

FINANCIAL STOCKS AND POLITICAL BONDS: STOCK MARKET PARTICIPATION
AND POLITICAL BEHAVIOR IN THE UNITED STATES AND BRITAIN

BY

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DISSERTATION

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Abstract

This dissertation investigates the effect of stock market participation on political behavior. Some observers claim that financial assets—stocks and mutual funds—have a causal effect on political behavior. The “investor class theory” asserts that as people invest in the stock market their partisan attachments shift rightward. The “asset effect theory” claims that financial investments increase political interest and participation. I examine these claims with longitudinal data from the United States and Great Britain covering a twenty-year period from the early 1980s through the mid-2000’s. I also examine the effect of financial asset ownership on political attitudes in the United States during the 2008 stock market crash. I find no evidence to support the argument that stock market participation has any causal effect on partisanship, participation, or political attitudes.

For Teresa, Melina, Kara and Neela.

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Chapter 1

Introduction

Until recently, few people played the stock market. At the time of the Great Depression, only two percent of American adults owned stocks. By the early 1950s, this figure had climbed to a mere four percent (Harrington 2008). Even as late as the early 1980s, only one-fifth of American households owned shares of companies listed on U.S. stock exchanges.¹ By 2005, however, one-half of U.S. households and one in three individuals directly or indirectly owned stock market equities (Investment Company Institute and Securities Industry Association 2005).

This trend was repeated in other parts of the world. Roughly one-half of Swedish households and a third in the U.K. were invested in stocks and mutual funds at the start of the new millennium (Guiso et al. 2003). Stockholding increased even in relative laggards such as the Netherlands, Italy, France and Germany to about 15-25% of households.² As a result of this global growth in household stock market participation, more people than ever before became directly exposed to the vicissitudes of the financial markets. The 2008 financial crisis, the most serious global economic catastrophe since the Great Depression, affected investors large and small from Wall Street to Main Street. As governments around the world scrambled to contain the fallout from one of the worst financial crises in history, their actions were judged by voters concerned about both their incomes and their stock portfolios.

The most rapid growth in U.S. household stock market participation occurred during the bull market of the 1990s. The percentage of American adults who owned stocks doubled

¹Directly held stocks and stocks owned through mutual funds, both inside and out of retirement accounts.

²In 2000, these six European states held approximately 90% of total financial wealth in the European Union (Guiso et al. 2003).

during this decade. It previously had taken a full twenty-five years for a change of this magnitude to occur—from 10 percent in 1965 to 21 percent in 1990 (Harrington 2008, p. 12). Observers attribute the growth in stock market participation in the United States since the early 1980s to the lower interest rates paid by bank savings accounts, greater accessibility and reduced costs of mutual funds, and introduction of “defined-contribution” retirement plans such as Individual Retirement Accounts (IRAs) and employer-sponsored 401(k)s (Keister 2005; Nadler 2000a; Guiso et al. 2002; Davis 2009).³

Many new investors doubtless were attracted to the stock market by the remarkable gains of equities in the 1990s. Between 1994 and 1999 alone the Dow Jones Industrial Average increased from a low of 3,600 to roughly 11,500—an increase of nearly 220%.⁴ Tables 1.1 and 1.2 illustrate this dramatic growth in stock ownership in America over the past three decades. These figures represent a conservative estimate of U.S. stock market participation as they include household equity investment but not other popular investments such as bonds or non-stock mutual funds.⁵ Overall, these data show significant growth in overall equity investment as well as significant increases across income and racial subgroups.

Equity ownership increased dramatically for white, black and Hispanic households. In 1983, only 4.3% of black households owned stocks; by 2007 roughly 30% of black households were stockholders. Hispanic stockholding increased more dramatically, rising from 1% in 1983 to 24% in 2007. Moreover, the data show that growth in stock market participation was not limited to the wealthy or middle class, but also included low-income households. Women went from making up just under a third of investors in 1990 to fifty percent by 1999 (Harrington 2008). Over three quarters of American voters in 2000 were stock market investors (Nadler 2000a).

³“Stock market participation” in this specific case refers to investments in a broad range of securities, including stocks, bonds and mutual funds. Equity ownership refers specifically to direct or indirect stock ownership.

⁴It previously had taken a decade (1984-1994) for the Dow to realize an increase of this magnitude.

⁵An early 2008 survey found that 60% of American households with financial assets held own both stocks and bonds (Investment Company Institute and Securities Industry Association 2008, p. 8).

Table 1.1: Characteristics of U.S. households owning stock market equities, 1983

Characteristics	All	White	Black	Hispanic	Low Income
Average age of household (years)	50.3	50.6	43.1	36	47.1
Average education of household head (years)	13.9	13.9	13.2	12.0	12.9
Median income (thousands of 2007 dollars)	44.9	43.7	49.9	81.2	0.0
Median financial net worth (thousands of 2007 dollars)	34.7	37.4	1.1	-54.0	9.6
Median total net worth (thousands of 2007 dollars)	226.7	236.9	70.7	131.1	44.3
Percent equity owners in population (households)	20.4	23.6	6.8	1.0	4.4
Millions of equity-owning households	17.2	16.2	0.7	0.03	0.74

Source: Survey of Consumer Finances 1983, weighted data.

Table 1.2: Characteristics of U.S. households owning stock market equities, 2007

Characteristics	All	White	Black	Hispanic	Low Income
Average age of household (years)	50.7	52.1	45.1	43.1	55.6
Average education of household head (years)	14.3	14.3	14.4	13.4	13.2
Median income (thousands of 2007 dollars)	56.6	54.5	60.7	64.8	0.0
Median financial net worth (thousands of 2007 dollars)	11.9	24.2	-21.5	-43.2	9.4
Median total net worth (thousands of 2007 dollars)	288.9	310.0	131.0	198.1	119.5
Percent equity owners in population (households)	51.1	57.5	29.8	24.2	13.5
Millions of equity-owning households	59.3	49.3	4.3	2.6	23.2

Source: Survey of Consumer Finances 2007, weighted data.

Expansion of mutual fund offerings and introduction of defined-contribution pension funds similarly increased stock market participation in Europe. Privatization of public enterprises in the 1980s and 1990s, the aging populations of many European states, and European Union directives on financial integration and liberalization also contributed to this growth (Guiso et al. 2003). Even countries with relatively lower overall stock market participation experienced significant growth over this period. Guiso et al. (2005) report that in the mid-to-late 1990s as many as one-third of Italian households were unaware of the existence of stocks, mutual funds and investment accounts. In 2000 only 7% of Italian households directly invested in stocks (Guiso et al. 2003, p. 138).⁶

Table 1.3 shows the changes in Italian household equity ownership over nearly a 20 year period. I focus on just the riskiest investments—stocks and stock mutual funds.⁷ Even with this narrow focus, equity ownership in Italy between 1987 and 2006 almost doubled from 5.7% to 9.4%. Moreover, this growth in financial investments was not limited to any one group. In terms of overall percentage change, stock investment grew most rapidly for the least educated Italians (from 2.3% to 4.5%). These changes in household portfolios were repeated elsewhere as stock market participation became much more common across households throughout the developed world (McCarthy 2004). By the mid-2000s, financial investments were no longer limited to the well-heeled and highly-educated.

⁶Total Italian household participation in 2000, including indirect investments in mutual funds, retirement accounts and other managed assets, was 15%.

⁷Excludes bonds, bond mutual funds, and money market mutual funds.

Table 1.3: Characteristics of Italian households owning stock market equities, 1987 & 2006

Characteristics	1987	2006
Average age of household (years)	48.6	52.8
Education of household head (%)		
Below high school	2.3	4.5
High school	13.1	14.2
College	21.5	23.5
Post-graduate	32.8	36.6
Median income (thousands of euros)	44.3	43.5
Median financial net worth (thousands of euros)	33.7	40.0
Median total net worth (thousands of 2007 dollars)	201.3	347.9
Percent equity owners in population (households)	5.7	9.4

Source: Surveys of Household Income and Wealth, weighted data

1.1 The puzzle

By the late 1990s, U.S. political analysts began to hypothesize about the political consequences of this dramatic increase in American household stock market participation. In the United States, the fastest growing group of new investors were young people and lower middle income families—core Democratic Party constituencies. Surveys appeared to show that investors were more likely to vote and more likely to lean Republican than non-investors (Glassman 2001). “Asset ownership was associated with increased G.O.P. affiliation among voters who were black, single, young, low-income, and female—all significant elements of the Democratic coalition” (Nadler 2000a, p. 1). Conservative policy analysts began to describe the growing ranks of stock market participants as a new “investor class” (Glassman 1999; Nadler 1999).⁸ On average, these investors had relatively limited portfolios. One-quarter of household portfolios were less than \$5,000; the median household (in terms of income) had only about \$15,000 in financial assets.⁹ Based on data from a 1999 Rasmussen survey, however, investor class advocates believed that political behavior began to change with portfolios as small as \$5,000 (Norquist 2004).

Conservative analysts developed the “investor class theory,” claiming that acquisition of stock market assets has a subsequent effect on their owners’ partisanship, participation, and political attitudes. Proponents asserted that stock market investments make investors more conservative in their political views, vote more often, and more likely to support the Republican Party (Glassman 2001; Nadler 2000a, 2005). These lay theoreticians, however, were not alone in drawing a connection between financial assets and politics. Scholars researching asset-based welfare policies in the 1990s also hypothesized about the effects of assets on political behavior (Sherraden 1991). Although these analysts made no claims about the effect of asset ownership on partisanship, they did suggest that financial assets

⁸Credit for this term is attributed by Ponnuru (2004) to Lawrence Kudlow, a former Reagan administration official and conservative print and television political commentator.

⁹Zogby (2008) finds relatively small portfolios even among self-identified American investors. Three in five have portfolios less than \$100,000; two in five have less than \$50,000.

have a positive effect on political interest and participation. Schreiner and Sherraden (2006) suggest “it is possible that the social and political effects of [asset] ownership matter even more than the individual economic effects” (p. 6).

Political scientists, however, offer different predictions. The partisanship literature finds that partisan attachments are very sticky and resistant to change. Green et al. (2002) liken party identification to a form of social identity similar to religious affiliation. Although a Catholic may not entirely agree with the Pope, it is relatively unlikely she will switch “parties” to become a Protestant. Likewise, after people reach young adulthood (roughly thirty years of age) party identification generally persists throughout the rest of their lives. The economic voting literature finds that personal financial interests have little to no effect on voting behavior. Much more important are perceptions of national economic conditions (Lewis-Beck and Nadeau 2000). Finally, the self-interest literature finds that personal economic welfare generally has little effect on attitudes. Apart from a few notable exceptions, neither subjective nor objective measures of self-interest appear to have any substantive effect on political attitudes (Sears and Funk 1991).

1.2 The question

Do financial assets influence political behavior? Does stock market participation affect partisanship, participation and political attitudes? A large literature finds that financial resources shape political participation (Campbell et al. 1960; Wolfinger and Rosenstone 1980; Verba and Nie 1972; Verba et al. 1995; Leighley 1995). This literature, however, largely focuses on *current income*; we know relatively little about the effect of *financial assets* on political behavior. Income refers to the flow of money (wages, salaries, government transfer payments, pensions, interest, dividends) over time (Keister 2005; Shapiro 2001). Financial assets such as stocks and mutual funds are part of the stock of stored-up wealth that is saved from

income (Sherraden 1991; Nam et al. 2008).¹⁰

People generally use income to pay for daily necessities such as shelter, food and clothing. Assets, however, represent stored up potential that allows their owners to make purchases (such as education, business property, or a home) which some analysts contend can improve their social and political status (Nam et al. 2008; Shapiro 2001; Keister 2005). Financial contributions are one important way that people are able to increase their visibility in the political process (Miler 2007). People arguably are more likely to make decisions about contributions based on their stored-up assets rather than current income. This distinction is important as income is not perfectly correlated with financial wealth (Nam et al. 2008).¹¹ Thus, a purely income-based approach might understate the effect of financial resources on political behavior. At present, political science knows very little about the relationship between financial assets and political behavior.

1.3 Motivation

In less than a decade, the investor class theory moved from the pages of conservative political journals to become the intellectual centerpiece of landmark domestic policy in the White House. Nadler (2005) describes President George W. Bush as a “disciple” of the investor class theory. The White House wrapped the investor class theory in the rhetoric of the “Ownership Society,” which emphasized the individual’s autonomous control of her own life through ownership of homes, businesses, and financial investments (Avsar 2008; Zogby 2008). Privatization of Social Security was the top domestic issue for President Bush at the start of his second presidential term in 2005. The Bush plan called for allowing workers to put a portion of their Social Security retirement contributions into private accounts invested

¹⁰Total wealth is the value of financial wealth plus tangible (or “real”) assets such as homes, cars, and real estate. Net worth is total wealth less total debts (mortgage, credit cards, etc.).

¹¹For example, based on data from the 1994 U.S. Panel Study of Income Dynamics, Conley (1999) found white American families in the top income quintile had a median net worth of \$308,000, while African-American families in the same income bracket had a median net worth of only \$114,600.

in the stock market. Philosophically, the Ownership Society agenda was consistent with the Republican Party's goal of reduction of long-term dependence on government programs (Davis 2009). Apart from the merits of the proposal to address the long-term solvency of this government entitlement program, however, Social Security privatization also had partisan motivations.

Among policy changes championed by investor class activists, such as the expansion of education, retirement and medical savings accounts, privatization of Social Security was the biggest prize. "Social Security reform is the key goal of an investor-class politics, since it would bring almost the entire population into the class" (Ponnuru 2004, p. 31). Ken Mehlman, President Bush's campaign manager in 2004, claimed that policies enacted under the ownership society agenda would create a pro-Republican Party constituency similar to what emerged for the Democratic Party after enactment of New Deal programs (Barnes 2004). "While the New Deal and the Great Society addressed the needs of the nation's poor and forgotten through an aggressive welfare state, the Ownership Society promised liberation for millions through jobs, home ownership, and a stock portfolio" (Zogby 2008, p. 39). The political calculus was quite simple for conservative activist Grover Norquist. Social Security privatization would create more stock owners and more stock owners would mean more Republicans, which would make the Republican Party "a true and permanent national majority" (Calmes 2005).¹²

President Bush invoked the investor class in stump speeches to promote his proposal for Social Security reform in spring 2005: "I believe everybody's got the capability of being in the investor class. I believe everybody should be allowed to watch their own assets grow, not just a few people. I like the idea of somebody opening up their statement on a quarterly basis and watching their asset base grow. It basically means they're going to pay

¹²Republicans were not the only ones who believed in the investor class theory. Former New York attorney general Eliot Spitzer was a strong investor class proponent and reportedly tried to get John Kerry during the 2004 presidential campaign to argue that the Democratic Party was the better voice for the investor class than the Republican Party (Continetti 2005).

closer attention to the fiscal policy of the government” (Bush 2005). Although President Bush ultimately was unsuccessful in his efforts to privatize the Social Security program, the question remains whether the Republican Party’s beliefs about the relationship between financial assets and political behavior were well-founded. Would Social Security privatization have created millions of new Republican voters?

1.4 Significance

Although the “Ownership Society” faded with the end of President Bush’s term in office, the investor class theory lives on. Pollster John Zogby’s surveys routinely include a question about whether respondents consider themselves as a member of the “Investment Class” (Zogby 2009). James Pethokoukis, former economics columnist and business editor at *U.S. News and World Report*, blogged in October 2008 that “there are at least two pretty effective ways to turn someone into a Republican: (1) get them married with kids and (2) get them to invest in the stock market” (Pethokoukis 2008). During the 2008 presidential campaign, conservative political commentator Lawrence Kudlow exhorted the Republican presidential candidate to reach out to the investor class (Kudlow 2008). Conservative activist Grover Norquist’s 2004 proclamation in *The Washington Monthly* magazine of a \$5,000 threshold for the effect of stock ownership on political behavior gained currency in the mainstream press. A November 2008 *BusinessWeek* article on the investor class notes “many pollsters define [the investor class] as those who have more than \$5,000 invested in the stock market” (Herbst 2008).

Although the proponents of the investor class theory were primarily focused on the United States, this theory has obvious global implications. If correct, the emergence of functioning stock markets may be as critical to the development of political and economic systems as political institutions. “[New] stockholders may acquire not only new financial instruments (stocks) but also new attitudes towards capitalism, private property, and reforms

that potentially enhance the value of corporations, liberalise labour markets and improve the functioning of the financial sector” (Guiso et al. 2003, p. 126).

1.5 Contribution

This dissertation is “an inquiry into the political economy of individual citizens” (Kinder and Kiewiet 1981, p. 130). Despite the global increase in stock market participation over the past thirty years, political science knows little about the effect of financial asset ownership on political behavior. Most efforts to date have focused on macro-level interaction between markets and politics. This dissertation responds to the exhortation by Scheve and Slaughter (2001) to “consider asset ownership” in the study of political economy. Economic models of political behavior are founded on the assumption that self-interest is *the* prime motivator of human behavior (Mansbridge 1990). Surprisingly, the overwhelming conclusion of the self-interest literature is that individual economic concerns have little effect on political behavior. Prior research on economic self-interest, however, largely focused on the effect of *negative* financial circumstances such as unemployment or inflation on political behavior (Sears and Funk 1991). This dissertation is among the first to investigate the possibility of “wealth effects” on political attitudes and actions. A large literature emphasizes the relationship between financial resources and political participation. This study expands the conceptualization of resources to include financial assets, an important part of household and individual wealth (Keister 2005).

This dissertation seeks to contribute to our understanding of comparative political behavior by investigating the effect of acquisition of stocks and mutual funds on political behavior in the United States and Britain. The American and British political systems have a great deal in common. Both countries share “relatively well-defined incumbents and oppositions, (approximate) two-party systems, and single-actor (executive) incumbents” (Anderson 2007, p. 283). Moreover, both political systems have a well-defined left-right ideological dimension

regarding economic and social policies, so investors can reasonably be expected to be able to identify which parties are presumed to be more “pro-market.”

This dissertation also seeks to contribute to policy discussions about the non-economic effects of asset ownership (Sherraden 1991; McKernan and Sherraden 2008). Research to date on this subjective is inconclusive largely due to challenges in research design. This dissertation seeks to elevate the level of empirical discourse on this important topic as well as invite additional research. Finally, this dissertation responds to the argument that political science needs to pay attention to debates in both academic journals and editorial pages. This concern is particularly acute in cases in which non-peer reviewed research is used to justify significant policy issues (Muir 1999). Reviewing the literature on this topic, Kaustia and Torstila (2008) conclude that “there is virtually no empirical research on the interplay of political views and investment behavior.” This dissertation begins to fill this void.

1.6 Organization of chapters

This dissertation has the following organization. Chapter 2 reviews the investor class and asset effect theories and examines the empirical evidence for their respective claims. This chapter situates these theories in political science literature and develops a theory of portfolio politics. It concludes with research questions and hypotheses concerning the effect of financial assets on political behavior. Chapter 3 explains the methodology and research design used to test a proposed theory of portfolio of politics. Chapters 4 through 6 analyze the effect of stocks and mutual fund ownership on partisanship, participation, and political attitudes. Chapter 7 analyzes the effect of financial asset ownership on attitudes in the United States during the financial crisis of 2008. In each of these chapters I review prior research on this topic and offer direct empirical tests of the investor class theory. Chapter 8 summarizes these findings and discusses implications for political science, policy and offers recommendations for future research.

Chapter 2

Theory

In this chapter I review the theoretical claims about the effect of financial assets—stocks and mutual funds—on political behavior. I begin with analysis of the investor class theory, which argues that as people become investors they vote more frequently and become more politically conservative. I also review a related proposition, the asset effect theory, which contends that accumulation of financial assets leads to greater political interest and participation. I find insufficient empirical evidence in the literature to support either theory. I offer an alternative theory of how assets shape political behavior grounded in the literature on self-interest. I review the limited social science literature on the relationship between financial assets and political behavior. The chapter concludes with research questions and hypotheses about the effect of financial assets on partisanship, participation, and political attitudes.

2.1 Definitions

“Asset” is a very slippery concept (Nam et al. 2008). Sherraden defines assets in terms of property: a right or claim to concrete or abstract property enforceable by custom, convention, or law (Sherraden 1991, p. 100). Conversely, Lindblom defines property in terms of assets: a set of rights to control assets—to refuse use of them to others, to hold them intact, or to use them up (Lindblom 1977, p. 26). “Asset” frequently is used interchangeably with closely related concepts such as investments, resources, ownership and wealth. In this dissertation, I focus on financial assets—stocks and mutual funds. Sherraden (1991) suggests this focus

on financial assets allows theory to more directly relate to economic theories and provides a practical focus for policy.

A great deal of government activity and social policy focuses on another type of investment not addressed in this dissertation—home ownership. The family home represents the largest financial investment for most households. There are important reasons, however, to make a distinction between financial and real assets such as owner-occupied housing. Aside from being an investment, the family home offers shelter and other current services to its owners (Keister 2005). Thus, the decision to purchase a primary residence arguably is driven by different motivations than the decision to invest in stocks and mutual funds. Whereas a home addresses current needs, financial investments require a deliberate decision to delay current consumption for future goals, such as retirement and college education. Investment in stocks and mutual funds offers both the promise of gain and the risk of loss.¹

Financial assets are fungible and can be used for a wide variety of purposes, such as campaign contributions. The social and political status gained from wealth arguably stems from this potential to convert financial assets into political power. Conversely, the economic value inherent in a house is not as readily obtained. To realize its economic value a house must first be sold, which requires the abdication of the non-economic benefits of home ownership.² A final distinction concerns the life cycle of asset acquisition. People acquire real and financial assets at different points during their lifetime. The life cycle model of household portfolios holds that younger households first acquire homes and then later after they've accumulated enough savings invest in stocks and bonds (Keister 2005). This distinction is further underscored by the fact that younger people tend to participate in politics at lower

¹Although housing values fluctuate over time, a reduction in appraisal value does not necessarily reduce a home's utility and is only realized if the house is sold.

²Political scientists offer contrary predictions about the effect of specific assets such as real property on political behavior. Hirschman (1970) and Alt and Gilligan (1994) contend that owners of specific assets are more likely to become involved politically because of the high cost of moving their investments. Other scholars, however, contend that owners of non-specific assets, such as stocks and mutual funds, have greater leverage over the political process due to their ability to harm the economy by moving their investments (Lindblom 1977; Bates and Lien 1985; Bates 2001; North and Weingast 1989; Boix 2003; Winters 1994).

levels than other groups (Wolfinger and Rosenstone 1980). “Assets” in this dissertation refer solely to stocks and mutual funds.

2.2 Investor class theory

The investor class theory emerged in the 1990s from a “cottage industry” of think tanks and conservative American political journals (O’Sullivan 2000; Continetti 2005). A lay theory of politics, it is grounded in fundamental beliefs about the role of self-interest to motivate behavior. The investor class theory contends that ownership of stocks and mutual funds “[acts] as an independent variable causing marginal shifts toward conservatism” (Nadler 1999, p. 23). Two mechanisms explain this shift. First, after an individual becomes a stock market investor, she needs *specialized sources of information* to track her financial investments. Nadler (2000a) cites investor surveys by the NASDAQ stock exchange that show investors change their sources of information as they accumulate financial assets. Nadler contends that the conservative editorial pages of popular financial news sources like *The Wall Street Journal* or *Investor’s Business Daily* might shape investors’ political views.³

Second, stock market participation *alters economic interests*. The financial interests of new investors change over time to become more oriented towards pro-market and “investor-friendly” policies that protect their investments (Nadler 2005; Ponnuru 2004). Nadler (1999) cites a survey that finds 65.7% of investors favor a capital gains tax cut compared to 45.9% of non-investors. He found higher levels of support for a capital gains cut among investors across racial, occupational, gender and party lines. Investor class advocates also contend that as people become more involved in managing their own investments, they become more wary of government attempts to direct or redistribute their financial assets.

According to the investor class theory, therefore, stock market participation has a causal effect on partisanship, participation and political attitudes. Proponents claim that own-

³Roberts (1990) argues that investors have incentives to consume *both* economic and political information that might affect their economic investments.

ership of stocks and mutual funds makes investors more conservative and more likely to lean Republican (Glassman 2001; Nadler 2000a, 2005). Ponnuru (2004) claims that “new investors were more Republican than they had been before they started investing.” Nadler (2005) argues that the probability of voting for the Republican party is higher for someone who invests in the stock market than someone who does not. Investor class theorists also claim investors are more likely to vote than non-investors (Nadler 2000a; Glassman 2001; Ponnuru 2004).⁴ For example, Ponnuru (2004) cites a poll that finds 70% of voters in the 2000 presidential election owned stock. He also notes that these voters supported George W. Bush over John Kerry by five points. Finally, the investor class theory claims that after acquiring new sources of information about his investments, the investor “changes his opinions on a variety of partisan and policy questions to favor market-based solutions and their political advocates” (Nadler 2000b). Glassman (1999) asserts investors are more interested in protecting their assets than non-investors and more opposed to income redistribution. Ponnuru (2004) claims that you can “[change] the mindset of voters by making them investors.”

Ponnuru hypothesizes that the type of investment affects the degree of impact on political behavior. Direct investment in stocks has a greater effect on political beliefs than more passive investments such as mutual funds or bonds. “Owners of individual stocks were more conservative than people who only owned 401(k)s, who were in turn more conservative than passive recipients of income from bonds” (Ponnuru 2004, p. 30) Moreover, the size of stock market investment might make a difference. Conservative activist Grover Norquist claims that if someone owns just \$5,000 in stock they are 18% less likely to be a Democrat and more likely to be a Republican. Norquist contends this relationship between stock ownership and Republican party identification exists across major demographic groups—race, gender, age, and income (Norquist 2004).

⁴Although not an “investor class theorist,” Davis (2009) also makes the (unsubstantiated) claim that “shareholders [are] substantially more likely than nonshareholders to vote in elections.”

Investor class theorists are careful, however, to hedge these strong claims. Norquist (in Nadler (2000b)) cautions the shareholder effect is not automatic:

When a wage worker enters the investor class through a work-based 401(k)...his opinions on vouchers, free trade, and entitlement privatization are no likelier to change overnight than his party affiliation. But as his plan assets grow, so do his expectations for their performance. He may at first study nothing more than the plan materials his employer provides. But over time, he actively seeks sources of information that will maximize his efficiency in his new vocation: that of a capitalist. It is this pursuit that changes his opinions on a variety of partisan and policy questions to favor market-based solutions and their political advocates.

The investor class theory fails to indicate when we should expect these changes to occur. Proponents simply contend that the longer investors are in the market, the more conservative they will become (Nadler 2000b; Ponnuru 2004).

Regarding the central claim that investments affect party identification, proponents contend they do not argue that all people who become investors will become Republican, but rather “will be several points more likely to vote Republican” (Nadler 2005). Glassman (1999) asserts that in political races with increasingly tight margins this difference in the aggregate can make the difference between a Republican and Democratic victory. Nonetheless, the investor class theory overlooks a fundamental characteristic of markets—they go up and down.⁵ People might have different reactions to market fluctuations. A theory of portfolio politics should specify how investors will react politically under conditions of gain *and* loss.⁶

⁵As American financier J. P. Morgan famously replied when asked by someone what the stock market would do that day: “It will fluctuate, young man. It will fluctuate.”

⁶O’Sullivan (2000) muses that a market downturn could affect investor political behavior. “As stocks fall, there will be a general drift in the investor class away from risk and towards security—a drift likely to take them in a Democratic direction.”

2.2.1 Existing evidence

Scant empirical evidence exists to support the claims that financial asset ownership causes changes in political behavior. The investor class theory is based entirely on cross-tabulations of cross-sectional surveys. For example, Nadler uses detailed cross-tabulations of investors and non-investors regarding party identification and attitudes toward a capital gains tax cut (Nadler 1999, 2000a). Ponnuru (2004) cites a January 1999 Rasmussen Research poll that finds differences in party identification between investors and non-investors. Male non-investors with income between \$20,000 and \$40,000 identified with the Democratic Party by twelve percentage points. Male investors in the same income range supported the Republican Party by three percentage points. The same survey showed that one-fifth of black investors supported the Republican Party versus six percent of black non-investors. Similar type of empirical evidence is offered in every other study used to support the investor class theory (Faucheux 1999; Glassman 1999; Nadler 2000b; Deane and Balz 2003; Nadler 2005; Zogby 2009).

There are three obvious problems with this approach. First, single point in time surveys cannot tell us anything about the causal effect of investments on change in behavior. Unless information is available about political behavior before and after the acquisition of investments, there is simply no way to determine whether investments shape political behavior. Second, these analyses do not account for importance differences between investors and non-investors that may also be related to differences in partisanship, participation or attitudes. Age, education, occupation, and income are well-known predictors of political behavior and stock market participation (Campbell et al. 1960; Wolfinger and Rosenstone 1980; Verba and Nie 1972; Verba et al. 1995; Leighley 1995; Campbell 2006). We cannot say definitively whether asset ownership shapes political behavior independent of differences in these important variables.

Third, we know nothing about the causal relationship between investments and political

behavior. Politics plausibly may shape investment behavior. Researchers argue that socially active households are more likely to own stocks (Hong et al. 2004) and that investment decisions are shaped by neighbor effects (Hong et al. 2005; Ivković and Weisbenner 2007; Brown et al. 2008). Guiso et al. (2005) found that people with higher levels of general trust are more likely to buy stocks. Keister (2005) found that households that regularly participate in religious services accumulate greater levels of assets. This demographic also constitutes a significant part of the base of the Republican Party.

Continetti found the investor class theory to be “long on assertion and supposition but short on evidence” (Continetti 2005, p. 29). An October 2003 *Washington Post* article on the investor class noted that prominent Democratic pollster Stanley Greenburg found no evidence of an investor effect on political behavior (Deane and Balz 2003). Based on the results of numerous polls on this topic, Zogby concluded that “investors aren’t anyone’s constituency...it’s not *politics* they care about so much as it’s *fairness* for workers and communities” (Zogby 2008, p. 40). Davis concurred, cautioning that investors are not “doctrinaire devotees of free market policies, and [are] largely indistinguishable from non-shareholders on a range of attitudinal issues” (Davis 2009, p. 221). Nonetheless, conclusive empirical evidence regarding the investor class theory requires longitudinal data to identify behavior before and after the acquisition of financial assets. Moreover, it requires a robust research design to isolate the effect of financial assets from other variables that also shape political behavior. Therefore, it remains an open question about whether financial assets affect political behavior or not.

2.3 Asset effect theory

These observers were not the only ones to hypothesize about financial assets and political behavior. In the early 1990s, social policy researchers concerned about the effect of changing U.S. social welfare policies on the poor also began to consider the potential effect of

assets on behavior. These scholars developed the “asset effect theory,” hypothesizing about the potential positive, non-economic effects of asset accumulation (Sherraden 1991).⁷ The asset effect theory predicts assets affect a broad array of social outcomes—economic status, social well-being, civic engagement, child welfare, and health (Lerman and McKernan 2008). “Assets improve economic stability, connect people with a viable, hopeful future; stimulate development of human and other capital; enable people to focus and specialize; provide a foundation for risk taking; yield personal, social and political dividends; and enhance the welfare of offspring. Simply put, people think and behave differently when they are accumulating assets” (Sherraden 1991, p. 148).

Sherraden (1991) asserts that assets provide resources and incentives to change political behavior. The accumulation of assets today leads to expectations of greater assets in the future.⁸ This expectation of improved future economic welfare has a feedback effect on current social and psychological welfare (Schreiner and Sherraden 2006). “People with assets have both greater incentives and greater resources to participate in the political process. Wealth leads to a greater effort to protect property, and ultimately, to greater political participation...People with assets become politically active in protecting those assets” (Sherraden 1991, p. 165).

Paxton (2001) hypothesizes that asset ownership leads to greater political interest, civic participation in local community groups and social influence. Assets increase their owner’s stake in society, elevate the seriousness with which they are regarded by others, and augment political power through enhanced social status. Lerman and McKernan (2008) hypothesize that asset ownership increases voting, political interest, and political knowledge. Sherraden

⁷The asset effect theory appears to have developed completely independent of the investor class theory. The only apparent connection between these two camps is a September 2000 conference at Washington University in St. Louis. Richard Nadler, a key investor class theorist, presented a paper (Nadler 2000a) at this conference organized by Michael Sherraden, who developed the asset effect theory in Sherraden (1991).

⁸Nadler (1999) similarly argues that financial investments shape the individual’s orientation toward the future and her preferences over government policies. Grover Norquist, president of Americans for Tax Reform argues that “[investment] changes your time horizon, over time it changes your party affiliation, and it also changes what you read and what you watch on TV” (Deane and Balz 2003).

suggests “it is possible that the social and political effects of [asset] ownership matter even more than the individual economic effects” (Schreiner and Sherraden 2006, p. 6). Like the investor class theory, however, the asset effect theory also fails to consider the conditional effect of pecuniary gain or loss on political behavior. Moreover, it does not make any predictions about the time lag required before the purported effects of financial assets on political behavior take effect.

2.3.1 Existing evidence

Little empirical evidence exists to support the contention that asset ownership affects political outcomes. Although generally much more rigorous than the investor class literature, researchers acknowledge the empirical studies in the asset effect literature suffer from their own methodological shortcomings such as selection biases and omitted variables (Lerman and McKernan 2008). Moreover, the implications of the asset effect theory are largely untested with regards to stock market investments. Assets in this literature generally are operationalized as home ownership or cash savings.⁹ Presumably, this is driven by the relative abundance of data on home ownership and cash transfer programs and comparative dearth of information about stocks and mutual fund ownership. As I argue in section 2.1, however, people buy homes and financial assets for different reasons. These distinct underlying motivations potentially have differential impacts on behavior.

Bynner (2001) appears to offer the only direct investigation into the effect of financial assets on political behavior. This study used data from the National Child Development Study, a longitudinal survey that tracks from birth to adulthood a group of 16,500 individuals born in Great Britain during the week of March 3-9, 1958. The surveys administered at ages 23 and 33 inquired about ownership of savings and investments. (Unfortunately, no distinction was made between types of investments.) Bynner compared information about asset ownership at age 23 with information collected at age 33 on various measures of health,

⁹For example, see Rossi and Weber (1996) and DiPasquale and Glaeser (1999).

parenting, labor market participation, and political behavior.

The survey queried respondents' level of political interest, political cynicism, and voting behavior. One interesting feature of the research design of this study is the disaggregation of the effects of assets on behavior by gender. Bynner (2001) found that financial investment is a strong predictor of political interest for men, but neither savings nor investments had any effect on voting or political trust among men. Financial investments similarly had no effect on women's voting, but did have a predictive effect on women's political interest. Moreover, both savings and investments had a positive effect on political trust for women. Bynner, however, offered no theoretical explanation for these findings.¹⁰

Moreover, this study suffers from serious methodological challenges. The study does not sufficiently account for the mechanism that explains why some people acquire financial assets while others do not. This is important because factors that influence the likelihood of financial asset ownership such as gender, education, and race also shape political behavior. Furthermore, there is a lack of data about political behavior prior to acquisition of financial assets at age 23. We cannot be certain of the effects of assets on behavior without some indication of behavior before these assets were acquired. Finally, the research design fails to clearly isolate the main independent variable of interest—financial asset ownership. Indeed, Bynner acknowledges the challenges of making valid causal inference based on a non-experimental study (Bynner 2001, p. 32).

In sum, the empirical evidence is very thin for the asset effect theory regarding political behavior. This is due to the dearth of good data and limitations of research design used in the empirical investigations to date. Moreover, these studies fail to address the mechanisms that affect the likelihood of asset ownership. Scholars acknowledge the dearth of strong empirical evidence for the theory (Paxton 2001; Bynner 2001; Lerman and McKernan 2008). “The body of knowledge on asset effects is still in an early stage of development” (Sherraden

¹⁰Burns et al. (2001), however, would suggest the possibility of separate processes through which investments affect the political behavior of men and women.

and McKernan 2008, p. 212).

In essence, the asset effect theory and the investor class theory make the same argument: as people acquire assets, they become more involved in the political process to protect their property. Both theories, however, fail to provide sufficient microfoundations to make this case. The threads connecting economic self-interest and political behavior are not well delineated. Moreover, these theories do not engage extant theories of political behavior in the political science literature. In the following section, I offer an alternative theory of portfolio politics to bridge this gap.

2.4 A theory of portfolio politics

In this section I develop a theory of how financial asset ownership shapes political behavior. I argue that stocks and mutual funds shape political behavior through three mechanisms: 1) creation of a vested interest in political actions that may affect economic welfare; 2) reduction of the cognitive capacity required to act rationally; and 3) increase in causal clarity between individual economic well-being and government action. Furthermore, I contend these mechanisms are mediated by two factors. First, people care more about future returns than past gains. Second, under specific conditions political behavior is motivated by pecuniary losses, but not by financial gains. Finally, I hypothesize about the effects of financial assets on partisanship, political acts and attitudes.

2.4.1 Vested interest

The central tenet of the investor class and asset effect theories is that political behavior follows economic self-interest. The key assumption is that investors will develop attitudes and take actions that protect and enhance (if not maximize) the value of their investments. Financial assets will affect political behavior, however, only if the individual has a *vested interest*, namely the perception of a personal consequence or relevance in their financial

investments (Crano 1997a). Self-interest is not the same as vested interest. Sears and Funk (1990a) define self-interest as the short-to-medium term impact of an issue (or candidacy) on the material well-being of the individual's own personal life or that of her immediate family.¹¹ “[People] who would benefit materially from the implementation of a social policy are expected to have more favorable attitudes of a social policy than are people who would not” (Miller 1999, p. 1054). Lewin (1991) characterizes self-interest as “[thinking] only of the consequences [of a given action] for oneself” (page 24). Vested interest, however, involves a deeper personal connection.

Lau and Heldman (2009) contend that a fundamental challenge of economic models of political behavior is that self-interest is not always obvious or widely understood. Lewin (1991) asserts that people sometimes don't know how to calculate their own self-interest or simply sometimes make mistakes. In order to have a vested interest in an object it must be “hedonically relevant,” namely that there are “perceived consequences of the attitude object for the daily life of the respondent” (Lehman and Crano 2002, p. 101). It is possible for two people to have identical objective self-interests in an object, but yet diverge in their subjective interests regarding the same object. Crano (1997a) illustrates this point using the example of opposition to forced busing in the 1970s. A common proxy for self-interest regarding school integration was the presence of school-aged children in the household. These children presumably would be directly affected by forced busing. Not all white parents of affected school-aged children, however, were necessarily opposed to busing black children to integrate schools. In fact, it is possible that highly vested parents existed at both extremes (for and against) regarding racial integration, which would yield contradictory attitudes toward busing.

