vironment, which in turn matters for the growth of financial markets, then financial development will ultimately depend on the interests of whoever is in power. This political economy problem relates to what political scientists call the "commitment problem." This means that any government strong enough to define and protect property rights is also strong enough to abrogate them when such an act is in its interest. Thus, changes in ruling coalitions can complicate the process through which governments commit to protect investors, which can ultimately hurt financial market growth. If politics are so important for financial market development, then the commitment problem can adversely affect economic welfare. This is particularly important if we consider that there are many causal links between financial market development and economic growth.

I end this dissertation by suggesting that socializing the benefits of financial markets is a good way for countries to commit to financial development. If more citizens have a stake in financial market development, then weakening investor protections is more costly for politicians. Workers and middle-class families are more inclined to defend financial markets if they can reap more benefits from them, for example, in the form of mutual and pension funds or through mortgage and debt securitization.

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Comments on Boustan, Frydman, and Murphy

When Gary Libecap asked me to convene the dissertation session for the Nevins Prize this year, I told him it would be my pleasure. When nine, very thick dissertations arrived on my doorstep I wondered what I had been thinking. Could I take them to the beach? I did. Could I take them to baseball games? I did. I took them everywhere. All kidding aside, I would like to thank Gary for giving me the opportunity to read what turned out to be a set of great dissertations. I learned a tremendous amount about a wide variety of subjects and feel quite happy that editors at various journals will be very busy for the next few years. Nonetheless, I really only had the summer to complete this and 100 other daunting tasks that I put off during the school year. I thought at first that I could easily disqualify some of the entries—perhaps they were poorly written or were not really economic history, or made poor use of statistics, or had no data. I was wrong. All nine proved to be very good submissions. So I read them and thought about them, and re-read some of them. Each of the dissertations that ended up

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on the panel is very different, yet represents some aspect of what I think represents good economic history.

Leah Platt Boustan examines the effect of black migration on northern cities and labor markets. She focuses on the period 1940–1970, during which 4 million blacks left the South for northern industrial cities. At the same time, central cities in the north were undergoing significant suburbanization. The first two essays in Boustan's dissertation focus on the link between these two events. The first essay documents the correlation between black in-migration and a rise in suburbanization, but wisely notes that it is difficult to sort out "white flight" from "migrant location choice." That is, while it may have been the case that whites left the inner city in response to black in-migration, it may have also been the case that whites were leaving anyway and blacks were attracted to the city by lower housing prices. Boustan's challenge is how to disentangle the effect of migrants' location decisions from actual white flight. She handles the problem by creating an instrument that predicts southern out-migration based on "push" factors; in this case, changes in southern agricultural conditions, and then assigns this migration stream to northern cities based on settlement patterns established during the earlier wave of black migration. Her IV estimates are larger than (although not statistically different from) the OLS estimates.

Questioning an instrument's validity is generally an easy way to criticize a paper, and the potential problem with this instrument is that it could be the case that migrants in the first Great Migration settled in cities that experienced positive economic shocks, and that these shocks were serially correlated with shocks that came later. Boustan addresses this problem using a variety of methods and her results are robust. Generally, she calculates that roughly 20 percent of the growth in suburbanization occurred because of white flight.

After reading the first essay, I wondered why whites would have to leave the city to escape blacks. For example, having lived in the Cincinnati area (I confess, in the suburbs) for the past eight years, I can say that Cincinnati is very racially segregated today, even within the central city. In other words, you do not have to leave the central city in order to live in a racially homogeneous neighborhood. Boustan's second essay addresses this exact question, with one of the more clever titles I read this summer. In the chapter entitled "Inside the Black Box of White Flight," Boustan explores *why* whites would leave racially diverse inner cities. Boustan argues white flight could occur if suburbs offered whites political autonomy that was not available even in segregated central cities. That is, whites may have wanted to avoid interacting with blacks in schools, or may not have wanted blacks to be able to impact local policy.

