ESSAYS ON POLITICAL AND CULTURAL INSTITUTIONS

A Dissertation

Presented to

The Faculty of the Department

of Economics

University of Houston

In Partial Fulfillment

Of the Requirements for the Degree of

Doctor of Philosophy

By

David Bostashvili

May 2015

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Abstract

This dissertation comprises two essays. The first examines the role of a bureaucratic institution—the civil service system—in dampening political budget cycles across U.S. state governments. Using new data on state-specific civil service reforms throughout the twentieth century enables me to account for cross-state institutional heterogeneity, as well as to exploit within-state variation in the precise timing of reforms. I uncover sizable electoral cycles before a state replaces its patronage system with a system of merit-based recruitment and civil service protections for state government employees. Once the civil service system is introduced, the cycles diminish significantly or disappear completely. Reform-induced heterogeneity in budget cycles is especially pronounced in expenditure categories with high voter appeal. For example, states with the patronage system spend, on average, 27.5 dollars more per capita on roads and highways (an increase of 8.5 percent relative to the sample mean) during the two years prior to gubernatorial elections compared to the two years following the elections, whereas states with the civil service system do not exhibit such cycles. The evidence suggests that reformed bureaucracies exert considerable influence on politicians' spending decisions.

The second essay examines the role of a cultural trait—individualism-collectivism—in shaping microeconomic preferences at the individual level. I adopt the "priming" method from social psychology to exogenously vary the salience of individualism and collectivism in laboratory settings. The findings indicate that subjects primed on collectivism make less risky and more patient financial choices, and report lower self-confidence than subjects primed on individualism. Findings from a supplemental experiment indicate that making collectivism salient does indeed strengthen subjects' collectivistic perceptions, lending support to the interpretation of the main results as the effects of collectivism. Finally, I

find that Hispanics and blacks are especially sensitive to being primed on collectivism, while Asians and whites are the least sensitive. This result is consistent with the evidence that Asian and white subjects in the sample are the most and the least group-oriented, respectively.

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Chapter 1

Introduction to Essays on Political and Cultural Institutions

The unifying theme of my dissertation is the role of institutions in explaining various economic outcomes. I am particularly interested in studying formal laws, rules and regulations that I refer to as "political institutions," and informal norms, beliefs and value systems that I call "cultural institutions." Because any given political process involves different parties with different interests, the rules and regulations produced by the collective decision-making process are not necessarily efficient. Examining the causes and economic consequences of these political institutions constitutes the first part of my research agenda, of which the first essay of this dissertation is a part. For the second part, I am interested in examining how cultural institutions shape individual behavior, and the resulting implications for determining political and economic equilibria. The second essay of my dissertation constitutes one step in this direction.

In the first essay, titled “Political Budget Cycles and the Civil Service in American State Governments.” I investigate how an important political institution—the civil service system—influences electorally motivated economic policy choices in American state governments. Civil service is an important bureaucratic institution to analyze because it directly affects most of public sector employees in the U.S., and there is an ongoing debate on further reforming the system. My essay seeks to contribute to the debate by empirically analyzing the implications of how the civil service system has affected politicians’ fiscal policy choices during the past several decades.

Specifically, I study the impact of the civil service reforms on politicians’ budgetary decisions during election periods. It is well known that the timing of elections and fluctuations in economic policy often coincide. That is, politicians like to spend more just before elections than during other times in office, as it presumably helps their reelection chances. This phenomenon is termed as “political budget cycles” in the economics literature. Contrary to simple theoretical predictions, however, previous research has failed to find strong empirical support for such cycles in the United States. In the essay, I show that the existing evidence is masked by substantial institutional heterogeneity across state governments. Using new data on civil service reforms throughout the twentieth century enables me to take such heterogeneity into account. I uncover sizable electoral cycles before a state replaces its patronage system with the system of merit-based recruitment and civil service protections for state employees. My findings suggest that the civil service system significantly limits politicians’ ability to use budgets for electoral gain.

In the second essay, titled “The Effect of Culture on Economic Behavior: Experimental Evidence on Individualism-Collectivism,” I am interested in identifying the causal effects of cultural values on individual-level economic behavior. The values of individualism-collectivism are particularly interesting to study because they strongly correlate with macroeconomic measures of well-being. Rich countries tend to be the ones where people have individualistic values (taking care of oneself and the immediate family only), whereas poor countries tend to be the ones where people favor collectivistic values (being taken care of by a particular social group in exchange for loyalty). In general, cultural values are tightly intertwined with other institutions in a given society. This makes the task of empirically identifying the causal effects of culture on economic outcomes hard with the use of non-experimental data.

In this essay, I take an alternative approach and adopt the "priming" method from social psychology to exogenously vary the salience of individualism-collectivism in a controlled laboratory experiment. My results suggest that subjects primed on collectivism make less risky and more patient financial choices and report lower self-confidence than subjects primed on individualism. Motivated by the "social identity" theory, I then explore whether the priming effects of individualism-collectivism are heterogeneous across subjects of different race. I find that white and Asian subjects are the least sensitive to being primed on collectivism, whereas Hispanics and blacks are the most sensitive. This result lends further support to the underlying theory of how individualism-collectivism relates to economic behavior.

Chapter 2

Political Budget Cycles and the Civil Service in American State Governments

2.1 Introduction

Reelection-minded politicians have an incentive to increase spending on politically salient expenditures before elections (Tufte, 1978; Rogoff, 1990; Drazen, 2008). This is the basic intuition for the existence of political budget cycles that researchers have documented in different countries across the world (Drazen, 2001). Despite the intuitive theoretical predictions and in contrast to the supporting empirical evidence, however, it has proved difficult to uncover election cycles in certain developed countries (Alesina, Roubini and Cohen, 1997; Brender and Drazen, 2005; Shi and Svensson, 2006). The evidence for the United States is particularly scant (Drazen, 2000, 2001). According to Drazen (2000, 244), "Taken as a whole, the econometric evidence presents a case for the existence of some opportunistic, pre-electoral manipulation of economic policy, ... [but] there is disagreement on how strong the case is."

The present paper shows that the lack of conclusive empirical support for electoral cycles in the U.S. can be explained by considering the relationship between bureaucrats and politicians. Although the importance of bureaucratic institutions in effective governance has been widely recognized, studies of political budget cycles have ignored the potential influence of bureaucrats on politicians' economic policy choices. I argue that a bureaucratic institution based on merit-based recruitment, political neutrality, and security of tenure—the civil service system—can significantly restrain politicians' electorally motivated behavior. Using data on the precise timing of civil service reforms in state governments, I show that this hypothesis is buttressed by the empirical evidence. I uncover sizable expenditure cycles before a state replaces political patronage with the system of civil service. The result demonstrates that the extent of political budget cycles in the U.S. depends on the bureaucratic organization of state governments.

The idea that a professional and politically neutral bureaucracy is key to good governance is not new (e.g., Evans and Rauch, 1999).[1] Already in the nineteenth century, the Progressive movement in the U.S. started advocating a bureaucracy where hiring, promoting, and other contracting decisions would be based on merit, and a bureaucracy that would be independent from political interference and accountable to the public at all times

[1]The International Monetary Fund stresses the importance of "promoting good governance in all its aspects, including by ... improving the efficiency and accountability of the public sector ... as essential elements of a framework within which economies can prosper" (International Monetary Fund, 1996). According to the World Bank, "Good governance is epitomized by ... a bureaucracy imbued with a professional ethos [and] an executive arm of government accountable for its actions" (World Bank, 1994).

(Woodrow Wilson, 1887). The Wilsonian ideal was partially implemented by Congress when it enacted the Pendleton Act in 1883. The law created a federal civil service system, mandated merit-based recruitment, and laid the foundation for political neutrality and security of tenure for federal employees. State and local governments also adopted similar "merit systems," albeit slowly.

Civil service reforms in state governments have recently become controversial. The controversy reflects theoretical tension between political neutrality and accountability of career civil servants. Detaching bureaucrats from elected policymakers, some scholars have argued, could make the bureaucrats unresponsive to the needs of voters (e.g., Heclo, 1977; McCubbins, Noll and Weingast, 1987; Lewis, 2007). It is thus no surprise that tenure, an important aspect of civil service regulations, remains the subject of an ongoing debate on overhauling the "dysfunctional" civil service, particularly at the state and local government levels. Governor Brewer of Arizona, for example, has declared: "The current 'Merit System' is a misnomer. It discourages our best State workers while protecting the weakest performers."[2,3] The American civil service, in short, has been "in flux" for the past two decades (Hays and Sowa, 2007; Battaglio and Condrey, 2007). Despite the immediate practical relevance, the existing empirical evidence is too scarce to inform the debate and shed light on the theoretical ambiguity of the optimal civil service design (Ujhelyi, 2014*a*).

[2]Office of the Governor, Phoenix, Arizona. February 14, 2012. "Governor Jan Brewer, Legislative Leaders Propose Sweeping Personnel Reform Plan" [Press release]. Retrieved from `http://www.azgovernor.gov/personnelreform.asp` (accessed April 25, 2014).

[3]By eliminating civil service protections and moving towards an "at will" employment system commonly used in the private sector, Arizona joined the growing group of "modernizing" states in 2012, together with Colorado and Tennessee (Maynard, 2012). Earlier transformations took place in Indiana (2011), Florida (2001), and Georgia (1996). Kansas and New Jersey are also debating similar changes.

The present study seeks to contribute to the debate by providing direct evidence regarding how the civil service system in state governments affects politicians' electorally motivated spending decisions. In particular, I estimate the impact of civil service reforms on political budget cycles, that is, the extent to which politicians increase spending just before elections to help their reelection chances. Pre-electoral fiscal interventions can take the form of boosting the overall level of spending or redistributing expenditures in favor of voter-friendly projects (Drazen and Eslava, 2010). Such cycles can be socially costly, however, as redistributing expenditures across voter groups or over time can, in the absence of perfect capital markets, undo any consumption smoothing benefits of government spending programs (e.g., Gruber, 1997). Answering the main research question of this paper—how the civil service system affects political budget cycles in U.S. state governments—is thus a step forward in understanding the welfare implications of civil service reforms.

The impact of the civil service system on electoral cycles is theoretically ambiguous. On the one hand, reformed bureaucracies might have a dampening effect on pre-electoral fiscal cycles. For example, governors might not be able to influence the exact timing of budgetary decisions or the distribution of expenditures within reformed state agencies. This is possible if the civil service system that makes bureaucrats relatively independent from politicians, also reduces the degree of effective control that politicians exercise over bureaucratic decisions of how much, where, and when exactly to spend. Additionally, if the reason for politicians to "manipulate" the pre-electoral budget is to simply appear competent in front of voters (Rogoff, 1990), professional bureaucrats can make it harder for politicians to signal their "type"—mitigating their incentives for producing budgetary cycles. On the other hand, it is also a possibility that civil service exacerbates, rather than dampens, budget cycles. Tenured bureaucrats might, without risking employment, deliberately implement inferior policies so that the electorate votes the politician out of office. Such policies might involve increasing spending too much before elections—up to electorally unpopular deficit levels (Peltzman, 1992). Strategically acting bureaucrats can also use the credible threat of undermining the politician to extract inflated budgets, thus potentially exacerbating expenditure cycles.

To assess the impact of the civil service system on political budget cycles empirically, I use novel data from Ujhelyi (2014*b*) on the precise timing of bureaucratic reforms in U.S. state governments. Different states adopting civil service in different years throughout the twentieth century generated rich time-series variation in bureaucratic systems across states (Figure 3.3). Exploiting the longitudinal dimension of this variation enables me to compare political budget cycles before and after the reform within each state. There are two reasons that make this difference-in-differences type of empirical strategy viable for recovering a consistent estimate of the impact of the civil service system on electoral cycles. First, unlike some countries where the timing of elections has been shown to respond to the underlying economic environment (Heckelman and Berument, 1998), gubernatorial election dates in the U.S. have historically been predetermined. Second, I take specific steps to address concerns about potential endogeneity of the reforms. Based on the literature in public administration and political science, I identify several widely agreed upon causes of civil service reforms across states and argue that, conditional on these factors, the reforms were orthogonal to other time-varying state characteristics affecting governors' expenditure decisions.

My main results suggest that the civil service system has a dampening effect on political budget cycles in U.S. states. I find that state governments operating with the patronage system experience relatively large electoral cycles compared to the average state (by about a factor of three), and that such cycles are significantly smaller in magnitude or not present at all in states with merit-based bureaucracies. The results are robust with respect to the definition of electoral cycles and the estimation method, as well as to controlling for a number of factors that could simultaneously affect the timing of civil service reforms and expenditure cycles.

The dampening impact of the civil service system on electoral cycles is statistically significant and economically meaningful. Patronage states increase spending on direct expenditures by 41.7 dollars per capita and on roads and highways by 27.5 dollars per capita (increases of two and 8.5 percent, respectively, relative to the sample means) during the two years prior to elections. After introducing the civil service system, the magnitude of the pre-electoral cycles in direct expenditures diminishes to 10.2 dollars per capita (0.5 percent relative to the sample mean), while the cycles in highway expenditures become statistically indistinguishable from zero. Given that the discretionary part of state expenditures is typically small (5–15 percent, according to Rosenthal, 1990), my findings imply large cycles in discretionary spending in patronage states and a large dampening impact of the civil service system thereon. For comparison, consider as an example the findings of Shi and Svensson (2006), who estimate that fiscal balance (expressed as a share of GDP) worsens by 1.3 percentage points in the election year in developing countries—a change of 30 percent relative to the sample mean—but does not change in developed ones. Compared to my results, this suggests that the difference between electoral cycles in discretionary spending in patronage and civil service states is at least half as large as the difference between election-year changes in fiscal balance in developing and developed countries.

My finding that cycles are much larger in politically salient highway expenditures in patronage states is consistent with the "signaling argument" for the existence of political budget cycles, whereby politicians, to appear competent and gain electoral support, increase spending on voter-friendly projects just before elections. In addition, that these large cycles are entirely offset in civil service states is consistent with the hypotheses that professional bureaucrats make signaling hard for politicians, or that politicians are not able to influence independent bureaucrats who enjoy a large degree of autonomy in making budgetary decisions about how much, where, and when exactly to spend. Finally, the results are not consistent with the alternative hypothesis that tenured bureaucrats might be trading off the threat of implementing inferior policies for large budgets for their agencies, potentially making election cycles more pronounced.

As an additional support for the signaling argument, I find no electoral cycles in politically "insignificant" expenditure categories. For example, combined state spending on police protection in 2010, that amounted to 13 billion dollars, was only 13.3 percent of combined state and local spending on police nationwide. More dramatically, the total state spending of 7 billion dollars on elementary and secondary education amounted to only 1.2 percent of combined state and local expenditures on schools. The state shares of police and school expenditures in combined state and local spending have been low across states and over time. Governors, knowing that such services are traditionally financed by local governments, are thus unlikely to use these expenditures as a main vehicle for influencing voters. My findings of no cycles in police and school expenditures, and of no dampening effect of the civil service system thereon, are consistent with the hypothesis that politicians do not find it worthwhile to alter politically insignificant policies before elections.

Politicians' primary purpose for producing political budget cycles is to gain additional electoral support and eventually win reelection (e.g., Drazen and Eslava, 2010). Electoral outcomes, however, are influenced by many other factors and changing fiscal policies might be "helpful" only at the margin. If the incumbent expects to win reelection easily (e.g., by 60 percent or more), it is unlikely that she will need to resort to the means of modifying the budget for gaining extra votes. Likewise, if the incumbent expects to lose to the challenger by a wide margin (with 40 percent or less), it is unlikely that increased expenditures will help her reverse the anticipated outcome. The incumbent will have the strongest incentive to increase pre-electoral spending when the race is expected to be relatively close, that is, when the probability of making the difference (winning versus losing the electoral race) is higher. Consistent with this hypothesis, I find that the reformed-induced heterogeneity in political budget cycles is much more pronounced during the election episodes in which the incumbent's vote share fell between 40 and 60 percent.

Overall, the evidence suggests that civil service reforms have significantly contributed to ameliorating politicians' electorally motivated behavior across U.S. states. A general policy implication is that further reforming state bureaucracies—e.g., by taking away tenure from professional civil servants—could result in "unintended" consequences above and beyond those that the reformers envision. The current debate that emphasizes the need for removing "unresponsive" bureaucrats from the policy process should not ignore the potential impact on politicians' electoral behavior and their incentives to execute economic policy.

2.2 Related Literature

My findings contribute to several strands of the literature. Most importantly, this is the first paper to connect two distinct and large bodies of the scholarship: one on bureaucracies and the other on political budget cycles.

First, the present study is one of only a handful of works to date that provide direct evidence on the impact of merit-based bureaucratic institutions on economic or political outcomes. Evans and Rauch (1999), for example, analyze a cross-section of 35 countries and show that "Weberianness" of public agencies, that is the extent to which these agencies rely on merit-based recruitment principles, is positively correlated with economic growth. Similar contributions include Rauch (1995), Krause, Lewis and Douglas (2006), and Ujhelyi (2014*b*), who study the impact of civil service rules on infrastructure investments across U.S. cities, the accuracy of state revenue forecasts, and the reallocation of spending from state to lower level governments, respectively. In contrast to these papers, I study the impact of the civil service system on political budget cycles.[4]

[4]More generally, the present paper is related to a large theoretical branch of the economics literature studying the role of bureaucrats in policy making in the principal-agent type settings (Wilson, 1989; Prendergast, 2007; Ujhelyi, 2014*a*). A number of papers in this literature are primarily concerned with the optimal delegation of powers between elected and appointed officials (Besley and Coate, 2003; Alesina and Tabellini, 2007; Vlaicu and Whalley, 2013). As opposed to studying what the level of delegation between politicians and bureaucrats should be, political scientists have studied why politicians do delegate power to bureaucrats to begin with. Gailmard and Patty (2007), for instance, argue that politicians have an incentive

The body of the literature on political budget cycles—to which this paper also contributes—is large.[5] The main finding in this literature is that there is some evidence for the existence of election cycles, albeit in different contexts.[6] An especially interesting result is that political budget cycles are "conditional" on the development level of a given country. Specifically, the cycles have been found to be large in developing nations, but small or nonexistent in developed ones (Brender and Drazen, 2005; Shi and Svensson, 2006).[7] According to Brender and Drazen (2005), the reason why the findings of cycles in a panel of countries are driven by the experience of developing countries is that voters in these countries are poorly informed and can not adequately evaluate fiscal manipulation. Alt and Lassen (2006) interpret this finding as a consequence of lower fiscal transparency within certain OECD economies. My results are consistent with a similar conceptual interpretation. Reforming state bureaucracies essentially mitigates the asymmetric information

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to delegate decision-making powers to bureaucrats so that the bureaucrats deem it worthwhile to acquire expertise and pursue careers in government (cf. Epstein and O'Halloran, 1999, Fox and Jordan, 2011, and Iyer and Mani, 2012; see also Gailmard and Patty, 2013).

[5]Drazen (2001) provides a thorough survey. Early versions of the political business cycle theory noted the possibility of exploiting the short-term trade-off between inflation and unemployment and thus potentially manipulating the economy. Nordhaus (1975) pioneered the theory by pointing out that if voters were sufficiently myopic, reelection-minded incumbent politicians would find it optimal to please the electorate and expand the economy right before elections. Later contributions, such as Rogoff and Sibert (1988) and Rogoff (1990), relaxed the assumption of voter myopia and rationalized political budget cycles by introducing a signaling argument. According to this theory, politicians would signal their high "competence" to voters by increasing spending during the election year, taking advantage of the fact that voters learn the true competency only with a lag.

[6]Analyzing a sample of 18 OECD economies, Alesina, Roubini and Cohen (1992) find "indications of 'loose' fiscal policy during election years." Drazen (2001) reports similar results. Schuknecht (2000) and Block (2002), on the other hand, study a cross-section of 24 developing and 44 sub-Saharan African economies, respectively, and find electoral cycles in fiscal deficits and public expenditures.

[7]See also Akhmedov and Zhuravskaya (2004), who examine political budget cycles in Russia and find strong cycles in public spending across regions. Khemani (2004), Veiga and Veiga (2007), Drazen and Eslava (2010), Sakurai and Naercio (2011), and Schneider (2009) find similar results in Indian, Portuguese, Colombian, Brazilian and German contexts, respectively.

problem that otherwise handicaps voters' ability to "adequately" evaluate electorally motivated fiscal policies.

Researchers have paid relatively little attention to studying political budget cycles in the United States. This might be because the cycles in the U.S., unlike in many other countries, have been hard to detect.[8] For example, Besley and Case (1995*a*) analyze the panel of U.S. states and find little evidence of electoral cycles or any "discernible behavioral pattern" (785). In contrast, Levitt (1997) shows that hiring police officers in U.S. cities is persistently higher during gubernatorial and mayoral election years than during non-election years. Reynolds (2014) shows that states spend *less* on tuition and fees during election years. Rose (2006) finds evidence for conditional political budget cycles—conditional on balanced budget rules. She shows that spending rises before elections in states that can carry deficits into the next fiscal year, but that this pattern, not surprisingly, does not exist in states that have strict balanced budget rules. In contrast to these papers, I ask whether political budget cycles in U.S. state governments are conditional on the civil service system.

Studying the impact of civil service on electoral cycles have several virtues. First, civil service has evolved over time across states, allowing me to compare cycles before and after the introduction of the civil service system within each state (cf. Rose, 2006, where balanced budget rules are fixed characteristics of states). Second, the civil service system directly affects hundred thousands of civil servants in the U.S. and indirectly affects many millions of citizens benefiting from the proper functioning of public service. Finally, civil

[8] For time-series analyses of political cycles in the U.S. federal government, see Borjas (1984) and Haynes and Stone (1989). They find some evidence for electoral-year increases in federal employee wages and macroeconomic outcomes, respectively.

service reforms remain a subject of an ongoing debate on further reforming bureaucracies. My results therefore have broad implications, as some of the countries that have already achieved the status of a "Weberian democracy" with merit-based bureaucracies, such as the U.S., are debating elimination of civil service protections, while many other countries are still aspiring to the Weberian model.

2.3 Civil Service in the United States

2.3.1 Historical Background

Until the twentieth century, the employment norm in the American government was based on political patronage or the so-called spoils system. Politicians dictated appointments of civil servants, who knew that they would almost certainly be removed from office if they ceased political activities in exchange for patronage. It was common for administrators to grant public servants paid leaves of absence, so that the employees could campaign in their home districts before elections (Hoogenboom, 1959). The early public servants in the U.S. were thus primarily motivated by local politics, not by career-building or professionalism.

President Garfield's assassination by a disappointed office seeker in 1881 helped elevate the issue of the civil service reform to the public agenda. In 1883, Congress passed the Pendleton Civil Service Act, intended to end the widespread spoils system. The Pendleton Act marked the beginning of the civil service system in the American government, also frequently referred to as the merit system. The system was designed to replace patronage practices with the practice of hiring employees based solely on merit and competitive examinations. The law also mandated political neutrality for all civil servants in the federal government. It required that public servants did not contribute to political funds or provide political services, and that public servants could not be removed for refusing to do so. The Act laid the foundation for tenure protections for civil service employees, although it did not initially guarantee tenure explicitly.

At the state level, adoption of the civil service system was initially slow. During the following three decades only seven states introduced civil service with general coverage for their public employees (Table 3.1). The initial wave of civil service reforms was led by the "gentlemen reformers" of the National Civil Service Reform League and did not at first have mass support (Freedman, 1994). In the late 1930s, though, civil service reforms resurfaced following a 1939 amendment to the Social Security Act of 1935. The amendment required that states receiving Social Security funds from the federal government place their unemployment security and public assistance employees under the civil service system, or forgo the funds. States obliged by adopting limited civil service systems covering relevant agencies. Some states, on the other hand, decided to introduce statewide civil service systems during the next few years (Figure 3.3).

According to scholars of public administration, the main determinant of state-level civil service reforms was the strength of the good-government movement among voters in a given state (Stahl, 1956; Tolchin and Tolchin, 1971; Ingraham, 1995). Politicians were unlikely to initiate the civil service reform themselves, as they would have had to forgo significant benefits associated with patronage (Folke, Hirano and Snyder, 2011). Still, some politicians may have advocated the reform for other reasons. According to Ruhil and

Camões (2003), these reasons were as follows: (i) party competition could have encouraged incumbents to reform the bureaucracy to "blanket in" their loyal employees under the civil service and prevent the challenger from firing them later on (see also Ting et al., 2012); (ii) if an economy was under-performing, there would have been increased demand for efficient public services, and civil service could have helped with increasing efficiency in the bureaucracy; and (iii) the reform could have been demanded by interest groups from rural areas to counter the cities' political dominance, where patronage was especially rife. In the empirical section, I check whether my findings are robust to accounting for these possible causes of civil service reforms.