I contend that financial assets meet this acid test of vested interest. Money is necessary both for the “mundane” activities of daily life as well as the special occasions (e.g. emer-

¹¹This definition focuses on the proximal, tangible aspects of well-being that directly affects an individual, excluding non-material interests to align it with common usage—what's in it for me? (Sears and Funk 1991, p. 64).

gencies, weddings, college). People generally have great incentive to pay attention to issues that affect their financial livelihood (Lockerbie 2008). Although all investors by definition are self-interested, however, not every investor necessarily perceives a vested interest in how politics affects their financial investments. Investors are not homogenous and come to the stock market with a variety of motives and levels of risk tolerance which may mediate any effects of financial asset ownership on their political behavior. A direct owner of a specific security arguably has a greater vested interest in how government actions affect the political fortunes of the company she's invested in than does another investor who passively owns shares in that same company through a mutual fund. Moreover, two investors may both be invested in the same mutual fund, but may differ in their vested interest in tax policy if one is invested through a tax-sheltered account and the other through a taxable account.

Thus, it is not enough to simply own financial investments; they have to hold meaning for the owner. This may be related to the size of investments; the greater the size of the stock market portfolio, the more vested a person may be in the outcomes. The motivation behind investment may also play a role in vesting. The more specific the reason for investment, the more likely someone is to be concerned about external factors that may affect their economic welfare. For example, a person is likely to have a greater vested interest in assets saved up for college, retirement or a major purchase than general purpose savings. Relatedly, the immediacy of need may play a factor. Two people saving for retirement may both be "vested" in their 401(k) accounts, but likely will vary in intensity of interest in retirement savings policies depending upon their proximity to retirement age.

Crano and colleagues found that when self-interest and vested interest are properly delineated, the effects of material interests on political behavior are readily apparent (Sivacek and Crano 1982; Crano 1997a,b; Lehman and Crano 2002). "[T]here should be greater congruence between policy-relevant attitudes and policy endorsements of highly vested (as opposed to nonvested) individuals...highly vested *attitudes* are stronger and therefore more likely to be expressed through behavior or policy endorsement" (Lehman and Crano 2002,

p. 103). Highly vested interests shape attitudes toward government employment policy, national health insurance and forced busing. Unlike theories of self-interest, which assume a direct effect of material interests on political behavior, vested interest posits a moderated effect on attitudes without prediction of outcomes. Although I use the two terms interchangeably, all subsequent references to self-interest in this chapter refer to the stricter variant of vested interest.

2.4.2 Cognitive capacity

The second mechanism through which financial assets shape political behavior is the reduction of the cognitive capacity required to make informed judgments about the economy. Economic models of political behavior assume that voters rationally evaluate the economy using formal indicators such as economic growth, inflation, and unemployment, as well as informal proxies like the state of their own personal finances. Voters weigh these objective and subjective indicators when making decisions whether to punish or reward incumbents or political parties (Lewis-Beck and Stegmaier 2007). The literature finds, however, that people have cognitive limits to their ability to act rationally (Jones 1999). Given the well established finding that voters' political knowledge is generally very low (Gaines et al. 2007), it is unrealistic to expect them to be any better informed about the economy. Although Achen (1992) contends people need not be "geniuses or saints" to be rational decision makers, the simplicity of stock market returns significantly reduces the cognitive requirements for rational decision making.

Stock market indices such as the Dow Jones Industrial Average and Standard & Poor's 500 popularly are used as measures of the overall health of the economy. These indices incorporate future expectations about the direction of the U.S. economy, responding on a daily basis to a dizzying array of economic information such as economic growth, trade deficits, inflation, unemployment, consumer sentiment, consumer spending, and productivity. Lockerbie (2008) argues that people are best informed on issues with direct bearing on their

financial well-being. As a simple heuristic, there are three possible outcomes of the value of stock market investments over any period: up, down or no change. Certainly, it is possible for voters to “systematically misjudge the state of the economy even when it is presented to them on a silver platter” (Anderson 2007, p. 280). The complexity of information, the 24-hour news cycle and proliferation of financial news sources permit simple movements of the stock market (up or down) to be interpreted in different and possibly contradictory ways. If interpretation of information lies between factual beliefs and policy opinions (Gaines et al. 2007), then there are multiple ways that stock investments can affect political behavior even when the facts are uncontested.

Nonetheless, daily stock market movements and periodic information about stock portfolio returns provide objective, low-cost and readily accessible information about individual economic welfare. Investors ostensibly can focus on their own portfolios, the broader market indices or both. Linn and Nagler (2005) suggest that “stockholders have essentially homogeneous preferences: maximize the stock price.” Although Sears (1997) dismisses the idea that routine economic activity provides useful information for political behavior,¹² Popkin et al. (1976) make the opposite case:

Voters do, however, acquire much political information in the course of their daily lives. Such information then helps voters to form political opinions and make decisions. It is not surprising that voters make use of such “free” information and feel most comfortable dealing with political choices in terms of these “mundane” life experiences, and there is no need to ignore such “gut” voting as a subject of serious analysis...Usable political information is acquired in the process of making individual economic decisions; housewives learn about inflation of retail prices, homebuyers find out the trends in mortgage loan interest rates, *and owners of*

¹²Sears (1997) contends “political choices are typically so distal from daily life” and that “the mundane, concrete, proximal world of private life, of electricity bills and trips to the grocery store, routinely offers choices engaging simple self-interest calculations: Which store is closer? Which box of dishwasher detergent is less expensive?” (page 495).

stocks follow the Dow-Jones averages [emphasis added]. On the basis of this information, voters can gauge the economy, and apply this [*sic*] data when they vote...it is the amount of “free” daily life information and individual need to know—motivation rather than just education level—which explains how informed about issues voters will be (Popkin et al. 1976, p. 788).

Financial asset ownership shapes political behavior in part by economizing the cognitive capacity required to apply informed appraisals of the economy to political judgments. Investors do not need to know exactly the actions or outcomes of alternative political choices. They simply need to probabilistically determine which alternative is likely better for their stock portfolio (Lockerbie 2008).

2.4.3 Causal clarity

The third mechanism through which financial assets shape political behavior is what I call “causal clarity,” namely the ability to draw clear connections between one’s personal finances and the larger political process. The core assumption of economic models of political behavior is that voters reward the government if the economy is doing well, but punish the government if the economy is doing poorly. The logic is straight-forward. Voters first attribute success or failure to the incumbent responsible for managing the economy. The incumbent can be a president, prime minister, or legislature. Second, voters observe and interpret economic conditions. Voters can reflect on their own personal financial conditions, national economic conditions, or both. Finally, voters reward or punish incumbents with their votes (Lewis-Beck and Stegmaier 2007). Voters can assign blame or praise to a single national figure like a president or prime minister, or to a collective group like a legislature or political party.

In order for vested interest to shape political behavior, however, individuals must be able to draw a connection between their personal economic well-being and government actions (Feldman 1982; Kiewiet and Rivers 1984; Sears and Funk 1990a). This a high bar to clear,

however, as people generally find it difficult to relate their personal financial circumstances to specific actions of the government. This explains why scholars repeatedly find no relationship between personal finances and political behavior (Feldman 1982, 1984; Sears and Funk 1990a, 1991; Lau and Heldman 2009). The literature, however, provides evidence that increased causal clarity can shape political behavior. For example, multiple studies consistently found that heavy tax burdens shape political attitudes and that people attribute responsibility for taxes to government action (Sears and Funk 1991).

The relevant question, therefore, is whether investors can attribute portfolio performance to government policy (Feldman 1982, 1984; Brody and Sniderman 1977). Popkin and colleagues illustrate this connection by contrasting the specific interests of farmers with the less focused interests of laborers. Farmers have a great deal of information about farming issues in relation to their business and can directly relate these concerns to specific government policies such as subsidies, import tariffs, or water runoff. Laborers, on the other hand, are not in the business of routine collection of economic information and thus are more likely to use party label as an informational shortcut (Popkin et al. 1976, p. 791).

The stock market solves this information problem by providing a simple heuristic about the effect of government actions on stock market portfolios. The market responds to proposed government policies and realized outcomes (e.g. inflation, growth, etc.) on a daily basis. Moreover, financial news media facilitate this connection by routinely drawing connections between market changes and government actions. Financial assets provide causal clarity regarding the relationship between personal economic welfare and the political process.¹³

Thus far, I've argued that financial assets shape political behavior by creating a clear motivation for political interest, reducing the cognitive capacity required to make rational decisions, and simplifying the relationship between economic well-being and the political

¹³Reporting on stock market investments is distinct from the general disconnection between information generated by mass media and individual self-interest cited by Sears and Funk (1991): "in political life information about the self may generally not be very accessible, because the mass media are our primary sources of information, and their focus is on national and collective affairs rather than on matters directly relevant to our own lives" (page 9).

process. In the next two subsections, I propose two mediating factors—future orientation of decision making and asymmetrical impact of financial loss and gain.

2.4.4 Future orientation

People require information to make political decisions. I argue that the stock market structurally shapes investors' perspectives so that they are prospective rather than retrospective. Economic models of political behavior contend that voters use measures such as economic growth, unemployment and inflation to make judgments about the state of the economy (Lewis-Beck and Paldam 2000; Lewis-Beck and Stegmaier 2007). Due to the time needed to collect and revise these data, these types of summary statistics are better measures of where the economy has been rather than where it is going. Thus, these types of indicators leave room for debate about whether rational decision makers look forward or backwards as they use economic information to make political decisions.¹⁴

By contrast, the structure of the stock market compels investors to be forward looking, focusing on future gains rather than past returns. Daily stock market prices and indices like the Dow Jones Industrial Average and the Standard and Poor's 500 report the market's expectations about *future* sales, earnings and profits. Investors commonly use the price-to-earnings (P/E) ratio to determine the relative costs of stocks. The P/E ratio is forward looking, and reflects the number of years of future earnings required to pay back today's purchase price. Companies that forecast lower than expected future earnings generally are not rewarded for their candor, but rather see their share prices pummeled by the market.

This does not mean that people are not influenced by previous performance. Despite

¹⁴Key (1966) argues people are retrospective and make rational decisions based on information from the past. Downs (1957) and MacKuen et al. (1992), however, argue that people are prospective, basing decisions on likely future benefits. Achen contends that past outcomes are useful only the extent that they can be used to predict future payoffs (Achen 1992). Other scholars maintain that voters are both retrospective *and* prospective in evaluations of future welfare under alternative political choices (Fiorina 1981; Kuklinski and West 1981; Lockerbie 2008). Although Fiorina (1981) is widely associated with the retrospective camp, his retrospective argument most squarely concerns party identification. With respect to voting behavior, however, Fiorina finds that future expectations weigh more heavily than retrospective evaluations (p. 197).

the common refrain that “past performance is no indication of future returns,” investors routinely buy into yesterday’s high-flying stocks and mutual funds in an attempt to chase higher returns. Nonetheless, investor orientation is inherently prospective. Prior returns are “sunk”; there is nothing that the investor can do to change these results. She can only try to shape tomorrow’s prospective returns by making decisions based on expectations of future gains or losses. Further, the investor is likely to care more about the immediate future. People tend to heavily discount the future (hyperbolic discounting), placing greater value on near-future returns than expected gains far out into distance (Thaler 1981; Laibson 1997). This suggests that any effect of concerns about stock market portfolios on political behavior should manifest in the near term.

2.4.5 Asymmetry of loss and gain

A key behavioral assumption of the self-interest literature is that people act to avoid pain and secure pleasure (Mansbridge 1990; Sears and Funk 1990a, 1991). These assumptions appear to be supported by science. The human brain is more stimulated by the anticipation of a financial gain than the actual realization of the gain itself (Zweig 2007). Brain scans reveal that the part of the brain stimulated by the prospect of making money is the same region activated by sexual pleasure and drug use (Critchley et al. 2001; Kuhnen and Knutson 2005; Knutson et al. 2005). Financial losses, however, have the opposite effect, stimulating the region in the brain that evaluates negative emotions like pain, disgust and guilt (Zweig 2007). People’s responses to loss and gain literally are hard wired in their brains (Loewenstein 2006).

Experimental findings support the biological evidence that people react differently to gains and losses. Psychologists report the impact of a financial loss is felt more heavily than an equivalent financial gain (Kahneman and Tversky 1979).¹⁵ Furthermore, the evidence

¹⁵Experiments, however, find diminishing sensitivity to loss and gain (Tversky and Kahneman 1991). The effect of movement (gain or loss) between \$0 and \$100 is greater than movement between \$1,000 and \$1,100.

suggests (at least in the American case) that investors will attribute positive returns to their own investment prowess, but shirk blame for their losses (Feldman 1982). In the event of losses, however, investors will not automatically blame the government for their portfolio balances (Sniderman and Brody 1977; Sigelman and Tsai 1981; Sears and Funk 1990a). This is explained in part by the finding that people routinely “edit” outcomes to subjectively maximize their welfare, i.e. make themselves feel happier. If an investor loses less than others or less than she expected, she routinely will interpret this loss as a “gain” (Thaler and Johnson 1990).¹⁶ Thus, we can expect financial assets to have an asymmetric effect on political behavior. Significant financial losses, which I define as drops in asset value of 25% or more, are much more likely to trigger a reaction than equivalent pecuniary gains.

In light of the evidence that people (at least Americans) do not hold the government accountable for their personal finances (Brody and Sniderman 1977; Kinder and Sears 1985; Sears and Funk 1990a), I suggest there is one major factor that explains whether financial losses become politicized or not—whether the individual believes the playing field is unfair. The self-interest literature provides tentative support for this proposition. People who feel particularly burdened by taxes or expect to benefit substantially from tax-reduction measures exhibit greater support for tax-cutting policies (Sears and Funk 1990b). Smokers and non-smokers generally are indistinguishable on their political views, but sharply diverge on issues concerning cigarettes (Green and Gerken 1989). Smokers are much more opposed to taxes and restrictions that target cigarettes. Unless investors feel their losses are the result of some unfair process, they are unlikely to politicize their financial losses. This is consistent with the finding that self-interest generally lays dormant unless primed. Kramer (1983) found that self-interest manifests when people have clear information and can specifically attribute changes in their personal economic welfare to government policies.

For example, imagine an investor in a company which has been targeted for antitrust

¹⁶Thaler and Johnson (1990) call this the “house money effect,” based upon the example of how gamblers rationalize losses after gains by shifting their reference point to the point in time at which they commenced gambling, i.e. whether they are still “up” from where they started.

violation by the government. The resultant loss in financial value of the company's stock is directly due to the government's action. In May 1998, the U.S. government sued Microsoft for alleged violation of a 1995 agreement related to earlier anti-trust investigations. Over the next two years, the value of Microsoft's shares dropped 40% due to the uncertainty of the case (Economides 2001). Investors in Microsoft stock in the 1990s likely had distinct views about the government's actions from non-owners of Microsoft stock. Moreover, the effect of the government action on political views potentially was greater for direct owners of Microsoft shares than passive owners of the stock in mutual funds.

2.5 Predicted effects

The theory of portfolio politics advanced thus far posits that financial assets have a moderated effect on political behavior through the mechanisms of vested interest, increased cognitive capacity, and causal clarity. It also asserts that investors are future-oriented, due to the structural influence of the stock market itself. Finally, it contends that investors are motivated more by loss than by gain. Financial losses, however, will not be politicized unless investors believe the playing field is fundamentally unfair. The theoretical claims to this point are made without any clear prediction of specific outcomes. In this section, I examine the predicted effect of financial assets on three different types of political behavior—partisanship, participation, and attitudes.

2.5.1 Partisanship

The partisanship literature finds that partisan attachments are relatively stable. A form of social identity, people think of themselves as members of partisan groups as much as they see themselves as members of other social groups (Campbell et al. 1960; Green et al. 2002).¹⁷ Parents generally influence initial partisan attachments, but these are subject to

¹⁷The revisionist model in the partisanship literature contends that partisan attachments are endogenous to individual interests and political party positions. Revisionists assert that partisanship is shaped by

change by social influences in the early adult years. By the time an investor reaches her early thirties, we can expect her party ID to be relatively stable for the rest of her life (Miller 1991; Abramson and Ostrom Jr. 1991; Green et al. 2002). When change does occur, it is more likely temporary support of another party or, in the American case, movement between Independents and the major political parties.

Accordingly, there is no theoretical basis on which to believe acquisition of financial assets has any effect on partisanship. A new investor is no likelier to change her party identification than she is her religious affiliation. A union member has no obvious reasons to disassociate herself with a labor organization once she acquires stocks and mutual funds. Due to the strength and durability of partisan attachments, and the relatively weak and conditional effects of financial assets on political behavior, I predict that in general stocks and mutual fund ownership have no effect on partisanship. The only caveat is the possible effect of financial assets on people in their twenties and thirties when the potential for partisan change is greatest (Green et al. 2002).

2.5.2 Participation

Political participation is defined as activity intended directly or indirectly to influence government action (Verba et al. 1995). This influence occurs through attempts to influence the development of public policies and/or selection of the people who make and implement these policies. A large literature finds that income (among other variables) is highly predictive of participation (Campbell et al. 1960; Wolfinger and Rosenstone 1980; Verba and Nie 1972; Verba et al. 1995; Leighley 1995). People with more income are more likely to get involved in the political process. However, the literature to date overlooks the distinction between current income and saved up assets. It's possible that financial assets have a distinct effect

rational prospective evaluations of how a political party will benefit individual interests (Achen 1992, 2002a; Franklin and Jackson 1983; Franklin 1984; Markus and Converse 1979; Abramowitz and Saunders 2006; Clarke et al. 2009). Although the empirical evidence in the literature weighs against this argument, even if revisionists were right the theorized weak effects of financial assets make any change in partisanship highly unlikely.

from current income on participation.

Political participation is costly. It generally takes time, effort and in some cases money to participate in the political process. Voting, volunteer work, contact with politicians, and campaign contributions all entail some degree of cost. The literature shows that people with vested interests are more likely to pay these costs of participation. For example, Green and Cowden (1992) found that people most opposed to forced busing to integrate schools were more likely to join anti-busing organizations. Similarly, Sivacek and Crano (1982) found that college students directly affected by a proposed increase in Michigan's legal drinking age were more likely to join protests against the change than those who would not be affected even though both groups of college students were similarly opposed.

The "least expensive" form of participation is voting, which is relatively (but not absolutely) costless. Indeed, voting is the most fundamental form of participation and the most common means of participation for most people. Given the findings in the literature that resources have no effect on voting and my argument about the conditional effects of financial assets on behavior, I predict that ownership of stocks and mutual funds has no effect on voting behavior. Regarding time-based acts like volunteering and contact with politicians, I predict that financial assets may have some effect only in the event of financial loss. Investors will be more motivated to spend time in politics if they feel they have been unfairly treated in the market. I am uncertain, however, about the effect of assets on campaign contributions. Verba et al. (1995) find that financial resources are needed to make financial contributions. However, people are more likely to make campaign contributions from their saved up assets than current income. Given my contention that financial loss is more likely to motivate political behavior, it is unclear why someone would desire to make campaign contributions after incurring significant financial loss.

2.5.3 Attitudes

Attitudes are summary evaluations of psychological objects, which are “anything that people can like or dislike, wish to protect or to harm, want to acquire or to reject” (Kahneman et al. 1999, p. 205). People form beliefs about objects based their perceived value (good-bad, likeable-unlikeable) (Ajzen 2001). Attitudes, therefore, are based in part on beliefs about the desirability of these object attributes (Katz 1960; Fishbein and Ajzen 1972). Attitude toward an object such as financial assets is derived from its perceived utility. Clarity, consistency, and immediateness of costs and benefits as they relate to an individual’s actions help to shape utilitarian attitudes. “The closer these objects are to actual need satisfaction and the more they are clearly perceived as relevant to need satisfaction, the greater are the probabilities of positive attitude formation” (Katz 1960, p. 171).

Attitude change involves a chain of events that include object evaluation, existing beliefs, and prior attitudes. The more beliefs and cognitive items that go into shaping an attitude, the more difficult it is to change (Katz 1960). Moreover, attitude change can occur in a variety of ways. The acquisition of new information about objects can result in replacement or overwriting of previous attitudes (Ajzen 2001; Druckman and Lupia 2000). Attitudes can be completely reversed or partially changed. Preferences and behavior can be affected (or not) in a variety of ways. Thus, although it is less costly to change attitudes, changes in political attitudes are difficult without sufficient stimulus. “Attitudes toward political parties and voting behavior are often difficult to change if there is no widespread dissatisfaction with economic conditions” (Katz 1960, p. 177).

The self-interest literature suggests that financial assets are unlikely to change people’s political attitudes. Researchers consistently found that self-interest does not have any effect on political attitudes (Sears et al. 1980; Sears and Funk 1990a, 1991; Feldman 1982; Kinder and Sears 1985). Although Green and Cowden (1992) found that self-interest explained *participation* in anti-busing organizations, they found no correlation regarding *attitudes* of

white Americans on forced busing in Boston and Louisville in the 1970s. Similarly, Sivacek and Crano (1982) found that although college students directly affected by a proposed increase in Michigan’s legal drinking age were more likely to join protests against this action, there was no difference in attitudes regarding the proposed law between these and older students not directly affected by the proposed change. Therefore, I predict that financial assets will have no effect on attitudes, save in special circumstances of financial loss. People who experience significant loss are more likely to change political attitudes on issues that directly affect their economic interests.

2.6 Political science and the stock market

Despite the dramatic increase in household financial asset ownership around the world over the past thirty years, political scientists have devoted little attention to the relationship between stock market participation and individual political behavior.¹⁸ In a recent literature review, Kaustia and Torstila (2008) found that “there is virtually no empirical research on the interplay of political views and investment behavior.”¹⁹ Barabas (2006) combined individual-level U.S. opinion poll data with broad stock market indices to find that overall stock market performance affects policy preferences. Public support in the U.S. for privatization of Social Security appears to rise and fall with the stock market. A pair of economists analyzed data on Finnish households that received shares of demutualized telecommunication companies in the late 1990s to find a small rightward shift in political ideology (Kaustia and Torstila 2008).

¹⁸Political science has not completely ignored the stock market. For example, Roberts (1990) uses aggregate stock market data to show that stock prices of defense-related firms responded to expected shifts in defense policy in anticipation of the outcomes of the 1980 U.S. presidential and legislative elections. Other scholars examine the relationship between aggregate stock markets and politics (Bernhard and Leblang 2006; Leblang and Mukherjee 2005; Jensen and Schmith 2005; Döpke and Pierdzioch 2006).

¹⁹Scheve and Slaughter (2001) make a similar observation for research on the political economy of trade policy, exhorting analysts to “consider asset ownership” in their theoretical models.

Davis and Cotton (2007) used survey data from the 1998-2004 American National Election Studies (ANES) to find that shareholders were about 30% more likely to identify with the Republican Party in 1998-2002 and 130% more likely in 2004. Other scholars examined the effect of privatized share ownership on voting (Studlar et al. 1990; Saunders 1995) and political issue positions (Earle and Gehlbach 2003). In the privatization literature, however, Earle and Gehlbach (2003) find “a relative paucity of research that directly relates the attitudes held by an individual to his or her experience of privatization policies, *ex post*.” Barabas (2006) sums up the literature, concluding that “scholarly works showing that the stock market affects political behavior are rare.”

2.7 Research questions and hypotheses

The main research question of this dissertation concerns the causal effect of financial assets on political behavior. Does acquisition of financial assets—stocks and mutual funds—have any discernible effect on partisanship, participation or attitudes? Both the investor class and asset effect theories make strong claims that assets shape behavior, but the evidence supporting these claims is weak. Political science has yet to weigh in on this subject. I offer and test the following hypotheses:²⁰

In the absence of significant financial loss (25% or more):

- Financial assets have no effect on partisanship with the possible exception of people under 34 years.
- Financial assets have no effect on voting.
- Financial assets have no effect on time-based forms of political participation.

²⁰I do not have any data on financial contributions in the data sets used in this dissertation, so I don't offer any testable hypothesis regarding the effect of financial assets on campaign contributions. However, I do offer an uncertain prediction about the effect of assets on campaign contributions. I expect greater levels of financial assets to have a positive effect on campaign contributions. However, I am uncertain about the effect of portfolio losses on campaign contributions.

- Financial assets have no effect on political attitudes.

In the event of significant financial loss (25% or more):

- Financial assets have no effect on partisanship with the possible exception of people under 34 years.
- Financial assets have no effect on voting.
- Financial assets have no effect on time-based forms of political participation.
- Financial assets have a causal effect on attitudes directly concerning economic welfare.

Chapter 3

Research Design and Methodology

3.1 Introduction

This chapter describes the research design and methodology used in this dissertation. The aim of this dissertation is to uncover the causal effects of financial asset ownership on political behavior and rule out plausible alternative explanations. Fundamentally, the research design of this dissertation seeks to address Teixeira’s trenchant inquiry about the investor class theory: “Does a voter with a \$50,000 annual income and a 401(k) worth \$15,000 really differ politically from a voter with the same income level who lacks that 401(k)?” (Teixeira 2003). The aim of the research design in this dissertation is causal inference, which takes into account potential outcomes in alternative causal states (Morgan and Winship 2007). The dissertation employs a quasi-experimental research design to attempt to overcome the limitations of observational data, using control groups, pre-tests and post-tests of political behavior to draw causal inferences. The dissertation treats the acquisition of financial assets as an intervention and investigates the effect of this “treatment” on political behavior when it is introduced into an experimental group compared to a control group.

I carefully select data sources with information about financial assets, political behavior and predictors of asset ownership to test my hypotheses about the effect of financial assets on political behavior. I use propensity score matching to make the treatment groups as alike as possible save for one difference—ownership of financial assets. I attempt to make my empirical results as transparent as possible to make the substantive implications of this research clear. Thus, I make relative comparisons about the effect of different types of

assets on political behavior across treatment groups rather than simple comparisons to null hypotheses. Finally, I test the effect of assets in the United States and Britain over two and a half decades on various measures of political behavior.

3.2 Instrumentation

Campbell (2006) identifies five characteristics of an ideal data set on household finances: good representation of an entire population (especially with regards to age and wealth); measures of total wealth including exhaustive categorical breakdowns; detailed information about asset classes and specific assets; highly accurate data; and, longitudinal data structure. To study the effect of financial assets on political behavior I also add to this wish list detailed information about voting, political interest, political acts, and positions on a wide range of political issues. Unfortunately, this ideal data set does not exist for finance scholars, much less for researchers interested in the intersection of investments and politics.¹ Nonetheless, I identify nine surveys with data about financial assets and politics. Table 3.1 summarizes the surveys used in this dissertation.

Table 3.1: Surveys of politics and investments

Survey	Country/Region	Structure
British Household Panel Study (1991-2005)	Britain	Longitudinal
National Child Development Study (1981-2000)	Britain	Longitudinal
American National Election Studies (2000-2002)	United States	Longitudinal
CBS News (December 2008)	United States	Cross-sectional
CBS News & New York Times (March 2008, January 2009)	United States	Cross-sectional
NBC News & Wall Street Journal (September 2008)	United States	Cross-sectional
USA Today & Gallup (January & December 2008)	United States	Cross-sectional

These surveys include samples from the United States and Britain. Three of these surveys are longitudinal; one follows a natural cohort over time and the other two follow randomly

¹The best sources of data on household finance do not provide detail on asset diversification, have incomplete data for significant percentages of households, and do not follow respondents over time (Campbell 2006, p. 1556).

selected representative panels. The other six surveys are cross-sectional. The longitudinal surveys cover roughly 25 years, starting from the early 1980s to the mid-2000s. The cross-sectional surveys were conducted within the last two years.

3.2.1 Longitudinal surveys

The British Household Panel Survey (BHPS) is a government-funded, nationally representative panel survey from Great Britain (Institute for Social and Economic Research 2008). The BHPS started in 1991 as an annual survey of each adult member (16 years and older) of a nationally representative sample of more than 5,000 households. A stratified random sample of households was drawn at the start of the survey. In subsequent years, all residents of these households were traced and re-interviewed. If departures from the original households occur, all adult members of the new households also are interviewed. I use BHPS data from 1993-2005. Most years include data about politics; the panels for 1995, 2000, and 2005 include detailed data about financial assets.²

The National Child Development Study (NCDS) is a longitudinal survey that tracks from birth a cohort of 16,500 individuals born in Great Britain (England, Scotland and Wales) during the week of March 3-9, 1958 (Centre for Longitudinal Studies 2008).³ There have been eight waves of data collection of members of the birth cohort about their physical, educational and social development. I use NCDS data from three waves administered between 1981-2000. Surveys administered at ages 23 (1981) and 33 (1991) include data about financial asset ownership and politics; the survey administered at age 42 (2000) includes only political data.

The American National Election Studies (ANES) is a series of biennial, nationally representative detailed surveys of the political behavior of Americans 18 years and older (The

²Unfortunately, one-third of the cases in my matched groups from 1995 and 2000 are missing for 2005, so I am limited to analysis using data from 1995-2000.

³The birth cohort was augmented in the follow ups at ages 7, 11 and 16 by including immigrants born this same week.

National Election Studies 2005). The survey dates to 1948 when its predecessor was first established at the University of Michigan, where it is currently housed and operated in partnership with Stanford University. The 2000-2004 panel includes 1,807 people who were first interviewed in 2000 and subsequently reinterviewed in 2002 and 2004. The survey in each of these years inquires about household stock market participation.

3.2.2 Cross-sectional surveys

The cross-sectional surveys used in this dissertation were conducted by American news media organizations, all of which were conducted by telephone. These surveys were put into the field between January 2008 and January 2009, a period of major financial crisis and economic uncertainty in the United States. Sampling for these surveys generally involved random dialing of both landline and cellular telephone numbers. The December 2008 CBS News survey is a national poll of 1,390 adults (CBS 2009). The March 2008 and January 2009 polls conducted by CBS News and *The New York Times* respectively included 1,368 and 1,112 adults (CBS and New York Times 2008, 2009). The September 2008 NBC News and *Wall Street Journal* poll includes 1,157 registered voters, including oversamples of Hispanic voters and voters who only use a cellular phone (NBC and Wall Street Journal 2009). *USA Today* and Gallup conducted polls in January and December 2008 that included 2,020 and 1,008 adults, respectively (USA Today and Gallup Organization 2008, 2009).

3.3 Research design

Observational studies are not well suited for analysis of cause and effect. Although the subjects in each of my data sets are randomly selected, by definition financial asset ownership in observational studies lacks *random assignment* (Shadish et al. 2002). This means I have no control over the mechanism that determined whether subjects acquired financial assets or not. There is near consensus in the household portfolio literature that certain factors

such as age, education and income greatly affect the likelihood of owning stocks and mutual funds (Campbell 2006). I can ignore this assignment mechanism and proceed to causal inference only if I can convincingly condition my analysis on the factors that affect ownership of investments (Gelman and Hill 2007, p. 184). This requires me both to imagine the hypothetical randomized experiment that led to the observed data as well as approximate the “randomized” decision rules that determined whether someone owns financial assets or not (Rubin 2008).⁴

The research design for this dissertation, therefore, is quasi-experimental. At root there are three variables—a pre-test of political behavior prior to asset ownership, a post-test of political behavior after asset ownership, and an asset ownership indicator.⁵ To make clear the potential outcomes in political behavior under different states of asset ownership, I identify “treatment” groups, consisting of those who acquired financial assets (“treated” group) and those who did not (“control” group). I “control” for differences between those who acquired assets and those who do not by accounting for factors which predict asset ownership such as age, income and education. Instead of using control variables, however, I use these predictors (or “covariates”) to create matched treatment groups. The treated and control groups on average are the same on all the predictors of asset ownership except for one difference—one group owns financial assets (treated) and the other group does not (control). I use these predictors of asset ownership to create propensity scores, which are used to create matched groups with equal likelihood of owning a given financial asset (stocks or mutual funds).

Propensity score matching works both as a method to facilitate quasi-experimental contrasts and a nonparametric method to adjust for the treatment assignment mechanism (Ho et al. 2007; Morgan and Winship 2007). Matching also improves the quality of the quasi-

⁴I follow Stouffer’s advice that “though we cannot always design neat experiments when we want to, we can at least keep the experimental model in front of our eyes and behave cautiously” (Stouffer 1950, p. 359).

⁵This approach heeds the advice of Achen (2002b) to limit causal analysis to no more than three explanatory variables.

experimental designs in this dissertation that use cross-sectional data with control groups but no pre-tests for prior behavior (Shadish et al. 2002). It is important to note that the creation of treated and control groups is done without any reference to the outcome of political behavior. I do not use any measure of political behavior to create the treatment groups, and use propensity score matching to pre-process my raw data nonparametrically. Ho et al. (2007) contend this approach improves the reliability of parametric methods and makes causal inference much less sensitive to model specification.

I use GenMatch, a genetic matching algorithm, in conjunction with the MatchIt package in the R statistical software program to create the matched groups (Sekhon Forthcoming; Ho et al. 2007, Forthcoming; R Development Core Team 2009).⁶ I employ matching with replacement, which in the case of GenMatch retains all of the treated cases and allows untreated cases to be matched to more than one treated case.⁷ Gelman and Hill (2007) suggest matching with replacement can create better balance between treated and control groups, which should yield estimates that on average are closer to the truth. Each untreated case is assigned a weight so that the treated and untreated groups are of equal size.

Since my interest is the effect of assets on change in political behavior, my general approach is to establish a baseline for when I know both treatment groups did *not* own a given asset and a second point in time when the treated group owns the asset but the control group does not. Although the ANES 2000-2002 panel is relatively short, it offers a good number of measures of the entire range of political behavior under investigation. I use 2000 as a baseline to create treatment groups based on cases who did not own assets in

⁶A key challenge of matching is creating balance between treatment groups on the predictor variables. The goal is to minimize the difference in the average values of predictor variables between treated and control groups. This can be an arduous task, requiring frequent tweaks, rebalancing and interactions between covariates. GenMatch reduces the analyst's burden considerably by substituting human mental work with computation by the computer. The GenMatch algorithm finds the combination of treated and control cases that creates the least degree of differences between treated and control groups across covariates (Sekhon 2009).

⁷This means that I do not use all of the "untreated" cases in my analysis. Achen (2002b) makes a vigorous argument in support of this practice: "Put bluntly, in most of our empirical analyses, some groups of observations should typically be discarded to create a meaningful sample with a unified causal structure" (p. 446)

2000 and either acquired assets over the next two years (treated) or continued not to own financial assets (control). Pre-test data on political behavior comes from 2000; post-test data are from 2002. The ANES includes pre- and post-test information on partisanship, political attitudes, voting, and other forms of participation. Unfortunately, no information is available about asset values.

The British Household Panel Study covers a longer period of time. In this case my baseline is 1995; the treatment groups are comprised of people who did not own financial assets in 1995, but either acquired them by 2000 (treated) or continued to not own any investments (control).⁸ The BHPS includes pre-test and post-test information about partisanship, voting, political interest, political activities, and political attitudes. The BHPS also does not include information on asset values.

The National Child Development Study includes information about financial assets in panels collected at ages 23 (1981) and 33 (1991). The 1981 panel is the baseline; treatment groups are based on those who did not own financial assets in 1981 and either acquired assets between 1981-1991 (treated) or did not (control). Available pre-test data include only voting and party identification. Although the NCDS offers rather limited information about political behavior, it does cover the formative years of the twenties and early thirties in which the respondents presumably are most likely to exhibit changes in political behavior (Green et al. 2002).⁹ Moreover, the NCDS includes information on asset values.

The lack of pre-treatment information inherent in cross-sectional data compels use of a “post-test only design” that is particularly weak for causal inference (Shadish et al. 2002; Morgan and Winship 2007). Thus, I limit the use of cross-sectional data to investigate change in political attitudes. This approach is similar to that employed in Doherty et al. (2006), which investigates the effect of lottery winnings on attitudes toward redistribution.¹⁰

⁸Unfortunately, the BHPS does not indicate when exactly these assets were acquired.

⁹The lack of additional political behavior pre-test data is unfortunate as the panels for ages 33-46 include data about various political acts, including demonstrations, attendance at political rallies, political campaign activity, and contact with political officials.

¹⁰Doherty et al. (2006) did not create treatment groups presumably due to the lack of predictors which

In this case, I use six media surveys to test the effect of asset ownership on attitudes during the 2008 financial crisis. As the survey questions I use in this dissertation are specific to the crisis, respondents can not reasonably be expected to have prior attitudes about government policies toward an unexpected major event. These surveys do not include information about asset values.

3.4 Data

In this section I describe the data used to create the matched groups and conduct empirical analysis. Fundamentally, I need three types of data to construct the matched treatment groups used in this dissertation: 1) data about asset ownership (treatment variable); 2) measures of political behavior (pre-tests and post-tests); and, 3) factors that influence financial asset ownership and political behavior (predictors or covariates). Each of the data sets selected for this dissertation offers at least nine predictors of asset ownership. Below I describe the variables constructed for this analysis.

3.4.1 Treatment indicators

The treatment variables in this dissertation are financial assets—stocks and mutual funds (Keister 2005). These are the riskiest types of financial assets and are distinguished from less riskier forms such as bonds or cash (Donkers and van Soest 1999; Campbell 2006; Shum and Faig 2006; Heaton and Lucas 2000). By focusing on the owners of riskier assets I attempt to identify people with the greatest vested interest in external (political) factors that may affect their material well-being (Crano 1997a).¹¹ I operationalize financial asset ownership

plausibly could be used to create matched groups. In my case, the finance literature provides well-researched predictors of financial asset ownership that can be used to create treatment groups.

¹¹Due to data limitations I am unable to include long-term government bonds and corporate bonds as risky assets (Guiso et al. 1996). Government paper of longer maturities has default risk; corporate paper entails bankruptcy risk.

as dummy variables.¹²

Ideally, I would like to have information that allows me to disaggregate financial assets to investigate the effect of assets on political behavior. This would be particularly useful as some analysts speculate that different types of assets may have more pronounced effects on political behavior (Ponnuru 2004). It would be important not only to distinguish between financial assets owned inside and outside of tax-sheltered accounts, but also to differentiate between types of stocks (foreign and domestic) and mutual funds (stock, bond, money market) to discern their differential effects on political behavior. Unfortunately, this level of detail is unavailable in the data sets used in this dissertation. Additionally, I would like to have data on the value of these assets. Unfortunately, I only have data on asset values for the NCDS data set. Notwithstanding these limitations, I am able to make a basic distinction between investors and non-investors and differentiate between ownership of stocks and mutual funds.