Although this seems like a satisfying answer, it is difficult to measure statistically. Suburbs offered more homogeneity, but they also offered newer houses (with bigger closets—which is why I live there), safer streets, and the like. How do you isolate the effect of white political autonomy from these other factors? Boustan does by using block-level Census data to compare housing prices on either side of municipal borders. Her identification strategy works only if you assume that while borders change discretely, the housing stock changes in a more continuous way. This seems to be a very strong assumption, and one that seems true only part of the time, at least based on my casual observations. In Cincinnati, driving across a municipal boundary literally means you leave an affluent suburb and land in an incredibly poor neighborhood. Boustan does address this concern in a variety of ways—by looking at changes in municipal-level racial composition across borders on changes in the housing price gap, by running regressions on housing quality across borders, and by constructing placebo borders. She finds that even after controlling for housing quality and other factors, the median homebuyer in 1970 was willing to pay more for an identical home in a racially homogeneous area. She attributes 25 percent of the effect to the demand for living with in an area with more affluent residents, an additional 25 percent to the demand for having white classmates in local high schools, and finds further evidence that the "cost" of urban diversity rose in cities with large amounts of riot activity during the 1960s.

In her final essay, Boustan takes her analysis a step further by examining the impact on northern labor markets of an increase in the supply of workers caused by migration. This essay is an interesting contribution in light of today's immigration debate. Boustan documents a negative correlation between black in-migration and the extent of earnings convergence between northern blacks and whites. Following Borjas, she uses variation in skill groups (defined by education and experience) to examine the impact of migration on northern wage gaps. As in her previous chapters, Boustan is thorough and conducts a variety of robustness checks, and performs both IV and OLS analysis. She finds that a 5 percent migration shock would decrease black relative earnings in the north by roughly 3.5 percent—which may help to explain why the North had weaker wage convergence compared to the South during the 1940s.

Carola Frydman creates the first panel dataset that tracks the compensation of executives in large firms from 1936 to 2004 in order to examine the economic history of executive pay. Clearly, her dissertation fills a large gap in the literature, and is quite timely given the spate of recent research examining rising rates of executive compensation. She provides interesting insights into not only historical trends in executive compensation, but also as to what factors influenced the structure and amount of executive pay over time.

In her first chapter, Frydman and co-author Raven Saks examine historical trends in executive compensation. Whereas much work in this area has only examined recent trends and tried to explain the explosion in executive compensation, Frydman and Saks add a new perspective. Creating a new data set from the proxy statements and 10K reports of publicly traded firms, they find that since 1936, executive compensation has faced a U-shaped pattern: declining during World War II, and increasing only very modestly through the 1970s, and then increasing rapidly during the 1980s and 1990s. They also examine the structure of executive compensation, and find that stock options have become a greater share of compensation over time. Frydman and Saks suggest that high marginal tax rates account for roughly a third of the growth in compensation during the postwar period. They hypothesize that marginal tax rates as high as 90 percent for some executives may have constrained executive pay and obscured the correlation between compensation and the market values of firms. They also find that a 1950 change in tax policy that gave preferential tax treatment to stock options increased the amount of stock options received by executives.

An advantage to examining executive pay over a long period is that it enables Frydman and Saks to looks more deeply at certain trends, and to measure whether recent gains in executive compensation have been equally shared by all executives. Indeed, they have not. They find that the compensation of the highest-paid executives (those above the ninetieth percentile) has been growing faster than other executives. In the earliest period (1936–1939), the average real value of total compensation in the tenth percentile of the distribution was 360,000 (real 2000$), compared to 1.74 million in the ninetieth. In other words, the highest-paid executives made about five times more than their lowest paid counterparts. This gap widened significantly over the century. From 2000–2003, the highest-paid executives made over 16 times what the lowest paid executives earned. It would be interesting, and perhaps informative, to delve into this gap somewhat further.

The second essay in Frydman's dissertation, also co-authored with Raven Saks, builds on the first by examining the long-term relationship between executive pay and firm performance. The authors find an interesting W shape in the relationship between executive pay and firm performance over time. They find a strong relationship between pay and performance in every decade except the 1940s and 1970s, even after controlling for firm size. Further, the relationship is stronger now than in any previous period. Their findings are robust to a variety of different measures. Although Frydman and Saks speculate as to what could explain the trends they see in the data, they largely leave its explanation to future work. I look forward to seeing their theory explaining the long-run relationship between pay and performance.