2.3.2 Data and Study Sample

New data on the timing of civil service reforms are taken from Ujhelyi (2014*b*) and are shown in Table 3.1. New York was the first state to adopt the civil service system in 1883, followed by Massachusetts two years later. West Virginia was the last reformer in 1989. Texas remains the only state that has never adopted a statewide civil service system. Half of the states adopted comprehensive civil service systems in the second half of the twentieth century. Most of the states did not reform their civil service according to the merit principles until the late 1930s—the period when a series of amendments to the Social Security Act of 1935 mandated that state agencies without the civil service system be ineligible for Federal aid.

Starting in the 1990s and continuing through the 2000s, some states started to repeal tenure security in their civil service systems—marking the beginning of the "reinventing government" movement (Kellough, 1998). The first "modernizing" state was Georgia in 1996. Every public employee in Georgia hired after July 1, 1996 started serving at will, outside the civil service system. A few other states followed Georgia's suit in the 2000s. The debate on whether public employees in other states should also serve at will has been ongoing since then. Including the post-1996 period in my sample would thus complicate interpretation of the results, since in the recent years a number of states have not been operating with the same type of mostly homogeneous statewide civil service systems as they did before 1996. For this reason, 1996 defines the upper cutoff of my study period. (The results, however, are not sensitive to the inclusion of post-1996 years in the estimating sample.) The lower cutoff of 1960 is dictated by data availability.

I further confine the study sample to four-year election terms only. That is, I exclude the episodes when some states elected governors every two years (New Hampshire and Vermont still do) because budget cycles within the two-year periods are likely to be too short, if they exist at all, to be detectable. This exclusion eliminates 15 percent of the sample observations. Most importantly, the timing of civil service reforms does not appear to be correlated with changes in the electoral term length from two to four years. Most of these changes occurred during the 1960s and 1970s, following the federal model of allowing the executive a four-year term (Beyle, 2004). The simple correlation coefficient between the civil service variable and the indicator variable for whether a given state follows a two-year election term in a given year (as opposed to a four-year term) is -0.17. As Figure 3.2 illustrates, out of 25 states that changed gubernatorial term length from two years to four years, only three states did so within five years of adopting the civil service system (and five states within 10 years of the reform), suggesting little, if any, relation between the changes in term length and civil service reforms.

2.4 Empirical Strategy

2.4.1 Main Specification

Consider the specification that allows for estimating the impact of the civil service system on political budget cycles in state governments:

$$y_{st} = \alpha Elections_{st} + \beta Elections_{st} \times Civil_{st} + \gamma Civil_{st} + \delta y_{s,t-1} + X'_{st}\rho + \lambda_s + \mu_{dt} + \varepsilon_{st}, \tag{2.1}$$

where y_{st} is a fiscal outcome variable of interest in state s in year t (in per capita real 2010 dollar terms). The two explanatory variables of particular interest are $Elections_{st}$ and $Elections_{st} \times Civil_{st}$. $Elections_{st} = 1$ if there were gubernatorial elections held in state s in year t or $t+1$ and 0 otherwise, and $Civil_{st} = 1$ if state s had the civil service system in place in year t and 0 otherwise. (d indexes the U.S. Census designated divisions, discussed below.)

To allow for a possibility that budgetary cycles may last for a shorter period than two years, or differ across different pre-election years, I also report results with an alternative definition of the election cycle variable. Instead of defining the cycle as an indicator variable for the two-year window immediately preceding elections, I introduce a vector of three binary variables indicating the year of elections, the pre-election year, and the pre-pre-election year (and the three respective interaction terms with the civil service variable). In this setup, the post-election year is an omitted category and serves as a comparison group.

The rest of the explanatory variables are defined as follows. X_{st} is a vector of time-varying state characteristics (described below); λ_s and μ_{dt} are state and Census division-specific year fixed effects, respectively, and ε_{st} is a disturbance term.[9] Note that the division-specific year fixed effects account for any division-wide shocks occurring in any given year that could have been correlated with civil service reforms. This is more desirable than to simply control for the nationwide shocks occurring to all states simultaneously (via year fixed effects). The reason is that the diffusion of institutional innovations at the state level was likely to contain a strong geographic component. States closest to Washington D.C. were, on average, more likely to follow the federal government in implementing statewide reforms first than states farther away (e.g., Walker, 1969). Moreover, states whose neighbor states had already implemented institutional reforms were more likely to implement their own reforms, perhaps because of the motives similar to "yardstick" competition (Besley and Case, 1995*b*). In any case, controlling for the division-specific year effects seems to be the most plausible way for holding constant any geographically confined shocks that could have been correlated with the civil service reforms within those geographic confines.

As the main outcomes of interest, I analyze two variables: real per capita direct expenditures (total expenditures less intergovernmental expenditures) and real per capita direct highway expenditures (direct expenditures on construction and maintenance of roads and

[9] Census divisions are nine geographic regions in the United States, designated by the U.S. Census Bureau, offering a geographic level of aggregation that is one level finer than the four Census regions (Northeast, Midwest, South, and West), and one level broader that the fifty states. These divisions are: New England and Middle Atlantic (both within the Northeast), East North Central and West North Central (both in the Midwest), East South Central, West South Central and South Atlantic (all three in the South), and Mountain and Pacific (both in the West).

highways). Direct expenditures account for about three-fourths of total expenditures in an average state. The rest comprises intergovernmental expenditures—funds that state governments transfer to lower level governments such as counties and municipalities. I focus exclusively on direct expenditures because these are the funds spent directly by the state governments and are therefore more likely to buy votes than intergovernmental expenditures.

In addition to studying total direct expenditures, I analyze highway expenditures for two main reasons. First, infrastructure projects, such as roads, are "politically salient" as they can easily be targeted to a broad base of voters (e.g., Knight, 2002). Secondly, most infrastructure projects in the U.S. are administered by contracting out to third-party businesses, and bureaucratic discretion plays an especially important role in the process of government procurement and selection of such contractors.[10] Direct state spending on roads and highways is therefore an ideal candidate for estimating the impact of the civil service system on electoral cycles.

As control variables, contained in the vector X_{st}, I use characteristics commonly used in the literature on institutions and policy outcomes (Besley and Case, 2003). In particular, I control for government resources such as the tax base, measured by state real per capita income (and its squared term), as well as for demographic variables—population size (and its squared term) and the fractions of state population that are school-aged (5–17) and elderly (over 65)—to capture the demand for government services. Table 3.2 reports the definitions and summary statistics of all the dependent and independent variables used in the empirical analysis. The data sources are given in Appendix B.1.

[10]For extensive anecdotal evidence on the role of bureaucratic discretion in American politics, see Kirst (1969).

The coefficients of particular interest in Equation 2.1 are α and β. α tests for the presence of political budget cycles in a state without the civil service system, that is, in a patronage state. β is identified by comparing changes in outcome y before and after elections in states with civil service to states with the patronage system. In other words, β represents the extent to which the effect of election timing on expenditures might differ across states with and without the civil service system. Therefore, $\alpha + \beta$ represents the effect of electoral cycles in states that had the civil service system in place. A negative β, together with a positive α, would be consistent with the hypothesis that civil service has a dampening effect on fiscal cycles.

2.4.2 Identification

If the econometrician can not fully account for all time-varying factors simultaneously affecting civil service reforms and spending decisions, the estimated cycles in states with and without civil service will likely be biased. Even in this case, however, it is possible to consistently estimate β, the differential impact of the civil service system on fiscal policy outcomes during pre-election versus post-election periods. The assumption for interpreting the effect of the civil service system on budget cycles as causal requires that changes in expenditures over the electoral cycle are not affected by the factors correlated with the civil service reform. The identification assumption would be violated if, in the absence of the reform, there were any changes in pre-election expenditures before and after the reform that were different from changes in post-election expenditures between the same periods (conditional on X_{st}). This is arguably a weaker assumption than the one that is required for consistently estimating the causal effect of civil service on spending in general. In that case, the econometrician needs to assume that, in the absence of the reform in a given state, either expenditures would not have changed at all, or any observed changes would have been fully explained by the time-varying control variables contained in X_{st}.

As discussed in Section 2.3.1, potential factors simultaneously affecting the timing of civil service reforms and expenditure outcomes include: the state of the economy, the extent of urban-rural divide, the dominance of a governing party, and the public demand for efficient government. I control for economic conditions in a given state by including state personal income in X_{st}. The urban-rural divide can be captured by the urbanization rate (i.e., the share of state population living in urban areas). The party dominance can be controlled for by three indicator variables: one for Republican control of both state legislatures, another for Democratic control of both state legislatures, and a third for the governor's partisan affiliation.[11]

To address the issue of the voter demand for good government affecting both civil service reforms and spending outcomes, I employ a widely used measure of citizen ideology compiled by Berry et al. (1998). Using the roll call voting scores of state congressional delegations, this index first rates the ideology of congressional candidates. It then uses the candidates' outcomes in congressional elections to compute a state-by-year ideology measure for the electorate. The index is not available prior to 1960, so 1960 is the lower cutoff date of my study sample. The coefficients on $Elections \times Civil$ and $Civil$ in Equation 2.1 are therefore identified from the civil service reforms that occurred in 16 states after 1960.

[11] Specifications with these party control variables exclude Nebraska which has a nonpartisan legislature.

2.4.3 Alternative Estimation Method

Fiscal outcome variables, such as expenditures and budget deficits, are highly persistent over time. For this reason, it is customary in the political budget cycles literature to control for the lagged dependent variable in specifications testing for political cycles in a given fiscal variable. As Nickell (1981) has shown, however, panel data models with fixed effects generate biased estimates in the presence of a lagged dependent variable, especially when the sample period is short.

I address the issue that potentially arises from the Nickell bias in two ways. First, using Monte Carlo simulations, I show that the Nickell bias is negligible in my sample that has a time dimension of 37 years. (The simulation results are reported in Appendix B.2.) This is in line with the previous findings that for panels with at least 25 years, the Nickell bias converges to zero (Judson and Owen, 1999). As a robustness check, I perform a one-step generalized method of moments (GMM) estimation of Equation 2.1. To do this, first consider eliminating fixed effects from Equation 2.1 by first-differencing both sides of the equation:

$$\Delta y_{st} = \delta \Delta y_{s,t-1} + \Delta \tilde{X}'_{st}\theta + \Delta\mu_{dt} + \Delta\varepsilon_{st}, \tag{2.2}$$

where $\tilde{X}_{st}$ is a vector containing all the explanatory variables from Equation 2.1: X_{st}, $Elections_{st}$, $Civil_{st}$, and $Elections_{st} \times Civil_{st}$.

Note now that because $y_{s,t-1}$ and $\varepsilon_{s,t-1}$ are correlated by construction, correlation between $\Delta y_{s,t-1}$ and $\Delta\varepsilon_{st}$ is not zero:

$$\mathbb{E}(\Delta y_{i,t-1}\Delta\varepsilon_{it}) \equiv \mathbb{E}((y_{i,t-1} - y_{i,t-2})(\varepsilon_{it} - \varepsilon_{i,t-1})) = -\mathbb{E}(y_{i,t-1}\varepsilon_{i,t-1}) = -\sigma_\varepsilon^2 \neq 0. \tag{2.3}$$

Thus, estimating Equation 2.2 via OLS would yield inconsistent results. Arellano and Bond (1991) suggest to use lagged levels of the dependent variable from period $t-2$ and earlier ($y_{s,t-2}$, $y_{s,t-3}$, $\ldots$) as instrumental variables for the difference $(y_{s,t-1} - y_{s,t-2})$ to address this issue. This approach has become a standard way of estimating dynamic panel data models with a short time dimension and has been used, among others, by Brender and Drazen (2005), Rose (2006), and Shi and Svensson (2006) in the political budget cycles literature. As I show below, the Arellano-Bond method yields results that are very similar to the OLS estimates.

2.5 Results

2.5.1 Direct Expenditures

I first test for the presence of conditional political budget cycles in the total amount of direct expenditures. Table 3.3 reports the results of estimating specifications in which the dependent variable is the real per capita direct expenditures (in real 2010 dollar terms). The first column is a benchmark specification that tests for the presence of a cycle in an average state, without differentiating between civil service and patronage states. In the second column, I add two other key independent variables—*Civil* and $Elections \times Civil$— to allow the cycle to differ between states with the patronage system and states with the civil service system. The third and fourth columns fit conceptually the same specifications as the ones reported in the first and second columns, with the only difference that the election cycle is accounted for by including a vector of three pre-election year dummy variables instead of a single indicator for the two-year window prior to elections.

The results suggest a relatively small election cycle in an average state. In particular, direct expenditures increase, on average, by \$13.3 per capita during the two-year period preceding gubernatorial elections (Column 1). The cycle represents an increase of only 0.6 percent over the sample mean. Once institutional heterogeneity is accounted for, though, it turns out that the finding of a small cycle in an average state is entirely driven by the cycles in patronage states (Column 2). The electoral cycle of \$41.7 in these states is precisely estimated and is about three times larger than in an average state—an increase of 1.9 percent over the sample mean. The cycle in civil service states, in contrast, amounts to only \$10.2 (= \$41.7 − \$31.5). That is, states that have the civil service system in place spend significantly less money on direct expenditures just before elections (by \$31.5 per capita) than states that have not adopted the system. This is a novel finding in the literature, and implies that civil service dampens political budget cycles in direct expenditures.

The findings are similar when election cycles are instead represented by the three pre-election year dummies (Columns 3 and 4, Table 3.3). The cycle of \$23 in an average state is concentrated in the election year specifically, with no detectable changes during pre-election and pre-pre-election years. After accounting for the institutional heterogeneity, however, the results suggest that states without the civil service system spend more not only during the election year itself—an increase of \$35, or 1.6 percent over the mean, but also during the pre-election year—an increase of \$59.2, or 2.7 percent. This large pre-electoral cycle is completely offset by civil service. Reformed states do not significantly change spending during the pre-election year (\$59.2 − \$71.4 = −\$12.2, a statistically insignificant amount). The election year cycle, by contrast, does not seem to be dampened by the civil service system ($\$35 - \$13.4 = \$21.6$, significant at the five percent level). These results are succinctly represented by Figure 2.3, which shows how the estimated cycles over the four-year electoral term differ between states with and without the civil service system.

The reason for the estimated cycle in patronage states to be offset by the civil service system in the pre-election year, but not in the election year itself, could be as follows. If governors holding the office for four-year terms would like to get the signal across voters in time, they would need to start spending early. According to Beyle (2004), a popular message about four-year gubernatorial terms was that "in your first year, you learn how to be the governor; in the second and third years you can do what you had hoped to do; then in the fourth year you are running for reelection" (para. 5). Politicians might pay particular attention to longer-term investment projects with clear electoral consequences early on, and career civil servants might exert stronger influence on such expenditures. Note that this is in line with the highway spending results, whereby significant election cycles both during the election year and the year before are completely dampened by the civil service system. These considerations, however, are speculative and warrant further research.

2.5.2 Highway Expenditures

I now examine political budget cycles in the category of expenditures related to construction and maintenance of roads and highways.[12] Analyzing highway expenditures is important because of two reasons. First, it is the third largest category in total direct expenditures by state governments, making up about 10 percent of total state spending on average.[13] Second, expenditures on infrastructure projects, such as roads and highways, can be especially appealing to voters. In other words, these spending categories are politically salient. Politicians are therefore likely to deem such expenditures as an effective tool for pleasing the electorate and create budget cycles by increasing spending prior to elections.

Table 2.4 reports the results of specifications in which the dependent variable is real per capita direct expenditures on highways. The table presents coefficients on key independent variables: electoral cycle, civil service, and the interaction term between the two. Control variables in all specifications are the same as in Table 3.3, and the table is organized in the same way.

As in the case of total direct expenditures, the results show a relatively small election cycle in highway expenditures in an average state. In particular, highway expenditures increase by $7.3 per capita during the two-year period preceding gubernatorial elections (Column 1). The cycle represents an increase of 2.2 percent over the sample mean. Once

[12] These expenditures are related to maintenance, operation, repair, and construction of non-toll highways, streets, roads, alleys, sidewalks, bridges, tunnels, ferry boats, viaducts, and related structures. *Source:* 1992 Government Finance and Employment Classification Manual for Federal, State, and Local Governments, the U.S. Census Bureau. Retrieved from `http://www.census.gov/govs/www/class.html` (accessed June 14, 2014).

[13] States spend more only on public welfare (38 percent) and higher education (18 percent). These "big ticket" categories, however, are not as well suited for testing budget cycles in state governments because the discretionary role of governors in these types of spending is especially limited (Rosenthal, 1990).

institutional heterogeneity is accounted for, once again, it turns out that the finding of a cycle in an average state is entirely driven by the cycles in patronage states (Column 2). The estimated cycle in states with political patronage is about four times larger than that in an average state—an increase of 8.5 percent over the sample mean—which is a much bigger change than in the case of direct expenditures. The coefficients are even larger in a specification with the more flexible definition of electoral cycles (Column 4 and Figure 2.4). The civil service system has a strong dampening effect on cycles in highway expenditures (Columns 2 and 4, Table 2.4), lending support to the idea that politicians intend to appeal to voters by increasing spending on voter-friendly projects prior to elections.

2.5.3 Economic Significance and Interpretation of the Results

State governments never have full discretion over the entire amount of expenditures in a given year. According to Rosenthal (1990), "The funds over which governors and legislatures really have discretion amount to only 5 to 15 percent of the budget" (132). The rest is either earmarked or otherwise locked in. This implies that the estimated pre-electoral cycles of about two percent in total direct expenditures are on the order of 13 to 40 percent of discretionary spending (relative to the post-election year).

Estimated electoral cycles seem to be largest in patronage states, but how does the magnitude of such cycles compare to, say, election cycles in developing nations as documented in the political budget cycles literature? Shi and Svensson (2006), for example, estimate that fiscal balance, expressed as a share of GDP, worsens by 1.3 percentage points in the election year in developing countries—a change of 30 percent relative to the sample mean (1372, Table 2, Column 4). Developed countries, by contrast, do not experience such changes. Compared to my results, this suggests that the difference between electoral cycles in discretionary spending in patronage and civil service states is at least half as large as the difference between election-year changes in fiscal balance in developing and developed countries.

Overall, the evidence suggests that U.S. state governments with the patronage system experience statistically significant and economically substantial fiscal cycles prior to gubernatorial elections and that these cycles are significantly smaller or absent in states with the reformed civil service. The evidence is particularly stark for funds spent on roads and highways, but this is not surprising given the political salience of such expenditures. The evidence for cycles is consistent with the standard theory of political budget cycles (Rogoff, 1990), whereby reelection-minded politicians seek to appeal to voters particularly eagerly before elections, perhaps by signaling their "ability" via increased spending on key projects.

The evidence for the dampening effect of civil service on cycles, on the other hand, is consistent with the hypothesis that reformed bureaucracies simply make it harder for politicians to exert effective control on the timing of budgetary decisions. It is also consistent with the idea that in states with independent and professional bureaucracies, politicians find it harder to signal their type. Governors in civil service states would thus have little incentive for engineering fiscal cycles. More research is needed to disentangle the exact mechanisms behind the dampening effect of the civil service system. In Section 3.8, I explore a few possibilities in this direction.

That budget cycles are significantly dampened by civil service is an original finding.

It is a finding that links the literature on political budget cycles and that on bureaucracies, and provides further evidence on "conditional" budget cycles. According to a number of studies in this literature, electoral cycles, when they exist, seem to be dampened by certain institutional characteristics of governments. For instance, Rose (2006) demonstrates how election cycles in U.S. states are dampened in the states that have strict balanced budget rules. Alt and Lassen (2006) show that the existence of election-year cycles in fiscal balance across countries depends on fiscal "transparency" of a given country: the more transparent the country, the more informed the voters, and, presumably, the more difficult for politicians to convince voters of their "competent type" via high deficit spending. In line with this interpretation, my results suggest that professional bureaucracies mitigate the asymmetric information problem between politicians and voters that otherwise handicaps voters' ability to "adequately" evaluate fiscal interventions.

2.6 Robustness of the Results

2.6.1 Arellano-Bond Estimates

I re-estimate the main results—reported in Tables 3.3 and 2.4—using Arellano and Bond's (1991) GMM method described in Section 2.4.3. This method was designed to solve the endogeneity problem caused by the presence of a lagged dependent variable as one of the explanatory variables in a specification that includes individual-specific fixed effects (see Section 2.4.3 for more details). Since my estimating sample has a long enough time series dimension for the OLS estimates to exhibit little bias, these estimates should not be very different from the supposedly consistent ones produced by the Arellano-Bond's GMM method.

The results are reported in Table 2.5. The first four columns present the GMM estimates of the same specifications as in Table 3.3, and Columns 5–8 re-estimate the specifications from Table 2.4. Comparing the corresponding OLS and GMM estimates, the findings are very similar numerically. For example, the results in Column 4 suggest that election cycles in total direct expenditures in patronage states amount to \$34.2 per capita in the election year and to \$45.5 per capita in the pre-election year. The corresponding OLS estimates are \$35 and \$59.2, respectively. Importantly, the magnitudes of the dampening impact of the civil service system are also similar. The Arellano-Bond estimates are $-\$21.1$ and $-\$61.3$ for the election year and the pre-election year, respectively, while the corresponding OLS estimates are $-\$13.4$ and $-\$71.4$. There are definitely no qualitative differences between the results as estimated by GMM and OLS. The main result, which is that the civil service system dampens political budget cycles in U.S. state governments, is upheld regardless of the estimation method. The findings are also quantitatively similar, lending further support to the robustness of the results.

2.6.2 Ruling Out Spurious Heterogeneity

Could it be that the different cycles observed in patronage versus civil service states are due to some other factors correlated with civil service reforms? In this exercise, I check whether the heterogeneity of cycles across the civil service dimension is preserved after controlling for heterogeneity across the set of control variables. That is, in addition to controlling for each variable contained in X_{st} (Equation 2.1), I control for each x variable interacted with the *Election Cycle* variable. This ensures that the dampening of budget cycles is attributable to the civil service system and not to any other factor in X_{st}.

The results are presented in Table 2.6. For comparison, Columns 1, 3, 5, and 7 repeat Columns 2 and 4 in Table 3.3 and Table 2.4. Columns 2, 4, 6, and 8 are specifications controlling for the heterogeneity of cycles over the X_{st} variables. Note that the unreported coefficients on election variables in odd-numbered rows do not have a straightforward interpretation since each of these are interacted with all the control variables, many of which are continuous. The coefficients of interest are the ones on the interactions terms between the election variables and civil service. These coefficients estimate the dampening impact of civil service on electoral cycles, just as in the specifications without controlling for $Elections \times X$. The results show that, both in the case of total direct expenditures and highway expenditures, the dampening impact of the civil service system is largely the same regardless of whether the extra interaction terms are introduced. This is a reassuring finding: the effect of civil service on budget cycles does not appear to be an artifact of failing to account for the heterogeneity across other observable characteristics of the states.

2.6.3 "Falsification" Test

In this exercise, I estimate the impact of the civil service system on electoral cycles in tax revenues (instead of expenditures). The idea behind the exercise is that, unlike expenditures, bureaucratic discretion does not play an important role in the process of tax collection. Tax rates are predominantly set by state legislatures, are mostly automated and formula-based, and are much less likely to be affected by bureaucratic discretion than expenditures. Therefore, I do not expect dampening effect of the civil service system on electoral cycles in tax revenues.

The results in Table 2.7 and Figure 2.6 show that, indeed, there is no evidence for the dampening of tax revenue cycles in state governments. All the interaction terms of the election variables with the civil service variable are statistically indistinguishable from zero. Interestingly, it appears that states collect significantly more tax revenues in all three years prior to elections compared to the post-election year. The magnitude of these increases is about two percent over the sample mean. The reason for states increasing tax revenues before elections might be that voters are "fiscal conservatives" (Peltzman, 1992). Politicians might be trying to avoid punishment at the polls for deficit spending by balancing higher spending with higher taxes, perhaps counting on the low political salience of certain types of taxes versus high salience of certain categories of expenditures. The relationship between political salience of taxes and electoral cycles, however, is likely to be more complex and is an interesting topic for future research.