3.4.2 Measures of political behavior

My dependent variables (post-tests) are measures of political behavior—partisanship, participation, and attitudes. My pre-tests are these same measures at an earlier point in time. Table 3.2 below lists the common types of political variables used in this dissertation by survey. Operationalization of these measures varies somewhat by survey. I follow Green et al. (2002) and create dummy variables for political party identification. I create scales for strength of partisan attachment in the U.S. and Britain along a linear left-right dimension. For participation I similarly create dummy variables to indicate whether a political act occurred or not. For attitudes toward political issues I create dummy variables or scales that indicate respondents' positions on issues. Examples include the role of government, support for the free enterprise system, and positions on specific policies. I provide specific details about each of these measures in each of the chapters that follow.

¹²The surveys are not uniformly clear about asset ownership. In some instances it is unclear whether the respondent's response indicates household or individual investments. I am also ignorant about potential bargaining within households over investment decisions (Campbell 2006).

Table 3.2: Measures of political behavior

Measure	Survey	Operationalization
Partisanship	BHPS, NCDS, ANES	Dummy variables
Political ideology	BHPS, ANES	Scales
Voting	BHPS, NCDS, ANES	Dummy variables
Other forms of participation	BHPS	Dummy variables
Political attitudes	All surveys	Dummy, Scales

3.4.3 Predictor variables

To create matched treatment groups I identify predictor variables that directly or indirectly affect the probability of investment ownership and/or certain types of political behavior. I use predictors (also called “covariates”) to attempt to make the treated and control groups as much alike as possible with regards to the assignment mechanism. Table 3.3 summarizes covariates identified in the finance literature. (Since the NCDS is a cohort study, all of the respondents are the same age.)

Financial assets

Age, income, and education are the most frequently cited predictors of asset ownership. The life-cycle model of investments posits that younger households limit investments to home ownership. Only later as they accumulate assets do they consider riskier financial assets such as stocks and mutual funds. As households mature they sell off riskier investments to preserve capital for their retirement (Keister 2005). Numerous scholars found evidence of a quadratic effect of age on investment ownership that supports the life-cycle model (McCarthy 2004; Shum and Faig 2006; Campbell 2006).¹³ Income refers to household income and often is captured in the surveys used in this dissertation as categorical data. I generally divide income into thirds to identify low, medium and high income based on the income distribution

¹³Focusing on stockholding, however, Haliassos and Bertaut (1995) and Ameriks and Zeldes (2004) find no effect of age.

Table 3.3: Predictors of financial asset ownership

Variable	Operationalization	ANES	BHPS	NCDS	USAT/G	CBS/NYT	NBC/WSJ	CBS	USAT/G	CBS/NYT
					Jan 2008	Mar 2008	Sep 2008	Dec 2008	Dec 2008	Jan 2009
Gender	Dummy (male, female)	X	X	X	X	X	X	X	X	X
Age	Years	X	X	N/A	X	X	X	X	X	X
Income	Scale (low, medium, high)	X	X	X	X	X	X	X	X	X
Education level	Scale (low, medium, high)	X	X	X	X	X	X	X	X	X
Ethnic minority	Dummy (yes, no)	X	X	X	X	X	X	X	X	X
Occupation level	Scale (low, medium, high)	X	X	X		S	S		S	S
Fulltime employment	Scale (low, medium, high)	X	X	X	X	S	S	S	X	S
Marital status	Dummy (yes, no)	X	X	X	X	X	X	X	X	X
Dependent children	Number of children	X	X	X	X	X	X	X	X	X
Home ownership	Dummy (yes, no)	X	X	X	X	X			X	X
Business/self-employ	Dummy (yes, no)		X	X						
Debt	Scale (low, medium, high)		X	X						
Health problem	Dummy (yes, no)		X	X						

Notes: X=Predictor available; S=Close substitute; N/A=Not applicable. ANES=American National Election Studies; BHPS=British Household Panel Study; NCDS=National Child Development Study; USAT/G=USA Today/Gallup; CBS/NYT=CBS/New York Times; NBC/WSJ=NBC/Wall Street Journal; CBS=CBS News.

in each respective data set.¹⁴ Campbell (2006) found that education is a strong predictor of stock ownership, even with controls for age, income and wealth. I operationalize education as one of three values: low (less than high school), medium (high school through some college), and high (undergraduate degree or higher).

Keister (2005) contends that occupation and employment status affect the likelihood of acquiring financial assets. People with higher occupations acquire more assets; the lack of full-time work negatively affects acquisition of financial assets. I follow Iversen and Soskice (2001) to operationalize occupation based on skill specificity using the third version of the International Standard Classification of Occupations adopted in 1988 by the International Labor Organization (ISCO 88). I generally divide occupation into three levels: low (elementary occupations), medium (clerks to machine operators), and high (managers and professionals).

Research shows that marital status affects asset ownership. Marriage tends to increase financial wealth (Keister 2005). I operationalize marriage as a dummy variable. Keister (2005) suggests that saving motives may affect the proclivity for investment. For example, households are likely to save and invest for children's education. I operationalize savings motives by including the number of dependent children in the household. Scholars found that the riskiness of the specific asset of a home might make households less likely to invest in similarly risky investments such as stocks and mutual funds (Heaton and Lucas 2000; Cocco 2005; Shum and Faig 2006). I operationalize home ownership as a dummy variable. Researchers found that business ownership is negatively correlated with financial investments such as stocks and mutual funds (Shum and Faig 2006; Campbell 2006). I operationalize business ownership or self-employment as a dummy variable based on occupational and/or investment data. Debt might also be a barrier to acquiring financial assets (Campbell 2006). I operationalize debt in three categories (low, medium, high) based on the distribution of the cases in each data set.

¹⁴This also facilitates logistic regression by reducing the possibility of empty cells.

Berkowitz and Qiu (2006) found that health status affects financial investments. The existence of a serious health ailment negatively affects household financial wealth. I construct a dummy variable to indicate the presence of a long-term serious illness. Analysts found that minorities in the United States own investments at much lower levels than whites (Chiteji and Stafford 1999; Yao et al. 2005; Keister 2005; Gutter et al. 1999; Gutter and Fontes 2006). For the American surveys I conceptualize ethnic minorities as blacks and Latinos. For the British surveys, ethnic minorities are conceptualized as non-whites—primarily blacks and Asians. For both sets of surveys I operationalize ethnic minorities with a dummy variable.

Politics

A large literature finds that socioeconomic status (e.g., income, education and occupation) is highly predictive of political participation (Campbell et al. 1960; Wolfinger and Rosenstone 1980; Verba and Nie 1972; Verba et al. 1995; Leighley 1995). People with higher SES are more likely to get involved in the political process. As noted above, I include income, education and occupation as predictors. Scholars also find differences between the political participation of men and women (Verba et al. 1995; Burns et al. 2001). I thus include gender as a predictor of political behavior, which I operationalize as a dummy variable.¹⁵

3.4.4 Omitted variables

There are several known or theorized predictors of financial asset ownership for which I unfortunately do not have data. The most significant omitted variable is wealth. Wealth is the sum of total property (financial and real assets) less total debts (Keister 2005). Most surveys include data about income but none for wealth. This is an important distinction since these two variables often are not highly correlated (Nam et al. 2008). Moreover, wealth may have an effect on the probability of owning particular investments. For example,

¹⁵The literature does not suggest that gender is a predictor of asset ownership. However, there is some debate as to whether men are overly confident investors compared to women. See Barber and Odean (2001) and Deaves et al. (2008).

Campbell (2006) found a quadratic pattern in the relationship between wealth and stock ownership. Low-wealth households are unlikely to own financial assets, but in high-wealth households there is a strong relationship between wealth and stock ownership. Omission of this important variable hopefully is mitigated by the inclusion of highly correlated variables such as age and education.¹⁶

Another omitted variable is awareness about investment options. Guiso and Jappelli (2005) show that nearly one-third of Italians in the mid-to-late 1990s were unaware of the existence of stocks, mutual funds and investment accounts. Individuals obviously cannot be expected to invest in stocks and mutual funds if they are unaware of the opportunity. These researchers find that awareness is positively associated with variables included in this study (education, income and age). Thus, although I do not directly measure this variable hopefully the inclusion of these correlates mitigates its omission.

Donkers and van Soest (1999) suggest that interest in finances might drive the investment decision. People who are interested in personal finance might be more aware and motivated to invest. Regrettably, I do not have data for this factor either. Although these scholars do not provide any information about correlates, I presume variables such as education, income, age, and occupation also are strongly related to interest in financial issues.

Keister (2005) found that the investment decisions of individuals are shaped by the investment behavior of their parents. In other words, an individual is more likely to participate in the stock market if as a child she observed her parents engaged in this activity. In fact, Chiteji and Stafford (1999) argue that the effect of parental investment behavior washes out some of the negative effects of race on asset ownership. Unfortunately, I do not have data on parent's investment behavior in any of the surveys used in this dissertation.

There is some evidence of social effects on investment ownership. Guiso et al. (2005) found that people with higher levels of general trust are more likely to buy stocks. Researchers

¹⁶For example, Campbell (2006) finds that education is a strong predictor of stock ownership, even with controls for age, income and wealth.

also argue that socially active households are more likely to own stocks (Hong et al. 2004) and that investment decisions are shaped by neighbor effects (Hong et al. 2005; Ivković and Weisbenner 2007; Brown et al. 2008). Keister (2005) found that households that regularly participate in religious services accumulate greater levels of assets. She argues religion shapes views about sexual behavior and marriage, which affect educational attainment. (As noted earlier, education is one of the strongest predictors of investment ownership.) Similar to Verba et al. (1995) Keister also argues that participation in religious organizations promotes skill development and social capital that may affect saving and investment. I do not have good data on social networks or religious participation for my surveys.

The final omitted variable is risk aversion. McCarthy (2004) defines risk aversion as “the desire of individuals for consumption to be similar in different states of the world.” In other words, individuals want to make investment decisions in ways that do not negatively affect their ability to realize the value of their assets over time. There is debate in the literature about the effect of risk aversion on investment behavior. Haliassos and Bertaut (1995) found that risk aversion does not explain investments in stocks. Guiso et al. (2005) also found that risk aversion does not predict stock ownership when they include measures of trust. Other scholars, however, found evidence that risk averse households are less likely to own risky assets such as stocks and mutual funds (Campbell 2006; Donkers and van Soest 1999) and hold more diversified investment portfolios (Dorn and Huberman 2005). Campbell (2006) and Heaton and Lucas (2000) found that households that are self-employed or have significant private business assets are more risk averse. Surveys typically use measures about respondents’ attitudes about hypothetical investment losses to proxy risk aversion. Unfortunately, I do not have any of these types of measures in the surveys used in this dissertation.

3.5 Matched groups

Valid causal inference using the potential outcomes model requires similarity between treatment groups (Gelman and Hill 2007; Morgan and Winship 2007). The treated and control groups on average should be exactly the same except for ownership of financial assets. Otherwise, we cannot rule out the possibility that there is some other difference between these groups that explains differences in political behavior. This condition is satisfied only to the extent that I successfully model the treatment assignment process. It is not enough to control for factors such as age, income and education that affect the probability of owning investments and political outcomes. I also need to make certain that the distribution of these characteristics is equal across both groups as much as possible. Estimates of the effects of the treatment indicator on outcomes will lead to incorrect causal inference if 1) there is a lack of complete overlap of the factors that affect asset ownership (e.g. income, education, age, etc.) between treatment and control groups or 2) these two groups are imbalanced (Gelman and Hill 2007).¹⁷

Tables 3.4 to 3.16 provide descriptive statistics for the matched groups by treatment indicator. In each table the first two columns provide the mean for each predictor and the propensity score before matching and the fifth and sixth columns provide the same after matching. The third and fourth columns provide two statistics to compare the pre-matched groups, and the two final columns provide the same for the matched groups. Although standardized mean differences are often used to compare means across different covariates, Imai et al. (2008b) argue this is a flawed approach to checking for balance between treatment

¹⁷A key assumption of the potential outcomes model is the stable unit value assumption (SUTVA). The causal effect of assets on the political behavior of one subject should not be affected by the assets of others in the sample. Several studies, however, argue for the presence of neighbor effects on individual investment decisions (Hong et al. 2005; Ivković and Weisbenner 2007; Brown et al. 2008). Scholars also suggest the possibility of cohort effects over the past 25 to 30 years as stock market participation became more common across households (McCarthy 2004; Starr 2008). Given the challenges of explicit modeling of causal effect exposure, I proceed with data analysis on the assumption of SUTVA. However, I keep this assumption in mind and “rather than consider SUTVA as overly restrictive...reflect on the plausibility of SUTVA [and] use such reflection to motivate a clear discussion of the meaning and scope of a causal effect estimate” (Morgan and Winship 2007, p. 38).

groups. Thus, I also include the mean of the empirical cumulative density functions (eCDF) for each predictor, which I use to check for covariate balance.¹⁸

The goal of propensity score matching is to minimize the differences between treated and control groups on each covariate. Although this difference should ideally be as close to nil as possible, in practice it is often difficult to achieve this benchmark. In assessing covariate balance, we generally want to see mean difference between treated and control groups of less than 10% across predictors. Especially important is balance on the propensity score, which represents the probability of being assigned to one treatment group or the other. In each of these tables, the mean eCDF for each covariate (column eight) should be 0.10 or less and the propensity score (bottom row in columns five and six) should be equivalent. In general, I achieve good balance between treated and control groups for each of my treatments.¹⁹

¹⁸The eCDF standardizes the quantile-quantile (QQ) plots, which compare the average distance between the empirical quantile distributions of the treated and control groups. The smaller the difference between these two distributions, greater the similarity of matched groups on the given covariate.

¹⁹Since I use the missingness of predictors as important information in the matching process, covariate means are not directly interpretable in these tables. This is because I code missing values for all predictors as “777.” In the case of the ANES data (Table 3.4), I include a missing data dummy indicator to account for slightly higher percentages of missing values for employment (13.61%) and occupation (7.69%). This allows me to make sure treated and control groups are balanced in the degree of missingness of these covariates.

Table 3.4: ANES group comparisons prior to and after matching on stock market investments treatment

Variable	Prior to matching				After matching			
	Treated <i>N</i> =92	Control <i>N</i> =345	Comparisons		Treated <i>N</i> =92	Control <i>N</i> =77	Comparisons	
	Mean	Mean	Std. Mean Difference	eCDF Mean	Mean	Mean	Std. Mean Difference	eCDF Mean
Gender (1=male, 0=female)	0.424	0.351	0.147	0.037	0.424	0.424	0.000	0.009
Age in 2002	47.293	52.820	-0.331	0.052	47.293	47.326	-0.002	0.021
Household income 2002 (3=high, 1=low)	44.228	28.438	0.089	0.167	44.228	52.250	-0.045	0.102
Education (3=high, 1=low)	10.652	1.968	0.108	0.070	10.652	2.239	0.104	0.016
Ethnic minority (1=yes, 0=no)	8.630	2.516	0.075	0.026	8.630	0.304	0.103	0.038
High skill occupation (1=yes, 0=no)	59.424	92.548	-0.160	0.032	59.424	59.380	0.000	0.011
Full-time employed (1=yes, 0=no)	110.326	113.000	-0.010	0.047	110.326	93.435	0.062	0.016
Married (1=yes, 0=no)	0.446	4.904	-8.922	0.015	0.446	0.446	0.000	0.002
Number of dep. children	0.652	2.870	-2.219	0.011	0.652	0.533	0.120	0.025
Homeowner (1=yes, 0=no)	0.717	5.130	-9.748	0.030	0.717	0.674	0.096	0.021
Employment missing variable indicator	0.141	0.145	-0.010	0.002	0.141	0.120	0.062	0.006
Occupation missing variable indicator	0.076	0.119	-0.160	0.021	0.076	0.076	0.000	0.001
Propensity score	0.266	0.196	0.486	0.168	0.266	0.254	0.083	0.016

Table 3.5: BHPS group comparisons prior to and after matching on stocks treatment

Variable	Prior to matching				After matching			
	Treated	Control	Comparisons		Treated	Control	Comparisons	
	<i>N</i> =789	<i>N</i> =4691			<i>N</i> =789	<i>N</i> =707		
	Mean	Mean	Std. Mean Difference	eCDF Mean	Mean	Mean	Std. Mean Difference	eCDF Mean
Gender (male=1, female=0)	0.485	0.422	0.127	0.032	0.485	0.487	-0.003	0.004
Average age during treatment period	44.894	45.055	-0.011	0.033	44.894	44.663	0.015	0.008
Avg. household income during treatment period	25.624	24.116	0.011	0.019	25.624	25.591	0.000	0.016
Education in 2000 (3=high, 1=low)	10.788	5.991	0.058	0.066	10.788	9.698	0.013	0.028
Race (non-white=1, white=0)	9.867	6.326	0.041	0.004	9.867	9.861	0.000	0.003
Average job skill level (3=high, 1=low)	9.209	18.303	-0.125	0.055	9.209	9.193	0.000	0.013
Employed fulltime entire treatment period (1=yes, 0=no)	12.265	15.4	-0.033	0.040	12.265	12.247	0.000	0.009
Married entire treatment period (1=yes, 0=no)	0.617	0.509	0.222	0.054	0.617	0.615	0.005	0.009
Highest number of children under 16 yrs	25.281	23.796	0.011	0.006	25.281	25.225	0.000	0.014
Homeowner anytime during treatment (1=yes, 0=no)	2.915	4.742	-0.047	0.059	2.915	2.905	0.000	0.004
Business owner anytime during treatment (1=yes, 0=no)	31.589	33.518	-0.013	0.005	31.589	31.579	0.000	0.003
High debt, top third of sample in 2000 (1=yes, 0=no)	25.954	32.798	-0.049	0.006	25.954	25.97	0.000	0.006
Health problem anytime during treatment (1=yes, 0=no)	11.028	14.872	-0.042	0.037	11.028	11.06	0.000	0.011
Propensity score	0.153	0.143	0.287	0.081	0.153	0.153	0.005	0.012

Table 3.6: BHPS group comparisons prior to and after matching on mutual funds treatment

Variable	Prior to matching				After matching			
	Treated	Control	Comparisons		Treated	Control	Comparisons	
	<i>N</i> =353	<i>N</i> =5912			<i>N</i> =353	<i>N</i> =338		
	Mean	Mean	Std. Mean Difference	eCDF Mean	Mean	Mean	Std. Mean Difference	eCDF Mean
Gender (male=1, female=0)	0.518	0.437	0.163	0.041	0.518	0.516	0.006	0.011
Average age during treatment period	49.602	45.439	0.308	0.089	49.602	49.602	0.000	0.012
Avg. household income during treatment period	15.187	25.325	-0.101	0.006	15.187	15.19	0.000	0.017
Education in 2000 (3=high, 1=low)	10.793	6.195	0.056	0.071	10.793	10.771	0.000	0.018
Race (non-white=1, white=0)	11.034	6.341	0.051	0.003	11.034	11.028	0.000	0.003
Average job skill level (3=high, 1=low)	17.819	16.966	0.008	0.075	17.819	17.802	0.000	0.019
Employed fulltime entire treatment period (1=yes, 0=no)	13.609	15.991	-0.024	0.017	13.609	13.629	0.000	0.001
Married entire treatment period (1=yes, 0=no)	0.64	0.536	0.218	0.052	0.64	0.635	0.012	0.002
Highest number of children under 16 yrs	14.955	24.997	-0.100	0.013	14.955	14.892	0.001	0.013
Homeowner anytime during treatment (1=yes, 0=no)	5.357	4.22	0.019	0.051	5.357	5.348	0.000	0.004
Business owner anytime during treatment (1=yes, 0=no)	33.133	32.525	0.004	0.018	33.133	33.099	0.000	0.015
High debt, top third of sample in 2000 (1=yes, 0=no)	13.479	32.006	-0.184	0.036	13.479	13.439	0.000	0.014
Health problem anytime during treatment (1=yes, 0=no)	11.184	14.21	-0.033	0.036	11.184	11.173	0.000	0.007
Propensity score	0.063	0.056	0.370	0.107	0.063	0.063	0.022	0.009

Table 3.7: BHPS group comparisons prior to and after matching on Personal Equity Plan treatment

Variable	Prior to matching				After matching			
	Treated	Control	Comparisons		Treated	Control	Comparisons	
	<i>N</i> =532	<i>N</i> =5549			<i>N</i> =532	<i>N</i> =486		
	Mean	Mean	Std. Mean Difference	eCDF Mean	Mean	Mean	Std. Mean Difference	eCDF Mean
Gender (male=1, female=0)	0.457	0.438	0.038	0.010	0.457	0.457	0.000	0.011
Average age during treatment period	48.712	45.229	0.263	0.088	48.712	48.646	0.005	0.006
Avg. household income during treatment period	22.404	25.17	-0.022	0.006	22.404	22.361	0.000	0.013
Education in 2000 (3=high, 1=low)	7.791	6.193	0.024	0.061	7.791	7.771	0.000	0.009
Race (non-white=1, white=0)	7.344	6.613	0.010	0.004	7.344	7.327	0.000	0.007
Average job skill level (3=high, 1=low)	6.752	17.364	-0.183	0.060	6.752	6.729	0.000	0.012
Employed fulltime entire treatment period (1=yes, 0=no)	10.658	15.748	-0.057	0.030	10.658	10.662	0.000	0.005
Married entire treatment period (1=yes, 0=no)	0.658	0.524	0.283	0.067	0.658	0.656	0.004	0.004
Highest number of children under 16 yrs	22.038	24.848	-0.023	0.007	22.038	21.987	0.000	0.017
Homeowner anytime during treatment (1=yes, 0=no)	0.959	4.713	-18.840	0.055	0.959	0.962	-0.019	0.000
Business owner anytime during treatment (1=yes, 0=no)	27.852	32.825	-0.034	0.014	27.852	27.838	0.000	0.007
High debt, top third of sample in 2000 (1=yes, 0=no)	10.487	32.962	-0.254	0.044	10.487	10.523	0.000	0.011
Health problem anytime during treatment (1=yes, 0=no)	10.429	14.845	-0.050	0.029	10.429	10.419	0.000	0.003
Propensity score	0.097	0.087	0.414	0.114	0.097	0.097	0.005	0.010

Table 3.8: NCDS group comparisons prior to and after matching on stocks treatment

Variable	Prior to matching				After matching			
	Treated	Control	Comparisons		Treated	Control	Comparisons	
	<i>N</i> =1498	<i>N</i> =7524			<i>N</i> =1498	<i>N</i> =1725		
	Mean	Mean	Std. Mean Difference	eCDF Mean	Mean	Mean	Std. Mean Difference	eCDF Mean
Gender (1=male, 0=female)	0.570	0.466	0.211	0.052	0.570	0.569	0.001	0.031
Net weekly household income in 1981 (3=high, 1=low)	38.352	31.972	0.039	0.039	38.352	37.816	0.003	0.012
Net weekly household income in 1991 (3=high, 1=low)	2.174	2.302	-0.154	0.045	2.174	2.172	0.002	0.009
Education level in 1991 (3=high, 1=low)	12.095	18.868	-0.078	0.073	12.095	12.083	0.000	0.015
Ethnic minority (non-white=1, white=0)	2.095	3.426	-0.033	0.001	2.095	2.091	0.000	0.004
Occupation level in 1991 (ISCO-88)	72.375	100.385	-0.180	0.094	72.375	72.337	0.000	0.019
Fulltime employed in 1991 (1=yes, 0=no)	1.251	1.222	0.001	0.043	1.251	1.248	0.000	0.024
Married (1=yes, 0=no)	12.683	26.284	-0.142	0.027	12.683	12.696	0.000	0.016
No. dependent children in 1991	58.289	64.416	-0.030	0.028	58.289	58.298	0.000	0.025
Homeowner in 1991 (1=yes, 0=no)	49.107	70.207	-0.113	0.054	49.107	49.107	0.000	0.006
Businessowner in 1991 (1=yes, 0=no)	0.643	0.732	-0.004	0.004	0.643	0.642	0.000	0.006
High debt [top third] (1=yes, 0=no)	14.828	25.522	-0.102	0.016	14.828	14.817	0.000	0.015
Longterm illness/disability in 1981 or 1991 (1=yes, 0=no)	1.724	1.311	0.012	0.002	1.724	1.197	0.015	0.015
Propensity score	0.180	0.163	0.370	0.105	0.180	0.180	0.002	0.029

Table 3.9: NCDS group comparisons before and after matching on mutual funds treatment

Variable	Before genetic matching				After matching			
	Treated	Control	Comparisons		Treated	Control	Comparisons	
	<i>N</i> =1498	<i>N</i> =7524			<i>N</i> =1498	<i>N</i> =1725		
	Mean	Mean	Std. Mean Difference	eCDF Mean	Mean	Mean	Std. Mean Difference	eCDF Mean
Gender (1=male, 0=female)	0.596	0.479	0.240	0.059	0.596	0.596	0.000	0.028
Net weekly household income in 1981 (3=high, 1=low)	28.641	33.089	-0.031	0.010	28.641	28.620	0.000	0.035
Net weekly household income in 1991 (3=high, 1=low)	2.150	2.282	-0.154	0.034	2.150	2.148	0.003	0.030
Education level in 1991 (3=high, 1=low)	12.609	17.897	-0.060	0.094	12.609	10.480	0.024	0.032
Ethnic minority (non-white=1, white=0)	0.037	3.400	-17.806	0.006	0.037	0.032	0.028	0.010
Occupation level in 1991 (ISCO-88)	63.736	96.688	-0.225	0.131	63.736	63.863	-0.001	0.025
Fulltime employed in 1991 (1=yes, 0=no)	0.757	1.241	-1.127	0.046	0.757	0.755	0.006	0.031
Married (1=yes, 0=no)	19.153	24.179	-0.042	0.006	19.153	19.172	0.000	0.035
No. dependent children in 1991	78.963	62.631	0.070	0.038	78.963	78.956	0.000	0.052
Homeowner in 1991 (1=yes, 0=no)	62.351	66.851	-0.021	0.039	62.351	62.343	0.000	0.015
Businessowner in 1991 (1=yes, 0=no)	0.103	0.739	-2.092	0.005	0.103	0.100	0.009	0.009
High debt [top third] (1=yes, 0=no)	18.763	24.173	-0.046	0.009	18.763	16.707	0.017	0.033
Longterm illness/disability in 1981 or 1991 (1=yes, 0=no)	0.195	1.330	-2.859	0.007	0.195	0.190	0.013	0.038
Propensity score	0.047	0.041	0.405	0.120	0.047	0.047	-0.004	0.025

Table 3.10: Group comparisons prior to and after matching: USA Today/Gallup January 2008 Poll

Variable	Prior to matching				After matching			
	Treated	Control	Comparisons		Treated	Control	Comparisons	
	<i>N</i> =1414	<i>N</i> =589	Std. Mean	eCDF	<i>N</i> =1414	<i>N</i> =346	Std. Mean	eCDF
	Mean	Mean	Difference	Mean	Mean	Mean	Difference	Mean
Gender (male=1, female=0)	0.525	0.463	0.124	0.031	0.525	0.529	-0.007	0.021
Age	62.153	57.961	0.047	0.067	62.153	60.170	0.022	0.042
Household income (1=low, 2=medium, 3=high)	71.897	72.589	-0.003	0.188	71.897	70.797	0.005	0.139
Education level (3=high, 1=low)	5.806	7.358	-0.031	0.105	5.806	5.205	0.012	0.066
Black or Latino (1=yes, 0=no)	4.487	2.829	0.028	0.033	4.487	1.800	0.046	0.025
Employed fulltime (1=yes, 0=no)	13.204	12.231	0.010	0.070	13.204	11.576	0.017	0.044
Married (1=yes, 0=no)	11.117	8.290	0.032	0.103	11.117	9.471	0.018	0.080
Dependent children (1=yes, 0=no)	12.986	10.803	0.022	0.034	12.986	11.305	0.017	0.035
Homeowner (1=yes, 0=no)	1.948	3.229	-0.044	0.086	1.948	1.381	0.019	0.061
Income missing variable indicator	0.090	0.092	-0.007	0.001	0.090	0.088	0.005	0.007
Propensity score	0.764	0.566	1.120	0.278	0.764	0.763	0.008	0.210

Table 3.11: Group comparisons prior to and after matching: CBS/New York Times March 2008 Poll

Variable	Prior to matching				After matching			
	Treated <i>N</i> =801	Control <i>N</i> =555	Comparisons		Treated <i>N</i> =801	Control <i>N</i> =335	Comparisons	
	Mean	Mean	Std. Mean Difference	eCDF Mean	Mean	Mean	Std. Mean Difference	eCDF Mean
Gender (male=1, female=0)	0.442	0.364	0.157	0.039	0.442	0.444	-0.005	0.045
Age group (1=18-29, 2=30-44, 3=45-64, 4=65+)	2.994	2.906	0.113	0.058	2.994	2.990	0.005	0.048
Household income (1=low thru 6=high)	56.537	46.099	0.054	0.220	56.537	55.450	0.006	0.161
Education level (3=high, 1=low)	5.401	7.699	-0.049	0.096	5.401	5.383	0.000	0.058
Black or Latino (1=yes, 0=no)	1.055	8.591	-0.275	0.042	1.055	1.044	0.000	0.010
Currently employed (1=yes, 0=no)	0.582	1.856	-2.581	0.042	0.582	0.594	-0.024	0.063
Married (1=yes, 0=no)	1.662	6.074	-0.161	0.073	1.662	1.666	0.000	0.048
Dependent children (1=yes, 0=no)	0.298	0.277	0.046	0.010	0.298	0.300	-0.003	0.030
Homeowner (1=yes, 0=no)	4.793	11.906	-0.130	0.069	4.793	4.789	0.000	0.025
Propensity score	0.600	0.577	0.369	0.069	0.600	0.600	0.000	0.038

Table 3.12: Group comparisons prior to and after matching: NBC/Wall Street Journal September 2008 Poll

Variable	Prior to matching				After matching			
	Treated <i>N</i> =660	Control <i>N</i> =447	Comparisons		Treated <i>N</i> =660	Control <i>N</i> =221	Comparisons	
	Mean	Mean	Std. Mean Difference	eCDF Mean	Mean	Mean	Std. Mean Difference	eCDF Mean
Gender (male=1, female=0)	0.518	0.450	0.137	0.034	0.518	0.515	0.006	0.022
Age group (1=18-24 thru 12=75+)	12.591	7.895	0.070	0.055	12.591	8.847	0.056	0.039
Household income	63135.080	39238.991	0.705	0.195	63135.080	63002.761	0.004	0.100
Education level (3=high, 1=low)	10.768	16.067	-0.067	0.088	10.768	7.157	0.045	0.064
Black or Latino (1=yes, 0=no)	7.252	5.595	0.022	0.064	7.252	3.800	0.047	0.044
Occupation level	17.153	21.322	-0.040	0.089	17.153	17.236	-0.001	0.056
Currently employed (1=yes, 0=no)	14.788	17.879	-0.030	0.055	14.788	14.770	0.000	0.032
Married (1=yes, 0=no)	6.608	7.468	-0.013	0.069	6.608	5.445	0.017	0.020
Dependent children (1=yes, 0=no)	5.083	9.000	-0.065	0.022	5.083	5.079	0.000	0.007
Propensity score	0.652	0.514	0.839	0.223	0.652	0.650	0.010	0.118

Table 3.13: Group comparisons prior to and after matching: NBC/Wall Street Journal September 2008 Poll (Form B)

Variable	Prior to matching				After matching			
	Treated <i>N</i> =318	Control <i>N</i> =234	Comparisons		Treated <i>N</i> =318	Control <i>N</i> =113	Comparisons	
	Mean	Mean	Std. Mean Difference	eCDF Mean	Mean	Mean	Std. Mean Difference	eCDF Mean
Gender (male=1, female=0)	0.531	0.444	0.174	0.044	0.531	0.538	-0.013	0.009
Age group (1=18-24 thru 12=75+)	11.657	9.295	0.039	0.071	11.657	11.484	0.003	0.062
Household income	65583.245	39093.175	0.801	0.213	65583.245	64404.000	0.036	0.129
Education level (3=high, 1=low)	9.881	15.423	-0.074	0.098	9.881	7.362	0.034	0.063
Black or Latino (1=yes, 0=no)	7.519	3.675	0.051	0.055	7.519	0.289	0.096	0.043
Occupation level	20.022	17.004	0.027	0.075	20.022	19.912	0.001	0.041
Currently employed (1=yes, 0=no)	17.774	13.829	0.035	0.044	17.774	17.808	0.000	0.017
Married (1=yes, 0=no)	5.607	7.150	-0.025	0.071	5.607	3.211	0.039	0.019
Dependent children (1=yes, 0=no)	2.830	10.269	-0.171	0.026	2.830	2.792	0.001	0.023
Propensity score	0.645	0.483	0.889	0.238	0.645	0.645	-0.001	0.142

Table 3.14: Group comparisons prior to and after matching: CBS December 2008 Poll

Variable	Prior to matching				After matching			
	Treated <i>N</i> =839	Control <i>N</i> =531	Comparisons		Treated <i>N</i> =839	Control <i>N</i> =349	Comparisons	
	Mean	Mean	Std. Difference	eCDF Mean	Mean	Mean	Std. Difference	eCDF Mean
Gender (male=1, female=0)	0.406	0.409	-0.005	0.001	0.406	0.398	0.017	0.005
Age group (1=18 to 29 thru 4=65+)	3.011	2.902	0.134	0.050	3.011	3.017	-0.007	0.033
Household income (1=low thru 6=high)	58.735	50.930	0.039	0.216	58.735	59.544	-0.004	0.151
Education level (3=high, 1=low)	6.232	6.497	-0.005	0.105	6.232	7.125	-0.017	0.083
Black or Latino (1=yes, 0=no)	12.120	6.058	0.063	0.041	12.120	4.746	0.077	0.004
Currently employed (1=yes, 0=no)	1.529	0.384	0.043	0.074	1.529	0.602	0.035	0.060
Married (1=yes, 0=no)	7.138	3.363	0.053	0.076	7.138	3.455	0.052	0.057
Dependent children (1=yes, 0=no)	2.133	1.706	0.011	0.013	2.133	1.176	0.025	0.024
Propensity score	0.641	0.567	0.610	0.146	0.641	0.640	0.011	0.093

Table 3.15: Group comparisons prior to and after matching: USA Today/Gallup December 2008 Poll

Variable	Prior to matching				After matching			
	Treated <i>N</i> =644	Control <i>N</i> =355	Comparisons		Treated <i>N</i> =644	Control <i>N</i> =192	Comparisons	
	Mean	Mean	Std. Mean Difference	eCDF Mean	Mean	Mean	Std. Mean Difference	eCDF Mean
Gender (male=1, female=0)	0.542	0.451	0.183	0.046	0.542	0.548	-0.012	0.024
Age	64.803	60.375	0.049	0.048	64.803	61.856	0.032	0.040
Household income (1=low, 2=medium, 3=high)	85.427	65.017	0.085	0.184	85.427	85.427	0.000	0.107
Education level (3=high, 1=low)	8.543	8.654	-0.002	0.106	8.543	8.526	0.000	0.061
Black or Latino (1=yes, 0=no)	6.110	19.865	-0.202	0.041	6.110	8.550	-0.036	0.024
Professional/business class (1=yes, 0=no)	6.616	13.400	-0.099	0.105	6.616	6.616	0.000	0.080
Fulltime employed (1=yes, 0=no)	1.716	4.701	-0.098	0.062	1.716	0.567	0.038	0.048
Married (1=yes, 0=no)	3.075	2.597	0.011	0.085	3.075	0.702	0.055	0.042
Dependent children (1=yes, 0=no)	9.941	8.989	0.011	0.019	9.941	9.930	0.000	0.020
Homeowner (1=yes, 0=no)	2.082	2.823	-0.024	0.081	2.082	2.075	0.000	0.038
Income missing variable indicator	0.107	0.082	0.082	0.013	0.107	0.107	0.000	0.001
Propensity score	0.727	0.496	1.275	0.274	0.727	0.725	0.007	0.186

Table 3.16: Group comparisons prior to and after matching: CBS/New York Times January 2009 Poll

Variable	Prior to matching				After matching			
	Treated	Control	Comparisons		Treated	Control	Comparisons	
	<i>N</i> =733	<i>N</i> =368	Std. Mean	eCDF	<i>N</i> =733	<i>N</i> =226	Std. Mean	eCDF
	Mean	Mean	Difference	Mean	Mean	Difference	Mean	
Gender (male=1, female=0)	0.445	0.378	0.135	0.034	0.445	0.457	-0.025	0.017
Age group (1=18-29 thru 4=65+)	2.959	3.011	-0.064	0.052	2.959	2.943	0.020	0.055
Household income	39.065	51.065	-0.075	0.213	39.065	36.407	0.017	0.162
Education level (3=high, 1=low)	2.521	6.245	-6.904	0.122	2.521	2.438	0.155	0.119
Black or Latino (1=yes, 0=no)	5.386	2.274	0.049	0.026	5.386	0.095	0.083	0.010
Social class group (1=low thru 5=high)	7.154	8.709	-0.027	0.090	7.154	7.042	0.002	0.067
Currently employed (1=yes, 0=no)	0.570	0.394	0.356	0.088	0.570	0.576	-0.011	0.068
Married (1=yes, 0=no)	1.733	4.595	-0.100	0.100	1.733	0.612	0.039	0.073
Dependent children (1=yes, 0=no)	0.304	0.215	0.195	0.045	0.304	0.297	0.015	0.041
Homeowner (1=yes, 0=no)	6.196	21.780	-0.244	0.077	6.196	6.156	0.001	0.039
Propensity score	0.682	0.633	0.510	0.114	0.682	0.681	0.016	0.075

3.6 Causal inference

The goal of causal inference is to evaluate the aggregate effect of the acquisition of financial assets (stocks and mutual funds) on change in political behavior. I accomplish this in two different ways. First, to make the results as transparent as possible, I evaluate change in the outcome variables across treatment groups. For example, I compare the percentage of people who changed their party preference from the Labour Party to the Conservative Party after acquiring stocks or mutual funds to those who did not acquire any financial assets. This simple analysis provides first-level evidence of any general trends that distinguish the two treatment groups.

Next, I follow Gelman and Hill (2007) and Rubin (2008) to make causal inference using regression on the treatment variable. I regress political behavior (the post-test or dependent variable) on financial asset ownership (treatment indicator) and prior political behavior (pre-test). I generally use binary logistic regression as most of my measures of political behavior are dichotomous. (A subject either voted in an election or she did not. She is or is not a political party member.) Logistic regression is the preferred method for situations with a dependent variable with two outcome categories and continuous or dichotomous independent variables. Where I have polychotomous dependent variables, such as questions concerning political attitudes, I use either ordered logistic regression or ordinary least squares regression (OLS).

