NBER WORKING PAPER SERIES

EDUCATION AND SAVING: THE LONG-TERM EFFECTS OF HIGH SCHOOL FINANCIAL CURRICULUM MANDATES

B. Douglas Bernheim
Daniel M. Garrett
Dean M. Maki

Working Paper 6085

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 1997

We are grateful to the National Science Foundation (grant number SBR94-09043 to the National Bureau of Economic Research and SBR95-11321), Merrill Lynch & Co., Inc., and an anonymous private foundation for financial support, and to Merrill Lynch & Co., Inc., for sponsoring the collection of the data on which this study is based. We would like to thank Peter Brady, Martha Starr-McCluer, John Pencavel, Jim Poterba, and Jonathan Skinner for helpful comments, Heather Nevin for excellent research assistance, and Hayden Green for valuable conversations concerning the consumer education movement. The views expressed in this paper are those of the authors and do not necessarily reflect the positions of any organization including the National Bureau of Economic Research. This paper is part of NBER's research programs in Aging and Public Economics.

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NBER Working Paper No. 6085
July 1997
JEL Nos. D12, E21, H31
Aging and Public Economics

ABSTRACT

Over the last forty years, the majority of states have adopted consumer education policies, and a sizable minority have specifically mandated that high school students receive instruction on topics related to household financial decision-making (budgeting, credit management, saving and investment, and so forth). In this paper, we attempt to determine whether the curricula arising from these mandates have had any discernable effect on adult decisions regarding saving. Using a unique household survey, we exploit the variation in requirements both across states and over time to identify the effects of interest. The evidence indicates that mandates have significantly raised both exposure to financial curricula and subsequent asset accumulation once exposed students reached adulthood. These effects appear to have been gradual rather than immediate -- a probable reflection of implementation lags.

B. Douglas Bernheim
Department of Economics
Stanford University
Stanford, CA 94305
and NBER
bernheim@leland.stanford.edu

Daniel M. Garrett
Cornerstone Research
1000 El Camino Real
Menlo Park, CA 94025

Dean M. Maki
Mail Stop 41
Federal Reserve Board
20th and C Streets, NW
Washington, DC 20551

1. Introduction

Between 1957 and 1985, 29 states adopted legislation mandating some form of "consumer" education in secondary schools. In 14 cases, these measures specifically required coverage of topics relevant to household financial decision-making, from budgeting, credit management, and balancing checkbooks to compound interest and other investment principles.[1] The objective of these curriculum mandates was to equip students with practical decision-making skills that would prove useful in their adult lives.[2]

Should one expect high school consumer/financial curriculum mandates to be effective in achieving this objective? There are reasons for both optimism and pessimism. On the one hand, there is some evidence that poor financial decisions frequently result, at least to some degree, from a failure to appreciate economic vulnerabilities (Bernheim, 1994, 1995). Education could alleviate this problem by contributing pertinent knowledge and/or specific decision-making skills. Early financial education may also increase comfort and familiarity with financial matters, thereby removing psychological barriers that impede proper decision-making. By way of analogy, the early acquisition of computer literacy appears to have a long-term effect on an individual's general level of comfort with computers. On the other hand, long-run effects of targeted educational initiatives have been difficult to detect in other contexts.[3] Broad mandates may have little real influence on curriculum content, classroom treatment of critical financial topics may be cursory, and/or students may fail to take the material seriously. Of course, all of these considerations, both positive and negative, are speculative. Virtually nothing is known about the long-term

[1]See Alexander (1979), Highsmith (1989), and Scott (1990) for a detailed description of these mandates. A more concise summary is presented in section 2 of this paper.

[2]For example, legislation in the state of Georgia states: "Each citizen should have the skills and knowledge to be an informed consumer in order to use available resources in an efficient and beneficial manner" (Alexander, 1989).

[3]The Head Start program is a good example; see e.g. the discussion in Currie and Thomas (1995).

behavioral effects of financial education in secondary schools.[4]

In this paper, we attempt to determine whether the curricula arising from high school financial education mandates have had any discernable effect on adult financial decision-making. Obviously, this issue has many dimensions, including the adequacy of saving, the appropriate management of credit, and prudent portfolio allocation. While we are interested in all of these dimensions, we focus here only on the first (the adequacy of saving). This focus is motivated in part by the widespread (though certainly not universal) concern that the typical American saves too little (see e.g. Bernheim, 1991, 1996a). Rather than develop a formal model to measure the adequacy of saving, we simply ask whether exposure to a high school financial curriculum leads to higher saving as an adult.

Our analysis is based on a unique cross-sectional household survey fielded in the Fall of 1995, which, among other things, solicited the state in which the respondent attended high school, as well as recollections concerning exposure to financial education. In attempting to detect the effects of curriculum mandates, we are aided by the fact that some states never adopted these programs, and that the others adopted them at different times. Our analysis is based on comparisons both across states and over time -- using the non-adopting states as a benchmark, we investigate whether the establishment of a mandate coincided with a departure from this benchmark among the affected cohorts.