2.7 Possible Mechanisms and Alternative Explanations

If the reason for politicians to engineer political budget cycles is to gain additional electoral support, then they need a useful instrument through which they can influence voters. This is why I expect governors to make use of politically salient expenditures as a primary tool for pleasing voters. The finding of especially large cycles in expenditures on roads and highways is consistent with this hypothesis. By the same logic, politically insignificant expenditures are unlikely to be perceived as an effective instrument for appealing to the electorate.

The two leading examples of such "insignificant" categories are expenditures on police protection and elementary and secondary education. Combined state spending on police protection in 2010, for example, amounted to 12.9 billion dollars that was only 13.3 percent of combined state and local spending on police nationwide. Total state spending of 6.7 billion dollars on elementary and secondary education amounted to only 1.2 percent of combined state and local expenditures on elementary and secondary schools. The state shares of police and school expenditures in combined state and local spending have been low across states and over time. Governors, knowing that such services are traditionally financed by local governments, are thus unlikely to increase these expenditures for the purpose of garnering extra electoral support.

I thus check whether the cycles that I estimated in total direct expenditures and highway spending behave differently in these insignificant expenditure categories. The results in Table 2.8 support the case that politicians do not find it worthwhile to intervene in politically insignificant policies. There is no evidence of electoral cycles in expenditures on police or secondary education in either patronage or civil service states. All the coefficients on the election cycle variables are small in magnitude and statistically insignificant at any reasonable level of confidence. This finding strengthens the signaling argument as a potential explanation of political budget cycles. If politicians were indifferent as to how voters reacted to different types of spending, then there would be no reason not to increase all types of expenditures or, at least, those expenditure categories that are more malleable.

It is also worthwhile to check whether the primary motive for politicians creating budget cycles is to gain an edge in elections. To this end, I explore whether the cycles in patronage states, and thereby the dampening effect of the civil service system, are even larger during the periods preceding more closely contested elections.[14] The intuition behind this hypothesis is as follows. If the incumbent knows that the reelection he is running for is going to be an easy win, his incentive to modify the budget will be weaker. Likewise, if an incumbent is sure to lose reelection, there is no point in trying to secure votes via fiscal cycles. On the other hand, in close races where stakes are high incumbents will be more tempted to resort to any means to gain voter support—increasing politically salient spending just before elections being one of the options.

Results of this exercise are presented in Table 2.9. For both types of expenditures, the findings are consistent with the hypothesis that electoral gains constitute the primary motive behind engineering budget cycles. Patronage states exhibit large cycles prior to competitive elections—34 percent larger than prior to average elections, but no cycles prior to non-competitive elections. This is consistent with the hypothesis that competitive elections are the times when incumbents find increasing spending to have the highest marginal benefit for gaining extra votes. Furthermore, these large cycles are entirely offset by the civil service system in civil service states—consistent with the original hypothesis regarding the restrictive role of reformed bureaucracies in governors' spending decisions.

[14] I define a given election to be competitive if either party's candidate received a vote share of less than 60 percent. Otherwise, the election is considered "noncompetitive." The results are similar when changing the definition of competitiveness with alternative cutoffs.

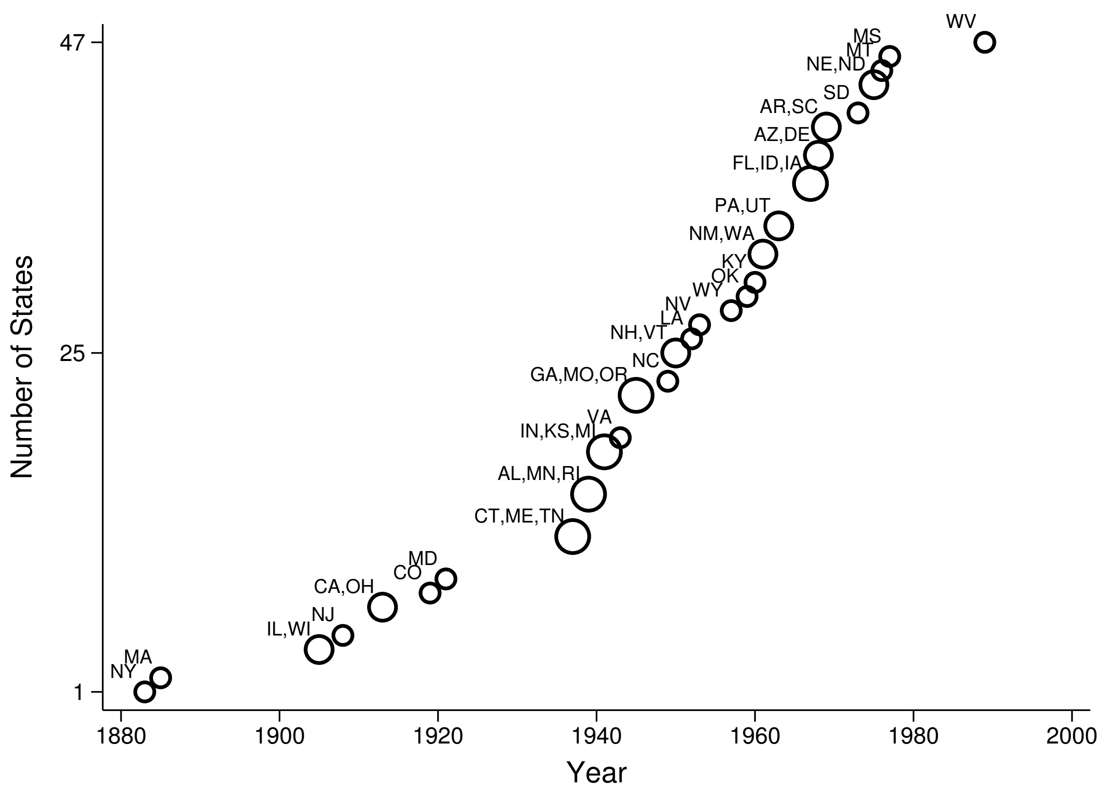
2.8 Conclusion

This paper has presented the evidence of electoral cycles in direct expenditures and expenditures on roads and highways in U.S. states. More importantly, I have shown that these cycles are conditional on an essential element of state bureaucracies—the civil service system. States with patronage-based bureaucracies tend to exhibit relatively large electoral cycles, while states with reformed civil service experience substantially smaller or no cycles. This is a novel finding in the political budget cycles literature that highlights the understudied role of bureaucratic institutions in economic policy making.

There can be several reasons for the civil service system to have a dampening impact on politicians' electorally motivated behavior. First, civil service might simply reduce the control of politicians over the timing of expenditure decisions in independent, professional bureaucracies. Politicians whose hands are tied by the civil service system, preventing them from freely using state budgets for electoral gain, spend less than politicians from the states without general merit-based coverage for their state employees. Second, the civil service system might be mitigating the imperfect information problem that voters otherwise face about the politician's quality. Estimating the relative importance of these or other potential explanations for the dampening effect of civil service presents an interesting avenue for future research.

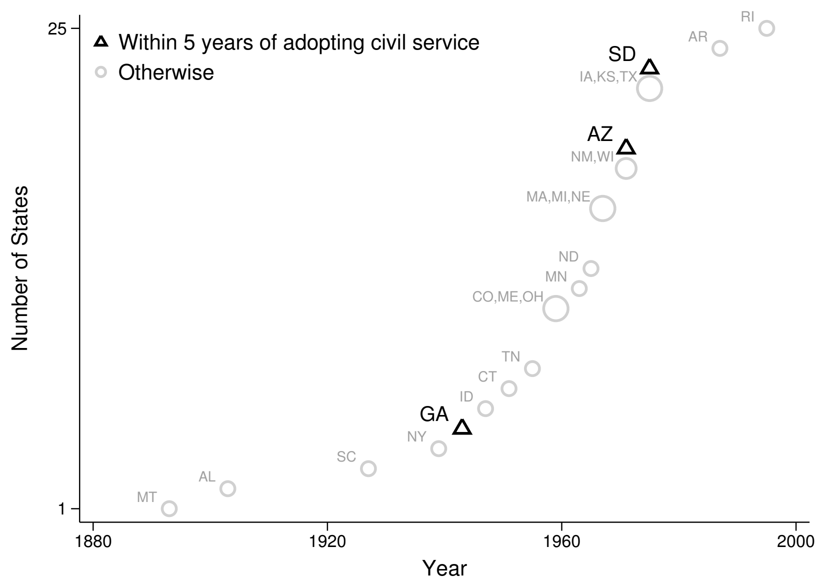
Another question that remains unanswered in this paper and in the political budget cycles literature is about welfare implications of electoral cycles. The general finding of large budget cycles in developing countries and small or non-existent cycles in developed countries makes it tempting to conclude that cycles are inefficient. A class of models pioneered by Rogoff and Sibert (1988) and Rogoff (1990), however, leaves open a possibility that cycles might represent a socially efficient mechanism for voters to screen out "low-ability" politicians out of office. Further research is needed for empirically quantifying welfare implications of political budget cycles and the role of bureaucratic institutions in dampening the cycles.

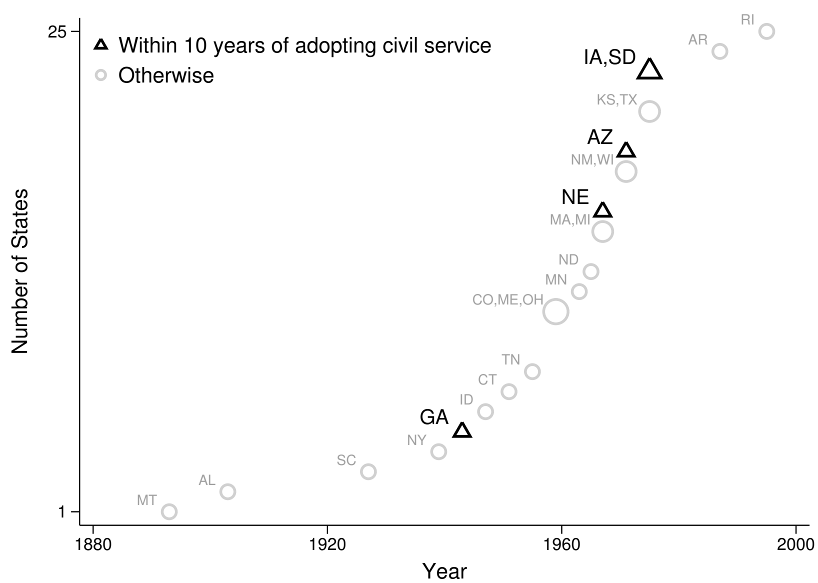
Figure 2.1: Civil Service Reforms across States over Time



Notes: Each circle in the figure shows the total number of states that had the civil service system in place in a given year. Labels on each circle denote the state(s) that adopted the system in the corresponding year. Size of each circle is proportional to the number of the most recently reformed states. The figure depicts the 48 contiguous states, excluding Texas where the comprehensive civil service has never been introduced. Appendix B.1 describes the data sources. Table 3.1 contains the exact dates of these reforms.

Figure 2.2: Changes in Gubernatorial Term Length and Civil Service Reforms





Notes: Each marker in both figures shows the total number of states that have switched from electing governors every two years to every fours years. Labels on each marker denote the state(s) that changed the term length in the most recent year. Size of each marker is proportional to the number of such changes most recently. In the upper figure, triangle-shaped markers denote the states that have switched from two- to four-year gubernatorial terms in a given year within the five-year period of adopting civil service. States switching gubernatorial terms within the 10-year period of adopting the civil service system are shown in the lower figure. Both figures depict a total of 25 contiguous states. The rest of the states have always had four-year election terms for their governors, except New Hampshire and Vermont that have always elected governors biennially.

Table 2.1: Dates of the Civil Service Reforms in the 48 Contiguous States

State	Year	State	Year
New York	1883	Vermont	1950
Massachusetts	1885	Louisiana	1952
Illinois	1905	Nevada	1953
Wisconsin	1905	Wyoming	1957
New Jersey	1908	Oklahoma	1959
California	1913	Kentucky	1960
Ohio	1913	New Mexico	1961
Colorado	1919	Washington	1961
Maryland	1921	Pennsylvania	1963
Connecticut	1937	Utah	1963
Maine	1937	Florida	1967
Tennessee	1937	Idaho	1967
Alabama	1939	Iowa	1967
Minnesota	1939	Arizona	1968
Rhode Island	1939	Delaware	1968
Indiana	1941	Arkansas	1969
Kansas	1941	South Carolina	1969
Michigan	1941	South Dakota	1973
Virginia	1943	Nebraska	1975
Georgia	1945	North Dakota	1975
Missouri	1945	Montana	1976
Oregon	1945	Mississippi	1977
North Carolina	1949	West Virginia	1989
New Hampshire	1950	Texas	N/A

Notes: For each of the 48 contiguous states, the table gives the exact year when a given state adopted the civil service system for its employees. States are sorted by the year of civil service adoptions. Texas never had a comprehensive civil service. Appendix B.1 describes the data sources.

Table 2.2: Variable Definitions and Summary Statistics, 1960–1996

Variable	Definition	Obs.	Mean	Std. Dev.	Min	Max
Dependent Variables						
Direct expenditures	Total minus intergov. expenditures per capita ($1,000)	1,474	2,187	798	614	5,286
Highway expenditures	Direct per capita expenditures on construction and maintenance of state highways and roads ($1,000)	1,474	324	164	95	1,402
Tax Revenues	Total per capita compulsory contributions exacted by a government for public purposes ($1,000)	1,474	1,562	508	441	3,398
Schools	Total per capita expenditures on elementary and secondary education ($1,000)	1,474	309	360	0	1,358
Police	Total per capita expenditures on preservation of law and order and traffic safety ($1,000)	1,474	27.4	14.3	6.2	92.7
Independent Variables						
Civil Service	= 1 if civil service with general coverage is in place	1,474	0.91	0.28	0	1
Income	Annual income per capita ($1,000)	1,474	25.3	6.0	9.0	45.6
Log population	Log state population (1,000)	1,474	8.1	1.0	5.7	10.4
Kids	Fraction of population aged 5–17	1,474	0.22	0.04	0.07	0.31
Aged	Fraction of population aged >65	1,474	0.11	0.02	0.04	0.19
Urbanization	Share of urban population in total state population (%)	1,474	67.5	13.8	36.1	93.8
Immigrants	Share of immigrants in total state population (%)	1,474	4.2	3.8	0.4	24.4
Governor	= 1 if the governor is Democrat	1,474	0.61	0.49	0	1
Control Democrats	= 1 if Democrats have a majority in both houses of the state legislature	1,474	0.59	0.49	0	1
Control Republicans	= 1 if Republicans have a majority in both houses of the state legislature	1,474	0.22	0.41	0	1
Ideology	Measure of citizen ideology by Berry et al. (1998) (higher values correspond to more liberal positions)	1,474	45.2	16.4	1.0	93.9
Competitiveness	= 1 if the governor receives between 40 and 60 percent of total votes cast in current (upcoming) elections	1,458	0.69	0.46	0	1

Notes: All monetary values are in real terms, measured in year-2010 dollars. See Appendix B.1 for the data sources.

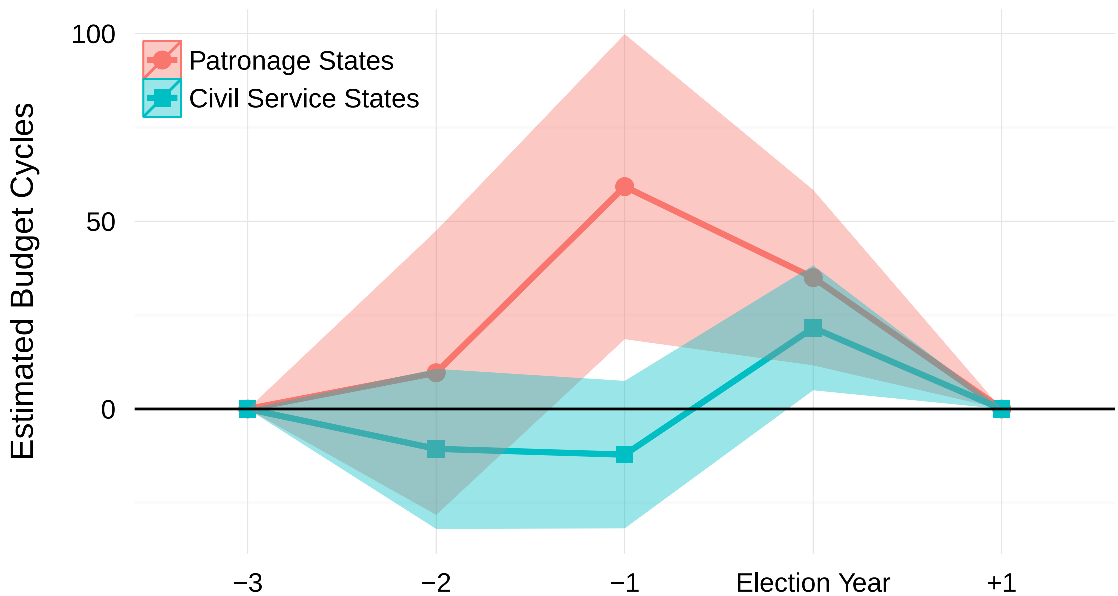
Table 2.3: Direct Expenditures, 1960–1996

Mean $= 2,187$, Std. Dev. $= 798$

	(1)	(2)	(3)	(4)
Election cycle	13.31**	41.67***		
	(5.03)	(12.32)		
Election cycle × Civil		−31.49**		
		(13.19)		
Election year			22.95***	34.98***
			(8.18)	(11.59)
Election year × Civil				−13.37
				(12.55)
Election year −1			−5.05	59.22***
			(7.90)	(20.16)
Election year −1 × Civil				−71.38***
				(25.31)
Election year −2			−8.24	9.65
			(9.42)	(18.78)
Election year −2 × Civil				−20.27
				(22.25)
Civil Service		63.20***		73.61***
		(17.35)		(19.85)
Observations	1,474	1,474	1,474	1,474
States	45	45	45	45
R^2	0.98	0.98	0.98	0.98
F-tests:				
Election cycle in civil service states		10.19**		
		(5.38)		
Election year in civil service states				21.62**
				(8.25)
Election year −1 in civil service states				−12.16
				(9.74)
Election year −2 in civil service states				−10.62
				(9.74)

Notes: The dependent variable in all four columns is total direct expenditures in real 2010 dollar terms. Direct expenditures are defined as total expenditures less intergovernmental expenditures (funds that state governments transfer to lower level governments such as counties, townships, etc.). Heteroskedasticity-robust standard errors are clustered at the state level and reported in parentheses. *p<.1, **p<.05, ***p<.01.

Figure 2.3: Cycles in Direct Expenditures in Civil Service vs. Patronage States



Notes: This figure is a graphical illustration of the estimated results from Column (4) of Table 3.3. The estimated cycles for each year over a four-year electoral term are obtained from a regression of direct expenditures against the civil service variable, indicator variables for pre-pre-election, pre-election, and election years (post-election year being the omitted category), and interaction terms of each of these indicator variables with the civil service variable, as well as additional controls (listed in Table 3.3). The shaded and dashed-line bands around the estimated effects represent 95 percent confidence intervals for the cycles in patronage and civil service states respectively. The figure shows significant cycles in states with the patronage system during the pre-election and election years, and no cycles in states with the civil service system (except a relatively small cycle in the election year).

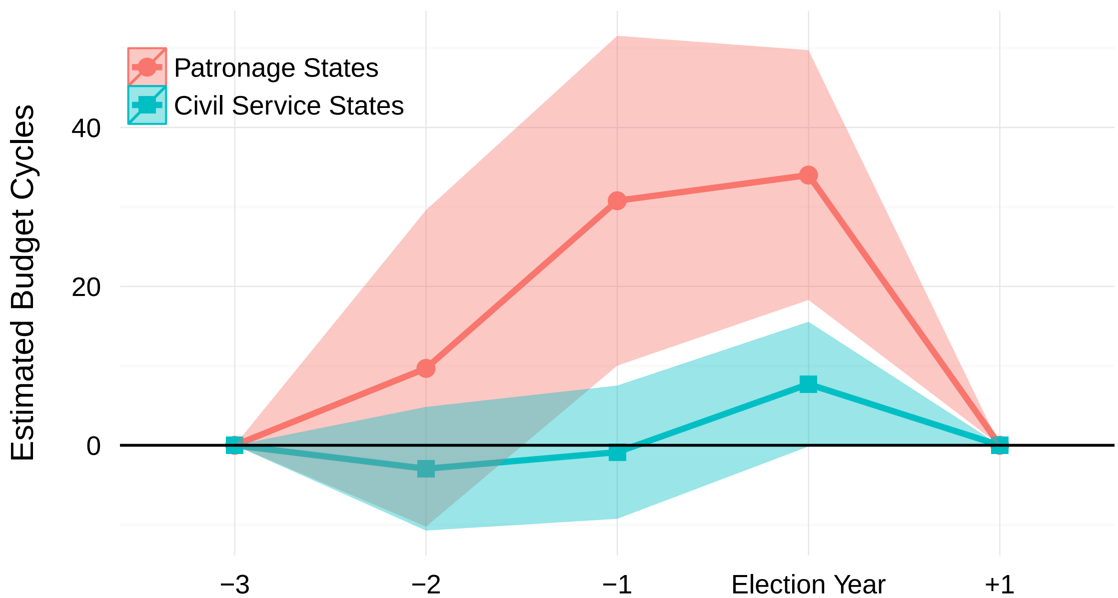
Table 2.4: Direct Highway Expenditures, 1960–1996

Mean = 324, Std. Dev. = 164

	(1)	(2)	(3)	(4)
Election cycle	7.25**	27.48***		
	(3.02)	(7.79)		
Election cycle × Civil		−22.53***		
		(8.13)		
Election year			10.39**	34.02***
			(4.00)	(7.80)
Election year × Civil				−26.32***
				(7.30)
Election year −1			2.34	30.78***
			(4.06)	(10.30)
Election year −1 × Civil				−31.65***
				(10.69)
Election year −2			−1.73	9.68
			(3.63)	(9.88)
Election year −2 × Civil				−12.64
				(10.25)
Civil Service		10.90		17.61**
		(9.92)		(7.75)
Observations	1,474	1,474	1,474	1,474
States	45	45	45	45
R^2	0.75	0.76	0.75	0.76
F-tests:				
Election cycle in civil service states		4.95		
		(3.09)		
Election year in civil service states				7.70*
				(3.90)
Election year −1 in civil service states				−0.87
				(4.16)
Election year −2 in civil service states				−2.95
				(4.16)

Notes: The dependent variable in all four columns is direct per capita expenditures on roads and highways in real 2010 dollar terms. Heteroskedasticity-robust standard errors are clustered at the state level and reported in parentheses. *p<.1, **p<.05, ***p<.01.

Figure 2.4: Cycles in Direct Highway Expenditures in Civil Service vs. Patronage States



Notes: This figure is a graphical illustration of the estimated results from Column (4) of Table 2.4. The estimated cycles for each year over a four-year electoral term are obtained from a regression of direct highway expenditures against the civil service variable, indicator variables for pre-pre-election, pre-election, and election years (post-election year being the omitted category), and interaction terms of each of these indicator variables with the civil service variable, as well as additional controls (listed in Table 2.4). The shaded and dashed-line bands around the estimated effects represent 95 percent confidence intervals for the cycles in patronage and civil service states respectively. The figure shows significant cycles in patronage states during the pre-election and election years, and no cycles in civil service states in any given year.

Figure 2.5: Cycles in Direct and Highways Expenditures in Average, Civil Service, and Patronage States

Notes: This figure is a graphical illustration of the main results.