Logistic regression uses maximum likelihood estimation to maximize the odds that the observed values of the dependent variable (political behavior) may be predicted from the observed values of the independent variables (financial assets). Logistic regression does not share some of the more challenging assumptions of OLS. It does not assume a linear relationship between dependent and independent variables, does not require normally distributed variables, and does not assume uniform variance of residuals. Logistic regression does assume, however, a linear relationship between the independent variables and the log odds

(logit) of the dependent variable. Otherwise, the customary concerns of OLS regression still apply, such as sampling error, multicollinearity, and omitted variables. Since I use matching with replacement, I use weighted logistic regression.²⁰

I evaluate the “substantive significance” of my findings by comparison of the effect of the intervention of financial asset ownership under alternative treatments (treated and control) (Morgan and Winship 2007). A common way to evaluate regression estimates is to compare the results to the null hypothesis of no effect. Although I follow convention and report point estimates, standard errors and p-values, in this dissertation empirical results are interpreted in terms of relative risk, a common method of reporting results of clinical trials (Zhang and Kai 1998; Davies et al. 1998).²¹ I calculate relative risk by the risk ratio, the probability of a given outcome if treated compared to the probability of the same outcome if untreated.²²

Risk ratios offer the advantage of literal interpretation of increased or decreased likelihood of an outcome between treated and control groups. For example, a risk ratio of 2 means a given outcome is two times more likely to occur in the treated group than in the control group. Conversely, a risk ratio of 0.5 means a given outcome is two times *less likely* to occur in the treated group than in the control group. The minimum threshold for a significant finding in this dissertation is a risk ratio of 2; risk ratios of 3 or higher represent substantively meaningful differences between treatment groups (Taubes 1995).²³ Since point estimates and

²⁰There appears to be little formal advice on this subject. Gary King’s online documentation for the MatchIt package makes a very limited reference to the need to use weights or fixed effects to account for the difference in numbers of treated and untreated cases. (gking.harvard.edu/matchit/docs/Conducting_Analyses-af.html). The only other reference on this subject that I found in the matching literature is a table note in Dehejia and Wahba (2002) that refers to the use of weighted least squares for data matched with replacement (see note C in Table 2 on page 155).

²¹Another way to think of relative risk is as the average treatment effect of a given financial asset on a given measure of political behavior.

²²Another way to compute relative risk is the odds ratio, which is the ratio of the odds of an outcome in the treated group to the odds of the same outcome in the control group. Logistic regressions compute log odds, the natural logarithm of odds ratios. Odds ratios, however, are frequently misinterpreted when calculating relative risk. Moreover, the odds ratio can give a biased interpretation of relative risk. “If the odds ratio is interpreted as a relative risk it will always overstate any effect size: the odds ratio is smaller than the relative risk for odds ratios of less than one, and bigger than the relative risk for odds ratios of greater than one” (Davies et al. 1998).

²³This minimum threshold accounts for both the uncertainty inherent to parametric estimates as well as the possibility of known or unknown confounds. Risk ratios above two represent greater certainty of actual

standard errors are arrived at through a process of data reduction, I avoid information loss only if my data are normally distributed. Simulation allows me to represent the uncertainty of my parameter estimates by summarizing statistical inferences using random numbers rather than point estimates and standard errors alone (Gelman and Hill 2007, p. 137). For each logistic regression I present the mean risk ratio and 95% confidence intervals of 100,000 simulations (Imai et al. 2008a).²⁴

3.7 Missing data

Missing data are a common challenge for observational studies. Incomplete data pose a change to internal validity (relationship between observed outcome and independent variables) and external validity (generalizability of findings) (McKnight et al. 2007). Hence, missing data are a direct threat to causal inference of the effects of financial assets on political behavior. I treat missing data differently prior to and after matching.

Prior to matching, I treat the missingness of my predictors (covariates) as information. In other words, I assume that by refusing to answer a question a respondent may disclose information about themselves related to their treatment status. Since my interest at the matching stage is to create like treated and untreated groups, I include a missing value indicator for this case and use this information in propensity score matching (D’Agosotino and Rubin 2000). If a given covariate has 10% or more missing values across cases I also include a separate binary indicator as a predictor to fit the propensity score model.²⁵ Haviland et al. (2007) indicate this approach helps balance the observed values of the predictors as well as the pattern of missingness between treatment groups.

After matching, I am now concerned about *all* missing variables—predictors and out-

²⁴Since I use multiple imputation to account for missing data (described in next section), these are mean simulated risk ratios and 95% confidence intervals for five multiply imputed data sets.

²⁵The binary indicator is applied to all observations for the affected covariate, with missing values coded as 1 and non-missing values coded as 0.

comes. The basic concern at this point is the missing data mechanism. Prior to matching, if a person was unwilling to answer a survey question about, say income, I could take this refusal as information about that person and use it to match her with another person similar in every way who also refused to answer information about his income. But for causal inference this is problematic. I can ignore *how* the data came to be missing only if the data are missing at random (MAR) and the factors that influence missingness are unrelated to the outcomes under analysis (Allison 2002; McKnight et al. 2007). Two people with low and high incomes could refuse to answer the same question about household income for very different reasons. Given the strong relationship between socioeconomic status and political behavior, this missingness is non-trivial.

My approach to missing data during data analysis is three-fold. First, I drop any variable in which more than 20% of the observations are missing. This decision rule does not affect any of the predictor or treatment variables, but does limit the availability of some of the pre-test and post-test variables. Second, I use the Amelia package in R to multiply impute missing values (King et al. 2001). Multiple imputation assumes the data at minimum are missing at random (even better would be “missing completely at random”). Although my data most likely do not completely fulfill this implied ignorability criterion, multiple imputation is generally considered a good solution for missing data (King et al. 2001; Allison 2002; McKnight et al. 2007). I create five imputation data sets, which is consistent with general recommendations in the statistics literature (McKnight et al. 2007, p. 201). I include in the imputation process propensity scores, predictor, treatment, and outcome variables that may be correlated with the variables that have missing data or associated with the probability that those variables have missing data (Allison 2002, p. 35).²⁶ Finally, I use the Zelig package in R to combine the multiply imputed data sets and perform regression analysis (Imai et al. 2008a).

²⁶The binary missingness indicator used during the matching process plays no role at this stage.

3.8 Limitations and delimitations

Methodologically, I follow Rubin's admonition that research design trumps statistical analysis for causal inference (Rubin 2008). I focus on quasi-experimental research design in place of more powerful and sophisticated statistical tools. For example, I do not use multilevel models to account for the effect of groups or regions in my estimate of the effect of financial assets on political behavior. I also seek to control for treatment assignment mechanisms instead of modeling selection (Heckman 1976). Despite the findings of neighbor effects on investment decisions (Shiller and Pound 1989; Hong et al. 2004; Ivković and Weisbenner 2007; Brown et al. 2008) and arguments for the spatial structure of political participation (Cho and Rudolph 2008), I do not attempt to conduct a spatial analysis of these data. Substantively, there is one issue from the literature that is beyond the scope of this dissertation. Scholars argue that political behavior is often motivated by economic issues, particularly for those with few or no resources (Huntington 1968; Gurr 1971; Scott 1985; Acemoglu and Robinson 2006). Although I rigorously examine the effect of owning financial assets on political behavior, I do not study the effect of having few or no assets.

3.9 Summary

Convincing causal inference requires careful attention to research design to rule out biases and alternative explanations. Although I have observational data, I keep the experimental model firmly in mind. I identify treatment groups and seek to make them as alike as possible. I address the assignment mechanism into treatment groups with predictors that represent potential "back-door" paths to the outcome variables. I evaluate the effect of assets by comparison of the effect of "intervention" of investments between treated and control groups. I use a combination of longitudinal and cross-sectional surveys from the United States and Britain over a 25-year period. In sum the research design of this dissertation intends to offer rigorous tests of the causal effects of financial assets on political behavior.

Chapter 4

Partisanship

4.1 Introduction

In this chapter I investigate the effect of acquisition of stocks and mutual funds on partisan identity in the United States and Britain. Both political systems have a well-defined left-right ideological dimension regarding economic and social policies, so investors can reasonably be expected to be able to identify which party is presumed to be more “pro-market.” In the U.S., the investor class thesis predicts that investors overtime will shift their support toward the Republican Party. Extension of the theory to Britain predicts that investors will exhibit similar behavior toward the Conservative Party. I hypothesize, however, that financial assets have no effect on partisanship, except possibly for people under 34 years for whom partisan identity is still somewhat in flux. Otherwise, there is no theoretical reason to expect that purchase of stocks and mutual funds will have any influence on partisan identity.

Partisanship is one of the most fundamental concepts in political science. “Partisanship [is] the central factor in explaining not just how people vote, but also how they see the political world” (Achen 2002a, p. 151). The investor class theory claims that as a person acquires stocks and mutual funds, her economic interests begin to alter her worldview. Over time her political preferences shift to support pro-market policies consistent with her financial interests. This shift in economic preferences also influences other beliefs, such as the proper role of government in the economy and society. This re-evaluation of interests and preferences over time leads to a shift in partisan support toward political parties that support these

positions (Nadler 2005; Ponnuru 2004).¹

The investor class theory fits squarely in the middle of the debate in the partisanship literature over the stability of partisan attachments. The political socialization model (also known as the “Michigan school”) contends that party ID is a form of social identity (Campbell et al. 1960; Green et al. 2002). People think of themselves as members of partisan groups in much the same way they see themselves as members of religious organizations (Green and Palmquist 1990; Green et al. 2002). They might over time not fully support all tenets of their faith but are relatively unlikely to switch religions. Partisan attachments are initially formed by parental partisanship (Franklin 1984; Achen 2002a), but later are shaped by social influences in the early adult years. Party ID generally settles down by the early thirties and for the most part does not change thereafter (Miller 1991; Abramson and Ostrom Jr. 1991; Green et al. 2002). In the U.S. context, when change does occur it is more likely to be movement between Independents and the major political parties rather than between the Democratic and Republican parties. Unless financial asset ownership somehow shapes social identity (Di Tella et al. 2007), the political socialization model predicts stocks and mutual funds will have no effect on partisanship.

The revisionist model, however, argues that partisan attachments are endogenous to individual interests and political party positions. “New issues, or old issues that become more salient, may divide the electorate in new ways, such that regardless of the positions the parties are perceived to adopt, they will lose some of their past supporters and gain new ones” (Franklin and Jackson 1983, p. 968). Revisionists assert that partisanship is not as stable as the political socialization model contends, and that party ID is shaped by rational prospective evaluations of how a political party will benefit individual interests (Achen 1992, 2002a; Franklin and Jackson 1983; Franklin 1984; Markus and Converse 1979; Abramowitz

¹Investor class proponents are careful not to claim that everyone will become a Republican or that this change will occur overnight. Rather, these analysts argue that the acquisition of stocks and mutual funds affects the *probability* of becoming Republican (Nadler 2000b, 2005; Ponnuru 2004).

and Saunders 2006; Clarke et al. 2009).²

This forward-looking aspect of the revisionist model accords well with both the investor class and asset effect theories. Nadler (1999) contends that financial investments shape the individual's orientation toward the future and her preferences over government policies. Sherraden (1991) asserts that the accumulation of assets today leads to expectations of greater assets in the future. Grover Norquist, president of Americans for Tax Reform, claims that “[investment] changes your time horizon, over time it changes your party affiliation, and it also changes what you read and what you watch on TV” (Deane and Balz 2003). The revisionist model predicts stocks and mutual funds can shape partisan attachments by shifting preferences towards parties that will best protect and enhance these financial interests.

4.2 Prior research

Scant research exists on the effect of financial asset ownership on political behavior in general, much less partisanship (Barabas 2006; Kaustia and Torstila 2008). Kaustia and Torstila (2008) used a natural experiment to examine the effect of involuntary stock ownership on voting for right-wing parties in Finland. In 1997-1998, tens of thousands of customers of three state-owned Finnish telecommunications companies received stock market shares from the privatization of these firms.³ These analysts investigated the change in right-wing voting from 1995 to 1999 in the 391 zip codes served by these companies. They used propensity score matching to identify treated and control groups for the intervention of telecom share ownership. Controlling for a range of possible alternative explanations for changes in voting patterns, these scholars found an increase ranging from 0.6 to 1.0 percent vote share for the conservative political party (National Coalition) in voting districts that received telecom shares.

²The notable exception is Fiorina (1981), who emphasizes *retrospective* evaluations of party performance.

³The value of these shares ranged from \$4,000 to \$8,000.

Saunders (1995), however, presented mixed findings regarding the effect of share ownership of privatized water and electricity companies on partisanship in Britain in the late 1980s-early 1990s. In 1989, the Conservative Party government privatized these public utilities despite majority public opposition to this change. Support for the government's privatization efforts never exceeded forty percent.⁴ The Labour Party, perhaps sensing a political opportunity, indicated that it would renationalize these industries if it gained control of the government in the 1992 General Election. Based on a 1989-1991 panel survey of 828 individuals in south and northwest England, Saunders found that Labour Party supporters who bought privatized water company shares were about five times more likely to switch support to the Conservative Party in 1991 than those who did not.⁵ Apparently, these Labour Party supporters perceived that their financial self-interest would be threatened by a Labour government. However, Saunders found no equivalent effect on voting intentions of supporters of other political parties or undecided voters. "Share purchase discouraged Labour supporters from voting Labour, but it did not encourage non-Conservatives to vote Tory" (Saunders 1995, p. 135).

The effect of financial assets on partisanship is most directly investigated by Davis and Cotton (2007) with data from the 1998-2004 American National Election Studies (ANES).⁶ Davis and Cotton construct a dichotomous measure of asset ownership from a question about stock market participation: Do you personally, or jointly with a spouse, have any money invested in the stock market right now—either in an individual stock or in a mutual fund? Partisanship is operationalized as a dichotomous measure of Republican party ID. These analysts estimate ordinary least squares regression of Republican party ID on stock market participation, controlling for age, income, education, race, gender, home ownership, political

⁴Despite widespread negative public opinion toward privatization, roughly 2.5 million people bought water company shares.

⁵Respondents were surveyed three months before and eighteen months after water industry privatization. Saunders did not appear to investigate whether party identification had any effect on the willingness to buy privatized shares.

⁶These scholars use a series of cross-sectional data and a three-wave year panel (2000-2004).

ideology, and prior party ID. Davis and Cotton find shareholders were about 30% more likely to identify with the Republican Party in 1998-2002 and 130% in 2004. They attribute this huge jump in 2004 to presidential campaign promises by President George W. Bush to privatize Social Security.

This conclusion is puzzling since nearly identical proportions of investors voted for the Democratic and Republican presidential candidates in 2000 and 2004 (Continetti 2005). Even the most ardent supporters of the investor class theory concede the effects of asset ownership likely take longer to manifest than two to four years (Nadler 2000b, 2005). Since the authors are unable to purge asset ownership from partisanship prior to 2000, we cannot discount the possibility that party ID is driving stock market participation. This is particularly important as analysts find that investment decisions may be shaped by social interactions (Hong et al. 2005; Ivković and Weisbenner 2007; Brown et al. 2008). Further, despite the use of a number of control variables for political orientation, Davis and Cotton do not sufficiently account for important differences that affect the probability of financial asset ownership. For example, employment, occupation level, and marital status are known predictors of stock market participation omitted in this study. As much as possible, we need to account for all known factors that affect financial asset ownership *and* partisan identity in order to make valid causal inference.

4.3 Data

To ascertain the causal effect of financial assets on party ID, I focus on the effects of *change* in financial asset ownership on subsequent *change* in partisanship. The important question is whether acquisition of stocks and mutual funds has any effect upon partisan attachments. I use longitudinal data from the American National Election Studies (2000-2002), the British Household Panel Survey (1993-2005), and the National Child Development Study (1981-2000). For each of these surveys I establish a baseline to identify respondents who report

not owning any financial assets. I use a subsequent reporting period to sort through these respondents to identify those that acquired financial assets and those that did not. I then use propensity score matching to divide respondents into treated and control groups based on the predictors of asset ownership listed in Table 3.3.

Treatment indicators vary by survey. The ANES asks the single question about stock market participation described in section 4.2. The baseline year for stock market participation in the ANES is 2000. The treatment period is 2000-2002, meaning that investors acquired stock investments during this two year period.⁷ The BHPS includes questions about ownership of stocks, mutual funds (known in Britain as “unit trusts”), and personal equity plan (PEP) accounts. PEPs were tax-privileged investment accounts available to British investors between 1986-1999.⁸ From 1992 to 1999 investors were able to invest in either single-company PEPs (annual limit of £3,000) or general PEPs that invested in mutual funds (annual limit of £6,000). The baseline for the BHPS is 1995; investors acquired these assets during the subsequent five-year period (1995-2000).

The NCDS includes questions about stock and mutual fund (unit trust) ownership. The baseline is 1981; investors acquired these assets during the following decade (1981-1991). In sum, the treatment periods for these three surveys range from two to ten years. Ideally, I would prefer much greater precision about exactly when these assets were acquired. Unobserved heterogeneity on duration of asset ownership potentially biases any findings and challenges the validity of causal inference. It is possible that the value of financial assets may interact with ownership to have an effect on partisanship. Unfortunately, I only have data on asset values for the NCDS data set. At minimum, however, for all three data sets I am at least able to distinguish between those who acquired financial assets during some defined treatment period from those who did not.

The pre- and post-tests for these interventions are measures of partisan identification.

⁷Although the ANES panel extends to 2004, I am limited to two years as one-third of the cases are missing for this third wave.

⁸PEP accounts were free of capital gains and income taxes.

The conceptualization of this measure is slightly different on either side of the pond. The American survey (ANES) asks a standard question about partisan identity: “Generally speaking, do you think of yourself as a Republican, a Democrat, an Independent, or what?” By contrast, the British surveys (BHPS and NCDS) inquire about vote choice—which party the respondent would be most likely to vote for if an election was held tomorrow. Respondents in these surveys generally choose from a half a dozen parties from Britain’s multiparty system.

This means these surveys measure different concepts, since partisan identity does not necessarily constrain vote choice (Green et al. 2002). In other words, it is possible for a Liberal Party supporter to vote for Labour or the Tories without changing her sense of partisan attachment. (It also may be rational if she perceives voting for the Liberals as a wasted vote.) So in a sense, the vote choice question is a noisy measure of partisanship that is less precise than partisan identity. Thus, we might find greater movement in vote choice over time than partisan identity. If financial assets do in fact have any effect on partisanship, the bar appears lower for the British data sets.

I operationalize party ID as a dichotomous variable indicating the presence or absence of support for a given political party. For the ANES data, the pre-test is partisan identification in 2000; the post-test is party ID in 2002. For the BHPS data, the pre-test is party ID in 1995; post-tests are party ID in 2000 and 2002. The pre-test for the NCDS data set is party ID in 1981; post-tests are party ID in 1991 and 2000.

4.4 Partisan stability

Before I begin causal analysis of the effect of assets on partisanship, I need to first ascertain the stability of partisan identity in my data sets. If party ID is unstable over time, this would challenge the validity of causal inference. Green et al. (2002) find partisanship to be relatively stable in both the United States and Britain over a thirty-year period (1960s-

1990s). Although I have no reason to suspect that partisanship operates differently in the data sets that I use in this dissertation, I examine the stability of partisan attachments to rule out possible alternative explanations for any findings. This is particularly important since these treatment groups were matched without any reference or prior knowledge of their partisan attachments. To ascertain the stability of party ID in my data sets, I follow Green et al. (2002) to compute the polychoric correlations of party identification over time. A polychoric correlation is a technique for estimating the correlation between two theorized normally distributed continuous latent variables from two observed ordinal variables.⁹ It measures the degree to which party ID for the same respondents correlate between successive waves of measurement.

At least three waves of panel data are necessary to compute polychoric correlations, although four waves or more are preferable. Since I only have two waves of data for the ANES data set, I limit computation of the polychoric correlation matrix to the BHPS and NCDS data sets. In Table 4.1 I divide the data into multiple panels based on treatment indicator and separate dichotomous party identification measures for the Conservative Party and Labour Party. This allows me to ascertain the stability of partisan identification for both major British political parties under three separate treatment conditions—stockholding, mutual fund holding and PEP account ownership. Since I use multiple imputation to account for missing data, I follow Roscino and Pollice (2004) and present the average association matrix of five polychoric correlation matrices separately computed for each of the multiply imputed data sets for the BHPS and NCDS matched groups.

To measure partisan stability, I do not examine the correlation between two successive periods, but rather *the rate of decline of this correlation over time*. If party identification changes over time without measurement error, in a three wave panel the correlation between the first and last waves (indicated as r_{13}) is the product of r_{12} and r_{23} . However, if parti-

⁹Since both observed variables are dichotomous technically this is a special case known as a tetrachoric correlation.

sanship is stable but measured with error, then r_{13} will be greater than $r_{12}r_{23}$ (Green et al. 2002, p. 67). For example, in the first panel of Table 4.1 identification with the Conservative Party among the stockholding treatment groups correlates at 0.898 between 1993 and 1994 (r_{12}). Between 1993 and 2002 identification with the Tories correlates at 0.781 (r_{16}). If Conservative Party ID was changing during this period I would expect to see a correlation of only 0.726 ($r_{12}r_{26}$). I find similar results for each set of treatment groups and partisanship measures in Table 4.1 (in some cases with greater differences).¹⁰ Although party ID at first glance appears less stable in the NCDS matched groups, r_{13} is greater than $r_{12}r_{23}$ in every panel in Table 4.2. Partisanship is stable under each treatment condition in both the BHPS and NCDS matched data sets. Therefore, I can safely rule out instability in partisan attachments as a rival explanation for any potential change in partisanship after acquisition of financial assets.

¹⁰According to Green et al. (2002) the discrepancy in correlation between successive panels is largely due to response or measurement error.

Table 4.1: Partisan stability in British Household Panel Study matched groups, 1993-2002

	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6	Mean	SD
Stocks 1993-2002 BHPS (N=1496)								
Conservative Party ID								
1993	1						0.191	0.393
1994	0.898	1					0.161	0.368
1995	0.898	0.900	1				0.152	0.360
2000	0.807	0.851	0.853	1			0.175	0.380
2001	0.799	0.807	0.840	0.922	1		0.157	0.364
2002	0.781	0.808	0.821	0.912	0.925	1	0.155	0.362
Labour Party ID								
1993	1						0.175	0.380
1994	0.908	1					0.193	0.395
1995	0.901	0.928	1				0.219	0.414
2000	0.821	0.858	0.852	1			0.209	0.407
2001	0.774	0.815	0.822	0.908	1		0.248	0.432
2002	0.802	0.826	0.842	0.929	0.928	1	0.223	0.416
Mutual Funds 1993-2002 BHPS (N=691)								
Conservative Party ID								
1993	1						0.266	0.442
1994	0.913	1					0.228	0.420
1995	0.861	0.927	1				0.230	0.421
2000	0.798	0.850	0.830	1			0.230	0.421
2001	0.787	0.839	0.840	0.929	1		0.214	0.410
2002	0.802	0.854	0.852	0.920	0.908	1	0.220	0.415
Labour Party ID								
1993	1						0.155	0.362
1994	0.941	1					0.162	0.369
1995	0.931	0.955	1				0.188	0.391
2000	0.898	0.879	0.902	1			0.188	0.391
2001	0.834	0.821	0.848	0.944	1		0.225	0.418
2002	0.860	0.808	0.869	0.959	0.941	1	0.210	0.408
PEP 1993-2002 BHPS (N=1018)								
Conservative Party ID								
1993	1						0.246	0.431
1994	0.898	1					0.201	0.402
1995	0.903	0.907	1				0.203	0.402
2000	0.873	0.886	0.877	1			0.220	0.415
2001	0.841	0.829	0.825	0.925	1		0.193	0.394
2002	0.860	0.849	0.861	0.948	0.953	1	0.200	0.401
Labour Party ID								
1993	1						0.175	0.380
1994	0.921	1					0.204	0.403
1995	0.922	0.935	1				0.221	0.415
2000	0.847	0.895	0.853	1			0.210	0.407
2001	0.832	0.879	0.850	0.940	1		0.239	0.426
2002	0.833	0.896	0.877	0.956	0.959	1	0.223	0.416

Source: British Household Panel Study.

Table 4.2: Partisan stability in National Child Development Study matched groups, 1981-2000

	Wave 1	Wave 2	Wave 3	Mean	SD
Stocks 1981-2000 NCDS (N=3223)					
Conservative Party ID					
1981 (23 yrs.)	1			0.220	0.414
1991 (33 yrs.)	0.613	1		0.437	0.496
2000 (42 yrs.)	0.541	0.719	1	0.283	0.450
Labour Party ID					
1981 (23 yrs.)	1			0.239	0.426
1991 (33 yrs.)	0.581	1		0.281	0.449
2000 (42 yrs.)	0.402	0.627	1	0.372	0.484
Mutual Funds 1981-2000 NCDS (N=988)					
Conservative Party ID					
1981 (23 yrs.)	1			0.236	0.425
1991 (33 yrs.)	0.589	1		0.456	0.498
2000 (42 yrs.)	0.496	0.703	1	0.295	0.456
Labour Party ID					
1981 (23 yrs.)	1			0.200	0.401
1991 (33 yrs.)	0.573	1		0.275	0.447
2000 (42 yrs.)	0.395	0.635	1	0.381	0.486

Source: National Child Development Study.

4.5 Partisan change

Now that I have established the stability of partisan identification in my data sets, I now turn to investigation of the relationship between financial assets and partisanship. I undertake two steps to examine whether acquisition of stocks and mutual funds has any effect on party identification. First, I present simple percentage comparisons of partisan change for the treated and control groups. Tables 4.3 through 4.5 summarize the percentage of partisan change under each treatment condition before and after the intervention period.¹¹

The investor class theory predicts that people become more conservative after they acquire

¹¹These percentages are weighted averages of five multiply imputed data sets.

financial assets. The key comparison, therefore, is percentage of party switchers (particularly toward conservative parties) in the treated and control groups after the treatment period.

Table 4.3 shows a complete lack of switching from the Democratic to Republican Party between 2000 and 2002 for the treated and control groups. I also find very low percentages of movement between Labour and the Conservative Party. In Table 4.4 I find less than one percent of treated cases in the BHPS data set switched from Labour to the Conservative Party. Moreover, the rate of party switching in most cases was very similar for the treated and control groups.

Table 4.3: Partisan change in the United States by treatment type 2000-2002, weighted percentages

Party ID	Treated <i>N</i> =92	Control <i>N</i> =77
Republican in 2000 and 2002	20.7	21.7
Democrat in 2000 and 2002	32.6	38.0
Republican 2000 to Democrat 2002	1.1	0.0
Democrat 2000 to Republican 2002	0.0	0.0

Source: American National Election Studies.

I find slightly higher percentages of party switching in the NCDS data set in Table 4.5. This supports earlier findings concerning the malleability of partisanship of people in their twenties and early thirties (Green et al. 2002).¹² Nevertheless, for both stocks and mutual funds I find the rate of change from Labour to the Conservative Party to be less than five percent. Moreover, the percentages of partisan change in Table 4.5 are virtually identical for treated and control groups.¹³ The evidence for all three data sets across treatment types strongly supports the contention of Green et al. (2002) that people generally do not change political parties. Not only is the percentage of people who switch parties low, but there

¹²Respondents in the NCDS survey were 23 years old in 1981 and 33 years old in 1991.

¹³In fact, a slightly smaller percentage of the treated group switched from Labour to the Tories than the control group for stocks (3.0% vs. 3.7%) and mutual funds (2.2% vs. 3.8%).

appears to be no significant difference in the very low percentage of people who changed parties across treated and control groups.

Table 4.4: Partisan change in Britain by treatment type 1995-2002, weighted percentages

Party ID	Stocks		Mutual Funds		PEP	
	Treated	Control	Treated	Control	Treated	Control
	<i>N</i> =789	<i>N</i> =707	<i>N</i> =353	<i>N</i> =338	<i>N</i> =532	<i>N</i> =486
Tory in 1995 and 2000	13.1	9.1	18.0	13.2	16.5	13.7
Labour in 1995 and 2000	15.2	14.9	13.6	14.3	12.2	19.1
Tory 1995 to Labour 2000	0.4	0.4	0.3	0.6	0.2	0.4
Labour 1995 to Tory 2000	0.3	0.0	0.3	0.0	0.2	0.0
Tory in 1995 and 2002	11.8	7.9	17.9	13.8	15.2	12.8
Labour in 1995 and 2002	15.9	14.8	13.1	15.1	13.4	20.3
Tory 1995 to Labour 2002	0.3	0.7	0.3	0.4	0.3	0.4
Labour 1995 to Tory 2002	0.2	0.2	0.5	0.1	0.4	0.2

Source: British Household Panel Study.

Table 4.5: Partisan change in Britain by treatment type 1981-2000, weighted percentages

Party ID	Stocks		Mutual Funds	
	Treated	Control	Treated	Control
	<i>N</i> =1498	<i>N</i> =1725	<i>N</i> =379	<i>N</i> =609
Tory in 1981 and 1991	20.2	13.8	22.8	14.9
Labour in 1981 and 1991	11.6	16.5	10.4	13.1
Tory 1981 to Labour 1991	1.1	1.7	1.0	1.7
Labour 1981 to Tory 1991	4.6	4.7	3.3	3.6
Tory in 1981 and 2000	14.8	10.0	17.0	9.4
Labour in 1981 and 2000	12.7	15.6	10.6	13.4
Tory 1981 to Labour 2000	3.8	3.5	4.5	4.4
Labour 1981 to Tory 2000	3.0	3.7	2.2	3.8

Source: National Child Development Study.

Next, I estimate a series of logistic regressions of the post-test (party identification after treatment) on the treatment indicator (financial asset) and the pre-test (party identification prior to treatment). The principal summary statistic of a causal effect of financial assets on party identification is the risk ratio: the probability of a given party attachment if in the treated group divided by the probability of the same party attachment if in the control group. The risk ratio equals one if the odds are even; greater distance from one in either direction indicates the relative greater or lesser probability of observing this outcome in the treated versus control group. A general rule of thumb is that the risk ratio must at least be greater than two in order to claim minimal evidence of a causal effect; a risk ratio of 3 or more would be more convincing evidence.¹⁴ To account for the uncertainty inherent in my parametric estimates, I simulate mean risk ratios and associated 95% confidence intervals.¹⁵

I begin with the ANES, which is the most limited data set in terms of the “crudeness” of the treatment indicator and relative brevity of the time horizon (two years). Contrary to Davis and Cotton (2007), I find no evidence in Table 4.6 that stock market participation affected partisanship among Americans who acquired financial assets between 2000 and 2002. The risk ratio for both Democratic and Republican party ID is approximately one.

¹⁴A risk ratio that indicates lesser probability of occurrence in the treated group compared to the control group would have to be smaller than 0.500 to meet the minimum threshold ($\frac{1}{0.500} = 2$) and smaller than 0.333 for convincing evidence ($\frac{1}{0.333} = 3$).

¹⁵I use the Zelig package in R to run 100,000 simulations (Imai et al. 2008a).

Table 4.6: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of stock market investments from 2000-2002 on partisan identification in 2002

	Republican 2002	Democrat 2002
Intercept	-2.832 (0.533)**	-2.338 (0.469)**
Investments	-0.204 (0.707)	0.031 (0.531)
Republican 2000	5.833 (0.834)**	
Democrat 2000		4.457 (0.534)**
RR ^a	1.004	1.084
RR 95% CI ^b	(0.272,2.656)	(0.531,2.007)
<i>N</i>	169	169

Source: American National Election Studies.

Notes: Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

Next, I turn to the BHPS matched groups. Tables 4.7 to 4.9 present the results of logistic regressions of party identification in 2000 and 2002 on acquisition of stocks, mutual funds and PEP accounts between 1995-2000. I find that none of these treatments had any effect on partisan attachment in either 2000 or two years later in 2002. In most cases the risk ratio is close to one and always less than two. Although mutual fund acquisition in the late 1990s appears to have a statistically significant negative effect on support for Labour in 2002 (column four of Table 4.8), the inverted risk ratio ($\frac{1}{0.659}$) is 1.5, meaning the control group was only one a half times more likely to vote for Labour than the treated group. This falls short of the minimum risk ratio threshold of 2.

Table 4.7: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of stocks from 1995-2000 on partisan identification in 2000 and 2002

	Tory 2000 (1)	Labour 2000 (2)	Tory 2002 (3)	Labour 2002 (4)
Intercept	-2.480 (0.137)**	-2.520 (0.141)**	-2.493 (0.140)**	-2.201 (0.136)**
Stocks	0.110 (0.170)	-0.003 (0.164)	0.059 (0.176)	-0.136 (0.159)
Tory 1995	3.372 (0.180)**		3.038 (0.182)**	
Labour 1995		3.327 (0.164)**		3.158 (0.164)**
RR ^a	1.109	1.011	1.066	0.907
RR 95% CI ^b	(0.815,1.486)	(0.775,1.313)	(0.778,1.394)	(0.693,1.168)
<i>N</i>	1496	1496	1496	1496

Source: British Household Panel Study.

Notes: Standard errors in parentheses; *p < 0.05, **p < 0.01.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

Table 4.8: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of mutual funds from 1995-2000 on partisan identification in 2000 and 2002

	Tory 2000 (1)	Labour 2000 (2)	Tory 2002 (3)	Labour 2002 (4)
Intercept	-2.397 (0.192)**	-2.874 (0.236)**	-2.280 (0.197)**	-2.075 (0.188)**
Mutual funds	0.273 (0.226)	0.046 (0.281)	-0.177 (0.276)	-0.518 (0.248)*
Tory 1995	3.007 (0.228)**		3.201 (0.239)**	
Labour 1995		3.960 (0.280)**		3.463 (0.265)**
RR ^a	1.243	1.097	0.888	0.659
RR 95% CI ^b	(0.859,1.783)	(0.652,1.755)	(0.580,1.270)	(0.432,0.943)
<i>N</i>	691	691	691	691

Source: British Household Panel Study.

Notes: Standard error in parentheses; * $p < 0.05$, ** $p < 0.01$.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

Table 4.9: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of PEP accounts from 1995-2000 on partisan identification in 2000 and 2002

	Tory 2000 (1)	Labour 2000 (2)	Tory 2002 (3)	Labour 2002 (4)
Intercept	-2.583 (0.174)**	-2.486 (0.171)**	-2.504 (0.173)**	-2.611 (0.181)**
PEP	0.305 (0.201)	-0.048 (0.197)	0.150 (0.203)	0.173 (0.204)
Tory 1995	3.468 (0.205)**		3.205 (0.216)**	
Labour 1995		3.277 (0.197)**		3.558 (0.205)**
RR ^a	1.316	0.971	1.145	1.167
RR 95% CI ^b	(0.923,1.807)	(0.674,1.321)	(0.801,1.593)	(0.814,1.593)
<i>N</i>	1018	1018	1018	1018

Source: British Household Panel Study.

Notes: Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

Since I don't know when these assets were acquired in the BHPS data set, it is possible that the duration of ownership could be fairly limited. Many people piled into the stock market in the waning years of the 1990s as the bull market continued apace. Thus, even a post-test in 2002 could actually be capturing a significant number of people who owned stocks and mutual funds for just two or three years. Fortunately, the NCDS data set allows me to examine a longer period of potential ownership. The treatment period is ten years and the second post-test in 2000 occurred nine years later, which allows me to test the effect of ownership after a reasonable period of time (from 9-19 years).

Tables 4.10 and 4.11 present the results of the effect of acquisition of stocks and mutual funds on party identification. Once again, I find no evidence of a causal effect of financial assets on party ID. Although stocks reach statistical significance in columns 1-4 in Table 4.10, the risk ratios fail to reach at least two in any of the models. The coefficients on mutual funds fail to reach statistical significance in any of the models in Table 4.11, save for Tory ID in 2000 in column three. However, once again the risk ratio is well below two. These assets were acquired when respondents were between 23-33 years old, the time period in which party identification is most malleable (Green et al. 2002). Even during these most impressionable years, acquisition of financial assets appears to have had a negligent effect on partisan attachments.

Table 4.10: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of stocks from 1981-1991 on partisan identification in 1991 and 2000

	Tory 1991 (1)	Labour 1991 (2)	Tory 2000 (3)	Labour 2000 (4)
Intercept	-0.777 (0.056)**	-1.272 (0.070)**	-1.404 (0.081)**	-0.740 (0.081)**
Stocks	0.214 (0.083)*	-0.389 (0.097)**	0.214 (0.083)*	-0.389 (0.097)**
Tory 1981	1.958 (0.118)**		1.631 (0.114)**	
Labour 1981		1.799 (0.104)**		1.186 (0.097)**
RR ^a	1.128	0.752	1.099	0.893
RR 95% CI ^b	(1.031,1.230)	(0.656,0.861)	(0.966,1.242)	(0.802,0.989)
<i>N</i>	3223	3223	3223	3223

Source: National Child Development Study.

Notes: Standard errors in parentheses; *p < 0.05, **p < 0.01.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

Table 4.11: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of mutual funds from 1981-1991 on partisan identification in 1991 and 2000

	Tory 1991 (1)	Labour 1991 (2)	Tory 2000 (3)	Labour 2000 (4)
Intercept	-0.762 (0.105)**	-1.285 (0.111)**	-1.479 (0.125)**	-0.688 (0.128)**
Mutual funds	0.280 (0.155)	-0.261 (0.163)	0.363 (0.169)*	-0.031 (0.174)
Tory 1981	1.853 (0.189)**		1.438 (0.172)**	
Labour 1981		1.833 (0.189)**		1.213 (0.183)**
RR ^a	1.168	0.830	1.309	0.986
RR 95% CI ^b	(0.991,1.364)	(0.649,1.041)	(1.033,1.631)	(0.810,1.198)
<i>N</i>	988	988	988	988

Source: National Child Development Study.

Notes: Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

4.6 Thresholds

In this section I investigate the possibility of threshold effects of financial assets on partisanship. The partisanship literature suggests that young adults are much more likely to change party identification than people past their early thirties. In the previous section I did not find any evidence that acquisition of financial assets affected the partisanship of respondents who were 23-33 years of age during the treatment period in the NCDS data set. To further investigate this possibility, I use the ANES and BHPS data sets to estimate separate regressions for cases under 34 years of age at the time the data were collected.¹⁶ Tables 4.12 to 4.13 report the results of these estimates for respondents who acquired assets at age thirty-three or younger for the ANES and BHPS data sets. Interacting financial asset ownership with the age dummy variable does not indicate any threshold effect for age. The risk ratio for financial assets in each case is close to one. As with the NCDS data sets, I find no effect of assets on partisanship regardless of age.¹⁷

¹⁶For the ANES data, this means respondents were under 34 years when they acquired assets during 2000-2002. For the BHPS, by the time asset ownership was reported in 2000, a respondent was no older than 33 years old, meaning that they were in their twenties or early 30s when assets were acquired during 1995-2000.

¹⁷Results for ages 34 and older not presented. These results also did not find any effect of age.