In the third essay, Frydman cleverly constructs a theoretical model to explain the trends highlighted in her first paper. The model suggests that the relative reward for general managerial skills (relative to firm-specific skills) has increased over time. It predicts that executives will be more mobile during their careers and more likely to attain a business education. To test her predictions, Frydman supplements her original dataset with biographical information on the three highest-paid executives among the top 50 firms from 1936–2003. She adds information on age, education, career path, and other characteristics. In order to measure general managerial skills, Frydman creatively constructs an index of "general human capital" and does find that mobility increased along with general skills, and that the correlation between general skills and managerial mobility was higher in the 1970s and 1980s than in earlier periods. Frydman herself notes that she does not answer the question of what caused the change in the type of skills over time, but she suggest that it could be related to the industrial and geographic diversification of firms over time.

Overall, Frydman's dissertation is an important contribution to an area in which there has been little work done, and understanding past trends helps in understanding the current structure. Putting together the dataset that she has created has undoubtedly been incredibly time-consuming, but it will be a valuable contribution to economic historians and modern labor economists as well. This is an area that seems rich for future work. Although the dissertation highlights very interesting trends, it leaves many open questions about why executive pay stagnated after 1940, and then surged greatly after 1980.

Sharon Murphy's almost 500 page dissertation is filled with remarkable detail on the genesis of an industry. Her data sources include personal correspondence as well as company and industry records. Using these sources, she weaves an enlightening tale of how the life insurance industry grew from a total of 100 policies nationwide in 1825 to insuring one in three adult men by 1870.

Part 1 of the dissertation describes the development of the industry, and focuses on the economic challenges the industry faced. Life insurance companies faced and surmounted numerous difficulties: the difficulty of insuring lives in an era when next to nothing was known about life spans and mortality; overcoming adverse selection and moral hazard; and overcoming public perception of the industry being associated with gambling, murder, and fraud. Murphy describes in much detail the development of the industry and how firms adapted. She discusses how, as the economy industrialized and urbanized, families increasingly came to rely upon men's wages, and could suffer severe hardship in the event of the male household head's death. Urbanization fractured familial and social ties, making it even more difficult for fatherless families to find aid in a world of strangers. These changes increased the demand for life insurance, but at the same time, as Murphy points out, they made it more difficult for life insurance companies to assess risk and monitor moral hazard. Early life insurance companies had individually examined applicants and required personal testimonies about their habits from known, respected community members. As urbanization increased and as companies began to insure people outside the cities of their home office, they had to find alternative means of selecting risk. Murphy combs through old applications and correspondence between agents and their home offices to show how life insurance companies overcame the challenge by using their agents not as salespeople, but as risk-assessors who could personally meet applicants to gauge risk.

Murphy's data seem to suggest that 1840s were a boom period for the industry. Life insurance in force tripled to $16 million between 1840 and 1845, and then rose to $93 million by 1850. Murphy argues that the 1840s were not atypical of the antebellum growth rates experienced by life insurance companies, but she also notes multiple reasons for growth in coverage during the period, including the advent of mutual companies, which marketed life insurance policies almost as investment goods, and changes in state laws that allowed widows to collect life insurance benefits on their husbands without the benefits being claimed by creditors. It would be useful in this section to apply statistical techniques to sort out the relative contribution of these various forces on the growth of the industry, especially in light of increasing state regulation that may have slowed life insurance sales between 1840 and 1850.

The second part of Murphy's dissertation focuses on how life insurance companies marketed their product in order to increase demand, and describes the kinds of people who held life insurance policies. Chapter 7 is particularly interesting; it details how some slaveholders insured the lives of valuable slaves, and how others used life insurance policies on slaves in order to buy the slave's freedom. Chapter 8 discusses the challenges the industry faced during the Civil War (such as whether to insure war risks, and what to do about Southern policyholders). She notes that several companies were reluctant to insure war risks because they believed diseases and habits acquired during the war could affect mortality years later—something she could test by comparing lists of policyholders with the Union Army dataset.

Overall, Murphy does an admirable job in describing the development and growth of the American life insurance industry through the Civil War. In the conclusion of the dissertation, she addresses the industry post-1870, and raises several additional questions. This seems like an equally interesting time period, and I am looking forward to reading Murphy's book on the subject.

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