Table 2.5: Total and Highway Expenditures, 1960–1996, Arellano-Bond Estimates

	Total Direct Expenditures Mean $= 2,187$, Std. Dev. $= 798$				Direct Highway Expenditures Mean $= 324$, Std. Dev. $= 164$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Election cycle	8.95**	34.50***			6.64**	27.59***		
	(4.09)	(9.51)			(2.77)	(6.91)		
Election cycle × Civil		−28.37***				−23.50***		
		(10.33)				(7.02)		
Election year			16.37***	35.23***			8.39**	32.09***
			(6.26)	(8.29)			(3.63)	(6.75)
Election year × Civil				−21.13**				−26.90***
				(9.33)				(5.97)
Election year −1			−9.84	45.52***			0.83	27.66***
			(6.74)	(15.27)			(3.70)	(9.33)
Election year −1 × Civil				−61.28***				−30.03***
				(19.65)				(9.45)
Election year −2			−11.02	9.92			−3.93	5.16
			(7.19)	(12.68)			(3.12)	(7.87)
Election year −2 × Civil				−23.57				−10.51
				(15.48)				(8.19)
Civil Service		41.63		54.73**		−1.25		4.63
		(25.92)		(24.54)		(15.59)		(13.18)
Observations	1,440	1,440	1,440	1,440	1,440	1,440	1,440	1,440
States	45	45	45	45	45	45	45	45

Notes: All specifications are estimated by the Arellano-Bond method described in Section 2.4.3. The dependent variable in Columns 1–4 is total direct expenditures per capita, and in Columns 5–8—direct per capita expenditures on roads and highways, both measured in real 2010 dollars. Heteroskedasticity-robust standard errors are reported in parentheses. *p<.1, **p<.05, ***p<.01.

Table 2.6: Total and Highway Expenditures, 1960–1996, Controlling for Heterogeneity in Observables

	Total Direct Expenditures Mean $= 2,187$, Std. Dev. $= 798$				Direct Highway Expenditures Mean $= 324$, Std. Dev. $= 164$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Election cycle × Civil	−31.49**	−34.48**			−22.53***	−22.45**		
	(13.19)	(15.59)			(8.13)	(8.64)		
Election year × Civil			−13.37	−15.44			−26.32***	−23.12**
			(12.55)	(20.50)			(7.30)	(9.09)
Election year −1 × Civil			−71.38***	−73.43**			−31.65***	−23.24*
			(25.31)	(33.40)			(10.69)	(13.40)
Election year −2 × Civil			−20.27	−26.44			−12.64	−3.65
			(22.25)	(26.59)			(10.25)	(11.52)
Controlling for Elections × X_{st}		Yes		Yes		Yes		Yes
Observations	1,474	1,474	1,474	1,474	1,474	1,474	1,474	1,474
States	45	45	45	45	45	45	45	45
R^2	0.98	0.98	0.98	0.98	0.75	0.76	0.76	0.76

Notes: The dependent variable in Columns 1–4 is total direct expenditures per capita, and in Columns 5–8—direct per capita expenditures on roads and highways, both measured in real 2010 dollars. Heteroskedasticity-robust standard errors are clustered at the state level and reported in parentheses. In addition to the reported regressors, all specifications include a lag of the dependent variable, state, year, and division-specific year fixed effects, and the variables contained in X_{st}: log state population and its square, real per capita income and its square, fractions of population aged 5–17 and aged 65 and over, share of immigrants and urbanized population, dummy variables indicating the control of state legislatures by Democrats and by Republicans, partisan affiliation of the governor, and a citizen ideology measure (Berry et al., 1998). *p<.1, **p<.05, ***p<.01.

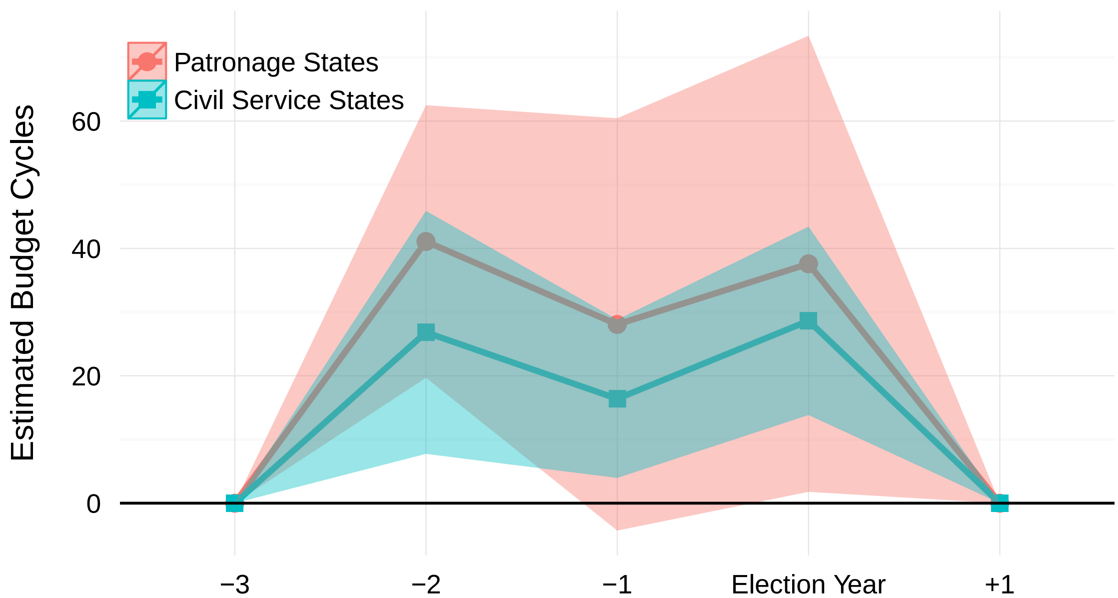
Table 2.7: Total Tax Revenues, 1960–1996

Mean $= 1,562$, Std. Dev. $= 508$

	(1)	(2)	(3)	(4)
Election cycle	9.14**	11.62		
	(3.87)	(10.49)		
Election cycle × Civil		−2.66		
		(11.81)		
Election year			29.48***	37.59**
			(6.90)	(17.77)
Election year × Civil				−8.95
				(18.06)
Election year −1			17.66***	28.07*
			(5.41)	(16.06)
Election year −1 × Civil				−11.68
				(18.52)
Election year −2			28.49***	41.10***
			(8.07)	(10.62)
Election year −2 × Civil				−14.27
				(15.41)
Civil Service		40.31**		47.49**
		(16.86)		(20.79)
Observations	1,474	1,474	1,474	1,474
States	45	45	45	45
R^2	0.97	0.97	0.97	0.98
F-tests:				
Election cycle in civil service states		8.96**		
		(4.24)		
Election year in civil service states				28.64***
				(7.34)
Election year −1 in civil service states				16.39**
				(6.17)
Election year −2 in civil service states				26.83***
				(6.17)

Notes: The dependent variable in all four columns is total tax revenues in real 2010 dollar terms. Heteroskedasticity-robust standard errors are clustered at the state level and reported in parentheses. *p<.1, **p<.05, ***p<.01.

Figure 2.6: Cycles in Total Tax Revenues in Civil Service vs. Patronage States



Notes: This figure is a graphical illustration of the estimated results from Column (4) of Table 2.7. The estimated cycles for each year over a four-year electoral term are obtained from a regression of total tax revenues against the civil service variable, indicator variables for pre-pre-election, pre-election, and election years (post-election year being the omitted category), and interaction terms of each of these indicator variables with the civil service variable, as well as additional controls (listed in Table 2.7). The shaded and dashed-line bands around the estimated effects represent 95 percent confidence intervals for the cycles in patronage and civil service states respectively. The figure shows that states with or without civil service tend to collect more in tax revenues throughout the electoral cycles compared to the post-election year.

Table 2.8: No Heterogeneity in "Prerogatives" of Local Governments

	Schools Mean = 309 Std. Dev. = 360		Police Mean = 27 Std. Dev. = 14	
	(1)	(2)	(3)	(4)
Election cycle	0.22		−0.18	
	(6.39)		(0.29)	
Election cycle × Civil	−0.08		0.22	
	(6.83)		(0.31)	
Election year		−9.77		0.49
		(6.55)		(0.61)
Election year × Civil		−0.19		−0.07
		(6.69)		(0.67)
Election year −1		3.33		0.14
		(4.51)		(0.49)
Election year −1 × Civil		1.75		−0.24
		(6.05)		(0.48)
Election year −2		−7.37		0.96
		(10.56)		(0.70)
Election year −2 × Civil		2.20		−0.74
		(10.62)		(0.72)
Civil Service	10.09	8.97	−0.16	0.23
	(7.40)	(8.28)	(0.83)	(1.01)
Observations	1,474	1,474	1,474	1,474
States	45	45	45	45
R^2	0.98	0.98	0.90	0.90

Notes: The dependent variable in Columns 1–2 is total per capita expenditures on elementary and secondary education, and in Columns 3–4—total per capita expenditures on police protection, both measured in real 2010 dollar terms. Heteroskedasticity-robust standard errors are clustered at the state level and reported in parentheses. In addition to the reported regressors, all specifications include a lag of the dependent variable, state, year, and division-specific year fixed effects, log state population and its square, real per capita income and its square, fractions of population aged 5–17 and aged 65 and over, share of immigrants and urbanized population, dummy variables indicating the control of state legislatures by Democrats and by Republicans, partisan affiliation of the governor, and a citizen ideology measure (Berry et al., 1998). *p<.1, **p<.05, ***p<.01.

Table 2.9: Expenditures in Competitive vs. Noncompetitive Elections, 1960–1996

	Direct Expenditures Mean $= 2,196$ Std. Dev. $= 798$	Highway Expenditures Mean $= 324$ Std. Dev. $= 164$
	(1)	(2)
Competitive election cycle	54.95***	35.64***
	(14.27)	(10.21)
Competitive election cycle × Civil	−50.27***	−34.18***
	(15.95)	(10.46)
Noncompetitive election cycle	−3.95	3.77
	(18.03)	(16.42)
Noncompetitive election cycle × Civil	25.56	6.65
	(22.58)	(16.39)
Competitiveness	−26.87	−3.35
	(24.07)	(20.14)
Civil Service	46.26*	8.47
	(23.59)	(21.37)
Competitiveness × Civil	26.72	4.78
	(27.32)	(21.03)
Observations	1,458	1,458
States	45	45
R^2	0.98	0.76

Notes: The dependent variable in both columns is total direct expenditures (total expenditures minus intergovernmental expenditures) in real 2010 dollar terms. Heteroskedasticity-robust standard errors are clustered at the state level and reported in parentheses. In addition to the reported regressors, all specifications include a lag of the dependent variable, state, year, and division-specific year fixed effects, log state population and its square, real per capita income and its square, fractions of population aged 5–17 and aged 65 and over, share of immigrants and urbanized population, dummy variables indicating the control of state legislatures by Democrats and by Republicans, partisan affiliation of the governor, and a citizen ideology measure (Berry et al., 1998). *p<.1, **p<.05, ***p<.01.

Chapter 3

The Effect of Culture on Economic Behavior: Experimental Evidence on Individualism-Collectivism

3.1 Introduction

Does culture affect economic well-being? There are numerous theoretical arguments about why "culture matters" (Guiso, Sapienza and Zingales, 2006; Fernández, 2011).[1] Most of the empirical evidence on the subject, however, is only descriptive or hard to interpret causally. One reason for cultural variables to be endogenous to economic outcomes can

[1] A common interpretation of "culture" in this literature is the set of beliefs, values, and norms, shared by members of a particular social group or society, and transmitted from generation to generation. Acemoglu, Johnson and Robinson (2005) discuss the relative importance of culture, institutions, and geography in explaining economic development across the world. Postlewaite (2011) discusses how social norms can be integrated into economic models.

be reverse causality. According to the "modernization theory," for example, economic development can cause trusting and participatory cultural values to replace traditional norms (Inglehart and Baker, 2000). A second and the most common concern for endogeneity arises because of omitted variables. Many unobserved or hard-to-measure factors, such as the quality of political institutions, are likely to be correlated with cultural values and also directly influence economic outcomes, thus confounding the causal effect of culture (e.g., Tabellini, 2008).[2]

In the present paper, I use an experimental method to address the aforementioned endogeneity concerns and study the causal role of individualism-collectivism in explaining economic behavior at the individual level. Individualism-collectivism (henceforth I-C) is one specific dimension of culture, but it is considered to be a useful descriptor of national cultures broadly defined (Gudykunst, Ting-Toomey and Chua, 1988; Triandis et al., 1988; Triandis, 1995). According to Hofstede (1980, 2001), I-C measures the extent to which individuals "are expected to take care of themselves and their immediate families only" (individualism), or "expect their relatives or members of a particular ingroup to look after them in exchange for unquestioning loyalty" (collectivism). At the aggregate level, a group can be said to be "individualistic" or "collectivistic" depending on the prevalence of corresponding beliefs within the given group. Individualistic societies (or individuals) value highly personal freedom, achievement, and the self-image of "I," the individual, while collectivistic societies (or individuals) place greater weight on conformity, harmony, and the self-image of "we," the group.

[2]For a review of why cultural values are endogenous to economic institutions, see Bowles (1998).

Hofstede's (1980) early work on individualism-collectivism found strong positive correlation between individualism and economic development across countries. The richest nations in his sample, such as the U.S. and Australia, were found to be also the most individualistic. The poorest ones, such as Guatemala and Bangladesh, were found to be the most collectivistic. The positive correlation between individualism and economic well-being has since garnered attention of many social scientists, including economists.[3] In an influential study, Greif (1994) has emphasized historical importance of individualist and collectivist beliefs in determining institutional organization of societies. More recently, Gorodnichenko and Roland (2013) have argued that individualism fosters innovation and consequently economic growth, providing cross-country empirical evidence to support their hypothesis. Fogli and Veldkamp (2012) have shown how I-C can affect economic development via technology diffusion.[4] The observational, non-experimental nature of the I-C data used in these studies, however, makes it challenging to interpret the evidence causally.

In this paper, I take a different, experimental approach to more directly address endogeneity of individualism-collectivism. In particular, I exogenously vary the salience of I-C in the laboratory to determine its causal effect on economic behavior at the individual level. Creating exogenous variation in the salience of I-C is achieved by the "priming" technique

[3]Politicians and political commentators have also expressed strong opinions about individualism. For example, Paul Ryan—a Republican vice presidential candidate in the 2012 U.S. presidential elections—has called individualism a "basic virtue of capitalism." During the 2012 election campaign, Ryan said: "Every fight we are involved in here, on Capitol Hill, ... is a fight that usually comes down to one conflict: individualism versus collectivism" (Atlas Society, 2012). In contrast, David Brooks (2013) has argued that individualism might be "the root of [today's socioeconomic] problem[s]," as overemphasizing individualistic values comes at the expense of sacrificing valuable community bonds.

[4]See also Licht, Goldschmidt and Schwartz (2007), Davis (2011), and Maseland (2013) for similar contributions.

commonly used in social psychology. The idea of priming is based on the "category accessibility theory," according to which "primes" temporarily make mental representations salient that then serve as interpretive frames in the processing of subsequent information (Higgins, Rholes and Jones, 1977; Srull and Wyer, 1979; Bargh and Chartrand, 2000; Bargh, 2006). That is, priming a subject with a certain concept influences the subject's decisions that she makes immediately after receiving the prime. By randomly assigning one group of subjects to the Prime A condition and the other group to the Prime B condition, an experimenter can identify the marginal effect of Prime A (relative to Prime B) on the outcomes of interest elicited right after priming the subjects. Recent examples of the use of priming in economics include Benjamin, Choi and Strickland (2010), who study the effects of ethnic, racial, and gender norms on American undergraduates' risk and time preferences, and Liu, Meng and Wang (2014), who investigate how Confucianism affects Chinese and Taiwanese subjects' risk aversion, loss aversion, impatience and trustworthiness.[5]

In contrast to the psychology literature on individualism-collectivism, I study the effect of the I-C salience on individual preferences for risk and time, and self-confidence. Social psychologists typically study the impact of the I-C salience on psychological value types (self-direction, achievement, power, etc.), self-concepts, and cognitive styles of individuals (Oyserman, Coon and Kemmelmeier, 2002; Oyserman and Lee, 2007; Oyserman and Lee, 2008). See also Mandel (2003), who finds that individuals whose independent selves are made salient take more risks, as measured by their responses to hypothetical

[5]See also Chen and Li (2009) and Chen and Chen (2011), who study the effect of group identity on social preferences and on efficiency in coordination games. Hoff and Pandey (2012) examine the effect of caste salience on cognitive ability in the Indian context. Benjamin, Choi and Fisher (2013) show how religious identity salience can affect economic behavior.

financial choice questions. An important difference is that I obtain incentive-compatible measures of economically important preference parameters as the outcome variables. I follow the common practice in experimental economics and use a multiple-price-list design with monetary payments to incentive-compatibly elicit subjects' risk aversion, time preferences, and self-confidence measures (Holt and Laury, 2002; Andersen et al., 2008; Andreoni and Sprenger, 2012*a*; Moore and Healy, 2008).

Empirical evidence suggests that risk and time preferences influence many individual-level outcomes such as income growth (Shaw, 1996), savings (Ashraf, Karlan and Yin, 2006), smoking behavior (Chabris et al., 2008), migration (Jaeger et al., 2010), and technology adoption (Liu, 2013). Yet these preferences have usually been taken as "given" and the question of how risk and time attitudes are determined has not received as much attention.[6] In addition to risk aversion and impatience, I measure the effect of I-C on self-confidence and overconfidence (or excessive self-confidence). Overconfidence has been argued to be an important determinant of an array of economic variables such as entry into competitive markets (Camerer and Lovallo, 1999), speculative trading (Scheinkman and Xiong, 2003), and corporate investment (Malmendier and Tate, 2005).[7] Showing how risk and time attitudes and confidence are affected by individualism-collectivism is therefore important to study the implications of such heterogeneity in a wide range of applications.

[6]Several studies have documented large individual heterogeneity in risk and time attitudes. Examples include Dohmen et al. (2011) and Cohen and Einav (2007) for risk preferences, Harrison, Lau and Williams (2002) and Laajaj (2012) for time preferences, and Dohmen et al. (2010) and Tanaka, Camerer and Nguyen (2010) for both.

[7]See also Biais, Hilton and Mazupier (2005) and Galasso and Simcoe (2011) who study the link between miscalibration (a form of overconfidence) and trading performance, and overoptimism and innovation, respectively. Compte and Postlewaite (2004) study welfare implications of overconfidence. Bénabou and Tirole (2002) and Köszegi (2006) give theoretical explanations for why overconfidence arises. Moore and Healy (2008) and Möbius et al. (2011) provide methodological and empirical contributions for properly defining and measuring overconfidence.

It is also important to pinpoint specific mechanisms through which I-C can affect economic outcomes.

My results show that priming subjects on collectivism causes them to make less risky and more patient financial choices and report lower self-confidence than subjects primed on individualism. The magnitude of the treatment effects—between 0.25 and 0.30 standard deviation of the dependent variable in the case of risk aversion—is sizable. For a comparison consider, for example, the findings of Hryshko, Luengo-Prado and Sørensen (2011). The authors show that having a parent who has obtained at least high school level education decreases the child's probability of being "very risk averse" in adulthood by 0.66 standard deviations (relative to the children whose parents did not receive high school level education).

Motivated by the "social identity" theory (Tajfel and Turner, 1979), whereby people derive a sense of identity from belonging to social groups, I then examine racial heterogeneity in the treatment effects of individualism-collectivism.[8] I show that receiving the collectivism prime causes blacks to make more patient intertemporal choices, having the opposite effect on whites. This is in line with the findings of Benjamin, Choi and Strickland (2010), who demonstrate that making the black identity salient to black subjects causes them to make more patient choices. I also find that priming blacks on collectivism makes them significantly less confident than other subjects. Hispanics, in contrast, are disproportionately more likely than non-Hispanics to choose less risky financial options after receiving the collectivism prime.

[8]Akerlof and Kranton (2000, 2005) develop an economic model of social identity and demonstrate its relevance in explaining a wide range of economic applications. See also Chen and Li (2009); Benjamin, Choi and Strickland (2010); Bénabou and Tirole (2011).

Finally, I hypothesize the priming effects of I-C to be more pronounced in subjects with average baseline levels of individualism-collectivism and more muted in subjects with extreme baseline levels of I-C. The intuition behind this hypothesis is that manipulating the salience of individualism or collectivism in an extremely individualistic or collectivistic person is harder than in a person with average baseline values of I-C. My findings suggest that Asians and whites are least sensitive to being primed on collectivism, while Hispanics and blacks are the most sensitive. This is consistent with the existing evidence that Asians are, on average, most collectivistic, whereas European Americans are the least collectivistic (Marín and Triandis, 1985; Coon and Kemmelmeier, 2001). In my experimental sample too, Asian and white subjects are the most and least group-oriented, respectively. Not surprisingly, then, priming subjects on a cultural value which they already have extreme baseline levels of does not make much difference.

The rest of the paper is organized as follows. Section 3.2 outlines the hypotheses about why one would expect I-C to influence risk and time preferences and self-confidence. (In Appendix B.1, I also present a simple theoretical framework clarifying the mechanism through which priming subjects on individualism-collectivism allows the researcher to detect treatment effects of I-C.) Sections 3.3 and 3.5 describe the experiment and the outcome variables. Section 3.6 outlines the empirical strategy, whereas Section 3.7 presents the results of the paper. Section 3.8 concludes.

3.2 Hypothesized Links between I-C and Economic Behavior

Building on the existing research on individualism-collectivism in the sociology and cultural psychology literature, I put forward hypotheses about how I-C might affect risk attitudes, time preferences and self-confidence.

H1. Collectivism is associated with lower risk aversion than individualism. Collectivism—as opposed to individualism—entails a norm of being tightly connected with the ingroup. Making collectivism salient to an individual would thus activate the individual's memory of such association with her ingroup. Because the ingroup can serve the individual as a safety net in case of a financial loss, the individual would be more willing to take financial risks when collectivism is made salient to her, than when it is not. This is referred to as "cushion hypothesis" (Hsee and Weber, 1999; Barr and Genicot, 2008).

H2. On the other hand, however, collectivism can be associated with higher risk aversion than individualism. Since collectivism entails being a part of a tight ingroup, an individual associated with such group will be more likely to conform to the group's social norms than a person not affiliated with a tight ingroup (Ybarra and Trafimow, 1998). Conforming to the norms, in turn, requires restraining the self in different situations (Markus and Kitayama, 1991), including when making a financial decision. Thus, making collectivism salient to an individual activates the individual's memory of her ingroup's norms, potentially making her more cautious and less willing to take financial risks than when individualism is made salient.

H3. Collectivism is associated with lower discount rate (more patience) than individualism. Collectivism—as opposed to individualism—also entails a norm of harmonious interdependence with the ingroup and restraining self, both of which require patience (discounting the future less) (Markus and Kitayama, 1991). The individual would thus tend to be more patient when collectivism is made salient to her, than when it is not.

H4. Individualism promotes self-confidence. This hypothesis is based on the evidence that individualistic societies place a premium on the individual whose self-esteem is enhanced when she sees herself as distinct or better than others (Singelis et al., 1999). Cultivating self-esteem, in turn, instills high self-confidence (Bénabou and Tirole, 2002). In contrast, collectivist values promote interpersonal harmony which is enhanced by self-effacement, not by self-confidence (Triandis, 2001). This follows from the fact that in collectivist cultures self-esteem is facilitated by adherence to the norm that one should "fit in"; that a person view him- or herself as having competence levels that are representative of the collective, not higher (Markus and Kitayama, 1991).

3.3 On-Campus Experiment

3.3.1 Procedures

Participants were 73 students attending the University of Houston—a large public university in Texas with a diverse international student body.[9] The experiment was run throughout January and February 2013 in 11 separate sessions, with a median of eight subjects

[9]The total enrollment as of 2013 was $39,540$ students. *Source:* `http://www.uh.edu/about/uh-glance/facts-figures` (accessed October 14, 2014).

per session. In each session subjects were seated in a large room with 40 seats (the same room was used for all sessions), ensuring that there was enough space between them to be unable to communicate during the experiment. Participants were instructed of the general experimental guidelines, practiced examples of experimental tasks, and were then asked to complete the actual experimental questionnaire in a paper-and-pencil format.[10]

The average subject took half an hour to complete her participation in the experiment, earning 23 dollars. These earnings included a fixed participation fee of 10 dollars. I used the random decision selection mechanism—paying the subject for one random decision-that-counts out of 30 choices they made—to determine each subject's earnings in addition to the participation fee (Azrieli, Chambers and Healy, 2012). All payments were made by a personal check given to the participant immediately following the experiment. Some decisions required a later payment. In such cases, the payment was implemented by giving the subject a post-dated check (Andersen et al., 2008), having reminded the subjects that they could cash a post-dated check only on or after the date on the check.