Table 4.12: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of stock market investments from 2000-2002 by people under 34 years of age on partisan identification in 2002

	Republican 2002 (1)	Democrat 2002 (2)
Intercept	-2.589 (0.557)**	-2.413 (0.496)**
Investments	-0.535 (0.800)	-0.057 (0.606)
Republican 2000	5.850 (0.861)**	
Democrat 2000		4.497 (0.545)**
Under 34 yrs	-1.888 (1.912)	-0.541 (0.942)
Investments x Under 34 yrs	2.203 (2.225)	0.446 (1.244)
RR ^a	1.216	1.095
RR 95% CI ^b	(0.288,3.538)	(0.537,2.028)
<i>N</i>	169	169

Source: American National Election Studies.

Notes: Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

Another possible threshold effect is the value of financial assets. People with greater financial assets might have greater vested interests and might be more likely to consider switching support to a political party that will protect their investments. Table 4.14 provides summary statistics for reported asset values of stocks and mutual funds in 1991 for the treated groups in the NCDS data sets (asset values are zero for control groups). On average the size of portfolios for stock and mutual funds was quite small, approximately £10,500 for stocks and £19,000 for mutual funds. These averages, however, are skewed as the median values for stocks (£800) and mutual funds (£2,000) were much smaller.

Table 4.13: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of stocks from 1995-2000 by people under 34 years of age on partisan identification in 2000 and 2002

	Tory 2000 (1)	Labour 2000 (2)	Tory 2002 (3)	Labour 2002 (4)
Intercept	-2.384 (0.148)**	-2.488 (0.154)**	-2.398 (0.154)**	-2.149 (0.144)**
Stocks	0.128 (0.185)	0.085 (0.181)	0.057 (0.186)	-0.177 (0.177)
Tory 1995	3.342 (0.181)**		3.008 (0.183)**	
Labour 1995		3.329 (0.165)**		3.148 (0.164)**
Under 34 yrs	-0.496 (0.354)	-0.181 (0.311)	-0.499 (0.376)	-0.260 (0.332)
Stocks x Under 34 yrs	-0.093 (0.471)	-0.459 (0.431)	0.035 (0.478)	0.201 (0.406)
RR ^a	1.112	1.002	1.069	0.907
RR 95% CI ^b	(0.818,1.498)	(0.764,1.465)	(0.772,1.448)	(0.707,1.144)
<i>N</i>	1496	1496	1496	1496

Source: British Household Panel Study.

Notes: Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

To analyze the potential threshold effects of financial asset values, I estimated a series of logistic regressions with dummy variables for £5,000, £25,000 and £50,000. Table 4.15 summarizes the results of these estimates. I limit presentation to those results in which inclusion of the asset value dummy results in a p-value less than or equal to 0.05 for the coefficient of the treatment indicator. (I am unable to interact asset values with the investment ownership dummy since the asset values for all members of the control group is zero.) Column five in Table 4.15 appears to show that acquisition of just £5,000 of stocks from 1981-1991 had a negative effect on support for the Labour Party in 1991. However, the risk ratio is short of

Table 4.14: Summary statistics of asset values for National Child Development Study treated group

	Stocks	Mutual Funds
Minimum	£1.00	£4.00
Maximum	£999,999.00	£1,999,998.00
Mean	£10,528.69	£19,063.41
Median	£800.00	£2,000.00
Standard deviation	£66,607.18	£130,251.27
Interquartile range	£2,170.00	£5,000.00

0.500, the threshold for the minimal case for causality. I find no difference between treated and control groups. In every other case in Table 4.15 the risk ratio also fails to reach at least two.¹⁸ Once again, the data provide no evidence that financial assets—regardless of value—make any difference on partisan identification.

¹⁸Risk ratios less than one would need to be .500 or smaller in order to reach this threshold.

Table 4.15: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of stocks and mutual funds from 1981-1991 on partisan identification in 1991 and 2000, controlling for asset values

	Tory 1991		Tory 2000		Labour 1991		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intercept	-0.776 (0.0561)**	-0.776 (0.0561)**	-1.480 (0.126)**	-1.481 (0.126)**	-1.271 (0.070)**	-1.271 (0.070)**	-1.272 (0.070)**
Stocks	0.194 (0.087)*	0.202 (0.086)*			-0.325 (0.103)**	-0.363 (0.097)**	-0.375 (0.097)**
Mutual funds			0.374 (0.168)*	0.392 (0.169)*			
Tory 1981	1.951 (0.117)**	1.954 (0.117)**	1.441 (0.172)**	1.445 (0.172)**			
Labour 1981					1.797 (0.104)**	1.798 (0.103)**	1.799 (0.104)**
£5,000					-0.444 (0.200)*		
£25,000	0.429 (0.324)		-0.171 (0.535)			-0.638 (0.372)	
£50,000		0.3507 (0.385)		-1.05 (1.024)			-0.470 (0.409)
RR ^a	1.116	1.121	1.318	1.336	0.789	0.767	0.760
RR 95% CI ^b	(1.016,1.22)	(1.022,1.226)	(1.037,1.641)	(1.052,1.661)	(0.683,0.910)	(0.668,0.878)	(0.663,0.871)
Treatment	Stocks	Stocks	M. Funds	M. Funds	Stocks	Stocks	Stocks
Asset threshold	£25,000	£50,000	£25,000	£50,000	£5,000	£25,000	£50,000
N	3223	3223	988	988	3223	3223	3223

Source: National Child Development Study

Notes: Standard errors in parentheses; *p < 0.05, **p < 0.01.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

4.7 Partisan strength

A dichotomous measure of party identification may obscure more subtle changes in partisanship. Although I find no evidence that financial assets affect party ID, it is possible that stocks and mutual funds might influence the strength of partisan attachments. In other words, if buying stocks and mutual funds does not cause someone to switch parties, perhaps it affects partisan fervor? If so, this effect may take place before or in place of wholesale change in partisan identification. I test this proposition with measures of partisan strength in two data sets (ANES and BHPS) that include this information.

For the ANES data set I use the standard seven-point measure of partisan identity. Strong Democrats are coded as -3 and strong Republicans are coded as 3. Independents are coded as 0, and intermediate levels of partisan attachment (weak and independent Democrats/Republicans) fill out the rest of the scale. For the BHPS data set I follow Green et al. (2002) to put the Labour and Conservative parties at either end of the scale (-3 and 3) and code respondents who did not identify with either of these major parties in the middle (0). Respondents who self-identify as fairly strong and not very strong partisans complete the rest of the scale.

Tables 4.16 and 4.17 provide descriptive statistics of partisan strength for the matched groups from the BHPS and ANES data sets. In both of these tables I show the mean partisan scales for all respondents and for partisans pre- and post-treatment.¹⁹ I further disaggregate these divisions into treated and control groups. The strength of partisan attachment in the United States and Britain appears largely unaffected by the acquisition of stocks or mutual funds. Whether we look at the short two-year period of the American data or the longer seven-year period of the BHPS data, the strength of partisan attachments appear very similar across treatment groups.

¹⁹These are weighted averages of five multiply imputed data sets.

Table 4.16: British partisan strength before and after treatment (1995-2000) of stocks, mutual funds and PEP accounts, weighted means

	Stocks			Mutual Funds			PEP		
	1995	2000	2002	1995	2000	2002	1995	2000	2002
All	0.006	0.024	-0.024	0.264	0.224	0.170	0.138	0.122	0.078
Treated	0.064	0.086	0.024	0.432	0.398	0.286	0.230	0.284	0.230
Control	-0.060	-0.050	-0.072	0.088	0.042	0.044	0.034	-0.052	-0.042
Tories	2.518	1.680	1.492	2.578	1.670	1.644	2.520	1.764	1.698
Treated	2.516	1.734	1.576	2.604	1.676	1.628	2.514	1.800	1.750
Control	2.512	1.594	1.366	2.540	1.660	1.672	2.522	1.720	1.630
Labour	-1.404	-1.512	-1.526	-1.474	-1.512	-1.552	-1.440	-1.534	-1.616
Treated	-1.396	-1.490	-1.518	-1.472	-1.436	-1.506	-1.436	-1.374	-1.434
Control	-1.412	-1.540	-1.534	-1.482	-1.582	-1.592	-1.446	-1.664	-1.762

Source: British Household Panel Study.

Notes: Stocks unweighted $N=1496$ (789 treated, 707 control); mutual funds unweighted $N=691$ (353 treated, 338 control); PEP accounts unweighted $N=1118$ (532 treated, 486 control).

Table 4.17: American partisan strength before and after treatment (2000-2002) of stock market investments, weighted means

	2000	2002
All	-0.46	-0.42
Treated	-0.34	-0.30
Control	-0.59	-0.55
Republicans	2.56	2.43
Treated	2.57	2.33
Control	2.55	2.55
Democrats	-2.59	-2.37
Treated	-2.62	-2.50
Control	-2.56	-2.24
Independents	-0.02	-0.10
Treated	-0.04	-0.10
Control	0.00	-0.50

Source: American National Election Studies.

Notes: Unweighted $N=169$ (92 treated, 77 control).

To investigate the causal effect of financial assets on partisan strength, I estimate a series of ordinary least squares regressions of partisan strength on acquisition of financial assets. As before, I use pre- and post-tests of partisan strength along with a dichotomous treatment indicator. Table 4.18 presents the findings for the British data. Substantively, the key statistic for this table is the 95% confidence interval of the point estimate.²⁰ In this case, only the PEP accounts in 2000 (column 3) appears to show any effect on partisan strength. However, the low end of the confidence interval is very small, reflecting the possibility that the size of any effect could be very, very small (3.5% of a standard deviation).²¹ Furthermore,

²⁰I am able to compute relative risk (risk ratios) only for logistic regressions.

²¹For reference I include a summary statistic for goodness of fit. Since the BHPS estimates are of five multiply imputed data sets, R^2 in columns 2-7 is the simple mean of five multiply imputed data sets; the range shows the spread of these estimates in each of the imputed data sets.

PEP accounts have no effect on partisan strength in the 2002 post-test. Table 4.19 shows acquisition of stock market investments have no effect on American partisan strength. The coefficient on stock market investments fails to reach the 5% level.

Table 4.18: Point estimates and 95% confidence interval from an ordinary least squares regression of acquisition of stocks, mutual funds and PEP accounts from 1995-2000 on British partisan strength in 2000 and 2002

	Partisan strength 2000			Partisan strength 2002		
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	-0.003 (0.044)	-0.021 (0.068)	-0.083 (0.048)	-0.027 (0.039)	-0.005 (0.068)	-0.068 (0.064)
Stocks	0.040 (0.056)			-0.016 (0.055)		
Mutual funds		0.115 (0.095)			-0.068 (0.076)	
PEP			0.184 (0.066)**			0.054 (0.067)
Partisan strength 1995	0.791 (0.019)**	0.702 (0.029)**	0.796 (0.023)**	0.312 (0.036)**	0.332 (0.035)**	0.311 (0.033)**
Partisan strength 2000				0.558 (0.029)**	0.545 (0.034)**	0.606 (0.030)**
Asset simulated 95% CI	(-0.068,0.148)	(-0.065,0.293)	(0.056,0.312)	(-0.114,0.124)	(-0.213,0.076)	(-0.070,0.179)
Adj. R ² ^a	0.55	0.49	0.49	0.63	0.66	0.71
MI R ² Range ^b	(0.54,0.56)	(0.47,0.50)	(0.48,0.49)	(0.62,0.65)	(0.63,0.69)	(0.70,0.72)
<i>N</i>	3223	988	1018	3223	988	1018

Source: British Household Panel Study.

Notes: Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$.

^a Mean R² of five multiply imputed data sets; ^b Minimum and maximum R² of five multiply imputed data sets.

Table 4.19: Point estimates and 95% confidence interval from an ordinary least squares regression of acquisition of stocks and mutual funds from 2000-2002 on American partisan strength in 2002

Intercept	0.000 (0.109)
Investments	0.019 (0.145)
Partisan strength 2000	0.927 (0.035)**
Investments 95% CI	(-0.268,0.302)
Adj. R ²	0.82
<i>N</i>	166

Source: American National Election Studies.

Notes: Standard errors in parentheses.

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

4.8 Partisan attachments and asset ownership

The final step in this analysis concerns the relationship between party ID and financial assets. The investor class theory claims that acquisition of financial assets has a causal effect on partisanship. However, it is possible that party ID affects the probability of owning financial assets in the first place. People who support pro-market positions associated with the Republican Party in the United States and the Conservative Party in Britain might have a greater propensity to purchase stocks and mutual funds. Any analysis concerning financial assets and partisan attachments must rule out the possibility of a reverse causal relationship.

I investigate this possibility by first examining the correlations between prior partisanship and subsequent financial asset ownership. Table 4.20 presents the correlations between party ID in 2000 and stock market investments over the next two years in the ANES data set. I find no evidence of any correlation between prior partisanship and acquisition of financial

assets.

Table 4.20: Correlation between prior partisanship and investment in the stock market between 2000-2002

	Republican	Democrat
Correlation	0.012	-0.092
T-Test	0.153	-1.187
DF	166	166
P-Value	0.878	0.237

Source: American National Election Studies.
Notes: Unweighted $N=169$.

For the British data, I focus on just the riskiest asset—stocks. Table 4.21 presents data from the BHPS data set. I find a statistically significant correlation between prior support for the Tories and subsequent stock purchases, but none for Labour. However, at a mere 8% this does not suggest a strong relationship between prior partisanship and the decision to invest in stocks. In Table 4.22 I present similar correlations for the NCDS data set. Although I find a statistically significant correlation for prior support for both the Labour and Conservative Party on subsequent stockholding, substantively this relationship is negligible (7% for Tories and -5% for Labour).

Table 4.21: Correlation between prior partisanship and purchase of stocks between 1995-2000

	Tory	Labour
Correlation	0.079	0.004
T-Test	3.044	0.162
DF	1494	1494
P-Value	0.003	0.872

Source: British Household Panel Study.
Notes: Unweighted $N=1496$.

Table 4.22: Correlation between prior partisanship and purchase of stocks between 1981-1991

	Tory	Labour
Correlation	0.069	-0.049
T-Test	3.792	-2.807
DF	3221	3221
P-Value	0.000	0.005

Source: National Child Development Study.
Notes: Unweighted $N=3223$.

Finally, I estimate a series of logistic regressions to investigate whether prior partisanship predicts the decision to acquire financial investments. In each case, the dependent variable is a dichotomous indicator of financial asset ownership: stock market investments for the ANES data set and stocks for the BHPS and NCDS data sets. The predictor variable is prior party ID. Once again, the key summary statistic is the risk ratio. The risk ratio should be greater than two to provide minimal evidence that prior partisanship affects the probability of owning a financial asset in the treated group compared to the control group.

In Table 4.23 I find that the risk ratio is approximately 1 for both Republican and Democratic party ID. Prior partisanship had no effect on the probability of owning stock market investments during 2000-2002. I make similar findings regarding prior partisanship on stockholding in Britain from 1995-2000 (Table 4.24) and 1981-1991 (Table 4.25). Although the coefficients reach statistical significance for Tory party ID in Table 4.24 (column one) and for Tory and Labour party ID in Table 4.25, the risk ratio in each case falls well short of the critical threshold of two. Whatever effect partisanship may have on political behavior, it apparently has little effect on the decision to invest in the stock market.

Table 4.23: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of prior partisan identification on acquisition of stock market investments between 2000 and 2002

	(1)	(2)
Intercept	0.150 (0.176)	0.324 (0.201)
Republican 2000	0.077 (0.840)	
Democrat 2000		-0.394 (0.317)
RR ^a	1.039	0.839
RR 95% CI ^b	(0.719,1.39)	(0.609,1.109)
<i>N</i>	169	169

Source: American National Election Studies.

Note: Standard errors in parentheses;

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

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets;

^bmean 95% confidence interval.

Table 4.24: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of prior partisan identification on acquisition of stocks between 1995 and 2000

	(1)	(2)
Intercept	0.047 (0.056)	0.101 (0.056)
Tory 1995	0.411 (0.147)**	
Labour 1995		0.042 (0.126)
RR ^a	1.197	1.020
RR 95% CI ^b	(1.058,1.336)	(0.905,1.139)
<i>N</i>	1496	1496

Source: British Household Panel Study.

Note: Standard errors in parentheses;

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

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets;

^bmean 95% confidence interval.

Table 4.25: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of prior partisan identification on acquisition of stocks between 1981 and 1991

	(1)	(2)
Intercept	-0.230 (0.040)**	-0.080 (0.041)*
Tory 1981	0.411 (0.087)**	
Labour 1981		-0.256 (0.084)**
RR ^a	1.232	0.869
RR 95% CI ^b	(1.133,1.333)	(0.790,0.951)
<i>N</i>	3223	3223

Source: National Child Development Study.

Note: Standard errors in parentheses;

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

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets;

^bmean 95% confidence interval.

4.9 Summary

This chapter empirically examined the claim that acquisition of financial assets—stocks and mutual funds—shapes partisan identification. The investor class theory claims that as people buy stock market investments over time they begin to shift their partisan preferences toward parties perceived more likely to protect or benefit these financial interests. The theoretical framework for this prediction is straightforward. New investors will eventually want to support political parties that will protect and enhance their financial self-interests.

I used survey data from the United States and Britain over a twenty-year period starting in the early 1980s to investigate this claim. I find no evidence to support the argument that investment in the stock market has any causal effect on partisan attachments. Partisanship is relatively stable among people who acquired financial assets in the 1980s, mid-to-late 1990s, and early 2000s. There is no difference in effect between ownership of stocks, mutual funds, or tax-privileged investment accounts. There also is no apparent differential effect of financial assets either by age or asset value. These findings support my hypothesis that financial assets have no effect on party identification.

These findings provide additional support for the political socialization model of partisanship. Financial assets play no significant role in explaining change in partisan support, whether measured by partisan identity, vote intention or strength of partisan identification. These results also buttress prior research findings that personal pocketbook issues play little to no role in shaping partisan attachments (Kinder and Kiewiet 1979, 1981; Sears and Funk 1991). It is unclear whether this is due more to the enduring strength of partisan attachments or because investors simply do not perceive parties as having much impact on their portfolio returns.

Chapter 5

Participation

5.1 Introduction

In this chapter I investigate the effect of acquisition of stocks and mutual funds on political participation in the United States and Britain. The investor class theory claims that as people acquire stocks and mutual funds they become more likely to vote and engage in the political process (Glassman 2001; Nadler 2000a, 2005). The theory asserts that the political preferences of new investors and their incentives to engage in the political process change over time. Financial asset ownership is predicted to lead to greater engagement in the political process as investors rationally increase their participation in the political process to protect their financial interests. The asset effect theory similarly claims asset ownership increases the likelihood of political participation. “Wealth leads to a greater effort to protect property, and ultimately, to greater political participation...People with assets become politically active in protecting those assets, which leads to social policy development” (Sherraden 1991, p. 165). As I argue in Chapter 2, however, I expect to find no effect of financial assets on political participation.

Political participation is activity intended directly or indirectly to influence government action. Participation is the means by which citizens “communicate information about their interests, preferences, and needs and generate pressure [on the government] to respond” (Verba et al. 1995, p. 1). This influence occurs through attempts to influence the development of public policies and/or selection of the people who make and implement these policies. Although voting is the most fundamental and common form of participation, par-

ticipation comprises a broad array of political acts including volunteer work on campaigns, financial contributions, contact with public officials, attendance at campaign rallies, and political demonstrations and protests (Verba et al. 1995).

The literature on political participation is roughly divided into the resource-based and rational choice camps.¹ The resource model of political participation distinguishes between the types of resources needed to engage in different modes of political participation.² This model emphasizes the opportunity structure for participation—motivation, capacity and recruitment. People have to want to be involved in politics and have the ability and rationale to do so. However, the ability to get involved is shaped by the availability of time, skills, and money (resources) needed to participate in various time-based, skill-based and money-based activities (Verba et al. 1995; Brady et al. 1995).

By contrast, the rational choice model emphasizes opportunity costs and benefits. In this view, rational actors compare the costs of participation with the benefits they expect to receive (Downs 1957; Riker and Ordeshook 1968; Ferejohn and Fiorina 1974). Political activity only occurs when the benefits clearly outweigh the costs. The fundamental challenge with the rational choice approach (the so-called “paradox of the rational voter”) is that it leads to predictions that no one should ever participate, because the odds of any one person deciding a political outcome through voting, campaign donations, or volunteer work are extremely low.³

Financial assets could fit into either the resource-based or rational choice explanations of participation. It is possible that financial assets might both pull and push their owners toward greater political participation. Scholars claim that financial assets can increase social

¹Prior to Verba et al. (1995), the literature referred to the former as the socioeconomic model of participation. By focusing on resources, Verba and colleagues, in the words of Brady et al. (1995) go “beyond SES” to explain variance in political participation by showing how resources are distributed differentially among groups defined by socioeconomic status. This resource-based model explains why socioeconomic status has traditionally been so powerful in predicting participation.

²Dalton (2000) notes that “this theoretical framework of participation modes has become the common foundation of participation research” (page 927).

³Rational choice scholars argue, however, that partisanship, group interests, values and preferences can increase the benefits of participation (Aldrich 1993).

standing and political influence (Keister 2005; Paxton 2001; Lerman and McKernan 2008; Sherraden 1991). People with financial assets might be pulled into the political process by virtue of the social status, visibility, and resources that come with financial assets. Fenno (1977) contends legislative representatives have multiple constituencies that vary in proximity and influence. Assets may pull their owner into the orbit of political actors. Indeed, Miler (2007) finds that time-constrained legislators focus their efforts on politically active and *resource-rich* constituents.

It is also possible that financial assets also might compel investors to become more active in the political process. Investors presumably have a strong vested interest to protect and enhance the value of their assets. Investors may want to ensure that political institutions will protect the future value of their current financial investments (Bates 2001). Investors have strong incentives to support policies consistent with their tangible financial interests (Kahneman et al. 1999). It is possible that financial investment can lead to greater awareness of economic issues that affect the value of investments. This initial interest in financial issues might “switch on” a related interest in political processes that shape economic policies (Roberts 1990). Nonetheless, I expect that under most circumstances financial assets have no effect on political participation.

5.2 Prior research

A large literature finds that political participation generally increases with income (Campbell et al. 1960; Wolfinger and Rosenstone 1980; Verba and Nie 1972; Leighley 1995). People with greater financial resources not only are more engaged in the political process, but “typically have more at stake in a variety of policy areas” (Campbell 2002, p. 565). For example, Verba et al. (1995) found that political activists tend to have higher incomes than the specific population or interest groups that they represent. Financial resources are cited as a principal explanation for the disparity in rates of participation between men and

women. Schlozman et al. (1994) argued that if the gender income gap was closed women's overall levels of political participation would be closer to men, especially with regards to financial contributions. Cho (1999) found that although certain socioeconomic factors (e.g. education) do not explain participation of certain immigrant sub-populations, higher income is a significant predictor of participation among Asian Americans and Latinos. Leighley and Vedlitz (1999) reached similar conclusions about the effect of income on participation among African Americans, Mexican Americans and whites.

Despite these general findings, Verba et al. (1995) found that household income has a very small direct effect on voting and time-based political acts such as contacting public officials, working on a campaign or volunteering to work on community issues.⁴ The only area where financial resources appears to make a substantively large effect is financial contributions to candidates or political parties. Thus, it appears that income is a proxy for other factors that affect the proclivity to participate. Moreover, the effect of income appears to be asymmetrical. Income appears to have the greatest influence on people without a college education, and has a relative threshold effect for those with college degrees (Wolfinger and Rosenstone 1980, p. 25).⁵

Income, however, is only one form of financial resource. Money can take the form of income or wealth. Income is the flow of resources through a household. It is measured by wages and salaries from work, government transfers and investment income (Keister 2005). Wealth, however, is the stock of assets stored up for future consumption (Sherraden 1991; Shapiro 2001; Nam et al. 2008). Financial wealth is the value of liquid assets such as stocks and mutual funds. The value of these stored up assets is their potential to be converted into

⁴For example, the effect of education on participation in time-based acts is twice as large than income; the effect of civic skills and political interest are four times as great. With regards to voting, the effect of religious attendance, political interest, and partisan strength were three to six times greater than household income (Verba et al. 1995, p. 358).

⁵In 2005 dollars, a household income of \$35,000 to \$46,662 increases the probability of voting for someone without a college degree by roughly 13 percentage points. Increases of income above \$46,667 for college graduates and \$70,000 for non-college graduates do not substantially increase the likelihood, respectively, that people with a college degree or with less education will vote (Wolfinger and Rosenstone 1980; Officer and Williamson 2009).

something else (e.g. power, influence, votes, etc.). This potential creates social and political opportunities and thus arguably is a more encompassing measure of financial resources than the standard socioeconomic measure of income (Shapiro 2001; Keister 2005). Moreover, assets and income often are used for different purposes (Conley 1999; Keister 2005). Individuals likely make decisions about campaign contributions based on their savings rather than current income. Few people, however, likely save up assets with the goal of making campaign contributions. More common are long-term goals like retirement, education, or major purchases. Therefore, the decision to use some of these saved-up assets for political purposes potentially is driven by distinct motivations than contributions from current income (Keister 2005; Nam et al. 2008).

5.3 Data

To ascertain the causal effect of financial assets on political participation, I focus on the effects of *change* in financial asset ownership on subsequent *change* in participation. The important question is whether acquisition of financial assets has any effect upon the level of participation in the political process. I use longitudinal data from the American National Election Studies (ANES), the British Household Panel Survey (BHPS), and the National Child Development Study (NCDS). Since the methodology described in Chapter 3 creates treatment groups without reference to political behavior, I use the same treated and control groups described in Chapter Four to investigate the causal effect of financial assets on participation. Once again the interventions are stock market investments for the ANES matched groups, stocks and mutual funds for the NCDS groups, and stocks, mutual funds and PEP accounts for the BHPS groups. Information about asset values is limited to the NCDS data set.

The pre- and post-tests for the interventions in this chapter are measures of political participation. I follow Verba et al. (1995) to construe participation broadly as four different

measures: voting in national elections, political party activity, political party membership, and participation in voluntary organizations. Each of these variables is operationalized as a dichotomous variable; a subject either engaged in that particular activity or not. Voting and political party membership are straightforward; coding is defined by whether a subject engaged in that act or not. For political party activity and voluntary groups, each subject is coded as a 1 if they indicate they are regularly active in that organization; otherwise they are coded as a 0. Pre-tests, post-tests, treatments, and treatment periods for voting are indicated in Table 5.1 below. I have voting data for all three data sets. Unfortunately, pre- and post-test data for the other measures of participation in this chapter are limited to the BHPS data sets. The pre-test for non-voting forms of participation is from 1994 and two post-tests are from 2001 and 2005.

Table 5.1: Pre-tests and post-tests of voting behavior

	ANES	BHPS	NCDS
Pre-test	2000	1987, 1992	1979
Post-test	2002	2001, 2005	1997, 2001
Treatment period	2000-2002	1995-2000	1981-1991
Treatment indicators	Stock market investments	Stocks, mutual funds, PEP	Stocks, mutual funds

Notes: ANES=American National Election Studies; BHPS=British Household Panel Study; NCDS=National Child Development Study.

5.4 Voting

In this section, I investigate the effect of acquisition of financial assets on voting behavior. Tables 5.2 to 5.4 provide descriptive statistics of reported voter turnout by treatment group in the ANES, NCDS and BHPS data sets. The first observation is the unusually high rate of voting in both the U.S. and British data sets. In Table 5.2 roughly 80% of the treated

and control groups reported voting in the 2000 national elections; over sixty percent of both treatment groups reported voting in the 2002 congressional elections. This is much higher reported turnout than the roughly half of eligible Americans who actually voted in U.S. national elections; the nearly 57% of Americans who turned out for the 2008 presidential election was the highest level of voter turnout in forty years (Infoplease 2008). British turnout in Tables 5.3 and 5.4 also appears inflated. In Britain three-quarters of eligible voters traditionally participated in national parliamentary elections, but for the General Elections in 2001 and 2005 turnout decreased to around sixty percent (UK Political Info 2009).

Table 5.2: Voting in U.S. national elections in 2000 and 2002, stock market investment treatment, weighted percentages

	Treated <i>N</i> =92	Control <i>N</i> =77
2000	0.84	0.79
2002	0.68	0.61

Source: American National Election Studies.

Table 5.3: Voting in British General Elections in 1979, 1997 and 2001, stocks and mutual funds treatments, weighted percentages

	Stocks		Mutual Funds	
	Treated <i>N</i> =1498	Control <i>N</i> =1725	Treated <i>N</i> =379	Control <i>N</i> =609
1979	0.75	0.72	0.77	0.71
1997	0.83	0.80	0.80	0.81
2001	0.82	0.76	0.84	0.79

Source: National Child Development Study.

Table 5.4: Voting in British General Elections 1987-2005, stocks, mutual funds and PEP treatments, weighted percentages

	Stocks		Funds		PEP	
	Treated <i>N</i> =789	Control <i>N</i> =707	Treated <i>N</i> =353	Control <i>N</i> =338	Treated <i>N</i> =532	Control <i>N</i> =486
1987	0.87	0.83	0.88	0.87	0.91	0.91
1992	0.85	0.82	0.88	0.87	0.90	0.88
2001	0.77	0.73	0.83	0.82	0.84	0.79
2005	0.80	0.77	0.85	0.83	0.84	0.84

Source: British Household Panel Study.

There are two possible explanations for these differences. First, scholars have long noted the disparity in survey-based reports of voting behavior and actual turnout (Campbell et al. 1960; Burden 2000; McDonald 2003). People tend to report higher individual levels of voting behavior than supported by aggregate measures of turnout. Second, it is possible these higher percentages of turnout could somehow be an artifice of the matching process. The genetic matching process described in Chapter 3 keeps all treated cases and creates a matched control group based on predictors of asset ownership. It is possible that the matched groups have higher levels of participation than one would ordinarily find in a more representative pool.

Voting turnout appears relatively comparable between treated and control groups in each of these data sets regardless of treatment type. In no case is there ever more than a difference of seven percentage points in voter turnout across treatment groups for either the American or British data sets. However, an interesting pattern emerges from these summary data. In almost every instance the turnout rate in the pre-test is higher for the treated group than the control group in Table 5.4 (the sole standout is the PEP treatment in the BHPS data set). Given that these treatment groups were pre-processed without any reference to political participation, this suggests that perhaps an important predictor of political participation

was omitted in the matching process (see Table 3.3). However, this slight gap in initial turnout (roughly 3% to 6% in the ANES and NCDS groups and 1% to 4% in the BHPS groups) does not appear to grow substantially after the treatment period (approximately 5% to 7% in the ANES and NCDS groups and 2% to 3% in the BHPS groups). In summary, although there are some small differences between treatment groups, I find no significant pattern in difference in the percentage of voting before and after intervention.

Next, I turn to investigation of the causal effects of financial asset acquisition on voting behavior. I estimate a series of logistic regressions of the post-test (voting after treatment) on the treatment indicator (financial asset) and the pre-test (voting prior to treatment). The principal summary statistic again is the risk ratio; the probability of voting if financial assets were acquired divided by the probability of voting if assets were not acquired. A risk ratio greater than two is needed for minimal evidence of a causal effect; three or higher is needed for convincing evidence of a causal effect. By contrast, a risk ratio of one means there is no difference between treatment groups.⁶ I begin with analysis of the American data set, which has a relatively short treatment period of two years. Table 5.5 presents the results of logistic regression of acquisition of stock market investments on voting. The coefficient on investments is not significant at the 5% level. Moreover, the risk ratio falls well below two. I find no evidence of any effect of acquisition of stock market investments between 2000-2002 on voting behavior in the 2002 U.S. congressional elections. There is no apparent short-term effect of acquisition of stock market investments on voting.

⁶I also provide simulated 95% confidence intervals to represent the uncertainty of my estimates. Similar to confidence intervals regarding the null hypothesis, a confidence interval that includes one means that regardless of the size of the risk ratio there is the possibility that the true risk ratio is one.

Table 5.5: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of stock market investments from 2000-2002 on voting behavior in U.S. congressional elections in 2002

Intercept	-2.003 (0.849)*
Investments	0.281 (0.444)
Vote 2000	3.038 (0.818)**
RR ^a	1.121
RR 95% CI ^b	(0.835,1.531)
<i>N</i>	169

Source: American National Election Studies.

Notes: Standard errors in parentheses;

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

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

Next I turn to the two British data sets. Table 5.6 summarizes the estimates of the effect of acquisition of stocks and mutual funds on the vote in the 1997 and 2001 British parliamentary elections using the NCDS data sets. The 1997 General Election witnessed the historic return of the Labour Party to government after eighteen years in opposition. Roughly sixty percent of the British electorate participated in the 2001 General Election, the first time in the previous fifty-five years in which turnout was below 70% (UK Political Info 2009). The coefficient on mutual funds does not reach the 5% level for voting in 1997 or 2001 (columns one and three). Although stocks reaches the 5% level for the vote in the 1997 General Election (column two), the risk ratio is roughly one for this model, as it is for all of the models in Table 5.6. The acquisition of stocks and mutual funds in the 1980s apparently had no effect on subsequent voting behavior in the late 1990s and early 2000s.

Table 5.6: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of stocks and mutual funds from 1981-1991 on voting behavior in British General Elections in 1997 and 2001

	Vote 1997		Vote 2001	
	(1)	(2)	(3)	(4)
Intercept	0.923 (0.165)**	0.709 (0.086)**	-0.427 (0.223)	-0.727 (0.121)**
Mutual funds	-0.063 (0.175)		0.395 (0.213)	
Stocks		0.220 (0.106)*		0.261 (0.131)
Vote 1979	0.763 (0.183)**	0.991 (0.102)**	0.645 (0.219)**	0.586 (0.121)**
Vote 1997			1.814 (0.289)**	2.091 (0.111)**
RR ^a	0.988	1.039	1.061	1.047
RR 95% CI ^b	(0.925,1.048)	(1.001,1.073)	(0.999,1.133)	(1.000,1.087)
N	988	3223	988	3223

Source: National Child Development Study

Notes: Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

Table 5.7 summarizes the results for the BHPS data set. The coefficients for each treatment type fail to reach the 5% level for any of the treatments. Moreover, the risk ratio is roughly one in each case. Acquisition of stocks, mutual funds, or PEP accounts in the late 1990s had no appreciable effect on voting behavior in the 2001 or 2005 parliamentary elections.

Table 5.7: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of stocks, mutual funds and PEP accounts from 1995-2000 on voting behavior in British General Elections in 2001 and 2005

	Vote 2001			Vote 2005		
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	0.227 (0.293)	-0.664 (0.195)**	-0.740 (0.261)**	-1.144 (0.336)**	-1.281 (0.218)**	-1.271 (0.392)**
Mutual funds	-0.078 (0.211)			0.226 (0.252)		
Stocks		0.184 (0.137)			0.065 (0.155)	
PEP			0.299 (0.172)			-0.031 (0.222)
Vote 1987	0.527 (0.354)	0.985 (0.218)**	1.274 (0.326)**	1.116 (0.596)	0.632 (0.251)*	1.530 (0.448)**
Vote 1992	1.010 (0.341)**	1.14 (0.190)**	1.14 (0.290)**	0.358 (0.616)	1.022 (0.222)**	0.148 (0.425)
Vote 2001				2.187 (0.250)**	1.950 (0.161)***	2.289 (0.280)**
RR ^a	0.988	1.044	1.053	1.020	1.025	0.997
RR 95% CI ^b	(0.918,1.06)	(0.982,1.109)	(0.993,1.117)	(0.957,1.086)	(0.972,1.080)	(0.936,1.057)
<i>N</i>	691	1496	1018	691	1496	1018

Source: British Household Panel Study

Notes: Standard errors in parentheses; *p < 0.05, **p < 0.01.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

The use of dichotomous treatment indicators overlooks the possibility that participation might be influenced by asset values. People with larger portfolios might have greater incentives to vote. To analyze this possibility I re-estimated the logistic regressions for the NCDS matched groups with dummy variables for £5,000, £25,000, £50,000 and £100,000.⁷ Table 5.8 summarizes the results of these estimates. I limit presentation to those results in which inclusion of the asset value dummy results in a p-value less than or equal to 0.05 for the coefficient of the treatment indicator.⁸ In every case the risk ratio is approximately one, indicating no greater likelihood of voting in the treated group than the control group. The data provide no evidence that the value of financial assets had any effect on voting behavior.

⁷As noted earlier, I only have asset values for the NCDS data set.

⁸The mutual fund treatment did not yield any estimate that met this threshold.

Table 5.8: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of stocks from 1981-1991 on voting behavior in British General Elections in 1997 and 2001, controlling for asset value

	Vote 1997			Vote 2001		
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	0.708 (0.086)**	0.708 (0.086)**	0.709 (0.086)**	-0.729 (0.121)**	-0.730 (0.121)**	-0.731 (0.121)**
Stocks	0.227 (0.108)*	0.225 (0.107)*	0.220 (0.107)*	0.279 (0.130)*	0.282 (0.129)*	0.275 (0.133)*
£25,000	-0.121 (0.333)			-0.324 (0.366)		
£50,000		-0.127 (0.392)			-0.516 (0.388)	
£100,000			0.030 (0.551)			-0.644 (0.561)
Vote 1979	0.991 (0.102)**	0.991 (0.102)**	0.990 (0.102)**	0.589 (0.122)**	0.590 (0.122)**	0.591 (0.122)**
Vote 1997				2.092 (0.112)**	2.092 (0.112)**	2.093 (0.122)**
RR ^a	1.040	1.040	1.039	1.050	1.050	1.050
RR 95% CI ^b	(1.003,1.076)	(1.003,1.078)	(1.003,1.081)	(1.007,1.092)	(1.008,1.092)	(1.002,1.094)
Asset threshold	£25,000	£50,000	£100,000	£25,000	£50,000	£100,000
N	3223	3223	3223	3223	3223	3223

Source: National Child Development Study

Notes: Standard error in parentheses; *p < 0.05, **p < 0.01.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

5.5 Other forms of participation

In this section I examine the effect of acquisition of financial assets on other modes of political participation: active involvement in political party activities, political party membership, and voluntary service groups. Valid causal inference requires that pre- and post-tests for these measures be administered clearly outside of the treatment period. Unfortunately, the NCDS data set does not offer any pre-tests for measures of political participation. Although the ANES offers valid pre-tests, I cannot be sure whether the post-tests occurred after the “intervention” of asset ownership. Thus, I limit my empirical investigation to the BHPS matched groups for which I can clearly establish that pre- and post-tests occurred outside of the treatment period of 1995-2000.

