pay all legal costs for both parties. Third, the dataset is limited to cases that resulted in a judgment. A serious selection effect arises because the vast majority of all disputes are not entered in court records, and only a minute fraction will further survive through to judgment. Such selection effects compromise our ability to make inferences about bias from the proportion of plaintiff wins. The authors are advised to engage with the prolific empirical and theoretical scholarship in law and economics on such issues and to integrate their own findings in this literature.

Overall, these three impressive scholars embody what Nevins called the "spirit of critical inquiry for the whole truth" and are all deserving of high accolades. The panelists are advised that any "criticisms are merely a request for more information," as a marketing manager once told me on a flight from Tunisia to Malta.

It has been a privilege to have the opportunity to read all of the dissertations that were submitted for the Nevins Prize. This process makes one reflect on the discrete changes in the field of cliometrics that have occurred over the past decade. It is evident that a "new new economic history" is unfolding that leverages machine learning, cheap access to large-scale data, and enhanced econometric techniques. At the same time, these technological developments are a mere gloss that has not altered the fundamentals of excellence in economic history. The best research studies demonstrate that history is more than a source of quasi-random variation to identify causality. The Nevins prize today, as always, recognizes originality and creativity, neurotic attention to getting the details right, clarity in economic reasoning, and research interests with broad social relevance.

I must confess that it is somewhat ironic that I should be the czar/czarina for the award of the Nevins Prize. My new book, *Inventing Ideas: Patents, Prizes, and the Knowledge Economy*, reveals the inefficiencies of administered systems like prizes, relative to market-oriented rewards (Khan 2020). And we certainly observe in this context many of the drawbacks of administered systems of awards. First, even though all dissertations might be deserving, no more than three can appear in this panel, and only one can win the prize. Second, a total of 20 people were directly involved in these three dissertations, including co-authors and supervisors, but only one author will be

rewarded. Third, decisions in administered systems clearly reflect the subjective preferences and standards of the judges in the process. Nevertheless, it is reassuring to note that all of these worthy dissertation candidates, regardless of whether or not they were nominated for this prize panel, will be compensated in the marketplace.

Despite these caveats, the award of the Nevins prize is especially important in a world where the validity of science and data themselves are being denied. When the members of the EHA gather to celebrate the achievements of Hugh Rockoff and our dissertation panel, we also reaffirm the commitment of the field of economic history to the pursuit of quality, fact-based inquiry, and the highest standards of academic integrity.

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