The experiment followed a simple between-subject design with two treatment groups. 44 out of 73 subjects were randomly assigned to the group receiving a collectivism prime. The other 29 subjects received an individualism prime.[11] After the priming stage, all subjects completed tasks that elicited their preferences for risk and time, and self-confidence.

[10]The experiment was conducted in full compliance with the University of Houston Committee for the Protection of Human Subjects (the local institutional review board). The script of instructions, experimental protocol, details about the experimental site (e.g., photos of the room), recruiting information and the IRB approval are all available from the author upon request.

[11]The reason for the sizes of the treatment groups to differ from 50 percent is as follows. Throughout the experiment, the two treatments were implemented in batches of sessions. E.g., the first two sessions were given the collectivism prime, the next two—the individualism prime, and so on. Because of unanticipated logistic reasons, however, some of the planned sessions were not possible to implement, hence the 50 percent balance between the treatment groups was not achieved.

Finally, subjects filled out a background questionnaire, providing information on their demographic characteristics.

3.3.2 Priming Instrument

Among the various methods of priming individualism-collectivism in social psychology, such as the "Sumerian warrior" (Trafimow, Triandis and Goto, 1991) and "scrambled sentence" (Srull and Wyer, 1979) tasks, pronoun circling (Brewer and Gardner, 1996; Gardner, Gabriel and Lee, 1999) has been particularly common (see Oyserman, Coon and Kemmelmeier, 2002 and Oyserman and Lee, 2008 for reviews). Its appealing feature is that there is only a slight yet straightforward difference between the individualism and collectivism versions of the task. The simple dichotomy allows the researcher to be precise about what cues the two versions elicit in subjects' minds.[12]

The task asks subjects to circle all personal pronouns, a total of 19, in the following paragraph:

> We go to the city often. Our anticipation fills us as we see the skyscrapers come into view. We allow ourselves to explore every corner, never letting an attraction escape us. Our voices fill the air and street. We see all the sights, we window shop, and everywhere we go we see our reflection looking back at us in the glass of a hundred windows. At nightfall we linger, our time in the city is almost over. When finally we must leave, we do so knowing that we

[12]The pronoun circling task has been commonly used by others in the psychology literature and has been validated in a number of experiments (e.g., Künen and Oyserman, 2002; Haberstroh et al., 2002). I still take specific steps to check the validity of the priming instrument. Please see Appendix B.2 for more details.

> will soon return. The city belongs to us.

This is the collectivist version of the priming instrument. The individualist version is the same as the above paragraph except for all plural personal pronouns replaced by the corresponding singular forms (*I* instead of *we*, *me* instead of *us*, etc.):

> I go to the city often. My anticipation fills me as I see the skyscrapers come into view. I allow myself to explore every corner, never letting an attraction escape me. My voice fills the air and street. I see all the sights, I window shop, and everywhere I go I see my reflection looking back at me in the glass of a hundred windows. At nightfall I linger, my time in the city is almost over. When finally I must leave, I do so knowing that I will soon return. The city belongs to me.

3.4 MTurk Experiment

Amazon Mechanical Turk, MTurk for short, is a large online marketplace where workers complete Human Intelligence Tasks (HITs) in exchange for small fees, typically as a means to supplement their incomes from other more conventional sources. Previous research has found that the MTurk worker population within U.S. is quite representative of the U.S. population.

I recruited 208 workers via MTurk throughout March 2015. The workers were asked to take a carefully designed Qualtrics survey in exchange for a fee. The fee was calibrated to at least match the prevailing effective hourly wage rate of $4.70 at MTurk. An average subject took 16 minutes to complete the survey and earned an equivalent of \$5.7 an hour (the pay consisted of a fixed \$0.75 payment and a variable payment averaging to \$0.80).

One of the advantages of collecting data via MTurk is that the demographic profile of the subject pool is considerably more representative of the general U.S. population than subject pools from typical lab experiments (Table 3.13). Geographic distribuation across U.S. is also fairly even.

3.4.1 Priming Instrument

I used a modified version of the pronounce circling task from the UH experiment. The paragraph was extended from 96 words to 204 words, and included increase in the number of personal pronouns from 19 to 32. The modified version of the priming paragraph is given below.

> We go to the city often. Every time we go there, we plan our trip carefully. If there is a football game in the city at the time of our visit, we reserve the tickets in advance. The night before the trip, we are so excited we can barely sleep. Next morning we have breakfast together and head to the city right away. The closer we get there, the merrier we become. As we see the skyscrapers come into view, our anticipation fills us completely. We allow ourselves to explore every corner, never letting an attraction escape us. One of the first things we do is visit the museum of fine arts. We then see all the other sights, we window shop, and everywhere we go we see our reflection looking back at us in the glass of a hundred windows. If there is no football game in the city that day,

> there is the thrill of an amusement park waiting for us instead. In the evening, we always dine at our favorite restaurant. At nightfall we linger, our time in the city almost over. When finally we must leave, we do so knowing that we will soon return. The city belongs to us.

This is the collectivist version of the priming instrument. The individualist version is the same as the above paragraph except for all plural personal pronouns replaced by the corresponding singular forms (*I* instead of *we*, *me* instead of *us*, etc.).

It took an average subject 65 seconds to read the priming paragraph. There were 10 subjects (4.8% out of 208) who spent less than 30 seconds on reading this paragraph. Because it is impossible to read the story in less than 30 seconds even if one reads very fast (without skipping words or sentences), in the subsequent analyses I restrict the sample to the remaining 198 subjects. Including those 10 subject in the analysis weakens the results somewhat, but it is not surprising because such subjects tend to be the ones who intend to complete the survey as fast as possible, not paying attention to the instructions. Indeed, it would be impossible to detect priming effects if subject were effectively not primed by not reading the priming paragraph fully. In later batches of the experiment, I started filtering out subjects spending less than 30 seconds on reading the priming paragraph.[13]

[13] One worker whose submission I didn't approve because he retook the survey after being aborted the first time wrote me the following message: "You're the first out of hundreds that caught me lying to add pennies to my sub-minimum wage earnings. Good work. Most people doing these are inept grad students that couldn't analyse their way out of a paper bag. I'm genuinely impressed."

3.5 Measuring Outcome Variables

3.5.1 Eliciting Risk Aversion

Risk preferences were measured by asking subjects to make 10 binary choices between a sure 10 dollars and a gamble with a 50-50 percent chance of winning a certain amount of money between 15 and 38 dollars or winning nothing. Table 3.1 gives the exact formulation of this "price list."[14]

A typical subject started by choosing a lower but safe payment in the first few options, as the risky alternatives were presumably not attractive enough. Since with each subsequent option the expected payoff of the offered gamble increases, subjects "switched" to accepting the gambles at some point. Note that once such a switch is made, there is no point in "switching back" to the safe option, as the subsequent gambles offer ever higher expected payoffs. Indeed, 69 out of 73 subjects (95 percent) in the UH experiment switched only once (or never). For the remaining four subjects who switched "back and forth," I use the first switching points for imputing their risk preferences. For the MTurk experiment, the survey software was configured to ask subjects to review their answers if they switched back and forth.[15]

Based on the financial choices subjects made in the risk aversion elicitation game, I compute risk premium as the expected return offered by the gamble the subject chooses in excess of the risk-free return in the switching row for that subject. For example, if John

[14] The complete experimental protocols of both UH and MTurk epxeriments are available as an online appendix at: `http://www.uh.edu/~dnbostas/research/online_appendix.pdf`.

[15] Several papers in the literature with similar price list designs also use the same approach. As a robustness check, though, I also analyze the sample without these four subjects.

prefers 22 dollars with a 50/50 chance over 10 dollars guaranteed, but not 20 dollars with the same chance, then he would choose safe options in Questions 1 through 3 (Table 3.1) and switch to choosing the gambles in Questions 4 through 10. John's risk premium thus equals $(11-10)/10 = 0.1$, that is the expected return offered by the gamble John chooses *in excess* of the risk-free return in the switching row. John's risk premium of 0.1 would be interpreted as an upper bound of his true premium. Had his true premium been larger than 0.1, John would not have preferred 11 dollars expected return over 10 dollars sure payoff. John's risk premium could be lower than 0.1, however, because it is impossible to tell whether he would have also chosen, say, 10.5 dollars expected return. The lower bound for John's premium would be zero: if it was lower than zero, he would have switched to choosing the gamble in the previous row.

Only one out of the 73 subjects did not accept any single gamble—not even the one offering nineteen dollars on average as opposed to 10 dollars guaranteed. Her risk premium could therefore, in principle, be infinity, with the lower bound of 0.9. However, given the interval nature of these data, I use appropriate estimation techniques specially designed for interval data to estimate the priming effects on risk (as well as time) preferences.

For the MTurk sample, there is considerable variation in financial choices between a sure payment and a gamble with varying expected outcome. Average Coefficient of Relative Risk Aversion, as imputed from the ten choices subjects make between safe and risky options, is 0.38 and is in the range of estimates obtained in other similar experiments (Table 3.5).

There is also considerable variation in intertemporal choices between immediate payments and payments a week or two later. An average subject makes more than 60 percent out of 14 intertemporal choices in favor of receiving a later higher payment, as opposed to a lower sooner payement.

These two observations imply that a typical subject does not have trust issues with the experimenter (if trust was a problem, most subjects would choose sure payments in the risk game and immediate payments in the intertemporal choice tasks).

3.5.2 Eliciting Time Preferences

Time preferences were measured by asking subjects to make seven binary choices between a certain amount of money received immediately after the experiment and a larger amount received two weeks later, and to make next seven binary choices between a certain amount of money received two weeks after the experiment and a larger amount received four weeks later. Table 3.2 gives the exact formulation of the time preference elicitation task.[16]

Based on the intertemporal choices subjects made in this game, I impute weekly interest rates in a similar way to computing the risk premia. For example, if John prefers 15 dollars in two weeks over 10 dollars today, but does not prefer 15 dollars over 11 or

[16] Note that when subjects earn later payments, they are paid via post-dated checks. It is generally possible to cash a post-dated check earlier than the date on the check. This might be a problem if the subject knows of this possibility and rationally chooses "later" larger payments, still being able to cash at the same time she would have cashed the earlier payment. To account for this issue, one of the exit survey questions explicitly asked subjects whether they were aware of such a possibility in general, and in particular when they were deciding to choose a sooner or later payment in the time preference game. Only three out of 73 subjects (four percent) answered in the affirmative. Even they did not choose later payments in the time game, however. In fact, paying a subject by a post-dated check right after the experiment is less costly and more trustworthy than promising the subject to mail her a check at a certain date in the future, and has been implemented by other experimental economists before (Benjamin, Choi and Strickland, 2010; Andersen et al., 2008; cf. Andreoni and Sprenger, 2012*a*, and Andreoni and Sprenger, 2012*b*).

more dollars today, then he would choose today's options in Questions 11 through 14 (Table 3.2) and switch to choosing the later payment in Questions 15 through 17. John's gross interest rate thus equals $15/10 = 1.5$, that is the minimum gross return that makes John forgo today's payment in exchange for the later payment in the switching row. John's gross interest rate of 1.5 would be interpreted as an upper bound of his true return. Had his true return been larger than 1.5, John would not have preferred 15 dollars in two weeks over 10 dollars today. John's interest rate could be lower than 1.5, however, because it is impossible to tell whether he would have also chosen 15 dollars in two weeks over, say, 10.5 dollars today. The lower bound for John's premium would be $15/11 \approx 1.36$: if it was lower than 1.36, he would have switched to choosing the later payment in the previous row. I repeat these calculations for the choices between two weeks from now and four weeks from now.[17] I then transform the resulting gross returns to minimum continuously compounded weekly interest rates.[18]

In addition to imputing subjects' time preferences measured in terms of weekly interest rates, I also impute parameters for the discount factor and present bias. It is the variation in the starting date of offered intertemporal choices (i.e, today vs. two weeks, then two weeks from today vs. four weeks) that allows for identifying the present bias separately from the discount factor, with certain assumptions on utility's functional form. Appendix B.3 provides the details.

[17] In the empirical section, I report results for the interest rates imputed from the choices between today and two weeks from today. The results for the interest rates imputed from the choices between two weeks from today and four weeks from today are very similar and are available from the author upon request.

[18] $\textit{Continuously compounded weekly interest rate} = \ln(\textit{Gross return over two weeks})/2$

3.5.3 Eliciting Self-confidence

Self-confidence was measured by asking subjects to answer six trivia questions and rate their confidence in the correctness of their answers to each question (Table 3.3). The six answers were part of the 30 decisions every subject had to make, one out of which was always randomly selected as a decision-that-counts for calculating experimental earnings for each subject. Correctly answering a question earned the subject 10 dollars, given that question was chosen as the decision-that-counts.

Despite incentivizing subjects for correctly answering the trivia questions, there was no built-in incentive in the given task for truthfully rating the confidence in the correctness of any given answer. To elicit participant's overconfidence incentive-compatibly, they were additionally asked to make two predictions. The first prediction had to be made about how many questions they thought they had answered correctly—promising them guaranteed five additional dollars in the case of a correct prediction. This mechanism gives subjects explicit incentive to reveal their confidence in the correctness of their answers truthfully. Taking thus elicited belief in own "ability" and subtracting the subjects actual "ability" (actual number of correctly answered questions) then results in a measure of overestimation, which is used as a proxy for overconfidence in the literature (Moore and Healy, 2008).

The second prediction had to be made about how many questions respondents thought the average subject in their experimental session would have answered correctly—again, promising them guaranteed five additional dollars in the case of a correct prediction. The rationale behind eliciting subjects' truthful prior about the average performance is to gauge their subjective perception of themselves *relative to* the average subject. The prediction results in a truthfully elicited belief of a given subject in others' "ability." This is a useful piece of information: subtracting this belief from the belief in own "ability" (the first prediction) results in the subject's belief in her "relative ability." But the experimenter also observes the subject's actual "relative ability"—difference between her actual number of correct answers and the average subject's number of correct answers. Adjusting the belief in relative ability by subtracting actual relative ability from it results in a measure of overplacement, which is yet another useful proxy for overconfidence.

For the MTurk sample, an average subject is confident 81 percent of the time (on a scale of 0 to 100), as imputed from six self-rated confidence levels (0 "no clue" 1 2 3 4 "I am sure") in the answers to six trivia questions. When average confidence is adjusted for the number of correct numbers, one obtains a measure of overconfidence, normalized so that 0 corresponds to no over- or under-confidence, positive values indicate overconfidence, and negative—underconfidence. An average subject is overconfident in the sense that his average rating in the correctness of his answers exceeds the actual number of correct answers.

To obtain incentive-compatible measures of confidence and overconfidence, subjects were also asked to predict the number of correct answers they think they got right, promising them a bonus for making the correct prediction. An average subject thinks he will get 4.3 (out of 6) answers right, but actually gets 2.8 correct. Thus, overestimation—an incentive-compatible version of overconfidence—is also positive on average.

3.6 Empirical Strategy

3.6.1 Data

The outcome variables—risk premium, one of the weekly interest rates and the present bias parameter, and three variations of overconfidence—are summarized in Figure 3.3 and Figure 3.2. Figure 3.3 plots the distribution of these outcomes by the treatment status, while Tables 3.4 and 3.5 contains descriptive statistics. Note that racial makeup of the UH experimental pool is evenly distributed among Asians, Hispanics, blacks and whites, while the MTurk pool is dominated by whites. Subjects in the UH experiment are mostly undergraduate students, 21 years old on average, significant part being born outside the U.S. Subjects in the MTurk experiment are 39 years old on average, tend to be slighlty worse-off than the U.S. population, but otherwise very similar (Table 3.13). Importantly, subjects in the collectivism and individualism prime conditions look statistically similar. In this sense, randomization seems proper. Please see Table 3.6 for the randomization check.

3.6.2 Econometric Specifications

The purpose of the experimental design is to answer the question: "What is the causal impact of the salience of collectivism on risk aversion, impatience, and self-confidence?" One can answer this question by comparing average outcomes across the treatment and control groups. The average treatment effect of the salience of collectivism on risk aversion is given by coefficient β from the following ordinary least squares regression:

$$y_i = \alpha + \beta c_i + X_i'\delta + \varepsilon_i, \tag{3.1}$$

where y_i is the outcome variable of interest (such as risk premium) of subject i; c_i is an indicator variable that takes the value of 1 if subject i has received a collectivism prime (subjects receiving an individualism prime serve as a comparison group). X_i is a set of subject i's individual characteristics such as gender and age. ε_i is a stochastic error term. The parameter of interest is β. It reflects the treatment effect of the collectivism prime, relative to the group receiving the individualism prime.

One can test various hypothesis based on the suggested specification. Testing, for example, the null that $\beta < 0$ directly tests the so-called "cushion hypothesis" (Hsee and Weber, 1999), whereby collectivism makes people less risk averse. Rejecting the null of $\beta < 0$ (when y is risk premium) would refute this hypothesis, favoring the *H2* hypothesis instead (discussed in Section 3.2).

When testing for heterogeneity in the treatment effects of I-C across racial groups, I estimate the following specification:

$$y_i = \alpha + \beta_1 c_i + \beta_2 (c_i \times race_i) + \gamma race_i + X_i'\delta + \varepsilon_i, \tag{3.2}$$

where $race_i$ is an indicator variable for subject i's self-reported racial category (black, white, Asian, Hispanic, or Other). The coefficients of interest here are β_1 and β_2. β_1 alone represents the treatment effect of collectivism prime on subjects of the omitted racial category, that is, whites. β_2 shows the deviation between the collectivism prime effects on subject i's race versus others. The total effect of collectivism prime on subject i' own racial category is given by $\beta_1 + \beta_2$.

In all specifications I calculate heteroskedasticity-robust standard errors clustered at the session (batch) level.

3.7 Results

3.7.1 Main Results

The main results show the priming effect of collectivism on subject's risk and time preferences, and confidence measures. I find that priming subjects on collectivism leads them to make safer financial choices compared to the subjects primed on individualism. Figures 3.3 and 3.2 illustrate this result. It plots kernel densities of all six dependent variables for subjects primed on collectivism and subjects primed on individualism. The upper-left panel of the graph shows that most subjects primed on individualism switch to choosing the gamble instead of a safe option at or just after the point where the expected payoff from the offered gamble equals the safe payoff. Subjects primed on collectivism, in contrast, are slower to accept gambles—they require higher expected payoffs to forgo the safe option.

The results of the estimated ordinary least squares regressions are given in Table 3.7 for the UH experiment and Table 3.8 for the MTurk experiment. The first column is the most parsimonious specification with the treatment status as the only independent variable. The second column estimates the same relationship as the first, but adds a set of control variables. The results are imprecisely estimated, but the magnitude is size and comparable the effects on other outcomes. An economic interpretation of the main coefficient of interest, 0.09 from Column 2, Table 8, for example , is that priming an individual on collectivism causes the person to require 0.09 higher CRRA, equivalent to an increase in CRRA by 0.29 standard deviations. For the economic interpretation of this magnitude, compare it, for example, to the causal effect of parental education on the child's probability of being "very risk averse" in adulthood, which has been estimated by Hryshko, Luengo-Prado and Sørensen (2011) to be 0.66 standard deviations.

Main resutls from the MTurk experiment are consistent with my hypotheses:

1. Collectivism is associated with more risk aversion (individualism with more risk taking);

2. Collectivism is associated with more patience (individualism with more impatience); no clear hypothesis about present bias;

3. Collectivism is associated with less self-confidence (individualism with more confidence), but not necessarily with less overconfidence.

3.7.2 Additional Results

According to the "social identity" theory (Tajfel and Turner, 1979), people derive a sense of self-esteem from belonging to social groups. Akerlof and Kranton (2000, 2005) develop an economic model of social identity and show its relevance in explaining various economic applications (see also Bénabou and Tirole, 2011). Benjamin, Choi and Strickland (2010) show that making racial identities salient to subjects of different races has a direct impact on their risk and time preferences. For example, blacks make more patient financial choices after receiving the black identity prime (cf. Chen and Li, 2009). In a similar spirit, I ask whether the treatment effects of individualism-collectivism are heterogeneous across racial groups.

I put forward a general hypothesis that the priming effects of I-C will be more pronounced in subjects with average baseline levels of individualism-collectivism, and more muted in subjects with extreme baseline levels of I-C. The idea is that manipulating the salience of individualism or collectivism in an extremely individualistic or collectivistic person is harder than in a person with average baseline values of I-C. Thus, priming subjects on a cultural value which they already have extreme baseline levels of should not make much difference in their resulting behavior. Appendix B.1 contains more details.

The results from the UH experiment are presented in Tables 3.9, 3.10 and 3.11. The results from the MTurk experiment are imprecise and are not reported.[19] Table 3.9 shows how the priming effect of collectivism in risk premium is different across racial groups that exhibit different levels of risk aversion to begin with. Columns 3 through 7 show that Hispanics are particularly sensitive to being primed on collectivism: the treatment effect of collectivism prime on their risk aversion is the biggest compared to other racial groups. The magnitude of the effect is 1/2 of a standard deviation of risk premium. On the other hand, Asians and whites do not react to priming, an observation that is consistent with the existing evidence that Asians and whites are, on average, the least and the most individualistic, respectively (Marín and Triandis, 1985; Coon and Kemmelmeier, 2001). Arguably, Asians and whites are "saturated" with high baseline degrees of collectivism

[19] Given that the racial make-up of the MTurk experimental pool is very uneven, it is not surprising to produce large standard errors in the estimation.

and individualism, so there is “no room” left for making them even more collectivistic or individualistic via priming.

Table 3.10 shows heterogeneity in the treatment effects of collectivism prime on time preferences across racial groups. At the baseline, blacks seem to be the least patient (Column 1), but as Columns 3–7 suggest, collectivism causes blacks to make significantly more patient intertemporal choices, having the opposite, even larger effect on whites. The two effects are large—about the size of a standard deviation of the log weekly interest rate between an immediate and a two-weeks-later payment. These findings are in line with the findings of Benjamin, Choi and Strickland (2010), who demonstrate that making the black identity salient to black subjects causes them to make more patient choices.

Table 3.11 demonstrates racial heterogeneity in the treatment effects of collectivism prime on overconfidence. Columns 3–7 suggest that collectivism causes blacks to rate overconfidence at significantly lower levels—by a full standard deviation lower than non-blacks.

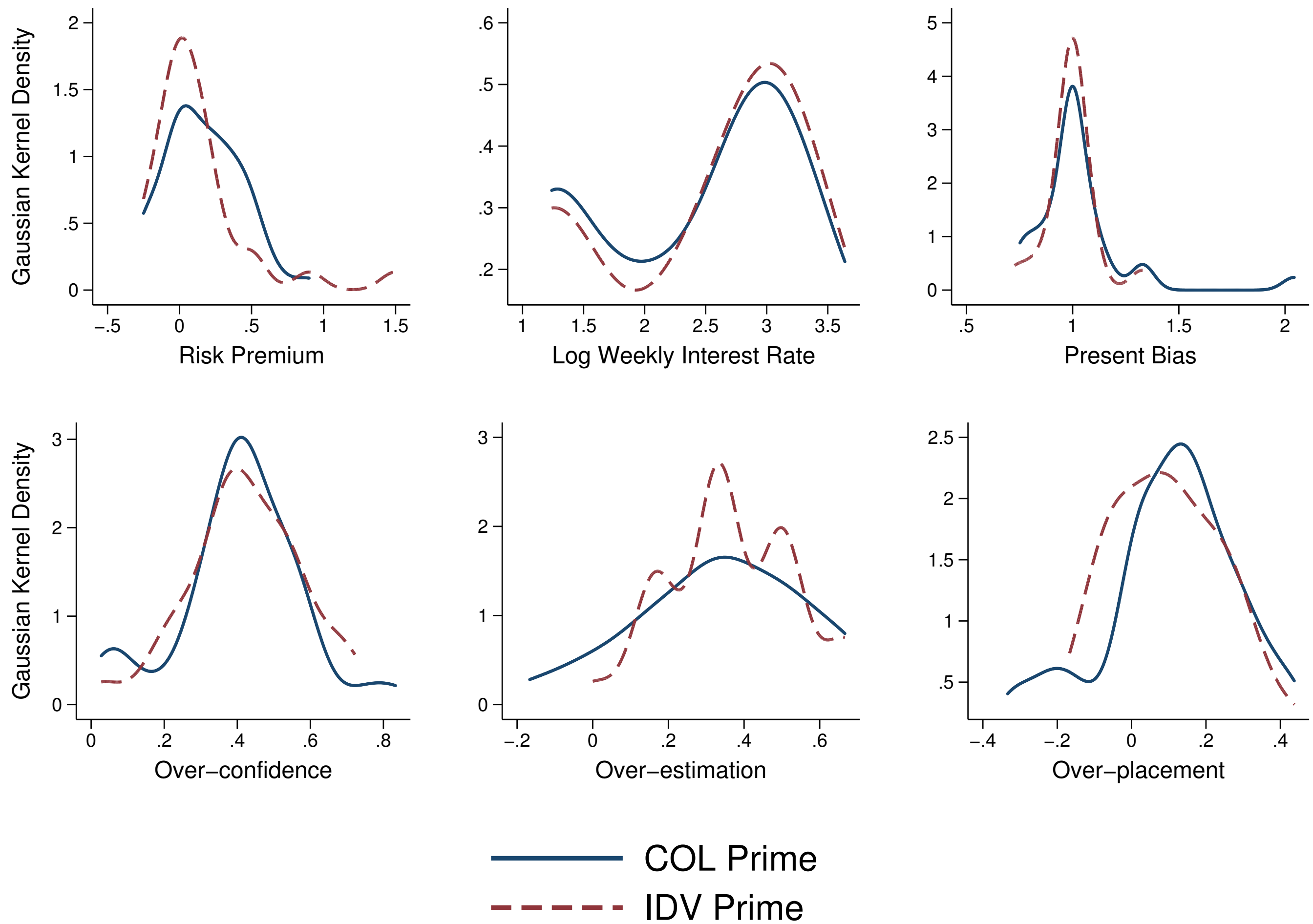
3.8 Conclusion

The primary goal of this paper has been to demonstrate that individualism-collectivism (I-C) has a causal impact on economic behavior at the individual level. To achieve this goal, I have experimentally created exogenous variation in the salience of I-C in a real-stakes laboratory experiment. I have then used multiple price-lists to incentive-compatibly elicit subjects’ risk aversion, time preferences and various measures of overconfidence as the outcome variables.