Tables 5.9 through 5.11 summarize the rates of participation in each of these activities by treatment group prior to and after the treatment period (1995-2000).⁹ The immediate observation is the relatively low level of participation in these different activities. In contrast with the remarkably high level of voting in the British matched groups, Tables 5.9 and 5.10 show that precious few are involved directly with British political parties.¹⁰ Similarly low numbers report regular involvement in voluntary service organizations in Table 5.11. No discernible difference is detectable prior to and after the intervention of asset ownership across treatment groups.

⁹These are simple averages of five multiply imputed data sets.

¹⁰Nonetheless, roughly 30% to 40% of the people in these groups reported support for one of the two major British political parties over this time period. See Table 4.1.

Table 5.9: Active political party participation 1994-2005 by treatment type, weighted percentages

	Stocks		Funds		PEP	
	Treated	Control	Treated	Control	Treated	Control
	<i>N</i> =789	<i>N</i> =707	<i>N</i> =353	<i>N</i> =338	<i>N</i> =532	<i>N</i> =486
1994	0.02	0.02	0.03	0.02	0.03	0.01
2001	0.01	0.01	0.04	0.03	0.03	0.01
2005	0.01	0.02	0.03	0.02	0.02	0.01

Source: British Household Panel Study.

Table 5.10: Political party membership 1994-2005 by treatment type, weighted percentages

	Stocks		Funds		PEP	
	Treated	Control	Treated	Control	Treated	Control
	<i>N</i> =789	<i>N</i> =707	<i>N</i> =353	<i>N</i> =338	<i>N</i> =532	<i>N</i> =486
1994	0.04	0.03	0.06	0.04	0.06	0.03
2001	0.04	0.03	0.06	0.05	0.06	0.02
2005	0.04	0.03	0.06	0.05	0.05	0.03

Source: British Household Panel Study.

Table 5.11: Active participation in voluntary groups 1994-2005 by treatment type, weighted percentages

	Stocks		Funds		PEP	
	Treated	Control	Treated	Control	Treated	Control
	<i>N</i> =789	<i>N</i> =707	<i>N</i> =353	<i>N</i> =338	<i>N</i> =532	<i>N</i> =486
1994	0.06	0.04	0.09	0.04	0.09	0.06
2001	0.04	0.03	0.06	0.06	0.09	0.06
2005	0.05	0.05	0.07	0.05	0.07	0.05

Source: British Household Panel Study.

To ascertain the causal effect of asset ownership on these various measures of political participation, I estimate a series of logistic regressions of the post-test measure of participation on the treatment indicator and relevant pre-tests. The results of these estimates are included in Tables 5.12 to 5.14. Table 5.12 summarizes the estimate of the regression analysis of stocks, mutual funds and PEP accounts on political party participation. In almost every case, the risk ratio fails to reach two. In column six, the risk ratio reaches 2.443 for PEP accounts for the post-test in 2005, however, the 95% confidence interval includes one, indicating the possibility of no difference between treated and control groups.¹¹ Thus, I cannot conclude that financial assets had any effect on active involvement with political parties.

Moving on to political party membership, in Table 5.13 I find no effect for financial assets. The coefficients on financial assets falls below the 5% level for all of the models. Finally, in Table 5.14 I find no evidence of any effect of financial assets on participation in voluntary groups. The coefficients for financial assets does not reach the 5% level for any of the models. Further, the risk ratio fails to reach the threshold of two for stocks, mutual funds and PEP accounts for both the 2001 and 2005 post-tests. In summary, I do not find any evidence that acquisition of financial assets has any effect on political participation.

¹¹Further, the coefficient on the PEP accounts fails to reach the 5% level.

Table 5.12: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of stocks, mutual funds and PEP accounts from 1995-2000 on political party participation in 2001 and 2005

	Party 2001			Party 2005		
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	-4.980 (0.468)**	-3.903 (0.376)**	-4.767 (0.577)**	-4.247 (0.359)**	-4.54 (0.485)**	-4.842 (0.605)**
Stocks	0.157 (0.523)			-0.260 (0.530)		
Mutual funds		0.080 (0.496)			0.198 (0.573)	
PEP			0.098 (0.639)			0.693 (0.697)
Party 1994	4.108 (0.566)**	4.255 (0.602)**	4.806 (0.639)**	2.195 (0.862)*	0.946 (0.896)	1.422 (0.986)
Party 2001				2.202 (0.807)**	3.591 (0.703)**	2.484 (0.091)**
RR ^a	1.335	1.209	1.324	0.874	1.420	2.443
RR 95% CI ^b	(0.430,3.231)	(0.421,2.752)	(0.346,3.607)	(0.309,2.103)	(0.406,3.646)	(0.564,6.942)
<i>N</i>	691	1496	1018	691	1496	1018

Source: British Household Panel Study

Notes: Standard errors in parentheses; *p < 0.05, **p < 0.01.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

Table 5.13: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of stocks, mutual funds and PEP accounts from 1995-2000 on political party membership in 2001 and 2005

	Party Member 2001			Party Member 2005		
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	-4.305 (0.317)**	-3.700 (0.352)**	-4.446 (0.440)**	-4.452 (0.424)**	-4.106 (0.482)**	-4.416 (0.592)**
Stocks	0.366 (0.361)			0.029 (0.448)		
Mutual funds		-0.306 (0.476)			-0.007 (0.566)	
PEP			0.628 (0.428)			-0.112 (0.592)
Party Member 1994	4.233 (0.369)**	4.855 (0.494)**	4.508 (0.422)**	1.785 (0.700)*	2.229 (0.756)**	1.393 (0.698)*
Party Member 2001				4.019 (0.486)**	3.952 (0.582)**	3.996 (0.681)**
RR ^a	1.521	0.823	2.023	1.123	1.146	1.052
RR 95% CI ^b	(0.733,2.851)	(0.303,1.799)	(0.830,4.226)	(0.450,2.332)	(0.356,2.851)	(0.336,2.875)
<i>N</i>	1496	691	1018	1496	691	1018

Source: British Household Panel Study

Notes: Standard errors in parentheses; *p < 0.05, **p < 0.01.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

Table 5.14: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of acquisition of stocks, mutual funds and PEP accounts from 1995-2000 on voluntary group participation in 2001 and 2005

	Volunteer 2001			Volunteer 2005		
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	-3.600 (0.233)**	-3.115 (0.264)**	-3.109 (0.235)**	-3.129 (0.193)**	-3.401 (0.329)**	-3.691 (0.281)**
Stocks	0.191 (0.291)			-0.119 (0.300)		
Mutual funds		-0.214 (0.361)			0.063 (0.391)	
PEP			0.309 (0.272)			0.226 (0.343)
Volunteer 1994	2.09 (0.343)**	2.601 (0.408)**	2.36 (0.294)**	0.827 (0.497)	1.879 (0.513)**	0.226 (0.343)
Volunteer 2001				2.023 (0.386)**	1.991 (0.477)**	2.37 (0.396)**
RR ^a	1.250	0.863	1.381	0.925	1.137	1.308
RR 95% CI ^b	(0.697,2.077)	(0.416,1.579)	(0.821,2.194)	(0.524,1.494)	(0.524,2.177)	(0.668,2.318)
<i>N</i>	1496	691	1018	1496	691	1018

Source: British Household Panel Study

Notes: Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

5.6 Summary

This chapter empirically examined the claim that acquisition of financial assets—stocks and mutual funds—shapes political participation. The investor class theory claims that financial asset ownership increases incentives for voting. The asset effect theory makes similar claims, arguing that asset ownership increases opportunity and motivation to engage in the political process. I used survey data from the United States and Britain over a twenty-year period starting in the early 1980s to investigate these claims. In sum, the evidence supports my hypothesis that investment in the stock market has no causal effect on voting or other forms of political participation.

These findings support the contention of Verba et al. (1995) that financial resources have weak effects on participation. Voting is a relatively costless political act and in contrast to other forms of participation requires no special resources. The fact that any single vote is unlikely to affect the outcome of an election means that at the individual level voting is instrumentally irrational (Bohanon and Van Cott 2002). The individual bears the “cost” of going to the polls and voting even though she most likely could enjoy the same electoral outcome by “free riding.” Thus, voting behavior apparently cannot be explained by financial assets since in a democratic system everyone has one vote regardless of the size of their stock portfolio. “What matters most for going to the polls is not the resources at voters’ disposal, but rather, their civic orientations, especially their interest in politics” (Verba et al. 1995, p. 361).

Financial assets also were unimportant for party membership or activity. Assets also had no effect on civic engagement in voluntary groups. The findings in this chapter support my argument that financial assets have no effect on political participation. These results also support conclusions of the self-interest literature that personal finances have relatively little effect on political behavior. As Dalton (2000) suggests, the fact that people participate politically at all suggests that more than pure economic rationalization is at play.

Chapter 6

Attitudes

6.1 Introduction

In this chapter, I investigate the effect of acquisition of financial assets on political attitudes. The investor class theory claims that people's political attitudes and preferences change after they acquire financial assets. Over time new investors begin to prefer pro-market and "investor-friendly" policies believed to protect and enhance their stock market investments (Nadler 2005; Ponnuru 2004). Further, as people become more involved in managing their own investments, they become more wary of government attempts to direct or redistribute their financial assets. The asset effect theory claims that acquisition of financial assets leads to greater political interest (Paxton 2001; Lerman and McKernan 2008). In this chapter I empirically evaluate these claims regarding political interest and attitudes toward economic policies. I hypothesize that acquisition of financial assets has no effect on political attitudes.

Attitudes are summary evaluations of psychological objects, which are "anything that people can like or dislike, wish to protect or to harm, want to acquire or to reject" (Kahneman et al. 1999, p. 205). Political objects can be abstract (parties, policies, institutions, events) or concrete (people) (Petty et al. 1997; Ajzen 2001). Accordingly, financial assets such as stocks and mutual funds are valid psychological objects. People form beliefs about objects based on relevant data about their characteristics: "good-bad, harmful-beneficial, pleasant-unpleasant, and likeable-unlikeable" (Ajzen 2001, p. 28). Beliefs are probability distributions of possible characteristics of objects, i.e. "the subjective values of the object's attributes in interaction with the strength of the associations" (Ajzen 2001, p. 30). Attitudes are based

in part on beliefs about the desirability of these object attributes (Katz 1960; Fishbein and Ajzen 1972).¹ Expressions of attitudes include statements of approval or disapproval, financial contributions, and answers to survey questions (Kahneman et al. 1999).

Katz (1960) proposes four functions of attitudes, one of which is the “adjustment” or “utilitarian” function.² Attitude toward an object (such as financial assets) is derived from its perceived utility. Clarity, consistency, and immediateness of costs and benefits as they relate to an individual’s actions help to shape utilitarian attitudes. “The closer these objects are to actual need satisfaction and the more they are clearly perceived as relevant to need satisfaction, the greater are the probabilities of positive attitude formation” (Katz 1960, p. 171). Lockerbie (2008) contends that money is a basic resource needed to fulfill a variety of daily human activities. “Like election is for politicians, financial well-being is a prerequisite for many of the goals we citizens have” (page 3). So we can safely argue that financial assets such as stocks and mutual funds are non-trivial objects.³

In theory, acquisition of financial assets could affect the evaluation of attributes of political objects, such as political parties or policies. Re-evaluation of these objects in turn would reshape attitudes, preferences and behavior. Figure 6.1 depicts this relationship between attitudes, beliefs, preferences and behavior.⁴ Attitudes are distinguished by their degree of differentiation. The more beliefs and cognitive items that go into shaping an attitude, the more difficult it is to change (Katz 1960). Thus, there is no guaranteed effect of the singular

¹Psychologists find that attitudes have both affective (emotional) and cognitive (belief) bases (Petty et al. 1997).

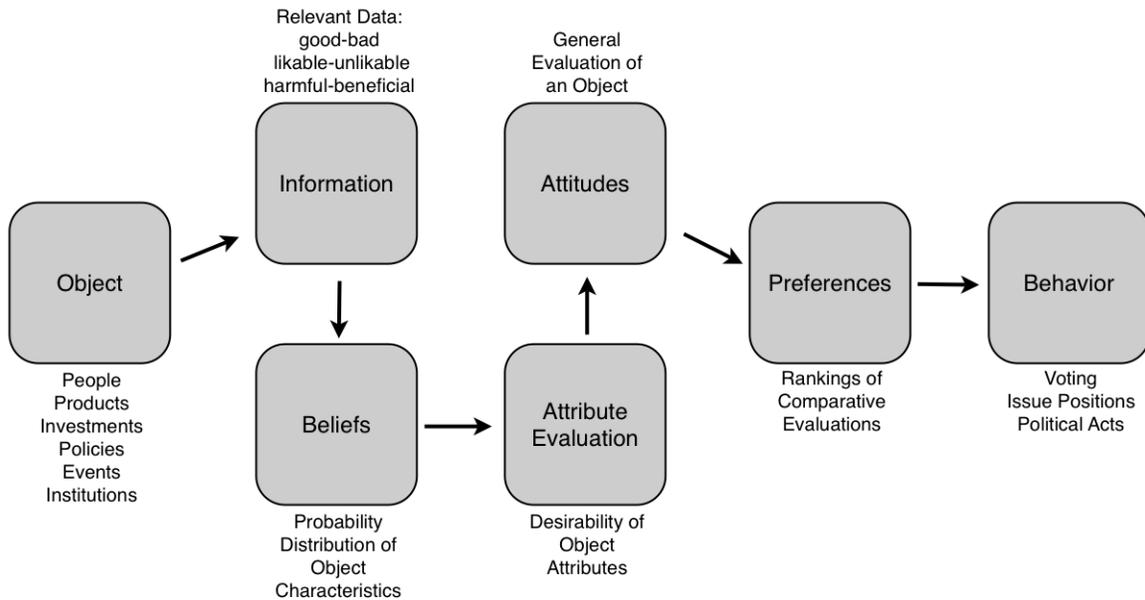
²The other three major functions are ego-defensive, value-expressive, and knowledge. The utilitarian function is grounded in the reward-punishment thesis upon which self-interest and rational choice theory is based (see section 2.4.1). Simply put, people seek to maximize rewards and minimize penalties. Campbell et al. (1960) refer to this as “primitive self-interest” (p. 205).

³Sears notes that money can have positive or negative connotations, prompting attitudes in favor of acquisition of more money, shame about not having enough money, or the belief that money is morally evil (Sears 1997, p. 495).

⁴This linkage between attitudes and behavior is complex. Leighley (1995) contends that attitudes sometimes are shaped by behavior; political activity sometimes can shape attitudes where none existed prior to the activity (e.g. protests). Katz (1960) argues that two people with similar attitudes may differ in their behavior. For example, two political party supporters may require different degrees of external motivation to get to the polls on election day.

act of investing in stocks or mutual funds on extant political attitudes. Furthermore, attitude change can occur in a variety of ways. The acquisition of new information about objects can result in replacement or overwriting of previous attitudes (Ajzen 2001; Druckman and Lupia 2000). Attitudes can be completely reversed or partially changed. Preferences and behavior can be affected (or not) in a variety of ways.

Figure 6.1: The relationship between attitudes and behavior.



Kahneman et al. (1999) note that people’s attitudes toward objects and their actions that affect these objects are usually consistent. In other words, it is not unreasonable to expect investors to support positions and policies that they believe will protect their investments. However, the manner in which an object is framed can result in separate attitudes toward the same object (Kahneman and Tversky 1979; Druckman 2001, 2004).⁵ This means we can not predict in advance political behavior based on separate attitudes toward multiple objects such as financial investments and politics (Kahneman et al. 1999, p. 206).

⁵This is consistent with findings that attitudes can be primed based on perceived self-interest (Sears and Funk 1991; Chong et al. 2001).

Attitude change occurs more readily when people realize that they can more easily achieve their objectives by updating their current attitudes (Katz 1960). In order for people to change utilitarian-based attitudes, however, one of two conditions must exist. Either the individual's current attitude and associated activities no longer provide the same level of satisfaction, or her aspirations have been elevated. Katz notes that changes in political behavior based on the utilitarian function may be difficult without sufficient motivation. "Attitudes toward political parties and voting behavior are often difficult to change if there is no widespread dissatisfaction with economic conditions" (Katz 1960, p. 177). The question therefore is whether acquisition of financial assets provides sufficient stimulus for attitudinal change.

6.2 Prior research

Di Tella et al. (2007) describe a natural experiment from Argentina that offers tentative support for the idea that asset ownership can shape attitudes. In 1981, approximately 1,800 families occupied a poor suburban area of Buenos Aires and sub-divided the land into small parcels. These families believed that the land belonged to the state at the time of the occupation. Subsequently, it was discovered that the land in fact was comprised of thirteen privately-held tracts. In 1984, a law was passed to expropriate the private owners' land for redistribution to the squatters. Land owners of eight tracts quickly accepted government terms, while owners of the other five tracts disputed the level of compensation through the courts. One of these five disputes was resolved in 1998.

As a result, some squatters received early property rights (early treated), others received property rights later on (late treated), and others did not receive property rights at all (control group). Each of the treatment groups on average had identical demographic characteristics. None could have known in advance what the outcome of their expropriation of seemingly "public" lands would be several years later nor predict which treatment group

they would be in. The relative weakness of property rights in Argentina makes this an especially interesting case. If people do not expect that the government is capable or willing to protect their property rights, holding legal title to the land should make little difference to their attitudes or behavior.

Di Tella et al. (2007) surveyed a random selection of households from each group to examine the effect of property ownership on beliefs across these treatment groups. These scholars found that households in both the early and late treatment groups had statistically significant different beliefs about self-efficacy, meritocracy, and materialism from those in the control group. Compared to the untitled group, property owners had stronger beliefs in individualism and materialism, and professed greater support for merit-based advancement. Galiani and Schargrotsky (2009) found these households substantially increased investment in their property, had smaller household sizes, and invested more in the education of their children compared to the control group.

The scant research on the effects of financial assets on attitudes offers mixed findings. Doherty et al. (2006) found that lottery winnings increased support for elimination of the estate tax but had no effect on general attitudes of lottery winners about income redistribution. Bynner (2001) analyzed data from the National Child Development Study to study the effect of financial investments on political attitudes. He found a positive effect of investments on political interest for both men and women and a negative effect of investments on “political cynicism” for women (but no effect for men).⁶ Davis (2009) analyzed data from the 2004 American National Election Study to compare differences between investors and non-investors on a range of economic issues. Outside of significant differences in support for tax cuts and Social Security privatization, he found no support for the claims of investor class proponents. “Shareholders, in short, were not doctrinaire devotees of free market policies, and were largely indistinguishable from non-shareholders on a range of attitudinal issues”

⁶Cynicism is measured on an attitudinal scale based on answers to the following questions: “politicians are mainly in politics for their own benefit”; “none of the political parties would do anything to benefit me”; and, “it does not make much difference which political party is in power.”

(Davis 2009, p. 221).

Researchers quite consistently find that self-interest does not have an effect on political attitudes. Sears and colleagues examined a range of issues on which we would expect to find evidence of self-interest effects: race, violence and economics. These analysts not only found that attitudes of white Americans regarding forced busing, affirmative action and bilingual education cannot be explained by self-interest, but in fact are better explained by general attitudes of racial intolerance (Sears and Funk 1990a, 1991). These large-N studies were further corroborated by Green and Cowden (1992) who found no effect of self-interest on attitudes of white Americans on forced busing in Boston and Louisville in the 1970s. Sears and Funk (1991) reported that having a personal concern about safety explained only 0.4 percent of the variance in general support for law and order policies.⁷ Researchers found that people with friends and relatives serving in the military during the Vietnam War were no more opposed to the war than their peers who did not have this very personal connection (Sears et al. 1980; Sears and Funk 1990a).

These studies also did not find an effect of economic self-interest on political attitudes. Sears and Funk (1991) found “economic discontent” (both subjective and objective measures of family finances) had no effect on attitudes regarding state tax and spending cuts. Moreover, public employees and recipients of government services, who purportedly should have direct interests in issues that affect the public sector, showed very limited additional support for the government sector compared to people without similar interests. Kinder and Sears (1985) found that people whose lives were disrupted during the energy shortages of 1974 in the United States were no more likely to support policies concerning energy conservation or development of new energy sources than those whose lives were unaffected by the energy crisis that year.

Kramer (1983) argues these types of negative findings are artifactual given that the gov-

⁷Operationalized as recent crime victims and individuals who reported fear of walking in their neighborhoods at night.

ernment is responsible for little of what affects people's daily material well-being. Indeed, there appear to be three exceptions to these general non-findings. First, researchers found evidence of self-interest effects on political attitudes when there are clear, substantial individual costs and benefits. Kramer (1983) suggests that government-induced and politically relevant issues are more likely to elicit self-interest effects on political attitudes. The best economic example is taxes; survey-based and experimental studies show consistent effects of self-interest on attitudes regarding taxes, especially when a political solution is clearly provided (Sears and Funk 1991). Analysts found that people who feel particularly burdened by taxes or expect to benefit substantially from tax-reduction measures exhibit greater support for tax-cutting policies (Sears and Funk 1990b). Green and Gerken (1989) also found that self-interest played a decisive role in their study of attitudes regarding smoking restrictions and cigarette taxes. Smokers and non-smokers generally are indistinguishable on their political views, but sharply diverge on issues concerning cigarettes.

Second, severe and ambiguous threats also can motivate self-interest. These are cases which combine considerable uncertainty about outcomes and significant individual costs. For example, Sears and Funk (1991) show that public employees in Massachusetts and California were much more opposed to state tax and spending referenda in the late 1970s than other voters. Presumably, the uncertainty public employees felt about their future welfare in the face of state spending cuts evoked a significant self-interest effect on their attitudes toward these policies.⁸ Similarly, white opposition to forced busing in Los Angeles, California in the 1970s was motivated by self-interest only during the uncertain period between the court mandate for busing and the announcement of a specific implementation plan (Sears and Funk 1991, p. 51).

Third, self-interest can manifest when it is politically activated. Sears and Funk (1990a) found that support for President Ronald Reagan in the 1980s was stimulated in part by direct

⁸This is a limited and focused effect. In general, the attitudes of these same public employees toward the public sector are indistinguishable from those of the general public.

appeal to voters' economic self-interests. This was a deliberate strategy during Reagan's entire time in office. Chong et al. (2001) similarly found that self-interest effects can be activated when people are "primed" to recognize and act upon individual costs and benefits. Attitudes about Social Security, mortgage interest deduction, and domestic partner benefits can be shaped by how the costs and benefits are framed.⁹ For example, the opinions of young and old survey respondents (and employed and retired respondents) about Social Security were significantly different depending upon whether respondents were primed or not. Opinions converged when primed to take into account the welfare of future generations.

Sears and Funk show that effects of self-interest can be manipulated by the item-order of survey questions. Placement of questions about personal finances immediately prior to questions about relevant political attitude questions can create artificially high levels of consistency between these two items (Sears and Funk 1991, p. 48).¹⁰ Boninger and colleagues in a series of studies found that self-interest can influence attitude importance; "one effective way to convince people to care deeply about political attitudes they hold may be to convince them that their self-interests are at stake" (Boninger et al. 1995, p. 75). Chong and colleagues reached similar conclusions. "Self-interest is more likely to matter when people actually have a stake in a policy and can see that they have a stake. Whether they can recognize those stakes depends upon the transparency of the policy, the clarity with which the policy is presented to them, and their capacity to understand the implications of the policy" (Chong et al. 2001, p. 544).

On the whole, however, these effects appear to be narrow and specific. For example, state public employee opposition to tax and spending cuts in California and Massachusetts was focused on defense of their jobs and wages, but did not extend to broader support of

⁹Sears and Funk (1991) note that priming helps to increase the accessibility of a construct such as Social Security to personal self-interests. However, "in political life information about the self may generally not be very accessible, because the mass media are our primary sources of information, and their focus is on national and collective affairs rather than on matters directly relevant to our own lives" (page 9).

¹⁰These authors also report higher effects of self-interest in exit polls than in pre-election polls. Voters apparently rationalize their votes post-elections in ways that are consistent with their interests rather than the other way around.

the government sector (Sears and Funk 1990a, 1991).¹¹ Moreover, responses to government induced economic changes sometimes provoke self-interested attitudes, but more often they do not as people do not or can not consistently attribute changes in their personal economic well-being to government actions (Sears and Funk 1991). More recent research continues to find no effect of self-interest on political attitudes (Lau and Heldman 2009). This accords with the conclusion of Chong and colleagues that when the stakes and impact of policies are unclear, people rely on “general political orientation” such as partisanship and ideology to make political decisions.¹²

6.3 Data

In this chapter I investigate the effects of *change* in financial asset ownership on subsequent *change* in attitudes. I use data from the British Household Panel Survey, which offer pre- and post-test data on political attitudes. The interventions are stocks, mutual funds, and PEP accounts for the BHPS matched groups. The pre- and post-tests for these interventions are measures of political attitudes. In some cases these attitudes are appropriately operationalized as dichotomous measures (approve-disapprove, yes-no). In other cases, respondents provided three or more possible responses, in which case I elect not to dichotomize their responses to avoid information loss.

The pre-tests for the BHPS groups (treatment period 1995-2000) are from either 1994 or 1995; post-tests are from 2001, 2003 or 2004. For the dichotomous measures I estimate binary logistic regression to infer the causal effect of assets on political attitudes. For the measures with non-binary responses, I estimate ordered logistic regressions. To facilitate interpretation of these results, I evaluate my findings in terms of risk ratios—the probability of a given attitude if treated compared to the probability of the same attitude if untreated.

¹¹Sears and Funk (1991) note that taxes prove to be the exception to this general finding. Tax burdens in one area appear to carryover to negative attitudes toward government spending in other areas.

¹²Sears and colleagues refer to this as “symbolic politics” (Sears and Funk 1990a, 1991).

6.4 Political interest

The investor class theory and asset effect theory argue that acquisition of financial assets increases interest in the political process. I use longitudinal data from the BHPS to test this proposition. Tables 6.1 to 6.3 provide descriptive statistics of the levels of political interest prior to and after the acquisition of stocks, mutual funds and PEP accounts. For stocks and PEP treatments, a slightly higher percentage of the treated group expressed interest in politics (fairly interested and very interested) prior to treatment. This raises the possibility that differences between these two groups may not have been totally purged by the matching process. The overall level of positive interest in politics (fairly interested and very interested) in the treated group, however, is largely unchanged in the 2003 post-test. The proportion of the treated group expressing interest in politics declines for both the stock and PEP treatments, and increases by roughly two percentage points for mutual fund holders.

Table 6.1: Political interest before and after intervention of stocks treatment, weighted percentages

	1995		2003	
	Treated	Control	Treated	Control
Not at all interested	13.8	18.2	17.3	22.4
Not very interested	33.1	34.5	33.1	32.0
Fairly interested	42.7	39.6	38.3	35.7
Very interested	10.4	7.7	11.3	10.0

Source: British Household Panel Study.

Notes: Unweighted $N=1496$ (789 treated, 707 control).

Table 6.2: Political interest before and after intervention of mutual funds treatment, weighted percentages

	1995		2003	
	Treated	Control	Treated	Control
Not at all interested	10.8	14.0	10.6	15.9
Not very interested	30.9	26.8	28.7	29.5
Fairly interested	47.0	45.8	47.0	39.2
Very interested	11.3	13.5	13.6	15.4

Source: British Household Panel Study.

Notes: Unweighted $N=691$ (353 treated, 338 control).

Table 6.3: Political interest before and after intervention of PEP treatment, weighted percentages

	1995		2003	
	Treated	Control	Treated	Control
Not at all interested	11.1	12.1	12.0	18.7
Not very interested	30.6	35.5	31.9	35.0
Fairly interested	50.4	43.2	44.8	35.9
Very interested	7.9	9.1	11.3	10.4

Source: British Household Panel Study.

Notes: Unweighted $N=1118$ (532 treated, 486 control).

To estimate the effect of acquisition of financial assets on political interest I estimate a series of ordered logistic regressions of the post-test (political interest in 2003) on the treatment indicator (financial asset) and pre-test (political interest in 1995). Tables 6.4 and 6.5 report the results of these estimates. Table 6.4 presents the point estimates for stocks, mutual funds, and PEP accounts as well as the predicted change in probabilities of reported levels of political interest if exposed to the “treatment” of each financial asset. Only the coefficient for PEP accounts reaches statistical significance in Table 6.4. However, Table 6.5 shows that the risk ratios for PEP accounts (as well as stocks and mutual funds) fails to

reach the critical threshold of two for any level of political interest. I find weak evidence that acquisition of financial assets has any causal effect on interest in politics.

Table 6.4: Log odds and change in predicted probabilities from ordered logistic regressions of stocks, mutual funds, and PEP accounts on levels of political interest, 1995-2003

Independent variables	Log odds	Change in Predicted Probabilities			
		Not at all interested	Not very interested	Fairly interested	Very interested
Stocks	0.051 (0.107)	-0.002	-0.007	0.005	0.005
Mutual funds	0.205 (0.163)	-0.003	-0.027	0.009	0.021
PEP	0.362 (0.134)**	-0.006	-0.053	0.022	0.037

Source: British Household Panel Study.

Notes: Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$.

Stocks unweighted $N=1496$ (789 treated, 707 control); mutual funds unweighted $N=691$ (353 treated, 338 control); PEP accounts unweighted $N=1118$ (532 treated, 486 control).

Table 6.5: Simulated risk ratio (RR) and 95% confidence interval from ordered logistic regressions of stocks, mutual funds, and PEP accounts on levels of political interest, 1995-2003

Attitude	RR ^a	Stocks		Mutual Funds			PEP		
		95% CI ^b		RR ^a	95% CI ^b		RR ^a	95% CI ^b	
		2.5%	97.5%		2.5%	97.5%		2.5%	97.5%
Not at all interested	0.973	0.783	1.195	0.828	0.597	1.118	0.707	0.544	0.905
Not very interested	0.978	0.825	1.152	0.856	0.656	1.096	0.759	0.616	0.924
Fairly interested	1.004	0.982	1.027	1.013	0.993	1.048	1.033	1.005	1.075
Very interested	1.035	0.851	1.250	1.210	0.906	1.591	1.387	1.094	1.736

Source: British Household Panel Study

Notes: Stocks unweighted $N=1496$ (789 treated, 707 control); mutual funds unweighted $N=691$ (353 treated, 338 control); PEP accounts unweighted $N=1118$ (532 treated, 486 control).

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets;

^bmean 95% confidence interval.

6.5 Economic policy

The investor class theory asserts that after people acquire financial assets, over time their political preferences shift to support more pro-market positions. I test this claim with a series of questions related to economic issues. Tables 6.6 to 6.9 provide descriptive statistics on attitudes regarding four free market “acid tests” before and after the acquisition of stocks, mutual funds and PEP accounts: government provision of employment, role of the private sector, privatization of public services, and government-imposed earnings limits. If assets move people toward a pro-market position, I would expect to see some effect of acquisition of financial assets on attitudes toward these issues. However, I find no significant difference in Tables 6.6 to 6.9 in percentage point change between the treated and control group for stocks, mutual funds or tax-privileged PEP accounts before and after the treatment period (1995-2000) for any of these issues.

Table 6.6: Attitudes about whether government has an obligation to provide jobs prior to and after intervention, weighted percentages

	Stocks				Mutual Funds				PEP			
	1995		2003		1995		2003		1995		2003	
	Treated	Control	Treated	Control	Treated	Control	Treated	Control	Treated	Control	Treated	Control
Agree	44.5	47.0	35.1	36.5	38.2	43.6	27.0	33.4	39.5	45.8	29.3	34.0
Neither	15.7	18.5	18.0	22.6	15.2	17.0	21.5	20.1	14.7	16.8	19.3	23.5
Disagree	39.8	34.5	46.9	40.8	46.6	39.4	51.5	46.5	45.9	37.4	51.4	42.5

Source: British Household Panel Study.

Notes: Stocks unweighted $N=1496$ (789 treated, 707 control); mutual funds unweighted $N=691$ (353 treated, 338 control);

PEP accounts unweighted $N=1118$ (532 treated, 486 control).

Table 6.7: Attitudes about whether “private enterprise solves economic problems” prior to and after intervention, weighted percentages

	Stocks				Mutual Funds				PEP			
	1995		2003		1995		2003		1995		2003	
	Treated	Control	Treated	Control	Treated	Control	Treated	Control	Treated	Control	Treated	Control
Agree	29.5	21.1	31.9	25.7	39.5	26.6	39.3	30.6	31.4	31.8	33.8	29.9
Neither	36.1	37.0	38.5	42.3	30.4	32.2	40.0	38.5	33.8	29.0	37.9	39.3
Disagree	34.5	42.0	29.7	32.0	30.1	41.2	20.6	30.9	34.8	39.1	28.2	30.7

Source: British Household Panel Study.

Notes: Stocks unweighted $N=1496$ (789 treated, 707 control); mutual funds unweighted $N=691$ (353 treated, 338 control);

PEP accounts unweighted $N=1118$ (532 treated, 486 control).

Table 6.8: Attitudes about whether public services ought to be state-owned prior to and after intervention, weighted percentages

	Stocks				Mutual Funds				PEP			
	1995		2003		1995		2003		1995		2003	
	Treated	Control	Treated	Control	Treated	Control	Treated	Control	Treated	Control	Treated	Control
Agree	48.5	48.9	40.9	40.7	40.2	50.6	33.3	39.9	44.2	50.6	37.9	41.0
Neither	23.4	24.8	27.8	30.2	24.2	21.8	29.3	27.6	22.3	19.1	28.6	29.0
Disagree	28.1	26.4	31.4	29.0	35.6	27.6	37.4	32.6	33.6	30.4	33.5	30.0

Source: British Household Panel Study.

Notes: Stocks unweighted $N=1496$ (789 treated, 707 control); mutual funds unweighted $N=691$ (353 treated, 338 control); PEP accounts unweighted $N=1118$ (532 treated, 486 control).

Table 6.9: Attitudes about whether the government ought to impose an earnings ceiling prior to and after intervention, weighted percentages

	Stocks				Mutual Funds				PEP			
	1995		2003		1995		2003		1995		2003	
	Treated	Control	Treated	Control	Treated	Control	Treated	Control	Treated	Control	Treated	Control
Agree	18.8	23.1	16.5	18.3	18.2	21.4	16.2	18.2	18.9	23.0	16.1	22.0
Neither	12.9	11.4	14.0	17.4	11.0	11.4	10.0	17.9	10.8	12.5	14.9	17.5
Disagree	68.3	65.5	69.5	64.3	70.8	67.2	73.8	63.8	70.3	64.5	69.0	60.5

Source: British Household Panel Study.

Notes: Stocks unweighted $N=1496$ (789 treated, 707 control); mutual funds unweighted $N=691$ (353 treated, 338 control);

PEP accounts unweighted $N=1118$ (532 treated, 486 control).

To examine the causal effect of acquisition of financial assets on attitudes, I estimate a series of ordered logistic regression equations of the post-test (attitude after intervention) on the treatment indicator (financial assets) and pre-test (attitude prior to intervention). The results of these estimates are summarized in Tables 6.11 to 6.16. For each question I separately analyze the causal effect of stocks, mutual funds and PEP accounts. I present two tables for each topic. The first table includes the coefficient from the logistic regression for each asset type as well as the change in predicted probabilities for possible responses to each question. The second table includes information about the risk ratio, which provides the clearest indication of the average treatment effect for each attitude.

For each of these “acid tests” of pro-market positions, I find no evidence of an effect of financial assets. Neither stocks, mutual funds, nor PEP accounts had any effect on attitudes regarding government provision of employment (Tables 6.11 and 6.10), the role of the private sector in the economy (Tables 6.13 and 6.12), privatization of public services (Tables 6.15 and 6.14), or government imposition of limits on earnings (Tables 6.17 and 6.16). In each case, the coefficient for each financial asset does not reach the 5% level, and the risk ratios fail to reach the critical threshold of two. The empirical evidence provides no support for the argument that acquisition of financial assets has a causal effect on attitudes toward economic policy.

Table 6.10: Log odds and change in predicted probabilities from ordered logistic regressions of stocks, mutual funds, and PEP accounts on attitudes regarding whether government has an obligation to provide jobs, 1995-2004

Indep. variables	Log odds	Change in Predicted Probabilities		
		Agree	Neither	Disagree
Stocks	0.108 (0.111)	-0.027	0.008	0.018
Mutual funds	0.133 (0.158)	-0.013	-0.013	0.026
PEP	0.180 (0.146)	-0.045	0.012	0.013

Source: British Household Panel Study.

Notes: Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$.

Table 6.11: Simulated risk ratio (RR) and 95% confidence interval from ordered logistic regressions of stocks, mutual funds and PEP accounts on attitudes regarding whether government has an obligation to provide jobs, 1995-2004

Attitude	RR ^a	Stocks		Mutual Funds			PEP		
		95% CI ^b		RR ^a	95% CI ^b		RR ^a	95% CI ^b	
		2.5%	97.5%		2.5%	97.5%		2.5%	97.5%
Agree	0.956	0.871	1.047	0.897	0.677	1.168	0.918	0.802	1.047
Neither	1.040	0.964	1.121	0.926	0.761	1.116	1.051	0.976	1.139
Disagree	1.092	0.921	1.285	1.038	0.955	1.129	1.153	0.932	1.410

Source: British Household Panel Study.

Notes: Stocks unweighted $N=1496$ (789 treated, 707 control); mutual funds unweighted $N=691$ (353 treated, 338 control); PEP accounts unweighted $N=1118$ (532 treated, 486 control).

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

Table 6.12: Log odds and change in predicted probabilities from ordered logistic regressions of stocks, mutual funds, and PEP accounts on attitudes regarding whether private enterprise solves economic problems, 1995-2004

Indep. variables	Log odds	Change in Predicted Probabilities		
		Agree	Neither	Disagree
Stocks	-0.060 (0.106)	0.007	0.008	-0.015
Mutual funds	-0.297 (0.172)	0.033	0.040	-0.073
PEP	-0.119 (0.148)	0.011	0.019	-0.030

Source: British Household Panel Study.

Notes: Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$.

Table 6.13: Simulated risk ratio (RR) and 95% confidence interval from ordered logistic regressions of stocks, mutual funds and PEP accounts on attitudes regarding whether private enterprise solves economic problems, 1995-2004

Attitude	RR ^a	Stocks		Mutual Funds			PEP		
		95% CI ^b		RR ^a	95% CI ^b		RR ^a	95% CI ^b	
		2.5%	97.5%		2.5%	97.5%		2.5%	97.5%
Agree	1.057	0.884	1.256	1.309	0.974	1.713	1.123	0.865	1.416
Neither	1.021	0.952	1.097	1.104	0.990	1.233	1.055	0.935	1.185
Disagree	0.971	0.872	1.077	0.854	0.710	1.017	0.948	0.834	1.080

Source: British Household Panel Study.

Notes: Stocks unweighted $N=1496$ (789 treated, 707 control); mutual funds unweighted $N=691$ (353 treated, 338 control); PEP accounts unweighted $N=1118$ (532 treated, 486 control).

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

Table 6.14: Log odds and change in predicted probabilities from ordered logistic regressions of stocks, mutual funds, and PEP accounts on attitudes regarding whether public services should be state-owned, 1995-2004

Indep.variables	Log odds	Change in Predicted Probabilities		
		Agree	Neither	Disagree
Stocks	0.023 (0.111)	-0.004	0.002	0.002
Mutual funds	0.092 (0.154)	-0.023	0.009	0.014
PEP	0.065 (0.136)	-0.014	0.006	0.007

Source: British Household Panel Study

Notes: Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$.

Table 6.15: Simulated risk ratio (RR) and 95% confidence interval from ordered logistic regressions of stocks, mutual funds and PEP accounts on attitudes regarding whether public services should be state-owned, 1995-2003

Attitude	RR ^a	Stocks		Mutual Funds			PEP		
		95% CI ^b		RR ^a	95% CI ^b		RR ^a	95% CI ^b	
		2.5%	97.5%		2.5%	97.5%		2.5%	97.5%
Agree	0.991	0.903	1.087	0.960	0.831	1.100	0.974	0.869	1.087
Neither	1.010	0.930	1.095	1.033	0.931	1.145	1.028	0.928	1.143
Disagree	1.023	0.856	1.212	1.086	0.847	1.371	1.062	0.854	1.311

Source: British Household Panel Study

Notes: Stocks unweighted $N=1496$ (789 treated, 707 control); mutual funds unweighted $N=691$ (353 treated, 338 control); PEP accounts unweighted $N=1118$ (532 treated, 486 control).

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

Table 6.16: Log odds and change in predicted probabilities from ordered logistic regressions of stocks, mutual funds, and PEP accounts on attitudes regarding whether government should impose an earnings ceiling, 1995-2004

Indep. variables	Log odds	Change in Predicted Probabilities		
		Agree	Neither	Disagree
Stocks	0.145 (0.124)	-0.009	-0.012	0.021
Mutual funds	0.352 (0.181)	-0.023	-0.026	0.050
PEP	0.308 (0.144)	-0.025	-0.027	0.052

Source: British Household Panel Study

Notes: Standard errors in parentheses. *p < 0.05, **p < 0.01.

Table 6.17: Simulated risk ratio (RR) and 95% confidence interval from ordered logistic regressions of stocks, mutual funds and PEP accounts on attitudes regarding whether government should impose an earnings ceiling, 1995-2004

Attitude	RR ^a	Stocks		Mutual Funds			PEP		
		95% CI ^b		RR ^a	95% CI ^b		RR ^a	95% CI ^b	
		2.5%	97.5%		2.5%	97.5%		2.5%	97.5%
Agree	0.880	0.698	1.093	0.731	0.519	1.000	0.762	0.587	0.974
Neither	0.900	0.746	1.075	0.773	0.584	1.000	0.812	0.666	0.980
Disagree	1.025	0.984	1.069	1.063	1.000	1.131	1.070	1.006	1.139

Source: British Household Panel Study

Notes: Stocks unweighted $N=1496$ (789 treated, 707 control); mutual funds unweighted $N=691$ (353 treated, 338 control); PEP accounts unweighted $N=1118$ (532 treated, 486 control).

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

6.6 Summary

This chapter empirically examined the claim that acquisition of financial assets has a causal effect on political attitudes. The investor class theory claims that financial asset ownership shapes political attitudes and leads to shifts toward pro-market policy preferences. The asset effect theory claims that acquisition of financial assets increases political interest. I used survey data from Britain in the late 1990s-early 2000s to examine these claims. I find no evidence to support the claims that investment in the stock market during this period had any causal effect on political interest, economic attitudes or political self-efficacy.

In previous chapters, I analyzed the effect of asset values on partisanship and participation. Unfortunately, the BHPS data set does not include asset values, so I cannot rule out the possibility that people with larger portfolios might have greater political interest and different views on economic policy. Due to data limitations, this question remains open for future research. The evidence so far, however, supports my hypothesis that financial assets have no effect on political attitudes.

These results also provide additional fodder for the self-interest literature, namely that personal financial well-being has little to no effect on political attitudes. Even on “acid test” free market issues, financial asset ownership had no obvious effect on attitudes. These findings mirror those of Davis (2009) who likewise finds no effect of stock market investments on political attitudes. In order for attitudes to be motivated by financial self-interest, investors need to clearly see the connection between their assets and politics (Chong et al. 2001). Investments appear to provide insufficient stimulus to reshape political attitudes.

Chapter 7

Market shocks

7.1 Introduction

In this chapter I investigate the effect of financial asset ownership on attitudes in the United States during the 2008 financial crisis. This focus on ownership rather than acquisition of assets is intentional. The data sets used in this chapter come from cross-sectional surveys by media organizations conducted from January 2008 through January 2009 described in Chapter 3. Cross-sectional surveys data do not allow me to define a “treatment period” and examine change in behavior before and after ownership of financial assets. Nonetheless, I believe these data sets allow for valid inference insofar that most people reasonably can be assumed to have not foreseen the major financial crisis that unfolded in 2008.

Although the self-interest literature generally finds individual economic welfare has no effect on attitudes, there are three notable exceptions. I argue that 2008 incorporates each of these conditions identified in the literature for self-interest to manifest (Sears and Funk 1991).¹ First, the threat of economic meltdown was severe and ambiguous. By the middle of 2008 there were legitimate fears that the United States was in imminent danger of entering an economic depression. Policymakers scrambled to find ways to avoid catastrophe; the U.S. Treasury Secretary at one point in September 2008 opined that if Congress didn’t pass emergency legislation that “we won’t have an economy on Monday.” Second, there were clear and substantial individual costs and benefits. The stock market dropped by over a third in 2008. This pain was felt acutely by investors large and small. Finally, self-interest

¹Note that the literature finds that only one of these conditions is necessary to prime self-interest.

was politically activated. The collapse of the stock market and slowdown of the economy occurred during a presidential election year. By mid-2008 the economy had taken priority in the political contest over other pressing concerns such as the ongoing wars in Afghanistan and Iraq. The Democratic and Republican presidential candidates each appealed directly to voters' economic interests. The airwaves were filled with news reports on the financial crisis. Thus, investors in 2008 arguably were well primed to be influenced by their stock market portfolios.

The investor class theory predicts that ownership of stocks and mutual funds will compel investors to support pro-market positions. This theory, however, was developed in response to the significant growth in household stock market participation in the United States during the bull market of the 1990s. During the second half of this decade the Dow Jones Industrial Average increased over 200%. Many people who jumped into the market did so with little to no experience or memory of significant downturn in the market. Indeed, the asset effect theory claims that assets shape the investor's political behavior in part based on her *expectations of greater assets in the future* (Sherraden 1991).

Stock markets, however, go up *and* down. Neither the investor class theory nor the asset effect theory indicate what we should expect to happen when markets fall.² The psychology literature suggests that the effect of stock market losses on political behavior indeed might be greater than gains. People frequently weigh losses more heavily than equivalent gains (Kahneman and Tversky 1979). I argue that if financial assets have any effect at all, it is on political attitudes after significant financial loss. Thus, the 2008 financial crisis offers a unique opportunity to test for the effect of stock market investments on political behavior.

²At least one supporter of the investor class theory fretted that a significant stock market downturn might compel investors to seek political safety: “[As] stocks fall, there will be a general drift in the investor class away from risk and towards security—a drift likely to take them in a Democratic direction” (O’Sullivan 2000).

7.2 Prior research

Like 2008, the 1973-1974 stock market crash was global in scope and one of the worst downturns in modern history. In the U.S., the stock market lost 45% of its value; it took over 20 years for the market to return to pre-crash levels. A number of studies on self-interest examined political behavior during this period. In addition to the stock market crash, the United States from 1973-75 experienced an economic recession, stagflation (high unemployment and high inflation), and a major energy crisis produced by the 1973 OPEC oil embargo. Despite these severe economic conditions, Feldman (1982) found no effect of economic self-interest on political attitudes in his analysis of survey data from the 1972-1976 CPS American National Election Study. Kinder and Sears (1985) reported similar results. People whose lives were disrupted during the energy shortages of 1974 were no more likely to support energy policies (conservation or new energy sources) than those whose lives were unaffected by the energy crisis that year.

Kinder and Kiewiet (1979) found virtually no effect of personal economic grievances (income or unemployment) on voting behavior in U.S. congressional elections in 1974 or 1976.³ Moreover, there was no evidence of economic self-interest on the vote for specific subgroups more likely to politicize their economic circumstances (Kinder and Kiewiet 1979, 1981).⁴ Sigelman and Tsai (1981) also did not find evidence that personal finances had any effect on the presidential vote in 1972 or 1976. Sears et al. (1980) concurred, finding that self-interest on a range of issues (guaranteed jobs, health insurance, school busing, and crime) added very little to explain variance in the 1976 presidential vote.⁵

Giuliano and Spilimbergo (2009) analyzed data from the General Social Survey from 1972-1993 to study the effect of economic recessions on political and social beliefs during

³The coefficients in their regression equation for 1976 reached statistical significance, but was relatively small (compared to party identification) and added little to explain the variance in voting in behavior.

⁴Self-defined working class, union members, independent voters, voters with high political interest, and voters who identified economic issues as national problems.

⁵These scholars did, however, find a much greater substantive effect of “symbolic attitudes” (partisanship, ideology, racial prejudice) on the presidential vote.

the formative years of 18 to 25. These analysts found that Americans who experience a recession during this period of their life have lower confidence in national legislative and executive political institutions. Growing up during a recession, however, had no apparent effect on political ideology (Giuliano and Spilimbergo 2009, p. 11).

7.3 Data

In this chapter I use cross-sectional data to ascertain the causal linkage between financial assets on political attitudes. Cross-sectional data sets usually are inappropriate for causal inference as they do not offer information about the outcome variable prior to intervention. Without some measure of political attitudes prior to acquisition of financial assets I cannot be certain that my estimates are not “contaminated” by possible confounders or selection biases. Shadish et al. (2002) suggest that matching is one way to improve the inherent challenge of this “post-test only” research design. In addition to use of matched groups, I also attempt to improve upon the weakness of cross-sectional observational data by focusing on contemporaneous issues that emerged during the financial crisis of 2008. People can reasonably be considered unbiased in their attitudes about novel policies that respond to an unforeseen crisis.⁶ Although I am ignorant about the treatment period, it is reasonable to expect that respondents in these surveys had no prior beliefs about the specific policies that developed in response to the rapidly changing political economic environment of 2008.⁷

I use data from six polls conducted by news media organizations from January 2008 through January 2009. Unlike the longitudinal surveys used in previous chapters, these surveys represent distinct “snapshots” over this time period. This offers the advantage of

⁶This approach is somewhat similar to that employed by Doherty et al. (2006) in their use of cross-sectional data to study lottery winners’ attitudes toward redistribution. The approach in this dissertation arguably is stronger since I use a control group whereas the lottery study does not.

⁷This is not to suggest that survey respondents did not have prior attitudes regarding economic policies. For example, someone who preferred libertarian economic policies likely had prior biases that affected their evaluation of government policies during the 2008 financial crisis. Nonetheless, I simply make the reasonable assumption that evaluation of specific policies and government actions occurred contemporaneously and can inform us about the comparative views of investors and non-investors during a market shock.

repeated testing of views about identical or similar issues over time. This allows me to investigate whether asset ownership had any differential effect on attitudes at various points in time as the crisis evolved during the year. The trade-off, however, is that results from these data sets are not directly comparable since the subject pool in each survey is unique. I try to address this concern through propensity score matching, which creates matched groups based on their propensity for financial asset ownership. The treatment indicator for each of these surveys is stock market investments (stocks or mutual funds, inside or outside of retirement accounts) derived from a generic question about household ownership of investments.⁸ This means that as much as possible the treatment groups across surveys are similar in their propensity to own stock market investments.

Compared to the other data sets used in this dissertation, these news media polls offer a more limited number of predictors of financial asset ownership. The six media surveys listed in Table 3.3 (columns 5-10) have only nine to ten of the thirteen predictors identified in Section 3.4.3 of Chapter 3. None of these surveys include information about household debt, health status, or business ownership. Only four of the six surveys include information about home ownership. This is an important omission, since the 2008 financial crisis at root involved the bursting of a housing asset bubble. Although all six surveys include information about employment status, only two surveys inquire whether the respondent is full-time employed. The balance simply ask whether the respondent is currently employed. None of the surveys inquire directly about the respondent's occupation. One survey inquires about occupation groups (managers and white collar, blue collar, farmers, etc.) and two surveys inquire about the respondent's social class. The lack of complete or preferred predictors challenges the validity of any findings based on these surveys, as I am unable to ascertain whether the treated and control groups vary in important ways other than financial asset ownership that might affect their political attitudes.

⁸Tables 3.10 to 3.16 provide descriptive statistics for matched groups from these six surveys. One of the questions from the NBC News-*Wall Street Journal* survey was administered to only half of the sample. I create separate matched groups for this reduced sample; summary statistics are included in Table 3.13.

Unfortunately, none of the data sets used in this chapter include information about asset values. This is a particularly important omission for two reasons. First, portfolio size plausibly might be related to vested interest. Larger portfolios might be more “hedonically relevant,” creating greater personal relevance between individual economic welfare and political attitudes. Second, the degree of loss or gain during a market shock might be an important factor. People who realized smaller percentage losses than the market as a whole might be expected to exhibit different attitudes from those whose losses matched or exceeded the market. However, financial loss might not have a linear effect on attitudes. Psychological experiments find that sensitivity to loss is greater between \$0 and \$100 than between \$1,000 and \$1,100 (Tversky and Kahneman 1991). So it is possible that the effect of loss is greater for people with smaller portfolios.⁹

For causal inference of the effect of stock market investments on attitudes, I estimate logistic regressions of the post-test (attitude after treatment) on the treatment indicator (stock market investments). I provide the point estimates, standard errors and p-values of these estimates. However, my principal summary statistic of a causal effect of financial assets on attitudes is the risk ratio: the probability of a given attitude if in the treated group divided by the probability of the same attitude if in the control group. A risk ratio of 3 or more is needed for convincing evidence of a causal effect. To account for the uncertainty inherent in my parametric estimates, I simulate mean risk ratios and associated 95% confidence intervals.

7.4 The 2008 financial crisis

The 2008 financial crisis, widely considered the most serious global economic situation since the Great Depression, was felt from Wall Street to Main Street. In the United States, the Dow Jones Industrial Average, which tracks the thirty largest and most widely held public companies in the United States, lost nearly a third of its value, the worst annual loss since

⁹Portfolio size might also be a proxy for experience in the market or the capacity to weather financial losses.

1931 and the third-worst in history. In Britain, the FTSE 100 Index, which follows the 100 most highly capitalized companies on the London Stock Exchange, fell by 31%. The MSCI All Country World Index (ACWI), comprised of 23 developed and 22 developing countries, declined by 44%. All told, U.S. and global stock markets in 2008 lost roughly \$7 trillion and \$29 trillion in value respectively (Krantz 2009; Adam 2009).

In the United States, a record three million homes had foreclosure filings, an increase of 81% from 2007 and 225% from 2006 (Armour 2009). In 2008, one in every fifty-four homes received at least one foreclosure notice. Unemployment rose from 4.9% in January 2008 to 7.2% in December 2008 as the U.S. economy shed 2.6 million jobs—the greatest annual decline in sixty years (Bureau of Labor Statistics 2009; Uchitelle 2009). The economic crisis also pushed the U.S. automotive industry into crisis, necessitating a \$17.4 billion government bailout of General Motors and Chrysler (and a line of credit for Ford).¹⁰

The origins of the financial crisis lie in a housing bubble exacerbated by very low interest rates and an explosion in “subprime loans” (virtually non-existent prior to 2000) to borrowers with marginal credit histories (Baily et al. 2008). Banks created new types of loans with low-to-no down payments and adjustable interest rates that allowed these borrowers to purchase homes with little to no assets. Financial institutions developed sophisticated instruments that sliced and repackaged these subprime mortgages into “securitized” investments that presumably lowered the risk of loan default.¹¹ The average individual investor, however, was oblivious to the existence or hidden danger in these financial innovations. Indeed, the risk inherent in these financial instruments often was beyond the comprehension of many who worked in government agencies charged with regulating the market.¹²

¹⁰Chrysler and General Motors eventually filed for bankruptcy in the second quarter of 2009.

¹¹This type of “mortgage-backed” security actually had existed for the previous twenty years but limited to “conforming” loans with a minimum principal and a credit score (Baily et al. 2008). The income stream from these mortgages went to investors who bought these securities. The securitization of home mortgages made it possible to loan more money for home purchases than if the loan originators held on to the mortgages for the duration of the loans.

¹²For example, “The Warning,” a public television documentary produced by Frontline, details how the inherent risk of financial derivatives was not well understood and dismissed by leadership at the U.S. Treasury, Federal Reserve Bank and Securities and Exchange Commission. This documentary is available for viewing

Three interrelated economic events came together to produce an economic tsunami in 2008: 1) the collapse of the housing bubble in 2005 and 2006; 2) the onset of the subprime mortgage crisis beginning in early 2007;¹³ and, 3) the start of an economic recession in the United States in December 2007 (although not officially recognized until a year later). The trigger was the increase in delinquent mortgage payments in three waves: 1) housing speculators hurt by the decline in housing prices; 2) subprime borrowers with expired introductory low interest rates subsequently adjusted higher; and, 3) (prime) borrowers with good credit scores who lost their jobs due to the recession (New York Times Website 2009c). As the number of delinquent mortgages began to rise in 2007, hundreds of billions of dollars in mortgage-related investments began to sour (New York Times Website 2009b). Financial institutions with significant obligations to insure these securitized mortgages were massively over leveraged, having too few assets to pay the claims of investors whose investments were negatively affected by rising defaults. Previously “insured” investments became uninsured debts, which resulted in credit downgrades by the rating agencies of the companies who owned these securities.

As the crisis unfolded, the average investor became buffeted by events largely beyond her control. The clearest indication of the severity of the crisis was the collapse of banks and other financial institutions. Bear Stearns, which had been forced to pledge up to \$3.2 billion to bail out one of its subprime hedge funds and negotiate loans for another similar fund in 2007, was the first major financial institution to fall (Creswell and Bajaj 2007). On March 16, 2008 the Federal Reserve engineered a deal in which the venerable 85-year old company was sold to JPMorgan Chase for \$2 a share. The next major financial institution to be affected was Lehman Brothers, a global financial services firm with a 158-year old history on Wall Street. After talks with the U.S. Treasury and Federal Reserve failed to yield a deal to save the company, Lehman Brothers collapsed on September 15, 2008. In its wake, already

online at <http://www.pbs.org/wgbh/pages/frontline/warning/>.

¹³Default and delinquency rates actually began to rise in 2006 (New York Times Website 2009b).

tight global credit markets began to seize as banks no longer knew who was solvent due to the relative opacity of the mortgage-backed security market. Banks refused to lend to other banks, consumers or businesses. The collapse of the housing market and related subprime mortgage defaults had developed in a full blown credit crisis that threatened to completely unravel the U.S. economy.

By the end of 2008, major financial institutions such as Washington Mutual, Wachovia, Countrywide, Lehman Brothers and Merrill Lynch were either out of business or sold to stronger banks, often with explicit support from the Federal Reserve and the U.S. Treasury. Twenty-five banks failed in 2008, roughly the total number of bank failures for the previous seven years. The U.S. government responded to the emerging economic crisis during a hard-fought primary and general presidential election.¹⁴ As the year wore on, the economic crisis took priority over ongoing wars in Iraq and Afghanistan in the minds of voters and in the presidential campaigns. Congressional votes regarding “bailouts” to homeowners and financial institutions were cast with an eye to the November ballot box. Indeed, voter interest in the 2008 presidential election was at an unusually high level. Turnout in the November presidential election was the highest in forty years. Table 7.1 summarizes the major financial and political events of the 2008 financial crisis along with the corresponding value of the Dow Jones Industrial Average.¹⁵

¹⁴This was the first presidential election in 56 years with an “open seat” in which neither a sitting president nor vice president contested the election.

¹⁵These are closing values of the Dow Jones Industrial Average on that day. In the event the market was closed that day, the value represents the closing value on the most recent market day.

Table 7.1: Timeline of the 2008 financial crisis in the United States

Date	Event	Dow Jones Industrial Average
1/3/08	Iowa caucuses	13,056.72
1/8/08	New Hampshire presidential primary	12,589.07
1/30-2/2/08	USA Today/Gallup poll	12,427.26
2/5/08	Super Tuesday presidential primaries in 24 states	12,265.13
3/10/08	Dow down 20% from peak 5 months earlier	11,740.15
3/16/08	JPMorgan Chase buys Bear Stearns at \$2/share	11,972.25
3/28-4/2/08	CBS News/New York Times Poll	12,608.92
5/9/08	Third bank failure of the year	12,745.88
5/30/08	Fourth bank failure of the year	12,638.32
6/30/08	Over 252,000 properties in foreclosure, up 53% from year ago	11,350.01
7/11/08	IndyMac Bank seized by FDIC, 4th largest failure in U.S. history	11,100.54
7/24/08	Congress passes \$300B housing bailout bill	11,632.38
8/28/08	Barack Obama accepts Democratic Party nomination for president	11,715.18
9/4/08	John McCain accepts Republican Party nomination for president	11,188.23
9/7/08	Government nationalizes Freddie Mac & Fannie Mae	11,510.74
9/14/08	Merrill Lynch sold to Bank of America	11,421.99
9/15/08	Lehman Brothers declares bankruptcy; Dow drops 500 points	10,917.51
9/17/08	Government loans \$85B to American Insurance Group	10,609.66
9/18/08	Treasury Secretary Paulson: "economy on Monday?"	11,019.69
9/19-9/22/08	NBC News/Wall Street Journal poll	11,015.69
9/25/08	John McCain & Barack Obama meet with Pres. Bush on the crisis; Washington Mutual seized by FDIC, largest failure in U.S. history	11,022.06
9/26/08	First presidential debate	11,143.13
9/29/08	Congress rejects financial bailout bill; Dow falls almost 800 points	10,365.45
10/2/08	Vice-presidential debate	10,831.07
10/3/08	Congress passes \$700B financial bailout bill, creates TARP	10,325.38
10/7/08	Second presidential debate	9,447.11
10/11/08	Worst week ever for Dow, down 22% in 8 days	9,387.61
10/15/08	Third presidential debate	8,577.91
11/4/08	Barack Obama elected president, Democrats take control of the U.S. House of Representatives and U.S. Senate	9,625.28
11/15/08	G20 meets in Washington, DC on the global financial crisis	8,273.58
12/4-12/8/08	CBS Newspann	8,934.18
12/11/08	Bernard Madoff arrested for purported \$50B Ponzi scheme	8,565.09
12/12/08	Twenty-fourth and twenty-fifth bank failures of the year	8,629.68
12/12-12/14/08	USA Today/Gallup poll	8,629.68
1/6/09	111th U.S. Congress begins; stimulus bill introduced in U.S. Senate	9,015.10
1/11-1/15/09	CBS News/New York Times poll	8,212.49

7.5 Role of government

Proponents of the investor class theory contend that stock market investments affect attitudes toward the role of the government. In this section, I investigate whether ownership of stock market investments had any effect on views about how the U.S. government should respond to the 2008 financial crisis. A March 2008 survey jointly conducted by CBS News and *The New York Times* inquired about whether respondents preferred a smaller government providing fewer services or bigger government providing more services.¹⁶ Although Table 7.2 shows a slightly larger percentage of the treated group preferred a smaller government than the control group, majorities of both groups clearly indicated a preference for smaller government.

Table 7.2: Attitudes regarding preferred size of government in March 2008, weighted percentages

	Treated <i>N</i> =801	Control <i>N</i> =335
Prefer bigger government	35.0	39.5
Prefer smaller government	65.0	60.5

Source: CBS News and *The New York Times*.

Table 7.3 presents three models of the effect of investments on attitudes regarding the role of government.¹⁷ In column one, I find investment had no effect on attitudes regarding the preferred size of government. This result is unchanged when I control for home ownership in column two. Column three includes a control for sociotropic view of the economy.¹⁸ In each model the risk ratio is approximately one, indicating no difference between investors and non-investors. Although I continue to find no effect of financial assets on attitudes

¹⁶“If you had to choose, would you rather have a smaller government providing fewer services, or a bigger government providing more services?”

¹⁷The dependent variable is coded 1 for prefer smaller government, 0 for prefer larger government.

¹⁸The rating of the economy is a scale from 1 for very good to 4 for very bad.

about the role of government, I do find that views about the national economy appears to create greater support for an active government role. The more pessimistic people were about the overall economic conditions, the less likely they were to express a desire for smaller government.

Table 7.3: Log odds, simulated risk ratio (RR) and 95% confidence interval from a logistic regression of stock market investments on support for smaller government, March 2008

	(1)	(2)	(3)
Intercept	0.429 (1.30)**	-0.059 (0.431)	2.488 (0.577)**
Investments	0.191 (0.153)	-0.134 (0.507)	-0.149 (0.545)
Homeowner		0.536 (0.479)	0.497 (0.513)
Investments x Homeowner		0.357 (0.553)	0.387 (0.603)
Rating of economy			-0.824 (0.112)**
RR ^a	1.077	1.075	1.08
RR 95% CI ^b	(0.966,1.204)	(0.963,1.202)	(0.962,1.217)
N	1136	1136	1136

Source: CBS News and *The New York Times*.

Notes: Standard errors in parentheses. *p < 0.05, **p < 0.01.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

A September 2008 survey by NBC News and *The Wall Street Journal* asked respondents about the proper role of government to solve problems.¹⁹ In the four days before this survey was administered, the Dow Jones index fell 500 points on the day Lehman Brothers collapsed, the government provided \$85 billion in emergency loans to insurance giant AIG,

¹⁹“I’m going to read you two statements about the role of government, and I’d like to know which one comes closer to your point of view. Statement A: Government should do more to solve problems and help meet the needs of people, OR Statement B: Government is doing too many things better left to businesses and individuals.”

and Treasury Secretary Henry Paulson openly mused about whether there would be a U.S. economy on Monday. Despite the gravity of this situation, Table 7.4 finds similar proportions of the treated and control group believed either the government was doing too much or too little.

Table 7.4: Attitudes regarding role of government in solving problems, weighted percentages

	Treated <i>N</i> =660	Control <i>N</i> =221
Government is doing too many things	45.6	43.1
Government should do some of both	7.4	12.7
Government should do more	47.0	44.2

Source: NBC News and *The Wall Street Journal*.

Tables 7.5 and 7.6 present the results of ordered logistic regressions attitudes about government actions on stock market investments. The coefficient on stock market investments in Table 7.5 falls short of the 5% level. Table 7.6 shows the risk ratio is roughly one for each possible response to this question. Financial asset ownership had no effect on attitudes regarding the proper role of government in responding to the crisis.

Table 7.5: Log odds and change in predicted probabilities from ordered logistic regressions of stock market investments on attitudes regarding role of government, September 2008

Independent variables	Log odds	Change in Predicted Probabilities		
		Govt doing too many things	Govt should do more	Some of both
Investments	0.006 (0.149)	-0.002	0.001	0.000

Source: NBC News and *The Wall Street Journal*.

Notes: Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$. Unweighted $N = 881$.

Table 7.6: Simulated risk ratio (RR) and 95% confidence interval from a logistic regression of stock market investments on attitudes regarding role of government, September 2008

Attitude	RR ^a	RR 95% CI ^b	
		2.50%	97.50%
Doing too many things	1.001	0.8528	1.175
Some of both	1.003	0.994	1.02
Should do more	1.008	0.8616	1.181

Source: NBC News & *The Wall Street Journal*

Notes: Unweighted $N = 881$. ^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

By late 2008 the U.S. government had created the Troubled Asset Relief Program (TARP) to purchase assets and equity from financial institutions to strengthen the financial sector burdened with non-performing subprime loans. Authorized by the Emergency Economic Stabilization Act of 2008 (enacted October 3, 2008), the U.S. Department of the Treasury spent \$247 billion of its allotted \$700 billion by the end of the year. \$178 billion of this amount was used to purchase shares of preferred stock and warrants from 214 financial institutions (Congressional Budget Office 2009). A December 2008 CBS News survey asked respondents whether they believed the government’s actions helped or hurt the economy.²⁰ As Table 7.7 shows, over 70% of both treatment groups believed the government’s efforts had no effect on the economy.

²⁰“Do you think the steps that the federal government has taken so far to assist banks and other financial institutions have helped the national economy, have hurt the national economy, or have they made no difference so far?”

Table 7.7: Attitudes regarding the impact of government actions to assist the U.S. economy in December 2008, weighted percentages

	Treated <i>N</i> =839	Control <i>N</i> =349
Hurt economy	12.1	13.8
Made no difference	71.4	73.5
Helped economy	16.5	12.8

Source: CBS News.

Table 7.8 shows that ownership of investments had no effect on views about the impact of government actions to help the economy. The coefficient on investments falls well below the 5% level. The risk ratios for possible responses to this question in Table 7.9 are roughly one, indicating no difference between investors and non-investors. In summary, I find no evidence that investors had different attitudes from non-investors regarding the role of the government throughout the crisis in 2008.

Table 7.8: Log odds and change in predicted probabilities from ordered logistic regressions of stock market investments on attitudes regarding impact of government actions to assist the U.S. economy, December 2008

Independent variables	Log odds	Change in Predicted Probabilities		
		Hurt economy	Helped economy	Made no difference
Investments	0.221 (0.156)	-0.026	0.027	-0.002

Source: CBS News

Notes: Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$. Unweighted $N = 1,188$.

Table 7.9: Simulated risk ratio (RR) and 95% confidence interval from a logistic regression of stock market investments on attitudes regarding impact of government actions to assist the U.S. economy, December 2008

Attitude	Risk Ratio	95% CI	
		2.50%	97.50%
Hurt	0.833	0.639	1.065
No difference	0.998	0.988	1.008
Helped	1.219	0.941	1.566

Source: CBS News.

Notes: Unweighted $N = 1,188$.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

7.6 Economic stimulus

In this section I investigate whether investors had different attitudes regarding efforts to stimulate the economy through government spending compared to non-investors. On February 7, 2008, the U.S. Congress passed the Economic Stimulus Act of 2008, signed into law by President George W. Bush six days later. The \$152 billion package consisted mainly of “tax rebate” checks mailed out to lower and middle income taxpayers in mid-2008. Almost exactly one year later, the U.S. Congress on February 13, 2009 passed a second stimulus measure, the American Recovery and Reinvestment Act of 2009, signed into law four days later by President Barack Obama. The much larger \$787 billion package included a mixture of federal tax cuts, unemployment benefits, and government spending for education, health care, and infrastructure projects. Support for both measures fell mostly along partisan lines. The 2008 stimulus passed with a majority of Democratic lawmakers and a minority of Republicans. The 2009 stimulus passed without any support from House Republicans and only three Senate Republicans. If investors took their cues from the Republican Party, we should see some difference in their support for economic stimulus by the government.

A January 2008 survey by *USA Today* and the Gallup Organization queried respondents' attitudes toward the proposed economic stimulus that year.²¹ A January 2009 survey by CBS News and *The New York Times* asked the same question of respondents prior to passage of the 2009 stimulus package. These two surveys provide bookends to the 2008 crisis. It is possible that investors had different views before and after the calamitous events of 2008. Table 7.10 summarizes the percentages of the treated and control groups who supported and opposed the stimulus in both years. At both points in time, similarly sized majorities of the treated and control groups favored an economic stimulus by the federal government. Interestingly, the level of support for stimulus declined somewhat in both treatment groups by early 2009 compared to a year earlier.

Table 7.10: Attitudes regarding economic stimulus by the government in January 2008 & 2009, weighted percentages

	January 2008		January 2009	
	Treated	Control	Treated	Control
	<i>N</i> =1414	<i>N</i> =346	<i>N</i> =733	<i>N</i> =226
Favor	76.3	78.2	69.1	66.6
Oppose	23.7	21.8	30.9	33.4

Sources: USA Today and Gallup (2008) & CBS News and *The New York Times* (2009).

Table 7.11 presents results of logistic regression of attitudes about government economic stimulus on stock market investment ownership. In January 2008 and January 2009, investments had no effect on attitudes regarding economic stimulus (columns one and four). Home ownership also had no effect on these views (columns two and five). Although views about the state of the national economy did not affect support for economic stimulus in early 2008, by January 2009 people who were more pessimistic about the economy were more likely

²¹ "Do you favor or oppose the federal government passing a bill that would attempt to stimulate growth in the economy?"

to support the stimulus package.²² Table 7.11 shows that financial asset ownership played no role in explaining support for economic stimulus either early in the crisis or after the financial carnage of 2008.

²²For the USA Today-Gallup survey, the sociotropic variable is a scale of rating of economic conditions in the country. People who viewed the economy as “getting better” were coded as 1, people who viewed the economy as “getting worse” were coded as 3. The coding for the CBS/New York Times survey is similar: from “very good” coded as 1 to “very bad” coded as 4.

Table 7.11: Log odds, simulated risk ratio (RR) and 95% confidence interval from logistic regressions of stock market investments on support for economic stimulus by the government, January 2008 & 2009

	January 2008			January 2009		
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	1.277 (0.149)**	1.120 (0.330)**	1.224 (0.409)**	0.689 (0.166)**	1.347 (0.625)*	-0.225 (0.726)
Investments	-0.111 (0.164)	0.338 (0.388)	0.334 (0.388)	0.116 (0.189)	-0.382 (0.677)	-0.561 (0.698)
Homeowner		0.191 (0.348)	0.186 (0.348)		-0.746 (0.645)	-1.052 (0.673)
Investments x Homeowner		-0.529 (0.406)	-0.527 (0.406)		0.568 (0.689)	0.779 (0.712)
Rating of economy			-0.031 (0.075)			0.516 (0.130)**
RR ^a	0.977	0.981	0.981	1.042	1.042	1.044
RR 95% CI ^b	(0.914,1.05)	(0.918,1.054)	(0.917,1.053)	(0.931,1.173)	(0.930,1.173)	(0.931,1.178)
<i>N</i>	1760	1760	1760	959	959	959

Sources: *USA Today* and Gallup (2008); CBS News and *The New York Times* (2009)

Notes: Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

7.7 Homeowner bailout

In this section, I investigate the effect of financial asset ownership on attitudes regarding government responses to the housing crisis. The extent of the subprime mortgage crisis became clearer as the year progressed. By the middle of 2008, a record 1.1 million homes were in foreclosure. Roughly three million borrowers missed at least one mortgage payment during the first five months of the year, of which about 737,000 were at least three months past due but not yet in foreclosure (Isidore 2008). On June 24, 2008, the U.S. Congress passed the Housing and Economic Recovery Act of 2008. The law authorized the Federal Housing Administration to guarantee up to \$300 billion in new 30-year fixed rate mortgages for subprime borrowers if lenders were willing to write down principal loan balances to ninety percent of current appraised value. The law also included a first-time home buyer refundable tax credit of up to \$7,500.

Three news media surveys in March 2008, December 2008 and January 2009 asked respondents about whether the government should help homeowners who were having trouble paying their mortgages due to increases in interest rates.²³ Table 7.12 appears to show clear differences between treatment groups on this issue.

Table 7.12: Attitudes regarding homeowner bailouts in March 2008, December 2008 and January 2009, weighted percentages

	March 2008		December 2008		January 2009	
	Treated <i>N</i> =801	Control <i>N</i> =335	Treated <i>N</i> =839	Control <i>N</i> =349	Treated <i>N</i> =733	Control <i>N</i> =226
Oppose	58.1	48.1	45.9	36.4	54.7	46.5
Support	41.9	51.9	54.1	63.6	45.3	53.5

Sources: CBS News and *The New York Times* (March), CBS News (December), and CBS News and *The New York Times* (January).

²³“Should the government help homeowners having trouble repaying mortgages because their rates went up?”

A majority of the control group consistently supported government aid for homeowners for all three time periods. Moreover, a greater percentage of the control group expressed support for homeowner bailouts than the treated group. Although a majority of the treated group appeared to support the bailout in December 2008, this possibly is due to the lack of a measure of homeownership in the CBS News survey. This means treated and control groups are likely unbalanced on this important covariate. A majority of the treated group expressed opposition to the “homeowner bailout” for the two surveys (March 2008 and January 2009) which include measures of home ownership. Table 7.13 summarizes the results of regression analysis of stock market investments on attitudes regarding “homeowner bailout” between March 2008 and January 2009.²⁴

In the base model in columns one, four and six in Table 7.13 the coefficient for stock market investments reaches the 5% level for statistical significance. However, the coefficient loses statistical significance when I control for home ownership (columns two and seven). I also check for the effect of sociotropic views of the economy.²⁵ Columns three, five and eight of Table 7.13 show perspectives about the state of the economy had a significant effect on views regarding homeowner bailouts. People who rated the economy more poorly were more inclined to support policies to help struggling homeowners. In terms of relative risk, however, the inclusion of additional controls had no effect. In each model the risk ratio falls well short of the critical threshold of 0.500 needed for minimal evidence that investors were more opposed to homeowner bailouts. Despite the lack of a covariate for homeownership in the CBS News survey, columns four and five of Table 7.13 yield nearly identical risk ratios to the other models. Financial assets had no causal effect on attitudes toward homeowner bailouts during the crisis.

²⁴The dependent variable is coded 1 for support of providing assistance to homeowners and 0 for opposition to this action.

²⁵The sociotropic variable is a scale rating of the national economy, from 1 (“very good”) through 4 (“very bad”).