I have found the following results. Priming subjects on collectivism causes them to make less risky and more patient financial choices and report lower self-confidence (but not overconfidence) than subjects primed on individualism. These effects are on the order of one-fifth to one-third of standard deviation of the corresponding dependent variable. Putting the magnitudes in perspective, the causal effect of parental education on the offspring's likelihood of being "very risk averse" has been estimated to be about two-thirds of the standard deviation. Secondly, in line with the social identity theory, I have found the priming effects of individualism-collectivism to be heterogenous across racial groups. Collectivism causes blacks to make more patient intertemporal choices, having the opposite effect on whites. Priming blacks on collectivism also makes them less overconfident than others. Hispanics, in contrast, are less likely than non-Hispanics to engage in risky financial behavior after receiving the collectivism prime. Finally, consistent with the existing evidence that Asians and whites are, on average, the least and the most individualistic, my results suggest that Hispanics and blacks are particularly sensitive to being primed on collectivism, while Asians and whites are the least sensitive.

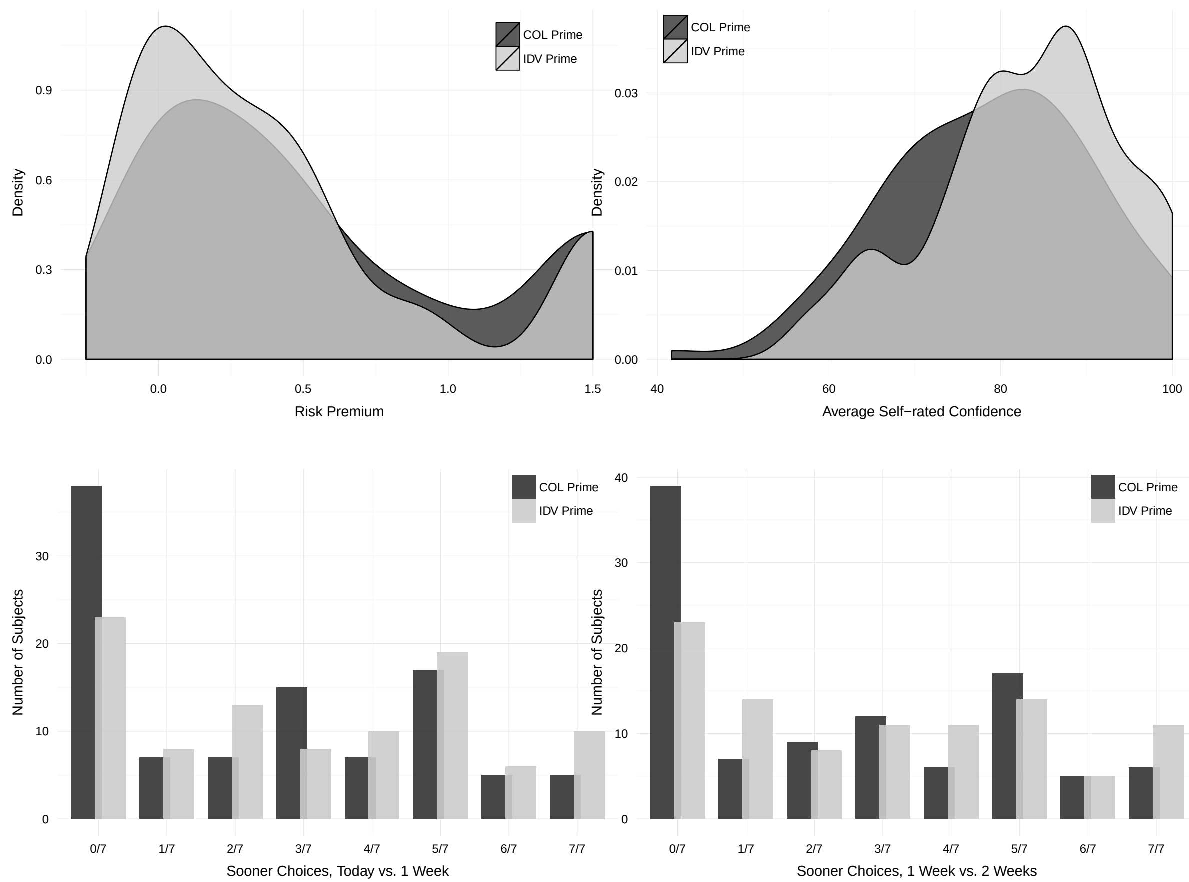
Future research needs to address the question of what the individual-level findings of this study mean in terms of macroeconomic implications of individualism-collectivism. Replicating the experiment in larger samples would increase the credibility of the current findings. I have already taken steps in this direction and plan to collect more experimental data in the near future.

Figure 3.1: Density Plots of Dependent Variables by the Prime Condition, UH Experiment



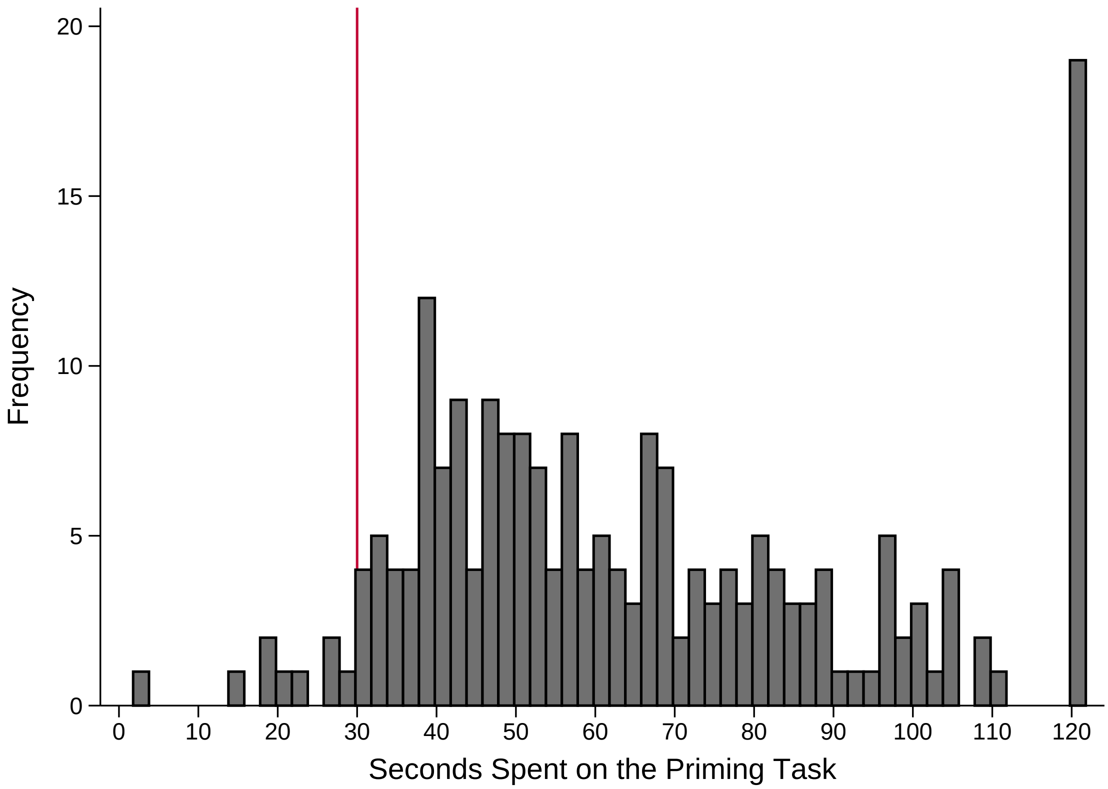
Notes: The figure plots Gaussian kernel density functions for subjects primed on collectivism and individualism separately for the variable given on the *x* axis of each graph. The density plot of risk premium for subjects primed on collectivism is particularly skewed to the right. This means that collectivism-primed subjects need higher expected payoffs from the offered gamble to forgo the safe option and accept the gamble.

Figure 3.2: Density and Histogram Plots of Dependent Variables by the Prime Condition, MTurk Experiment



Notes: The figure plots Gaussian kernel density functions for subjects primed on collectivism and individualism separately for risk premium and average self-confidence in the upper two images, and histograms for the fraction of sooner choices in the intertemporal tasks in the lower two images. That the density plot of risk premium for subjects primed on collectivism is has fatter right tail means that collectivism-primed subjects need higher expected payoffs from the offered gamble to forgo the safe option and accept the gamble, whereas subjects primed on individualists “switch” to choosing gambles faster: smaller expected payoffs of the early gambles are attractive enough for them.

Figure 3.3: Distribution of the Time Spent on Reading the Priming Paragraph

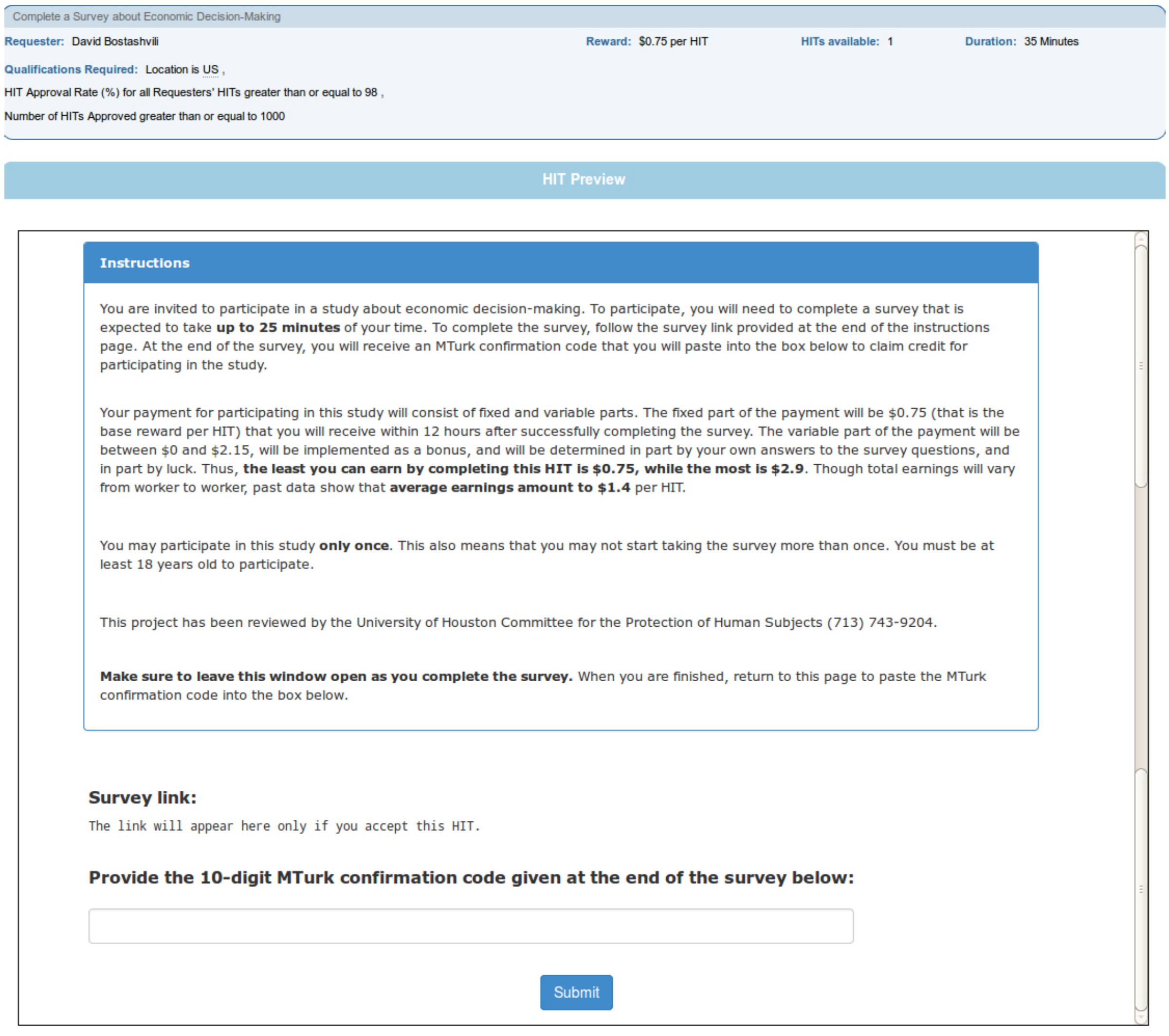


Notes: The figure plots a histogram of the time spent on reading the priming paragraph. The data are from the experiment conducted online via Amazon Mechanical Turk in March 2015 with a total of 208 subjects.

The priming paragraph consists of 204 words, plus additional 49 words of instructions—a total of 253 words for the entire task. It is practically impossible to read 253 words in less than 30 seconds even at a very fast reading speed, while two minutes is more than enough time to read the task once at a slow pace, or twice at an average speed (*source:* `http://readtime.eu/`). Indeed, the average and median subjects spend 65 and 59 seconds on reading the task. Still, 10 out of 208 subjects spend less than 30 seconds. I refer to these subjects as "weak primes," as without properly reading the priming paragraph one can not be adequately primed.

The reason for some bunching at 120 seconds is that the priming task auto advances to the following task if subjects do not choose to continue sooner. This is to avoid subjects spending too much time on the priming page, potentially multi-tasking and being distracted from the prime.

Figure 3.4: Instructions for the Main MTurk Experiment



Complete a Survey about Economic Decision-Making

Requester: David Bostashvili **Reward:** $0.75 per HIT **HITs available:** 1 **Duration:** 35 Minutes

Qualifications Required: Location is US ,

HIT Approval Rate (%) for all Requesters' HITs greater than or equal to 98 ,

Number of HITs Approved greater than or equal to 1000

HIT Preview

Instructions

You are invited to participate in a study about economic decision-making. To participate, you will need to complete a survey that is expected to take **up to 25 minutes** of your time. To complete the survey, follow the survey link provided at the end of the instructions page. At the end of the survey, you will receive an MTurk confirmation code that you will paste into the box below to claim credit for participating in the study.

Your payment for participating in this study will consist of fixed and variable parts. The fixed part of the payment will be $0.75 (that is the base reward per HIT) that you will receive within 12 hours after successfully completing the survey. The variable part of the payment will be between $0 and $2.15, will be implemented as a bonus, and will be determined in part by your own answers to the survey questions, and in part by luck. Thus, **the least you can earn by completing this HIT is $0.75, while the most is $2.9**. Though total earnings will vary from worker to worker, past data show that **average earnings amount to $1.4** per HIT.

You may participate in this study **only once**. This also means that you may not start taking the survey more than once. You must be at least 18 years old to participate.

This project has been reviewed by the University of Houston Committee for the Protection of Human Subjects (713) 743-9204.

Make sure to leave this window open as you complete the survey. When you are finished, return to this page to paste the MTurk confirmation code into the box below.

Survey link:

The link will appear here only if you accept this HIT.

Provide the 10-digit MTurk confirmation code given at the end of the survey below:

Submit

Notes: The image depicts the preview of the Human Intelligence Task (HIT) at Amazon Mechanical Turk that workers see before deciding to accept the HIT. The instructions clearly specify expected time commitment (up to 25 minutes) and potential earnings ($1.40 on average) for taking the survey. If accepted, the HIT displays a link to an external Qualtrics survey. Subjects then continue to complete the survey and obtain a randomly generated code to use at the submission of the HIT as a proof for completing the survey. Completed surveys are then reviewed and corresponding HITs approved within a few hours of submission, after which workers collect their earnings. Any additional earnings on top of the base reward of $0.75 (including later payments) are paid out as bonuses via the MTurk payment system.

Table 3.1: Risk Aversion Elicitation

	Option A (safe option)	Option B (risky option)		Difference in Expected Payoff (A−B)
Question	100% probability	50% probability	50% probability	
1	10	0	15	2.5
2	10	0	18	1.0
3	10	0	20	0.0
4	10	0	22	−1.0
5	10	0	24	−2.0
6	10	0	26	−3.0
7	10	0	28	−4.0
8	10	0	30	−5.0
9	10	0	33	−6.5
10	10	0	38	−9.0

Notes: The table presents ten financial choices subjects make as part of the risk preference elicitation task. The numbers were actual dollar values for the paper-and-pencil experiment conducted at the University of Houston during January-February 2013. For the online experiment conducted via Amazon Mechanical Turk during March 2015, the numbers were points, converted to dollars at ratio of 20 points for 1 dollar.

A typical subject starts choosing the safe option in the first few questions, switching to choosing the risk option in later questions as the expected pay-off increases. The switching row determines a given subject's preference for risk.

Table 3.2: Time Preference Elicitation

Question	Option A (sooner payment)	Option B (later payment)	Gross Interest Rate (B/A)
1	14	15	1.07
2	13	15	1.15
3	12	15	1.25
4	11	15	1.36
5	10	15	1.50
6	9	15	1.67
7	7	15	2.14

Notes: The table presents seven of the 14 financial choices subjects make as part of the time preference elicitation task. The numbers were actual dollar values for the paper-and-pencil experiment conducted at the University of Houston during January-February 2013. For the online experiment conducted via Amazon Mechanical Turk during March 2015, the numbers were points, converted to dollars at ratio of 20 points for 1 dollar.

In the first seven questions, the sooner payments were same-day payments, while later payments were potential earnings payable two weeks from the day of the session in the UH experiment, and one week from the day of the survey completion in the MTurk experiment. The following seven questions were about choices between "two weeks from today" and "four weeks from today" in the UH experiment, and "a week from today" and "two weeks from today" in the MTurk experiment.

Altogether subjects made 14 choices. A typical subject starts choosing sooner payments in the first question, switching to the later payments as the interest rate increases. The switching rows provide information on the subject's time preferences, discount factor, and present bias. See Section B.3 for details.

Table 3.3: Self-confidence Elicitation

For each question, your first task is to circle one answer you think is the correct one. Your second task is to rate your confidence in answering each question correctly by circling a number between 0 and 4, whereby circling

0 means: "I have no clue if I'm right or wrong;"

1 means: "I have a hunch, but I'm probably wrong;"

2 means: "I'm somewhat sure I'm right, but I could be wrong just as well;"

3 means: "I'm pretty sure I'm right, but I might still be wrong;"

4 means: "I'm absolutely sure I'm right."

That is, the surer you feel about the correctness of your answer, the higher the circled number should be.

#25. Which of the forty-eight contiguous states is the northernmost? (Circle one answer)

Maine Minnesota Washington

How confident are you that your answer is correct? (Circle one number)

0	1	2	3	4
("No clue")				*("I am sure")*

#26. What is the most abundant metal on the earth? (Circle one answer)

aluminum copper iron

How confident are you that your answer is correct? (Circle one number)

0	1	2	3	4
("No clue")				*("I am sure")*

#27. What is the fastest land animal? (Circle one answer)

cheetah jaguar leopard

How confident are you that your answer is correct? (Circle one number)

0	1	2	3	4
("No clue")				*("I am sure")*

#28. What color is the flight data recorder, also know as the "black box," in commercial airplanes? (Circle one answer) black orange red

How confident are you that your answer is correct? (Circle one number)

0	1	2	3	4
("No clue")				*("I am sure")*

#29. Which one of the following cities currently has the largest population? (Circle one answer)

Dallas, TX Phoenix, AZ San Diego, CA

How confident are you that your answer is correct? (Circle one number)

0	1	2	3	4
("No clue")				*("I am sure")*

#30. Where was Adolf Hitler, the leader of the Nazi Party, born? (Circle one answer)

Austria Germany Switzerland

How confident are you that your answer is correct? (Circle one number)

0	1	2	3	4
("No clue")				*("I am sure")*

Table 3.4: Descriptive Statistics, UH Experiment

	Observations	Mean	Std. Dev.	Min	Max
Demographic Characteristics					
Female	73	0.52	0.5	0	1
Age	73	21	3.3	18	35
Graduate student	73	0.07	0.25	0	1
Married	73	0.01	0.12	0	1
Parents' income, $1,000	73	63	34	10	110
Born in U.S.	73	0.7	0.46	0	1
English native language	73	0.63	0.49	0	1
White	73	0.21	0.41	0	1
Black	73	0.23	0.43	0	1
Hispanic	73	0.22	0.42	0	1
Asian	73	0.29	0.46	0	1
Survey Characteristics					
Total earnings, $	73	23	8.2	10	48
Number of uncircled pronouns	73	1	3.8	0	19
Cashed the check early	31	0.32	0.48	0	1
Subjects per session	73	8.9	3.7	1	15
Outcomes					
Percent safe choices in the risk game	73	34	25	0	100
CRRA, imputed	73	0.06	0.34	−0.71	1.2
Percent sooner choices, today vs. 1 week	73	39	31	0	100
Percent sooner choices, 1 week vs. 2 weeks	73	38	29	0	100
Present bias $\hat{\beta}$, imputed	49	1	0.2	0.73	2
Discount factor $\hat{\delta}$, imputed	53	0.69	0.21	0.00	1.1
Average self-rated confidence	73	76	11	50	100
Predicted number of correct answers (out of 6)	72	4.1	1.1	2	6
Overconfidence	73	0.41	0.16	0.03	0.83
Overestimation	72	0.35	0.2	−0.17	0.67
Overplacement	70	0.1	0.17	−0.33	0.44

Notes: These data were collected from an experiment conducted on campus at the University of Houston in a paper-and-pencil format throughout January-February 2013. Out of 73 subjects, 44 were randomly selected to receive the collectivism prime and 29 received the individualism prime. $\hat{\beta}$ and $\hat{\delta}$ are parameter estimates of present bias and discount factor. The reason for missing observations is that it is not possible to impute these parameters for subjects always choosing later payments—see Appendix B.3 for details. Overconfidence is measured as the difference between the subject's self-assessment of correctness of her answers to the trivia questions and her average percentage of getting the correct answers. Overestimation is measured as the difference between the subject's guess of the number of correct answers to the trivia questions and her actual average number of correct answers. Overplacement is measured as the difference between the subject's self-assessment of her relative performance compared to the session average and her actual relative performance.

Table 3.5: Descriptive Statistics, MTurk Experiment

	Observations	Mean	Std. Dev.	Min	Max
Demographic Characteristics					
Female	208	0.48	0.5	0	1
Age	208	39	13	18	75
Married	208	0.38	0.49	0	1
Employed full-time	208	0.57	0.5	0	1
Education: college or higher	208	0.51	0.5	0	1
Household income $<$ \$35,000	208	0.42	0.49	0	1
Salary main source of income	208	0.43	0.5	0	1
Born abroad	208	0.07	0.25	0	1
English native language	208	0.87	0.34	0	1
White	208	0.76	0.43	0	1
Black	208	0.1	0.3	0	1
Hispanic	208	0.05	0.22	0	1
Asian	208	0.07	0.25	0	1
Survey Characteristics					
Hourly earnings, \$	208	5.8	2.7	1.2	15
Minutes completing the survey	208	16	5.6	5.8	38
Seconds spent on the priming task	208	65	28	1.8	121
Spent $<$ 30 seconds on the priming task	208	0.05	0.21	0	1
Seconds spent on the risk game	208	50	33	5.6	231
Seconds spent on the time game	208	46	27	14	189
Seconds spent on the trivia task	208	68	37	19	240
Selected all safe and sooner choices (distrust)	208	0.03	0.17	0	1
Batch size	208	23	5.8	10	31
Outcomes					
Percent safe choices in the risk game	208	55	32	0	100
CRRA, imputed	208	0.38	0.46	-0.71	1.2
Percent sooner choices, today vs. 1 week	208	39	34	0	100
Percent sooner choices, 1 week vs. 2 weeks	208	38	35	0	100
Present bias $\hat{\beta}$, imputed	127	0.98	0.2	0.00	1.6
Discount factor $\hat{\delta}$, imputed	142	0.66	0.3	0.00	1.1
Average self-rated confidence	208	81	12	42	100
Predicted number of correct answers (out of 6)	204	4.3	1	2	6
Overconfidence	208	0.34	0.21	-0.19	0.83

Notes: These data were collected from an experiment conducted online via Amazon Mechanical Turk in March 2015. Out of 208 subjects, 105 were randomly selected to receive the collectivism prime and 103 received the individualism prime. $\hat{\beta}$ and $\hat{\delta}$ are parameter estimates of present bias and discount factor—see Appendix B.3 for details. Overconfidence is measured as the difference between the subject's self-assessment of correctness of her answers to the trivia questions and her average percentage of getting the correct answers.

Table 3.6: Randomization Check, UH and MTurk Experiments

	UH Experiment			*MTurk Experiment*		
	COL prime	Constant	Observations	COL prime	Constant	Observations
Demographic Characteristics						
Female	−0.05	0.55***	73	0.11	0.43***	208
	(0.12)	(0.09)		(0.07)	(0.05)	
Age	−1.29	21.79***	73	1.64	37.75***	208
	(0.89)	(0.84)		(1.83)	(1.28)	
Married	−0.03	0.03	73	−0.03	0.40***	208
	(0.03)	(0.03)		(0.07)	(0.05)	
Employed full-time				0.09	0.52***	208
				(0.07)	(0.05)	
Education: college or higher				0.02	0.50***	208
				(0.07)	(0.05)	
Household income < \$35,000				−0.19***	0.51***	208
				(0.07)	(0.05)	
Salary main source of income				0.14**	0.36***	208
				(0.07)	(0.05)	
Born abroad	0.16	0.21***	73	0	0.07***	208
	(0.11)	(0.08)		(0.03)	(0.02)	
English native language	−0.1	0.69***	73	−0.01	0.87***	208
	(0.12)	(0.09)		(0.05)	(0.03)	
White	−0.12	0.28***	73	0.08	0.72***	208
	(0.10)	(0.08)		(0.06)	(0.04)	
Black	0.1	0.17**	73	0.01	0.10***	208
	(0.10)	(0.07)		(0.04)	(0.03)	
Hispanic	0.08	0.17**	73	−0.05	0.08***	208
	(0.10)	(0.07)		(0.03)	(0.03)	
Asian	−0.04	0.31***	73	−0.02	0.08***	208
	(0.11)	(0.09)		(0.03)	(0.03)	
Survey Characteristics						
Subjects per session / batch	0.58	8.55***	73	−0.16	22.66***	208
	(0.76)	(0.34)		(0.81)	(0.58)	
Minutes completing the survey				−0.25	16.43***	208
				(0.78)	(0.60)	
Seconds spent on priming				1.21	64.45***	208
				(3.84)	(2.84)	
Spent < 30 seconds priming				−0.02	0.06**	208
				(0.03)	(0.02)	

Notes: The table presents Ordinary Least Squares estimates with heteroskedasticity-robust standard errors in parentheses. In the UH Experiment, 44 out of 73 subjects were randomly selected to receive the collectivism prime and 29 received the individualism prime. In the MTurk Experiment, 105 out of 208 subjects were randomly selected to receive the collectivism prime and 103 received the individualism prime. As the results show, the status of receiving collectivism prime is not correlated with almost any of the background or survey characteristics in either of the experiments. That is, treatment and control groups are statistically balanced across a broad range of observable characteristics.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.7: Priming Effects of Collectivism, UH Experiment

	CRRA		Daily r_1		Daily r_2		Present Bias		Self-confidence		Overconfidence	
$\hat{\sigma}$	0.38	0.33	1.83	1.77	1.81	1.68						
Std. Dev.							0.20		10.8		0.16	
COL prime	0.07	0.08	−0.14	−0.09	−0.11	−0.06	0.03	0.05	0.43	0.95	−0.01	0.01
	(0.07)	(0.06)	(0.56)	(0.47)	(0.40)	(0.26)	(0.07)	(0.08)	(1.56)	(1.59)	(0.03)	(0.03)
Controls		Yes		Yes		Yes		Yes		Yes		Yes
Observations	73	73	73	73	73	73	49	49	73	73	73	73

Notes: The table presents results of the experiment conducted on campus at the University of Houston in a paper-and-pencil format throughout January-February 2013. The experiment was conducted in 11 separate sessions with an average of 9 subjects per session.

The first dependent variable is a Coefficient of Relative Risk Aversion (CRRA), imputed from 10 financial choices subjects make in the risk preference game—see Section 3.5.1 for details. Daily r_1 and Daily r_2 are daily interest rates imputed from 14 financial choices subjects make between sooner and later payments—seven choices between immediate and two-weeks-later payments in case of r_1, and the other seven between two-weeks-later and four-weeks-later payments in case of r_2. Self-confidence is the average of self-rated confidence levels subjects report after answering each of the six trivia questions, higher values corresponding to higher confidence. Overconfidence is constructed by adjusting average self-confidence by the number of correct answers to the trivia questions.

Control variables include background characteristics such as age, binary variables for female, Black, Hispanic and Asian, and whether the student circled all pronouns in the priming task, as well as the session size.

Specifications with the dependent variables CRRA, Daily r_1 and Daily r_2 are estimated by the interval regression method to accommodate the censored nature of these variables. The magnitude of estimated coefficients can be interpreted relative to $\hat{\sigma}$—the estimated standard deviation of the latent dependent variable. The rest of the specifications are estimated by Ordinary Least Squares. Heteroskedasticity-robust standard errors clustered at the session level are reported in parentheses for all specifications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.8: Priming Effects of Collectivism, MTurk Experiment

Panel A: Sample without 10 "Weak Primes"

	CRRA		Daily r_1		Daily r_2		Present Bias		Self-confidence		Overconfidence	
$\hat{\sigma}$	0.32	0.31	4.90	4.81	5.13	5.05						
Std. Dev.							0.20		11.6		0.21	
COL prime	0.07	0.09*	−1.60***	−1.57**	−1.46**	−1.48**	0.02	0.02	−4.20***	−3.38**	0.01	−0.00
	(0.05)	(0.05)	(0.60)	(0.74)	(0.65)	(0.74)	(0.03)	(0.03)	(1.09)	(1.17)	(0.04)	(0.04)
Controls		Yes		Yes		Yes		Yes		Yes		Yes
Observations	198	198	198	198	198	198	122	122	198	198	198	198

Panel B: Full Sample

	CRRA		Daily r_1		Daily r_2		Present Bias		Self-confidence		Overconfidence	
$\hat{\sigma}$	0.33	0.31	4.99	4.89	5.28	5.22						
Std. Dev.							0.20		11.7		0.21	
COL prime	0.05	0.06	−1.45**	−1.46**	−1.44*	−1.48*	0.03	0.02	−3.49**	−2.56*	0.01	0.00
	(0.05)	(0.05)	(0.60)	(0.71)	(0.74)	(0.80)	(0.03)	(0.03)	(1.32)	(1.22)	(0.04)	(0.03)
Controls		Yes		Yes		Yes		Yes		Yes		Yes
Observations	208	208	208	208	208	208	127	127	208	208	208	208

Notes: The table presents results of the experiment conducted online via Amazon Mechanical Turk during March 2015. The experiment was conducted in 10 separate batches throughout several days, with an average of 23 subjects per batch. Panel A reports results for the sample excluding "weak primes." These 10 excluded observations are subjects who spend less than 30 seconds or more than two minutes on reading the 253-word priming task, effectively avoiding the priming manipulation. Panel B reports results for the the full sample.

The first dependent variable is a Coefficient of Relative Risk Aversion (CRRA), imputed from 10 financial choices subjects make in the risk preference game—see Section 3.5.1 for details. Daily r_1 and Daily r_2 are daily interest rates imputed from 14 financial choices subjects make between sooner and later payments—seven choices between immediate and a-week-later payments in case of r_1, and the other seven between a-week-later and two-weeks-later payments in case of r_2. Self-confidence is the average of self-rated confidence levels subjects report after answering each of the six trivia questions, higher values corresponding to higher confidence. Overconfidence is constructed by adjusting average self-confidence by the number of correct answers to the trivia questions. Control variables include background characteristics such as age, binary variables for female, White, whether the subject has attained college-level education, whether the subject lives in the Northeast (as opposed to Midwest, South or West), whether the subject lets the survey system automatically advance from the priming task to the next page in two minutes, and time spent completing the survey. $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.9: Racial Heterogeneity in the Treatment Effect on Risk Aversion

	Risk Premium Mean: 0.15, Std. Dev.: 0.30						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
COL×Asian			−0.295				−0.154
			(0.210)				(0.232)
COL×Hispanic				0.142			0.115
				(0.138)			(0.158)
COL×Black					0.089		0.121
					(0.160)		(0.206)
COL×White						−0.038	
						(0.152)	
COL prime			0.124*	0.019	0.034	0.025	−0.019
			(0.073)	(0.097)	(0.086)	(0.101)	(0.124)
Asian	0.254**		0.371*				0.376
	(0.105)		(0.218)				(0.256)
Hispanic	0.145*			−0.094			0.076
	(0.076)			(0.109)			(0.159)
Black	0.067				−0.104		−0.025
	(0.084)				(0.133)		(0.153)
White		−0.155**				−0.104	
		(0.067)				(0.137)	
Controls			Yes	Yes	Yes	Yes	Yes
Number of subjects	73	73	73	73	73	73	73
COL effect if:							
Asian			−0.171				−0.173
Hispanic				0.161*			0.096
Black					0.123		0.102
White						−0.013	−0.019

Notes: The table presents ordinary least squares estimates with heteroskedasticity-robust standard errors in parentheses. Each column represents a separate regression. Control variables include: age, age squared, dummy variables for gender, graduate student status, marital status, having been born in the U.S., having a mother born in the U.S. and having a father with at least college level education, whether English is the subject's native language, whether the subject believes cashing a post-dated check early is a possibility, as well as the survey measure of risk attitudes.

* $p < 0.10$, ** $p < 0.05$.

Table 3.10: Racial Heterogeneity in the Treatment Effect on Impatience

	Log Weekly Interest Rate Mean: 2.42, Std. Dev.: 0.84						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
COL × Asian			0.014				−1.088*
			(0.465)				(0.640)
COL × Hispanic				−0.537			−1.161
				(0.624)			(0.726)
COL × Black					−1.054***		−1.949***
					(0.388)		(0.550)
COL × White						1.382***	
						(0.469)	
COL prime			0.096	0.232	0.262	−0.153	1.169**
			(0.310)	(0.264)	(0.286)	(0.285)	(0.483)
Asian	−0.074		−0.140				0.001
	(0.306)		(0.389)				(0.517)
Hispanic	−0.163			−0.035			−0.118
	(0.326)			(0.536)			(0.696)
Black	0.243				1.155***		1.232***
	(0.307)				(0.282)		(0.371)
White		−0.020				−0.515	
		(0.256)				(0.413)	
Controls			Yes	Yes	Yes	Yes	Yes
Number of subjects	71	71	71	71	71	71	71
COL effect if:							
Asian			0.11				0.081
Hispanic				−0.305			0.008
Black					−0.792***		−0.78**
White						1.229***	1.169**

Notes: The table presents ordinary least squares estimates with heteroskedasticity-robust standard errors in parentheses. Each column represents a separate regression. Control variables include: age, age squared, dummy variables for gender, graduate student status, marital status, having been born in the U.S., having a mother born in the U.S. and having a father with at least college level education, whether English is the subject's native language, whether the subject believes cashing a post-dated check early is a possibility, as well as the survey measure of risk attitudes.

* $p < 0.10$, ** $p < 0.05$.

Table 3.11: Racial Heterogeneity in the Treatment Effect on Overconfidence

	Overconfidence Mean: 0.413, Std. Dev.: 0.161						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
COL×Asian			0.062				−0.082
			(0.077)				(0.109)
COL×Hispanic				−0.061			−0.152
				(0.077)			(0.117)
COL×Black					−0.201**		−0.284**
					(0.088)		(0.113)
COL×White						0.149	
						(0.099)	
COL prime			−0.035	−0.015	0.037	−0.048	0.115
			(0.051)	(0.052)	(0.047)	(0.048)	(0.090)
Asian	0.026		−0.007				0.074
	(0.051)		(0.062)				(0.098)
Hispanic	−0.006			0.132**			0.145
	(0.048)			(0.066)			(0.102)
Black	−0.056				0.063		0.112
	(0.060)				(0.076)		(0.079)
White		0.026				−0.062	
		(0.044)				(0.068)	
Controls			Yes	Yes	Yes	Yes	Yes
Number of subjects	73	73	73	73	73	73	73
COL effect if:							
Asian			0.027				0.033
Hispanic				−0.076			−0.037
Black					−0.164**		−0.169**
White						0.101	0.115

Notes: The table presents ordinary least squares estimates with heteroskedasticity-robust standard errors in parentheses. Each column represents a separate regression. The dependent variable in all eight columns is overconfidence, measured as the difference between the subject's self-assessment of correctness of her answers to six trivia questions and the actual average percentage of her getting the correct answers. Control variables include: age, age squared, dummy variables for gender, graduate student status, marital status, having been born in the U.S., having a mother born in the U.S. and having a father with at least college level education, whether English is the subject's native language, whether the subject believes cashing a post-dated check early is a possibility, as well as the survey measure of risk attitudes.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.12: Results of the Validation Experiment

	Index 1		Index 2		Alternative Index 1		Alternative Index 2	
	Panel A: Sample without "Weak Primes" $N = 128$							
Mean	0.07		−0.08		−0.08		−0.09	
(Std. Dev.)	(2.09)		(2.12)		(1.94)		(1.99)	
COL prime	0.46	1.14**	0.83**	1.50***	0.79***	1.14***	0.86***	1.25***
	(0.42)	(0.41)	(0.35)	(0.29)	(0.24)	(0.29)	(0.28)	(0.31)
Batch fixed effects		Yes		Yes		Yes		Yes
	Panel B: Full Sample $N = 186$							
	Index 1		Index 2		Alternative Index 1		Alternative Index 2	
Mean	0.00		0.00		−0.00		0.00	
(Std. Dev.)	(2.06)		(2.06)		(2.06)		(2.06)	
COL prime	0.20	0.50	0.41	0.78**	0.36	0.48	0.31	0.52
	(0.29)	(0.32)	(0.33)	(0.33)	(0.34)	(0.42)	(0.31)	(0.33)
Batch fixed effects		Yes		Yes		Yes		Yes

Notes: The table presents results of the validation experiment conducted online via Amazon Mechanical Turk during March 2015. The experiment was conducted in 14 separate batches throughout several days, with an average of 13 subjects per batch. Panel A reports results for the sample excluding "weak primes." These 58 excluded observations are subjects who spend less than 30 seconds or more than two minutes on reading the 253-word priming task, effectively avoiding the priming manipulation. Panel B reports results for the full sample.

The goal of the experiment was to test whether making collectivism salient actually makes subjects' collectivistic perceptions more pronounced. These perceptions were categorized as collectivistic based on subjects' answers to five simple tasks, four of which were image-categorization tasks (Figure 3.5) and the fifth was to write ten open statements about oneself (answering the question, "Who Am I?"). Each of the four outcomes of interest—Index 1, Index 2, Alternative Index 1 and Alternative Index 2—was obtained by summing up four leading principal components of five respective variables based on the five tasks. For each of the four indexes, the four leading principal components explain 85 percent of variation across the respective five variables. Four of these five variables are always binary classifications of four-image tasks as collectivistic. The fifth variable differs across the four indexes—see Section B.2 in the appendix for more details.

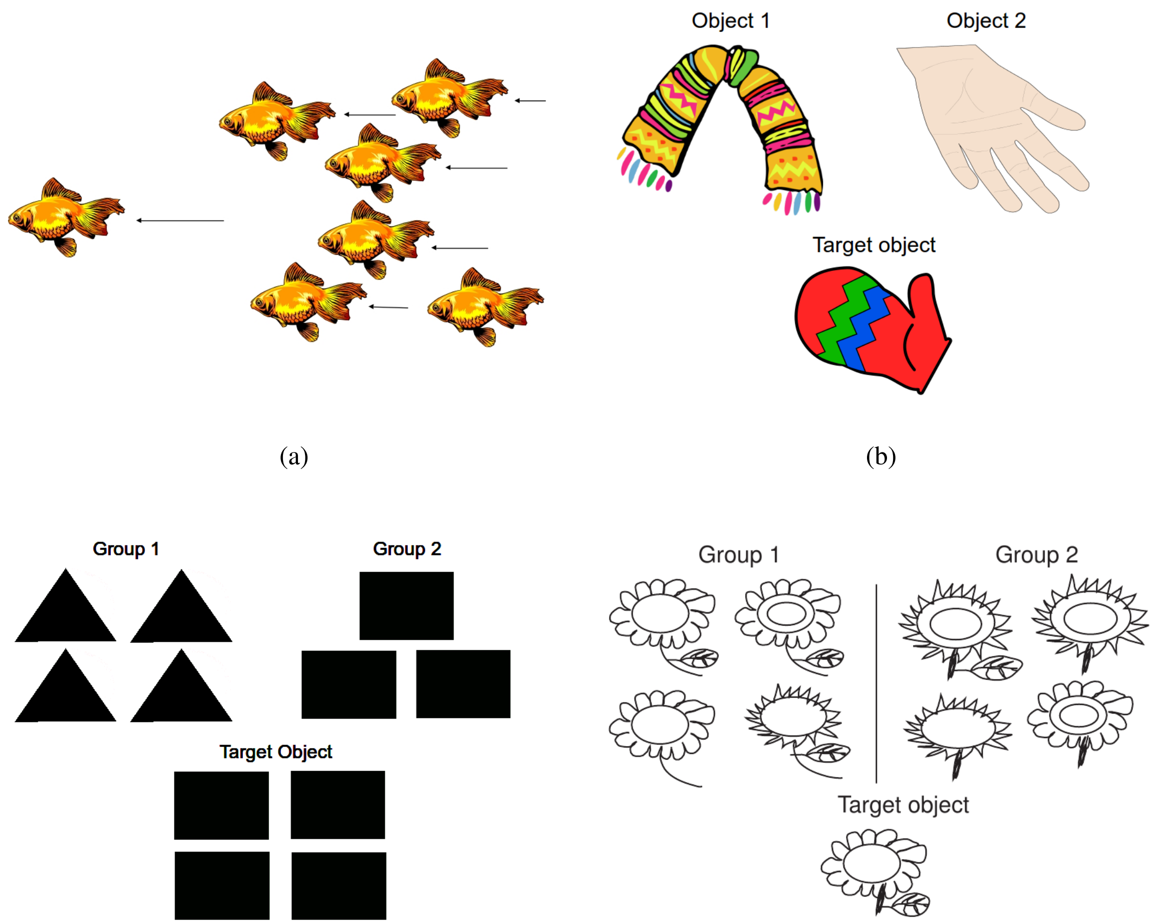
Both Panel A and Panel B report coefficients from parsimonious specifications with the collectivism prime as the main independent variable, and one of the four indexes as the respective dependent variable, estimated by Ordinary Least Squares with heteroskedasticity-robust standard errors clustered at the batch level and reported in parentheses. For each dependent variable two specifications are estimated: with and without batch fixed effects. (Batch fixed effects are always jointly significant at least at five percent.) * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.13: Comparison of the Experimental Samples to the U.S. Population

	MTurk	UH	US ACS	MTurk vs. US	UH vs. US	MTurk vs. UH
	$N = 208$	$N = 73$	$N \approx 3mil$	*p-values of the differences*		
Demographic Characteristics						
Female	0.48	0.52	0.51	0.44	0.83	0.56
Age	39	21	38	0.68	0.00***	0.00***
Married	0.38	0.01	0.39	0.96	0.00***	0.00***
White	0.76	0.21	0.74	0.46	0.00***	0.00***
Black	0.10	0.23	0.13	0.27	0.01***	0.00***
Hispanic	0.05	0.22	0.17	0.00***	0.27	0.00***
Asian	0.07	0.29	0.06	0.65	0.00***	0.00***
Born Abroad	0.07	0.30	0.14	0.00***	0.00***	0.00***
English is the native language	0.87	0.63	0.74	0.00***	0.03**	0.00***
Employed	0.76		0.65	0.00***		
Education: college or higher	0.51		0.21	0.00***		
Household income under $35,000	0.42		0.29	0.00***		
Salary is the main source of income	0.43		0.46	0.35		
Northeast	0.26		0.20	0.02**		
Midwest	0.18		0.21	0.28		
South	0.32		0.37	0.20		
West	0.23		0.21	0.40		
Survey Characteristics						
Number of subjects in a session	23	8.9				0.00***
Minutes for completing the survey	16	27				0.00***
Earnings, $	5.8	23				0.00***
Outcomes						
% Safe choices in the risk game	55	34				0.00***
Imputed CRRA from the risk game	0.38	0.06				0.00***
% Earlier choices, today vs. 1 week	39	39				0.97
% Earlier choices, 1 week vs. 2 weeks	38	38				0.89
Present bias	0.98	1				0.27
Discount factor	0.66	0.69				0.42
Average self-rated confidence	81	76				0.00***
Predicted number of correct answers	4.3	4.1				0.39
Overconfidence	0.34	0.41				0.01***
Overestimation	0.24	0.35				0.00***

Notes: The first three columns report sample means of the listed variables for three different samples. The MTurk sample ($N = 208$) contains survey data collected online via Amazon Mechanical Turk during March 2015. The UH sample ($N = 73$) contains data collected in a paper-and-pencil format at the University of Houston throughout January-February 2013. The US ACS sample contains data ($N = 3,132,795$) from the 2013 wave of the American Community Survey (Ruggles et al., 2010), reweighted at the individual level to represent the entire U.S. population. The last three columns report p-values of the differences between the two respective samples. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 3.5: Four Image-categorization Tasks



(a) (b) (c) (d)

Notes: The four pictures were presented to subjects one by one after they had received the primes. For Picture (a), subjects were asked to choose one of the two suggested sentences they thought best described the picture: "The individual is leading a group," or "The group is pressuring an individual." For Pictures (b), (c), and (d), subjects were asked to pick one of the two objects (groups) they thought was most closely related to the target object. The following answers were categorized as collectivistic: "The group is pressuring an individual" in (a) (Morris and Peng, 1994); Object 2 in (b) because of relational as opposed to categorical pairing (Talhelm et al., 2015); and Object 1 in both (c) and (d) because of holistic as opposed to analytic matching (Nadler and Minda, 2011; Nisbett and Miyamoto, 2005).

This experiment was conducted online via Amazon Mechanical Turk in March 2015 ($N = 186$), and preceded the main experiment measuring the effects of the salience of collectivism vs. individualism on the economic outcomes of interest (risk and time preferences and self-confidence). Subjects who participated in the first experiment, and were thus exposed to the priming instrument, were not allowed to participate in the main experiment.

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Appendix A

A.1 Data Sources and Definitions

A.1.1 The Civil Service

For all states, the sources include *The Book of the States*, Lexington, KY: The Council of State Governments (various issues), as well as Civil Service Assembly of the United States and Canada: *Civil Service Agencies in the United States*, Pamphlets No. 11 (1937), 16 (1940), and 17 (1943). For additional state-specific sources, please see the appendix in Ujhelyi (2014*b*).