Table 7.13: Log odds, simulated risk ratio (RR) and 95% confidence interval from logistic regressions of stock market investments on support for homeowner bailout, March 2008, December 2008 and January 2009

	March 2008			December 2008		January 2009		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	0.076 (0.141)	1.148 (0.448)**	-0.593 (0.524)	0.494 (0.141)**	-1.439 (0.331)**	0.140 (0.144)	0.801 (0.543)	-0.348 (0.605)
Investments	-0.402 (0.158)*	-0.720 (0.517)	-0.740 (0.528)	-0.329 (0.153)*	-0.411 (0.169)*	-0.327 (0.159)*	-0.564 (0.554)	-0.729 (0.568)
Homeowner		-1.164 (0.471)*	-1.144 (0.481)*				-0.759 (0.564)	-0.969 (0.587)
Investments x Homeowner		0.340 (0.550)	0.343 (0.561)				0.328 (0.580)	0.474 (0.594)
Rating of economy			0.576 (0.096)**		0.578 (0.106)**			0.371 (0.120)**
RR ^a	0.811	0.808	0.803	0.874	0.849	0.852	0.858	0.857
RR 95% CI ^b	(0.701,0.941)	(0.698,0.938)	(0.693,0.933)	(0.783,0.983)	(0.756,0.959)	(0.735,0.989)	(0.740,0.997)	(0.737,0.999)
<i>N</i>	1136	1136	1136	1188	1188	959	959	959

Sources: CBS News and *The New York Times* (March), CBS News (December), and CBS News and *The New York Times*. (January)

Notes: Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

7.8 Banks bailout

In this section, I investigate whether investors had different views about how the government should respond to the financial industry crisis. The U.S. Treasury and Federal Reserve Bank played the leading roles in the government's response to the unfolding financial crisis. Unlike the Federal Deposit Insurance Corporation (FDIC), which had an established protocol to deal with bank failures, the government's initial approach early in 2008 was to respond to emerging crises on a case-by-case basis (Nocera and Andrews 2008). For example, in mid-March 2008, Treasury Secretary Henry Paulson and Federal Reserve Chairman Ben Bernanke intervened to direct the sale of Bear Stearns to JPMorgan Chase for \$2 a share to avert what they believed would be systemic losses in the global stock market due to that firm's heavy exposure to collateralized debt obligations.²⁶

The government did not intervene, however, to prevent the collapse of Lehman Brothers on September 15, 2008. After the Bear Stearns deal, government leaders were concerned about the "moral hazard" that would ensue if financial institutions believed that the government would bail them out regardless of the riskiness of their actions (Nocera and Andrews 2008). In response, the Dow Jones Industrial Average fell by 500 points at the close of the market that day. Two days later, the government provided a \$85 billion bailout to the American Insurance Group (AIG), a large financial industry conglomerate with significant involvement in credit default swaps world wide.²⁷ The government finally moved to a more systematic approach with the \$700 billion Troubled Asset Relief Program (TARP) in October 2008.²⁸

²⁶On June 22, 2007, Bear Stearns pledged a collateralized loan of up to \$3.2 billion to "bail out" one of its hedge funds hurt by the collapse of the housing bubble and bad bets on subprime mortgage loans. At the time it was the biggest rescue of a hedge fund since the collapse of Long-Term Capital Management in 1998 (Creswell and Bajaj 2007).

²⁷A downgrade in AIG's credit rating required the company to post additional collateral with its counterparties. AIG did not have sufficient capital to meet this obligation, which resulted in a serious liquidity crisis. The bailout gave the government nearly 80% equity of the company and the right to suspend dividend payments to previously issued common and preferred stockholders.

²⁸Johnson (2009) argued that the U.S. government failed to develop a transparent, systematic approach to deal with the financial crisis. "The response so far is perhaps best described as 'policy by deal': when a major financial institution gets into trouble, the Treasury Department and the Federal Reserve engineer a

Over the course of the crisis, three surveys polled Americans about their attitudes regarding bailout of the financial industry in the wake of the subprime mortgage crisis. A March 2008 survey by CBS News and *The New York Times* asked respondents about whether the federal government should aid financial institutions that made bad loans.²⁹ Table 7.14 shows that large majorities of both treatment groups were opposed to a banks bailout.³⁰

Table 7.14: Attitudes regarding bailout of financial institutions in March 2008, weighted percentages

	Treated <i>N</i> =801	Control <i>N</i> =335
Opposed to bailout	75.1	73.6
In favor of bailout	24.9	26.4

Source: CBS News and *The New York Times*.

A September 2008 poll, however, found roughly equivalent proportions of attitudes among treated and control groups concerning proposed legislation to help the financial institutions.³¹ Table 7.15 shows approximately forty percent of both treatment groups supported government bailout of the banks, with the balance roughly split between opposition and undecided. A December 2008 poll (Table 7.16) finds both treatment groups roughly evenly split about the TARP.³² A simple majority of the treated group opposed the plan; an equally simple majority of the control group supported the effort.

bailout over the weekend and announce on Monday that everything is fine...Treasury and the Fed did not act according to any publicly articulated principles, but just worked out a transaction and claimed it was the best that could be done under the circumstances. This was late-night, backroom dealing, pure and simple” (page 52).

²⁹“Should the federal govt help banks and mortgage companies that made loans to people who can’t afford to pay them back?”

³⁰This position did not change when the question was re-framed with the goal of providing financial assistance to the banks to limit an economic recession.

³¹“Do you approve or disapprove of plan for government to take over bad mortgages and other troubled investments from financial firms?” Only one half of the sample received this question.

³²“Do you approve or disapprove of the federal government providing money to banks and other financial institutions to try to help fix the economy?”

Table 7.15: Attitudes regarding bailout of financial institutions in September 2008, weighted percentages

	Treated <i>N</i> =318	Control <i>N</i> =113
Approve	41.2	46.4
Uncertain/no opinion	29.9	25.2
Disapprove	28.9	28.5

Source: NBC News and *The Wall Street Journal*.

Table 7.16: Attitudes regarding bailout of financial institutions in December 2008, weighted percentages

	Treated <i>N</i> =839	Control <i>N</i> =349
Disapprove	52.7	48.5
Approve	47.3	51.5

Source: CBS News.

Tables 7.17 to 7.20 provide estimates of the causal effect of financial asset ownership on attitudes regarding support to the financial industry by the federal government. For the March (Table 7.17) and December (Table 7.20) surveys I estimate binary logistic regressions of attitudes on the treatment indicator. For the September poll I estimate ordered logistic regressions (Table 7.18) and summarize the risk ratios for the possible responses to the survey question (Table 7.19). In each case, the coefficient on stock market investments falls short of the 5% level for statistical significance and the risk ratios are approximately one. I find no evidence of a causal effect of stock market investments on attitudes regarding the government's bailout of the financial industry. Although I find sociotropic views of the economy had an effect in December 2008 (column two of Table 7.20), the losses investors personally incurred over the course of 2008 were apparently not enough to overcome the

general sense of malaise about the economy felt by investors and non-investors alike.

Table 7.17: Log odds, simulated risk ratio (RR) and 95% confidence interval from logistic regressions of stock market investments on support for banks bailout, March 2008

	(1)	(2)	(3)
Intercept	-1.024 (0.132)**	-0.4665 (0.396)	-0.872 (0.587)
Investments	-0.082 (0.157)	-0.493 (0.498)	-0.494 (0.500)
Homeowner		-0.619 (0.426)	-0.605 (0.428)
Investments x Homeowner		0.459 (0.526)	0.460 (0.526)
Rating of economy			0.130 (0.131)
RR ^a	0.949	0.948	0.947
RR 95% CI ^b	(0.756,1.183)	(0.755,1.182)	(0.753,1.182)
<i>N</i>	1136	1136	1136

Source: CBS News and *The New York Times*.

Notes: Standard errors in parentheses. *p < 0.05, **p < 0.01.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

Table 7.18: Log odds and change in predicted probabilities from ordered logistic regressions of stock market investments on support for banks bailout, September 2008

Independent variables	Log odds	Change in Predicted Probabilities		
		Approve	Disapprove	Uncertain/no opinion
Investments	0.139 (0.205)	-0.034	0.027	0.007

Source: NBC News and *The Wall Street Journal*.

Notes: Standard errors in parentheses. *p < 0.05, **p < 0.01.

Unweighted N = 1,188.

Table 7.19: Simulated risk ratio (RR) and 95% confidence interval from ordered logistic regressions of stock market investments on support for banks bailout, September 2008

Attitude	Risk Ratio	95% CI	
		2.50%	97.50%
Approve	0.9336	0.745	1.169
Uncertain/no opinion	1.0266	0.9704	1.119
Disapprove	1.1186	0.8315	1.493

Source: NBC News and *The Wall Street Journal*.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

Table 7.20: Log odds, simulated risk ratio (RR) and 95% confidence interval from logistic regressions of stock market investments on support for banks bailout, December 2008

	(1)	(2)
Intercept	0.058 (0.120)	-0.909 (0.316)**
Investments	-0.164 (0.143)	-0.197 (0.145)
Rating of economy		0.286 (0.087)**
RR ^a	0.923	0.909
RR 95% CI ^b	(0.808,1.056)	(0.793,1.040)
N	1188	1188

Source: CBS News.

Notes: Standard errors in parentheses.

*p < 0.05, **p < 0.01.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

7.9 Auto bailout

In this section, I investigate whether investors had different views than non-investors about government support for the auto industry during the 2008 financial crisis. The automotive industry was on life support by the beginning of the fourth quarter of 2008. The increase in global oil prices in the previous years resulted in major drops in purchases of sports utility vehicles and pickup trucks, which contributed significantly to the bottom line of the Big Three automotive companies. Moreover, these companies were heavily burdened by legacy retirement and employee health insurance expenses (New York Times Website 2009a). Auto sales dropped as the recession took hold in 2008 and credit markets froze up. Sales in October 2008 dropped nearly 32% to their lowest level in twenty-five years. The auto makers idled factories and closed dealerships as they burned through their cash reserves.

The potential collapse of the U.S. auto industry posed enormous economic and political challenges for policymakers. The Big Three auto companies, their suppliers and dealers collectively employed roughly 1.6 million people (Meyerson 2008). During an election year the plight of auto industry workers was a potent political issue. The auto industry also represented a significant portion of the American manufacturing base. Collapse of an entire industry doubtless would create ripples throughout the rest of the U.S. economy. In November 2008, the Big Three auto companies (General Motors, Chrysler and Ford) indicated they would need \$25 billion to avoid bankruptcy. After an auto industry bailout bill failed to pass in the U.S. Senate on December 11, President Bush on December 19 used his executive authority to direct \$13.4 billion in emergency loans from the TARP to General Motors and Chrysler, with another \$4 billion slated for February 2009.

Three surveys asked respondents about their attitudes regarding the “auto industry bailout.” An early December 2008 survey by CBS News occurred while details of the legislation was being worked out between Congress and the White House.³³ Table 7.21 shows that

³³“Do you approve or disapprove of the federal government providing money to the Big Three U.S. auto makers to prevent them from going out of business or bankruptcy?”

majorities of both treatment groups opposed the auto bailout. The treated group, however, is close to evenly split on the issue. An even greater proportion of the control group (by eleven percentage points) opposed the bailout than the treated group.

Table 7.21: Attitudes regarding proposed auto bailout in early December 2008, weighted percentages

	Treated <i>N</i> =839	Control <i>N</i> =349
Disapprove	51.1	62.6
Approve	48.9	37.4

Source: CBS News.

A mid-December poll by *USA Today* and the Gallup Organization occurred after the U.S. Senate rejected the auto bailout bill but before executive action by President Bush. Table 7.22 shows that while roughly the same simple majority of the treated group was opposed to the bailout of the Big Three, a majority of the control group now wished that the failed legislation had passed.³⁴ A January 2009 poll by CBS News and *The New York Times*, however, found a large majority of both treatment groups resoundingly opposed additional aid to the auto companies beyond the initial amount provided in December 2008 (Table 7.23).³⁵

³⁴“Would you rather have seen the auto legislation pass or fail?”

³⁵“Should the federal government give major auto companies more money than \$13 billion already provided if they ask for more?”

Table 7.22: Attitudes regarding failed auto bailout legislation in mid-December 2008, weighted percentages

	Treated <i>N</i> =644	Control <i>N</i> =192
Preferred to see fail	53.8	42.1
Preferred to see pass	46.2	57.9

Source: *USA Today* and Gallup.

Table 7.23: Attitudes regarding additional aid to auto companies in January 2009, weighted percentages

	Treated <i>N</i> =733	Control <i>N</i> =226
Opposed to more aid	78.9	72.7
In favor or more aid	21.1	27.3

Source: CBS News and *The New York Times*.

Table 7.24 reports the results of logistic regressions of attitudes regarding bailout of the auto industry on financial asset ownership. The coefficients on investments for both surveys in December are statistically significant at the 1% level (columns one and three). The coefficients maintain statistical significance (at the 5% level) when I control for home ownership and sociotropic views of the economy.³⁶ At first glance, it appears that investors were initially in favor of an auto bailout when it was under consideration in Congress (columns one and two), but were opposed to President Bush’s decision to support the auto companies out of TARP funds (columns three through five). By January 2009, the coefficients for investments fail to reach the 5% level for any of the models (columns six through eight). A closer examination of the risk ratio, the summary comparison between investors and non-investors,

³⁶For the CBS News and CBS/NYT surveys, the sociotropic question is how respondents rate the national economy. I create a scale ranging from 1 for “very good” to 4 for “very bad.” For the USA Today/Gallup survey, the question is how respondents would describe the current economic situation in the U.S. I create a scale ranging from 1 for “not a major problem” to 4 for “the biggest crisis ever.”

finds that none of the models clear the minimal threshold of 2 (or 0.500) in December or January. There is no difference in the probability of support or opposition for auto bailout between investors and non-investors. I find no evidence that financial asset ownership had any causal effect on views about government support for the automotive industry.

Table 7.24: Log odds, simulated risk ratio (RR) and 95% confidence interval from logistic regressions of stock market investments on support for auto bailout, December 2008 and January 2009

	Dec. 4-8, 2008		Dec. 12-14, 2008			January 2009		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	-0.515 (0.134)**	-2.096 (0.346)**	0.320 (0.149)*	1.072 (0.462)*	0.375 (0.539)	-0.979 (0.155)**	-2.072 (0.622)**	-2.733 (0.733)**
Investments	0.470 (0.163)**	0.435 (0.168)*	-0.474 (0.170)**	-1.187 (0.518)*	-1.214 (0.521)*	-0.338 (0.190)	0.518 (0.700)	0.457 (0.706)
Homeowner				-0.855 (0.487)	-0.921 (0.491)		1.220 (0.650)	1.120 (0.663)
Investments x Homeowner				0.810 (0.546)	0.832 (0.549)		-0.958 (0.735)	-0.887 (0.743)
Rating of economy		0.463 (0.093)**			0.224 (0.087)**			0.207 (0.149)
RR ^a	1.314	1.286	0.801	0.793	0.789	0.782	0.792	0.792
RR 95% CI ^b	(1.101,1.57)	(1.075,1.539)	(0.691,0.931)	(0.683,0.923)	(0.680, 0.919)	(0.594,1.019)	(0.597,1.040)	(0.596, 1.04)
<i>N</i>	1188	1188	836	836	836	959	959	959

Sources: CBS News (Early December), and *USA Today* and Gallup mid-December), and CBS News and *The New York Times* (January).

Note: Standard error in parentheses; *p < 0.05, **p < 0.01.

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets; ^bmean 95% confidence interval.

7.10 The stock market

Despite the severity of the financial crisis of 2008, investors apparently did not differ in their attitudes regarding government actions to rescue the economy. In this final section, I investigate whether investors held different views about the stock market from non-investors. 2008 was one of the worst years ever for stock market investors. The Dow Jones Industrial Average declined by nearly a third, and both the NASDAQ and Standard and Poor's 500 indexes fell by roughly 40%. Compared to opening share values on January 1, 2008, over 90% of the five hundred public companies that comprised the S&P 500 declined in value by the close of the stock market on December 31, 2008 (Krantz 2009). Although the stock market decline had broad effects on the U.S. economy, investors arguably had greater vested interest in matters concerning the stock market given their direct financial losses (Crano 1997a). To evaluate the effect of financial assets on attitudes regarding the stock market, I use a question from three surveys affiliated with CBS News in March and December 2008 and January 2009. The surveys asked respondents whether they think investment in the stock market generally is safe or risky.

Table 7.25 summarizes the responses of the treated and control groups. Across all three time points in time I find large majorities of both treatment groups believe the stock market generally is a risky place for investments. What's interesting, however, is the change in the percentage of both treatment groups that express the belief that the stock market is generally a safe investment. In March 2008 roughly 40% of the treated group believed the stock market is generally safe compared to nearly a quarter of the control group (columns 1 and 2). At the time this poll closed on April 2, 2008, the Dow Jones average was at roughly 12,600, down only about 400 points since the beginning of the year.

By the December poll, the Dow was down to just under 9,000 points—a decline of nearly 30% from the time of the March survey. Columns 3 and 4 in Table 7.25 show the proportion of the treated group who viewed the stock market as safe decreased by nine percentage points

to just under thirty percent. By contrast, *just over 7% of the control group* saw the stock market as safe by the end of 2008. This divergence continued in January 2009, as roughly 28% of the treated group viewed the stock market as generally safe while only three percent of the control group expressed the same attitude.

Table 7.25: Attitudes regarding general riskiness of the stock market, weighted percentages

	March 2008		December 2008		January 2009	
	Treated <i>N</i> =801	Control <i>N</i> =335	Treated <i>N</i> =839	Control <i>N</i> =349	Treated <i>N</i> =733	Control <i>N</i> =226
Risky	61.1	75.96	70.12	92.54	72.54	96.84
Safe	38.9	24.04	29.88	7.46	27.46	3.16

Sources: CBS News and *The New York Times* (March), CBS News (December), and CBS News and *The New York Times* (January).

In Table 7.26 I report the results of binary logistic regression of attitudes about the riskiness of the stock market on ownership of stock market investments. Column one shows that in March 2008 financial asset ownership had no causal effect on views about the stock market. The risk ratio was only about 1.6—short of the minimum threshold of two. By the end of the year, however, there was a clear divide between stock market investors and non-investors. The risk ratio in column 3 of Table 7.26 shows that stock market investors in December 2008 were four times as likely than non-investors to indicate they believed that the stock market was a safe place for investments. The 95% confidence interval for the risk ratio ranges from just below three to close to six. Column five in Table 7.26 shows that in January 2009, by which time the Dow Jones had fallen another 1,000 points, investors were almost ten times as likely to view the stock market as safe than non-investors. The 95% confidence interval in this case spans from a low of approximately four to just over twenty. In each case, inclusion of sociotropic views of the national economy actually increases the risk ratio (columns two, four and six).³⁷

³⁷The sociotropic variable is a scale rating of the state of the national economy. “Very good” is coded as

Table 7.26: Log odds, simulated risk ratio (RR) and 95% confidence interval from logistic regressions of stock market investments on attitude that the stock market generally is safe for investments in March 2008, December 2008 and January 2009

	March 2008		December 2008		January 2009	
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	-1.155 (0.212)**	0.970 (0.312)**	-2.521 (0.212)**	-0.911 (0.384)*	-3.446 (0.462)**	-1.748 (0.609)**
Investments	0.702 (0.220)**	0.756 (0.238)**	1.667 (0.227)**	1.775 (0.231)**	2.474 (0.466)**	2.508 (0.470)**
Rating of economy		-0.738 (0.122)**		-0.498 (0.104)**		-0.488 (0.129)**
RR ^a	1.645	1.74	4.100	4.557	9.770	10.12
RR 95% CI ^b	(1.233,2.151)	(1.264,2.334)	(2.72,5.983)	(2.987,6.727)	(3.97,20.66)	(4.059,21.540)
N	1136	1136	1188	1188	959	959

Note: Standard error in parentheses; *p < 0.05, **p < 0.01.

Sources: CBS News and *The New York Times* (March), CBS News (December), and CBS News and *The New York Times* (January).

^aMean risk ratio computed from 100,000 simulations of five multiply imputed data sets;

^bmean 95% confidence interval.

One possible explanation for this finding is survivorship bias. Roughly one-fifth of American households reduced their exposure to stocks and five percent completely got out of the stock market (Ameriks et al. 2009). In other words, the “bulls” stayed in the market as the “bears” got out or reduced their exposure to the stock market as the crisis unfolded over the course of the year. Indeed, Ameriks et al. (2009) found that roughly 17% of households *increased* stock holdings during the crisis. I believe two factors mitigate against this explanation. First, as Table 7.25 shows, the proportion of the treated group who believe the stock market is generally risky increased by about 20% from March 2008 to January 2009. This change is not consistent with a greater concentration of confident bulls over the course of the year in the treated group. Second, the percentage of investors in the full samples of these three surveys actually *increased* over this time period (58.6% in March 2008; 60.4% in December 2008; 65.9% in January 2009). This means these findings are not the result of fewer investors in the pool.³⁸ Thus, although I find that investors did not have distinct views about government policies and actions over the course of the 2008 financial crisis, investors were more likely to take a favorable view of the riskiness of the market compared to non-investors. These differences became more stark as the crisis unfolded.

7.11 Summary

I find no effect of stock market participation on attitudes during the unfolding financial crisis in 2008. In many ways, the 2008 crisis was an ideal test of the investor class theory as investors clearly had vested interests in the collapse of the U.S. stock market (Crano 1997a). Moreover, government action in response to the crisis received heightened media attention during the 2008 presidential election. The half dozen surveys used in this chapter provide good coverage of the year as the crisis unfolded in different parts of the economy. Investors

1, “very bad” is coded as 4.

³⁸Moreover, matching with replacement creates matched groups that are weighted to make them equivalent in size in regression analyses.

consistently failed to evince different attitudes from non-investors about the government’s response to the unprecedented crisis in the housing, financial and auto sectors. This is not to suggest that economic concerns did not shape political attitudes. People who rated the economy more poorly were more likely to support government actions across the board to respond to the economic crisis. Sociotropic views explained variance in support for economic stimulus, homeowner bailout, banks bailout, auto bailout, and an active government role.

In order for individual financial interest to affect political behavior, however, investors must see a clear connection between government action and their economic welfare. In other words, investors have to hold the government accountable for what happens in the economy. A December 2008 poll by USA Today and the Gallup Organization asked respondents whether under the best scenario (assuming political leaders made all of the right decisions) government can improve the economy.³⁹ As Table 7.27 below shows, over seventy percent of both treatment groups did not believe the government could make much of an impact on the economy. This arguably reflects the deep American values of economic individualism, the belief that one’s success stems from one’s own efforts and not help from the government (Sniderman and Brody 1977; Feldman 1984).

Table 7.27: Attitudes about how much the government can do to improve the economy, weighted percentages

	Treated <i>N</i> =644	Control <i>N</i> =192
A great deal	5.4	2.2
A fair amount	17.4	25.0
Not much	43.3	38.6
Nothing at all	33.9	34.3

Source: *USA Today* and Gallup, December 2008.

³⁹Regardless of who is in power, if the leaders in the federal government make the right decisions, how much do you think the government can do to improve the economy?

My sole finding is a clear divergence of views regarding the riskiness of the stock market between investors and non-investors. In the first quarter of 2008 there was no discernible difference between investors and non-investors. By early December 2008 investors were four times as likely than non-investors to view stock market as safe; this disparity grew to nearly ten times by mid-January 2009. Clearly where it mattered, investors were able to make clear distinctions about their financial interests. It just did not manifest in their attitudes about economic policies developed in response to the financial crisis.

This poses a puzzle. If investors had divergent views about the stock market compared to non-investors, why didn't this spill over into attitudes about government policies? If investors are more bullish than non-investors, should this attitude manifest regarding economic stimulus or bailout of the financial industry? If it is not investments that explain this difference, then is there some other unobserved variable such as risk tolerance that explains this difference?

These questions can only be answered with more and better data than are currently available. Despite this ambiguity, this chapter provides the clearest evidence of a divergence between investors and non-investors in this dissertation. Data limitations prevent me from making definitive judgments about investor attitudes during financial crises. A key challenge is incomplete data on predictors of financial asset ownership. The analysis particularly would be strengthened by the inclusion of data about financial loss and gain. At minimum, however, the tentative results of this chapter invite more research on this understudied topic.

Chapter 8

Conclusion

“Theories on political behavior are best left to CNN, pollsters, pundits, historians, candidates, political parties, and the voters.” United States Senator Tom Coburn.

8.1 Revisiting the argument

This dissertation critically examines claims about the effects of financial assets on political behavior. The investor class and asset effect theories claim that after people acquire stocks and mutual funds they become more interested and involved in the political process to protect and enhance their economic interests. This in turn leads to changes in their partisanship, participation and political attitudes. Campbell et al. (1960) refer to this mechanism as “primitive self-interest.” This dissertation offers a more nuanced argument, namely that stock market investments shape political behavior through three mechanisms: 1) creation of a vested interest in political actions that may affect the investor’s economic welfare; 2) reduction of the cognitive capacity required to act rationally on this interest; and 3) an increase in the “causal clarity” between individual economic well-being and specific government actions.

These mechanisms collectively help people to act rationally by making clear the connections between their individual economic welfare and the political process. I argue that the structure of the market compels investors to be forward looking. Further, investors place greater value on near-future returns than expected gains far out into distance. This suggests that the effects of assets on behavior should occur in the near-term. However, there is one

important caveat. I contend the effect of assets on behavior is conditionally triggered by financial loss. Neuroscience research and psychological experiments show that people react differently to loss and gain. People feel losses more acutely than equivalent gains. People frequently reinterpret these losses, however, to subjectively improve their welfare (Thaler and Johnson 1990). This asymmetry between gain and loss explains prior findings that investors are more likely to attribute positive returns to their own prowess, but shirk blame for their losses (Feldman 1982). I argue, however, that losses become politicized only when investors believe that the playing field is somehow unfair. In most cases, people do not make the connection between their individual economic welfare and government actions. The empirical evidence in this dissertation supports this argument, finding that financial assets generally did not shape political behavior in the United States or Britain from the early 1980s through the late 2000s.

8.2 Contribution

This dissertation contributes to the self-interest literature, which until now had not considered the potential for “wealth effects” on self-interest. The theory of portfolio politics developed in this dissertation builds upon and extends the self-interest literature to consider the potential for stored-up assets to shape political behavior. This project is among the first to consider financial assets in analysis of economic self-interest and political behavior. This project also contributes to the political economy literature by offering what Kinder and Kiewiet (1981) describe as “an inquiry into the political economy of individual citizens.” Political science previously had not explored the potential impact of stock participation on individual political behavior. This dissertation is an initial step in this direction and invites more work on this topic.

This study also contributes to the participation literature by expanding the conceptualization of resources to include financial assets. Research to date focuses on current income,

which is not always correlated with saved-up assets. This project encourages the study of wealth effects on engagement in the political process. Finally, this project contributes to scholarship about the non-economic effects of asset ownership. It helps to further build this literature by demarcating how specific types of assets—stocks and mutual funds—appear to have little effect on specific types of behavior.

Empirically, this project offers a robust test of existing theories about the relationship between assets and politics. It employs a quasi-experimental research design with longitudinal data covering over twenty-five years. The experimental approach used in this dissertation is akin to a clinical drug trial. By comparison of the treatment effect on both treated and control groups, I offer direct tests of the causal effects of acquisition of stocks or mutual funds on partisanship, participation, or political attitudes. One of the cautionary tales of this project concerns over-reliance on tests of the null hypothesis for causal inference. On numerous occasions my parametric estimates yielded “statistically significant” results. But for the methodological decision to focus on relative risk to ascertain the average treatment effect of financial assets on political behavior, I would have “followed the stars” and claimed causality where none in fact exists.

8.3 Relevance

The quotation at the beginning of this chapter by U.S. Senator Tom Coburn was made in October 2009 to support his proposed amendment to eliminate funding for political science from the National Science Foundation (Coburn 2009). Senator Coburn’s argument concerns the relative value of political science to society. Senator Coburn singled out the American National Election Studies at the University of Michigan for criticism. “The University of Michigan may have some interesting theories about recent elections, but Americans who have an interest in electoral politics can turn to CNN, FOX News, MSNBC, the print media, and a seemingly endless number of political commentators on the internet who pour over this

data and provide a myriad of viewpoints to answer the same questions. There is no shortage of data or analysis in this field that would require the government to provide funding for additional analysis.”

The investor class theory is just the type of theory Senator Coburn argues is better left to pollsters and pundits. In December 2000, John O’Sullivan, Editor-at-large of the conservative opinion magazine *National Review* took credit for his publication’s role in promulgation of the theory: “In recent years National Review has pioneered two of the most fruitful theories of electoral behavior: namely, those based upon the “investor class” and the “impact of immigration” (O’Sullivan 2000). The investor class theory was developed far away from political science departments and peer-reviewed academic journals. It was hyped in political opinion pages and think tank white papers until it reverberated within the halls of Congress and the West Wing of the White House.

Muir (1999) argues that political science needs to weigh in on important policy issues outside of the pages of academic journals. This is particularly important when non-peer reviewed research is used as the basis for important social policy. Although the investor class theory was not used publicly to make the case for Social Security privatization, it clearly motivated for the Bush Administration and the Republican Party to action. The findings of this dissertation reveal, however, that confidence in this theory was misplaced. Contrary to the popular belief that “you become a conservative when you have something to conserve,” this dissertation finds no evidence to support the belief that acquisition of stock market investments has any effect on political actions or beliefs. Political scientists need to shed light on these types of debates to not only demonstrate the value of our science, but also to add expertise where it can make a valuable contribution to society.

8.4 Future research

Although I find no evidence that financial assets play a role in shaping political behavior, these findings are very limited in scope for several reasons. First, the data are limited to the United States and Britain for very specific periods of time. The U.S. data in particular are limited in duration. More robust data sets are needed to draw more definitive conclusions. These findings of course are limited to the United States and Britain. Financial assets may have different effects on behavior in less developed economies. It is also possible that political context may mediate the effects of assets on politics. The U.S. and Britain are both advanced democracies. Financial assets may shape behavior in different ways in other political contexts.

Second, the effects of assets on behavior may vary for different sub-groups. For example, researchers contend there are distinct processes through which gender interacts with socio-economic factors. Due to the different roles of men and women in the home and work place, it is possible that the social processes through which financial assets affect political behavior may be different for men and women (Burns et al. 1997; Scholzman et al. 1999). By extension, assets may also have distinct effects on the political behavior of ethnic, racial and social subgroups. This is an important research question given the well-established finding that women and minorities participate at lower levels in the political process. Finally, more work is needed to fully investigate vested interest. Better data about asset types, asset values, and investment motivations could help to disaggregate investors and define different degrees of vesting.

8.5 An ideal survey

I conclude with an outline of an ideal survey instrument to investigate the effect of financial assets on political behavior. The ideal survey instrument has three features: detailed data about financial assets; a broad range of predictors of financial asset ownership and political

behavior; and, good coverage of questions about political behavior (partisanship, participation and attitudes). Ideally, this survey would be administered to a panel that can be tracked over time. An ideal candidate in the United States is the General Social Survey (GSS), which already asks some political and personal finance questions. The GSS is part of the International Social Survey Program (ISSP), a collaborative international comparative survey research project comprised of 43 countries. Inclusion of these survey questions in the ISSP would provide a rich source of data for comparative analysis.

8.5.1 Financial investments

Stocks

- Do you directly own any stocks?
 - In retirement accounts?
 - Outside of retirement accounts?
 - What is the value of stocks you own in retirement accounts?
 - What is the value of stocks you own outside of retirement accounts?

- Do you own domestic stocks, foreign stocks, or both?
 - In retirement accounts?
 - Outside of retirement accounts?
 - What is the value of each?

- How many companies are represented by the stocks you directly own?
 - In retirement accounts?
 - Outside of retirement accounts?
 - What is the value of each?

- Do you own any small-cap stocks?
 - In retirement accounts?
 - Outside of retirement accounts?
 - What is the value of small-cap stocks you own in retirement accounts?
 - What is the value of small-cap stocks you own outside of retirement accounts?

- Do you own any mid-cap stocks?
 - In retirement accounts?
 - Outside of retirement accounts?
 - What is the value of mid-cap stocks you own in retirement accounts?
 - What is the value of mid-cap stocks you own outside of retirement accounts?

- Do you own any large-cap stocks?
 - In retirement accounts?
 - Outside of retirement accounts?
 - What is the value of large-cap stocks you own in retirement accounts?
 - What is the value of large-cap stocks you own outside of retirement accounts?

- Do you own any “value” stocks?
 - In retirement accounts?
 - Outside of retirement accounts?
 - What is the value of the “value” stocks you own in retirement accounts?
 - What is the value of “value” stocks you own outside of retirement accounts?

- Do you own any “growth” stocks?

- In retirement accounts?
- Outside of retirement accounts?
- What is the value of “growth” stocks you own in retirement accounts?
- What is the value of “growth” stocks you own outside of retirement accounts?

Bonds

- Do you directly own any bonds?
 - In retirement accounts?
 - Outside of retirement accounts?
 - What is the value of bonds you own in retirement accounts?
 - What is the value of bonds you own outside of retirement accounts?
- Do you own domestic bonds, foreign bonds, or both?
 - In retirement accounts?
 - Outside of retirement accounts?
 - What is the value of each?

Stock Mutual Funds

- Do you directly own any stock mutual funds?
 - In retirement accounts?
 - Outside of retirement accounts?
 - What is the value of stock mutual funds you own in retirement accounts?
 - What is the value of stock mutual funds you own outside of retirement accounts?
- Do you own domestic stock mutual funds, foreign stock mutual funds, or both?

- In retirement accounts?
 - Outside of retirement accounts?
 - What is the value of each?
- Are the foreign stock mutual funds that you own in developed markets, emerging markets, or both?
 - What is the value of emerging market foreign stock mutual funds that you own?
 - What is the value of developed market foreign stock mutual funds that you own?
- Do you own any small-cap stock mutual funds?
 - In retirement accounts?
 - Outside of retirement accounts?
 - What is the value of small-cap stock mutual funds you own in retirement accounts?
 - What is the value of small-cap stock mutual funds you own outside of retirement accounts?
- Do you own any mid-cap stock mutual funds?
 - In retirement accounts?
 - Outside of retirement accounts?
 - What is the value of mid-cap stock mutual funds you own in retirement accounts?
 - What is the value of mid-cap stock mutual funds you own outside of retirement accounts?
- Do you own any large-cap stock mutual funds?
 - In retirement accounts?
 - Outside of retirement accounts?

- What is the value of large-cap stock mutual funds you own in retirement accounts?
- What is the value of large-cap stock mutual funds you own outside of retirement accounts?
- Do you own any “value” stock mutual funds?
 - In retirement accounts?
 - Outside of retirement accounts?
 - What is the value of the “value” stock mutual funds you own in retirement accounts?
 - What is the value of “value” stock mutual funds you own outside of retirement accounts?
- Do you own any “growth” stock mutual funds?
 - In retirement accounts?
 - Outside of retirement accounts?
 - What is the value of “growth” stock mutual funds you own in retirement accounts?
 - What is the value of “growth” stock mutual funds you own outside of retirement accounts?
- (Repeat questions for actively managed stock mutual funds and index stock mutual funds.)

Bond Mutual Funds

- Do you directly own any bond mutual funds?
 - In retirement accounts?
 - Outside of retirement accounts?

- What is the value of bond mutual funds you own in retirement accounts?
- What is the value of bond mutual funds you own outside of retirement accounts?

8.5.2 Politics

Partisanship

- Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?
- On a scale with strong Democrats on one end and strong Republicans on the other, and with Independents in the middle, where would you place yourself (Strong Democrat, Democrat, Weak Democrat, Independent, Weak Republican, Republican, Strong Republican)?
- Generally speaking, do you usually think of yourself as a Democrat or Republican?
- No matter how you voted in the last couple of national elections or how you think you might vote in the next national election, do you basically think of yourself as a Republican, a Democrat, an Independent, or what?
- In politics, as of today, do you consider yourself a Republican, a Democrat, an Independent, or what?
- (These questions should be interspersed throughout the survey.)

Participation

- Did you vote in the most recent presidential election?
- Did you vote in the most recent congressional election?
- In the last 12 months, have you had contact with a public official in writing or by telephone?

- In the last 12 months, have you had contact with a public official in person?
- In the last 12 months, have you made a financial contribution to a political candidate?
- In the last 12 months, have you made a financial contribution to a political party?
- In the last 12 months, have you made a financial contribution to a political action group?
- In the last 12 months, have you volunteered for a political candidate?
- In the last 12 months, have you volunteered for a political party?
- In the last 12 months, have you participated in political protest?
- In the last 12 months, how would you describe your activity in a political party (a lot, some, a little, none)?
- In the last 12 months, how many hours per month have you spent in activity with a political party?
- Are you a registered member of a political party?
 - Which political party?
 - Length of membership in political party?

Attitudes

- Level of interest in politics (very interested, fairly interested, not very interested, not at all interested)?
- The free market can handle complex economic problems without government help (strongly agree, agree, neither agree nor disagree, disagree, strongly disagree)?

- Private enterprise solves economic problems (strongly agree, agree, neither agree nor disagree, disagree, strongly disagree)?
- Public services ought to be state-owned (strongly agree, agree, neither agree nor disagree, disagree, strongly disagree)?
- The government should redistribute income from the wealthy to the poor (strongly agree, agree, neither agree nor disagree, disagree, strongly disagree)?
- Ordinary people don't get a fair share of the nation's wealth (strongly agree, agree, neither agree nor disagree, disagree, strongly disagree)?
- Big business benefits owners at the expense of workers (strongly agree, agree, neither agree nor disagree, disagree, strongly disagree)?
- Society do whatever is necessary to make sure an equal opportunity for all to succeed (strongly agree, agree, neither agree nor disagree, disagree, strongly disagree)?
- The government should guarantee jobs and standard of living (strongly agree, agree, neither agree nor disagree, disagree, strongly disagree)?
- The government ought to set a limit on earnings (strongly agree, agree, neither agree nor disagree, disagree, strongly disagree)?

8.5.3 Demographics

- What is your gender?
- What is your age?
- What is your annual household income?
- What is your education level (less than high school, high school, some college and/or technical training, undergraduate degree, advanced degree)?

- What is your race or ethnic background?
- What is your occupation?
- What is your employment status (part-time, full-time, unemployed, student, homemaker, retired)?
- On average, how many hours a week are you employed?
- What is your marital status (single, married, separated, divorced, widowed)?
- How many children do you have under the age of 18 years?
- Do you own or rent your home (or neither)?
 - If homeowner: What is the value of your home?
- Do you own your own business? If so, how many employees do you have?
 - If business owner: What is the value of your business?
- Do you have a mortgage?
 - If so, how much do you owe on your mortgage?
- What is your total non-mortgage debt?
- Do you have any serious health problems?
 - If married: Does your spouse have any serious health problems?
 - If dependent children: Do your children have any serious health problems?
- What is your estimated net worth (financial and real assets less debt)?
- How much do you know about investment options (a great deal, some, not much, nothing)?

- How would you describe your interest in personal finance and investing (a great deal, some, not much, none)?
- Were your parents invested in the stock market when you were a child?
- On a scale of one to ten, how many of your friends and family are also invested in the stock market?
- How often do you discuss the stock market with your friends and family (a great deal, some, not much, never)?
- How often do you attend religious services (more than once a week, once a week, more than once a month, once a month, a few times a year, never)?
- Which level of risk are you willing to take when saving or making investments: substantial risk expecting to earn substantial returns, above average risk to earn above average returns, average risk expecting to earn average returns, or not willing to take any financial risks?

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