A.1.2 Elections and political party control

The three sources for data on gubernatorial elections, governor's party, and party control of state legislatures are:

1. *CQ Voting and Elections Collection*, The Congressional Quarterly Press (various

years), `http://library.cqpress.com/elections/`.

2. Burnham, W. Dean, "Partisan Division of American State Governments, 1834–1985," Conducted by Massachusetts Institute of Technology, ICPSR ed. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [producer and distributor], 1986. All variables were merged, so that they reflect party composition in a given year (for election years party composition reflects the pre-election situation). Before 1975, this requires shifting the variables forward by one year.

Manual corrections: Maine, 1960—Republican (John H. Reed, Dec. 30 1959 to Jan. 5 1967); New York, 1943—Republican (Thomas Dewey from Jan. 1, 1943 to Dec. 31, 1954); New York, 1955—Democrat (W. Averell Harriman from Jan. 1, 1955 to Dec. 31, 1958); New York, 1959—Republican (Nelson Rockefeller from Jan. 1, 1959 to Dec. 18, 1973); Utah, 1965–1969—Democrat (Calvin L. Rampton from Jan. 4, 1965 to Jan. 3, 1977); Wisconsin, 1943—Republican (Walter S. Goodland from Jan. 4, 1943 to Mar. 12, 1947); Wyoming, 1973—Republican (Stanley K. Hathaway from Jan. 2, 1963 to Jan. 6, 1975).

3. *The Book of the States*, Lexington, KY: The Council of State Governments (various issues); and *The Statistical Abstracts* series by the U.S. Census Bureau, `http://www.census.gov/compendia/statab/cats/elections.html`.

A.1.3 Other data

— State expenditures and taxes. *Source:* U.S. Census Bureau, Annual Survey of State Government Finances and Census of Governments, `http://www.census.gov/govs/`

`state/definitions.html`. Direct expenditures are defined as payments to employees, suppliers, contractors, beneficiaries, and other final recipients of government payments (i.e., all expenditure other than Intergovernmental expenditure).

— Income and population. *Source:* Bureau of Economic Analysis: Regional Economic Accounts, `http://www.bea.gov/regional/spi/`. State Annual personal income. Population figures reported in this source are midyear estimates by the U.S. Census Bureau.

— Age and kids. *Source:* U.S. Census Bureau. The post-1970 data were compiled by List, J.A., and D.M. Sturm (2006): "How Elections Matter: Theory and Evidence from Environmental Policy," *Quarterly Journal of Economics*, 121(4), 1249–1281. The pre-1970 data were entered from Population Projection (P25) Reports. Measures, respectively, the fraction of population aged 5–17 and 65 and above. Imputed years: 1941–49, 1959, 1969.

— Consumer Price Index (CPI). *Source:* U.S. Department of Labor: Bureau of Labor Statistics, `http://www.bls.gov`. Consumer Price Index for all Urban Consumers, not seasonally adjusted. Yearly values obtained by averaging across months. 2010=100.

— Citizen ideology. *Source:* Berry, William D., Evan J. Ringquist, Richard C. Fording and Russell L. Hanson. 1998. "Measuring Citizen and Government Ideology in the American States, 1960–93." *American Journal of Political Science*, 42, 327–348. This index uses ideological ratings of congressional candidates by the Americans for Democratic Action and the AFL/CIO's Committee on Political Education and their vote shares to estimate the ideological composition of electoral districts; these are then aggregated to form a statewide measure of citizens' ideology (degree of liberalism), on a scale of 0–100.

A.2 Monte Carlo Simulations

Estimating dynamic panel data models with individual fixed effects by ordinary least squares (OLS) results in a bias when a lagged dependent variable is included in the model as one of the explanatory variables. The bias, also known as the Nickell bias after Nickell (1981), is more severe the shorter the time dimension of a panel. In sufficiently long panels, however, the bias is negligible (Judson and Owen, 1999). In this section, I describe the Monte Carlo exercise which I conduct to verify that in my estimating sample that spans 37 years the Nickel bias is, indeed, trivial in magnitude.

A.2.1 Setup of the Exercise

The exercise consists of the following conceptual steps:

1. Create a data generating process with the existing data and pre-specified coefficients.
2. Simulate the error term that completes the data generating process sufficiently many times (say, k times), resulting in k models of the data generating process.
3. Estimate the simulated models via OLS and obtain k estimates of the pre-specified coefficients; derive the Nickell bias by comparing the distribution of the estimated OLS coefficients to the preset coefficients.
4. Repeat Step 3 with Arellano and Bond's (1991) generalized method of moments (GMM) technique to juxtapose the GMM estimates with the OLS results.

I now describe each step in more detail. First, using the main estimating sample with $s = 45$ and $t = 37$,[1] I generate the dependent variable according to the following data generating process (the "true" model):

$$y_{st} = \tau y_{s,t-1} + Elections - Elections \times Civil + Civil + \lambda_s + u_{st}, \tag{A.1}$$

where $u_{st} = e_{st} + 2\lambda_s$, $e_{st} \sim N(0,1)$ for each s. That is, to generate the error term u of the data generating process (Equation A.1), simulated values are first drawn from a Normal distribution with the mean of zero and the variance of one, and then these values are added to the (doubled) state-specific fixed effects for each state s.[2] Thus, $\mathbb{E}(u_{st}, \lambda_s) \neq 0$ by construction, necessitating the inclusion of individual fixed effects for consistently estimating Equation A.1.[3] The rest of the variables (*Elections* and *Civil*) are the same as described in Section 2.4.1. The *Elections* variable takes on a value of one for the two-year period immediately prior to elections and zero otherwise, and the *Civil* variable equals one if a given state in a given year has had the civil service system in place for its civil service employees and zero otherwise.

Note that I arbitrarily set the values of coefficients of the data generating process to 1, -1, and 1 for *Elections*, $Elections \times Civil$, and *Civil*, respectively. This is not, however, crucial to the exercise. Choosing any coefficients will accomplish the same goal, which is

[1]The exercise sample consists of the same observations that are used to obtain the main results of the paper (Tables 3.3 and 2.4). This means confining the original data sample to a sub-sample without the missing values in the explanatory variables (X_{st} in Equation 2.1).

[2]Note that the nature of correlation between u_{st} and λ_s generated in this way is arbitrary. Instead, one can use the "parametric bootstrap" method to estimate the variation in state fixed effects, and then use this information for generating more realistic correlation between u_{st} and λ_s. Preliminary results based on the bootstrap method are similar to the results reported here.

[3]A presumption here is that state-specific fixed effects are correlated with the *Civil* variable, capturing the idea that the reasons for different states reforming their bureaucracies at different points in time most likely include unobserved time-invariant characteristics of the states.

to compare the estimated coefficients to the pre-specified values to infer the magnitude of the bias in the estimands. I also arbitrarily set the value of τ—the coefficient on the lagged dependent variable, or the "persistence" parameter—that captures the auto-regressive nature of the state finances variables. In particular, I vary the values of τ throughout the exercise from low to high—0.2, 0.5, 0.8, 0.9, and 0.95—to compare the extent of the bias across the range of this auto-regressive term.

Given the data generating process, the second step is to set the number of simulations to a sufficiently large number k ($k = 5,000$) and simulate the values of the error term k times. As a result, I obtain the k number of samples with different dependent variables (and the same independent variables) generated according to the true model (Equation A.1).

The third step is to estimate Equation A.1 by OLS for each sample k and each τ, resulting in the k number of OLS estimates of the coefficients for each independent variable for each τ. I then compute the bias in each estimand as a percentage deviation from the pre-specified coefficient for each sample k and each τ (e.g., if the estimated coefficient on $Elections \times Civil$ is -0.9, then the bias equals 10 percent[4]). Finally, I average out the biases for each coefficient across the k samples (for each τ), and report the results in Table B1.

The last step is to repeat the previous step by estimating the true model by Arellano-Bond's GMM method, instead of OLS, to compare the results to the OLS estimates.

[4]The bias in this example is computed as follows:

$$Bias = \frac{-0.9-(-1)}{100} \times 100\% = 10\%.$$

A.2.2 Simulation Results

The results of this exercise are summarized in Table B1 on the following page. Panel A reports the percentage bias in OLS estimands relative to the "true" coefficients. Panel B reports the results as estimated by the GMM method. Of particular interest are the estimates of the election cycle variable and its interaction with the civil service variable (the two middle columns of the table). Both of these coefficients, as shown in Panel A, can be estimated by OLS with only 1–3 percent bias (depending on the magnitude of the auto-regressive parameter). The estimates of the lagged dependent variable and the civil service variable, in contrast, exhibit larger biases, but for the purposes of the present paper, these results are inconsequential.

Interestingly, coefficients produced by Arellano and Bond's (1991) GMM method exhibit larger biases compared to the OLS estimates, as seen in Panel B. This, however, is not too surprising as the Arellano-Bond method is primarily designed for estimating short panels. As has already been demonstrated in the literature (Judson and Owen, 1999), the GMM technique is, in fact, outperformed by the OLS estimation with fixed effects in longer panels, such as the one used in this paper. The intuition behind this result is that the longer the panel, the longer the list of instruments generated by the GMM method, leading to the "proliferation of instruments" problem—overfitting endogenous variables and biasing the estimates (Roodman, 2009).

Table B1: Simulation Results

The dependent variable is y. The true model is:

$$y_{st} = \tau y_{s,t-1} + Elections - Elections \times Civil + Civil + \lambda_s + u_{st},$$

where $u_{st} = e_{st} + 2\lambda_s$, and $e_{st} \sim N(0,1)$.

Panel A: OLS Estimates

	$y_{s,t-1}$	*Elections*	*Elections*× *Civil*	*Civil Service*
$\tau = 0$	—	0.2	−0.3	3.2
$\tau = 0.20$	−18.9	−1.9	2.1	0.8
$\tau = 0.50$	−9.7	−1.2	1.3	2.9
$\tau = 0.80$	−7.8	−1.5	1.5	10.2
$\tau = 0.90$	−7.3	−3.0	3.3	16.6
$\tau = 0.95$	−6.0	−2.0	1.7	21.2

Panel B: GMM Estimates, Arellano-Bond Method

	$y_{s,t-1}$	*Elections*	*Elections*× *Civil*	*Civil Service*
$\tau = 0$	—	−0.0	−0.3	−9.1
$\tau = 0.20$	−37.4	−3.0	3.1	−17.2
$\tau = 0.50$	−26.7	−5.5	5.5	−19.1
$\tau = 0.80$	−34.8	−13.4	13.3	−13.3
$\tau = 0.90$	−37.2	−16.8	17.2	15.7
$\tau = 0.95$	−19.8	−7.9	8.1	53.1

Notes: Numbers in the table represent percentage deviations of the given estimates from pre-specified values of the corresponding coefficients. The number of simulations is 1,000. Fixed effects are also estimated, but not reported. $s = 45$, $t = 37$, and the estimating sample consists of the same observations used to estimate Equation 2.1. The *Elections* variable is an indicator variable for the two-year period immediately prior to elections, and the *Civil* variable is an indicator variable for whether a given state in a given year has had civil service in place for its civil service employees.

Appendix B

B.1 Priming Sensitivity

The purpose of this section is to clarify how the priming effects of individualism-collectivism might differ across racial categories.

As in Benjamin, Choi and Strickland (2010), let x be some course of action (such as how much to avoid risks) that an individual chooses to maximize her utility U:

$$U(x) = -w(s)(x - x_C)^2 - (1 - w(s))(x - x_I)^2, \tag{B.1}$$

where x_C is the social "norm," that is, the preferred action prescribed by the individual's ingroup (family or any other social group the individual most commonly associates herself with). x_I is the bliss point of the "pure individualist," that is the individual's preferred action in the absence of any group affiliations. $s \geq 0$ is an ordinal measure that denotes the strength of the individual's "attachment" to, or the affiliation with the ingroup, whereas $0 \leq w(s) \leq 1$ is the weight the individual places on the social norm when choosing her own action. $w(s)$ can therefore be interpreted as the degree of collectivism: the more important the ingroup's opinion in the person's own decision on what action to choose, the more collectivistic that person is. Conversely, $1 - w(s)$ can be interpreted as the degree if individualism: the more important the individual's personal opinion on what action to choose, the more individualistic that person is.[1]

Of course, if the relationship between $w(s)$ and s were linear, there would have been no reason to parameterize s in terms of $w(s)$; one could have assumed, without loss of generality, that s is both the strength of attachment to the group and the degree of collectivism. The reason for differentiating $w(s)$ from s, however, is to let $w(s)$ be a non-linear function of s. I hypothesize a specific form of non-linearity, illustrated in Figure B.1, so that the function can admit the possibility of priming sensitivity to be different for people having different baseline degrees of "attachment" to the group. Specifically, the given shape implies that people with weak or strong group ties (low and high s) are relatively insensitive to being primed on individualism-collectivism: they are already "saturated" with extreme baseline levels of individualism and collectivism, whereas the rest of the people (medium s) are the most sensitive to priming.

This is a hypothesis that I am be able to test indirectly by examining heterogeneity in the priming effects across racial groups. Indeed, Asians and whites respond the least to being primed on collectivism, while blacks and Hispanics respond more. This result is consistent with the evidence that Asians are, on average, the most collectivistic and whites are the most individualistic, which, in terms of the described framework, means that they

[1] I assume that $w(0) = 0$ and $w'(s) > 0$. That is, if the person is not affiliated with any group whatsoever (Robinson Crusoe), there is no one else's opinion to take into account when choosing an action other than the personal opinion; the person is "pure individualist." The more the person is getting attached to the ingroup, though, the more important the social norm becomes in deciding what action to take. In other words, deviating from the group norm causes disutility that is increasing in s, the strength of one's affiliation with that group.

are already "saturated" with high degrees of collectivism and individualism, respectively. They are, as a result, relatively insensitive to the manipulation of the salience of I-C.

The priming mechanism. As an aside, consider how the presented model explains the priming mechanism. Assume that $s = \bar{s} + p$, where $\bar{s}$ is some steady-state value of s (people are "born" with a certain degree of attachment to their ingroup) and p is a situational, context-dependent, manipulable part of the person's attachment to her ingroup. A given person's level of s can thus be temporarily perturbed away from $\bar{s}$ by a positive or negative prime p. This is possible by manipulating the salience of collectivism (positive p) or individualism (negative p) in a person's mind, that is, by priming the person on collectivism or individualism.

Note that Equation 3 implies the optimal action to be:

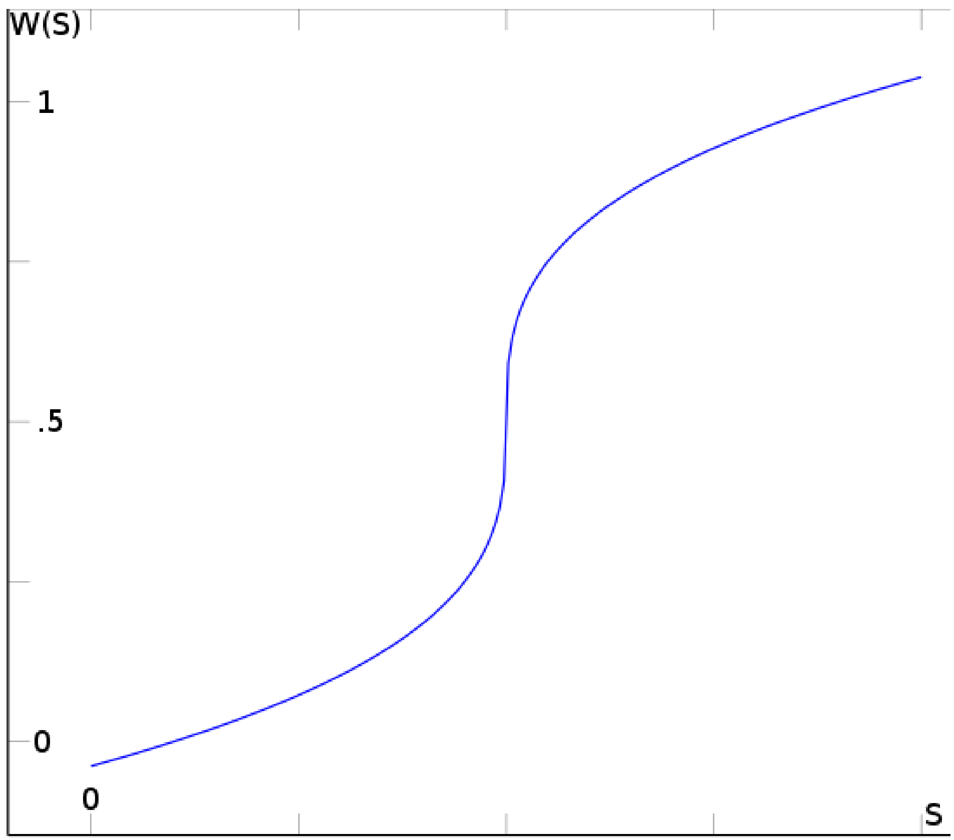
$$x^* = w(s)x_C + (1 - w(s))x_I. \tag{B.2}$$

In words, a person should always choose an action that is a weighted average of her ingroup's preferred action and her own preferred action, the weight being an increasing function of the person's attachment to the group. In terms of the priming effects, this condition implies the following. Since $s = \bar{s} + p$, collectivism prime ($p > 0$) increases s and $w(s)$ and thus moves x^*, the optimal action, closer to x_C, the social norm. Likewise, individualism prime ($p < 0$) decreases s and $w(s)$ and thus moves x^* farther away from x_C, closer to x_I. In other words, priming reveals marginal behavioral effects of individualism-collectivism on observed actions. This is why priming manipulations are a useful experimental procedure for studying how I-C affects observed outcomes.[2]

[2]More specifically, the treatment effect of priming equals, to the first-order Taylor approximation, $x^*(\bar{s}+$

$p) - x^*(\bar{s}) \approx (dx^*/d\bar{s}) \cdot p = w'(s)(x_C - x_I)p$. An implication is that, since $w'(s) > 0$, the sign of the treatment effect of collectivism prime ($p > 0$), $w'(s)(x_C - x_I)p$ depends only on the sign of $x_C - x_I$. So, even if $\bar{s}$, x_C, x_I and $w(\cdot)$ of the sample differ from those of the general population, the directional effects of priming the sample will generalize to the population as long as $x_C - x_I$ has the same sign for both groups.

Figure B.1: Degree of Collectivism, *w(s)*, as a Non-linear Function of the Strength of Attachment to the Ingroup, *s*



Notes: The figure plots $0 \leq w(s) \leq 1$, the weight a person places on her own preferred action versus her ingroup's preferred action (the norm), as a non-linear function of s, an ordinal measure of the strength of the person's attachment to her ingroup. The shape of the function admits the possibility of priming sensitivity to be different for people having different baseline degrees of "attachment" to the group. Specifically, the given function implies that people with weak or strong group ties (low and high s) are relatively insensitive to being primed on individualism-collectivism: they are already "saturated" with high baseline levels of individualism and collectivism, whereas the rest of the people (medium s) are the most sensitive to priming.

B.2 Validity of the Priming Instrument

The pronoun circling task has been commonly used by others in the psychology literature to prime subjects on individualism-collectivism. Still, it would be reassuring to show whether priming subjects on collectivism does indeed make them think more "collectivistically" than a comparable set of subjects primed on individualism. To achieve this, I conducted an experiment that checks the validity of the priming instrument.

The validity check was implemented as a supplemental experiment via Amazon Mechanical Turk in March 2015. None of the participants had participated in or had heard of the main experiment conducted earlier. The goal of the experiment was to test whether making collectivism salient actually makes subjects' collectivistic perceptions more pronounced. These perceptions were categorized as collectivistic based on subjects' answers to five simple tasks, four of which were image-categorization tasks (Figure 3.5) and the fifth was to write ten open statements about oneself (answering the question, "Who Am I?").

Each of the four outcomes of interest—Index 1, Index 2, Alternative Index 1 and Alternative Index 2—was obtained by summing up four leading principal components of five respective variables based on the five tasks. For each of the four indexes, the four leading principal components explain 85 percent of variation across the respective five variables. Four of these five variables are always binary classifications of four-image tasks as collectivistic. The fifth variable differs across the four indexes. For Index 1, the fifth variable is a binary classification of the first of the ten statements as collectivistic. For Index 2, it is a weighted average of binary classifications of all ten statements, whereby the first statement received the highest weight, the second statement received the second highest weight and so on, the tenth statement receiving the lowest weight. For Alternative Index 1 and Alternative Index 2, the fifth variable is the same as in Index 1 and Index 2 respectively, except that a slightly narrower definition of “collectvistic” was used to classify the statements. For Index 1 and Index 2, a statement is considered collectivistic if it states membership in a social or demographic category (e.g., “I am a citizen,” “I am college-educated,” etc.). For the Alternative Indexes 1 and 2, a statement is considered collectivistic only if it states membership in a social category. Statements about personal traits (“I am smart”) and interpersonal relationships (“I am a boyfriend”) are always categorized as non-collectivistic.

The results are presented in Table 3.12. The results of the sample that excludes “weak primes” support the hypothesis that the collectivism prime indeed increases the likelihood of the collectivistic way of thinking in experimental subjects.

B.3 Estimating Present Bias and Discount Factor

As in Laibson (1997), suppose the utility function for different time periods are time separable and take the following form (see also Andreoni and Sprenger, 2012*a*):

$$U(c_t, c_{t+k}) = \frac{1}{\alpha} c_t^{\alpha} + \beta \delta^k \frac{1}{\alpha} c_{t+k}^{\alpha}, \tag{B.3}$$

where $t = \{0, 2\}$ indexes time in weeks, $k = 2$ is the delay between the two possible payments (in weeks) for each intertemporal choice, and c_t and c_{t+k} are experimental earnings. α is the curvature parameter in the Constant Relative Risk Aversion utility form.[3,4] The parameters of interest are β—present bias, and δ—weekly discount factor. A value of $\beta < 1$ indicates present bias, and when $t > 0$, present bias does not influence choice.[5]

The presumed budget constraint subject to which (B.3) is maximized takes the form:

$$(1+r)c_t + c_{t+k} = m, \tag{B.4}$$

where $(1+r)$ is the gross return, and m is the experimental budget for each intertemporal decision (\$15).

Maximizing (B.3) subject to (B.4) yields the tangency condition:

$$\frac{c_t}{c_{t+k}} = \begin{cases} (\beta\delta^k(1+r))^{\frac{1}{\alpha-1}} \text{ if } t = 0 \\ (\delta^k(1+r))^{\frac{1}{\alpha-1}} \text{ if } t > 0 \end{cases} . \tag{B.5}$$

[3] The CRRA utility function is typically given as $\frac{c^{1-\theta}}{(1-\theta)}$, where θ represents the coefficient of relative risk aversion and is related to α as $\theta = 1-\alpha$.

[4] I also assume that Stone-Geary consumption minima, or background consumption levels for both t and $t+k$ periods are zero. As Andreoni and Sprenger (2012*a*) show, estimating the parameters of present bias and discount factor—the parameters of interest here—are not sensitive to restrictions placed on the Stone-Geary parameters (though the α parameter is).

[5] Frederick, Loewenstein and O'Donoghue (2002) review the literature on time discounting and conclude that preferences are largely time-inconsistent, with strong evidence for diminishing impatience (i.e., present bias).

Taking logs of (B.5) results in:

$$\ln(c_t) - \ln(c_{t+k}) = \begin{cases} \frac{\ln(\beta)}{\alpha-1} + \frac{\ln(\delta)}{\alpha-1}k + (\frac{1}{\alpha-1})\ln(1+r) \text{ if } t = 0 \\ \frac{\ln(\delta)}{\alpha-1}k + (\frac{1}{\alpha-1})\ln(1+r) \text{ if } t > 0 \end{cases} . \tag{B.6}$$

By stacking all decision level observations for each subject, it is straightforward to estimate (B.6) by ordinary least squares in two steps. First, I estimate the second part for $t > 0$ (i.e., the decisions from the two weeks vs. four weeks game). Note that k is constant, so $\hat{\delta}$ is easily obtained from the estimated constant, using the estimated α from the risk game. In the second step, I estimate the part for $t = 0$ (i.e., the decisions from the today vs. two weeks game). $\hat{\beta}$ is then imputed from the estimated constant, based on $\hat{\delta}$ from the first step and $\hat{\alpha}$ from the risk game.