We begin our investigation by studying exposure to financial education. There is no indication that mandates are correlated with pre-existing inclination to offer, require, or take financially oriented courses. Students in the typical state that enacted a mandate were no more likely, prior to the mandate, to have been exposed to financial education than were students in the typical state that never enacted a mandate. However, differences in reported exposure rates do appear after mandates are introduced, and increase

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[4]To the extent previous studies have explored the effects of consumer/economic education, they have focused on the short-term, and have usually examined measures of knowledge or attitudes rather than behavior. See e.g. Peterson (1992), Soper and Brenneke (1981), Kohen and Kipps (1979), and Langrehr (1979). One exception is Fast, Vosburgh, and Frisbee (1989), who analyzed the effects of consumer education on consumer search.

steadily with the passage of time. This suggests that the effect of a mandate is gradual, perhaps reflecting lags associated with curriculum development, adoption, and compliance.

Data on self-reported rates of saving mirror this pattern extremely closely: there are no differences (either in levels or age-trends) across states before the adoption of a mandate, but there is a steady and significant divergence after adoption, in the expected direction. A somewhat different pattern is predicted for the accumulation of net wealth; however, the basic predictions (no systematic difference from the benchmark among those too old to have been exposed, significant difference for those young enough to have been exposed) find considerable support in the data. Overall, the evidence suggests that adoption was not systematically related to pre-existing preferences -- that it is probably best regarded as exogenous. Irrespective of that issue, specifications that control for systematic differences across states provide strong indications that mandates not only increased exposure to financial education, but also systematically altered adult behavior by stimulating greater saving.

Although this is, to our knowledge, the first study to examine the effects of secondary school education on adult financial decisions, it is certainly related to several strands of the literature. One pertinent strand is concerned with the evaluation of policies to increase saving. There is an extensive literature on tax incentives, which reaches somewhat ambiguous conclusions.[5] There is also a smaller, but more closely related literature that investigates the relation between saving and education. Much of this is anecdotal and/or inconclusive. For example, although there is some indication that the post-War increase in saving by Japanese households may have been at least partially attributable to an extensive educational and promotional campaign, there are other competing explanations for the same phenomenon (see Bernheim, 1991, or Central Council for Savings Promotion, 1981). Likewise, correlations between an individual's general level of educational attainment and his or her rate of saving (documented by Bernheim and Scholz,

[5]For recent surveys of this literature, see Bernheim (1996b), Hubbard and Skinner (1996), Poterba, Venti, and Wise (1996), and Gale and Scholz (1996).

1993, and by Hubbard, Skinner, and Zeldes, 1995) may be attributable to other related factors, such as rates of time preference. A more promising branch of this literature has focused specifically on the behavioral effects of retirement education in the workplace (Bernheim and Garrett, 1996, Bayer, Bernheim, and Scholz, 1996, Bernheim, 1996c, Clark and Schieber, 1996, and Yakoboski, 1996). Subject to some qualifications, these studies have identified significant effects on saving. The growing body of evidence concerning the effects of education on financial decision-making has recently led the Department of Labor to launch "a national pension education program aimed at drawing the attention of American workers to the importance of taking personal responsibility for their retirement security" (Berg, 1995, p. 2).

The paper is related also to the more general literature on the returns to education. Much of this literature has focused on the relation between wages and either training or schooling. A central issue in this literature concerns the likelihood that both of these variables are related to some underlying unobservable characteristic, such as ability (see e.g. Ashenfelter, 1978, Lalonde, 1986, or Card, 1995). Causal inferences about the effects of education are potentially misleading unless they are derived from sources of variation in education that are plausibly exogenous. Naturally, identical issues arise in the context of the current investigation. In certain respects, our approach is similar in spirit to that of Angrist and Krueger (1991, 1995), who attempt to infer the effects of secondary education from variation in state mandatory schooling laws, which they treat as exogenous. Notably, since financial curriculum mandates vary both across states and over time, our investigation does not require us to make a similar assumption concerning exogeneity. Instead, we investigate whether the imposition of mandates correlates with changes in behavior. This general strategy has been used in a variety of other contexts; for example, Hoxby (1996) identifies the effect of teachers' unionization on educational performance by studying the variation in pertinent state laws.

The paper is organized as follows. Section 2 summarizes the history and content of state financial education mandates. Section 3 describes the household survey data used in our analysis. Section 4 examines the effects of mandates on exposure to financial education. Section 5 studies the effects of mandates on rates of saving and wealth accumulation. Section 6 concludes.

2. State Financial Curriculum Mandates

The financial education mandates analyzed in this paper are a subset of the larger set of consumer education mandates. Consumer education covers a wide range of topics.[6] Alexander (1979) and Scott (1990) refer to four sub-fields of consumer education: Consumer decision-making, economics, personal finance, and consumer rights and responsibilities.

Consumer decision-making includes instruction in assessing the consumer's goals and needs, as well as training in the decision-making skills used to meet the goals and needs. This area often focuses on factors that should be considered in choosing among various goods and services, such as prices, product features, warranties, and operation and repair costs. Consumer decision-making also may cover the analysis of advertising messages and product labels. Economics, the second area of consumer education, typically deals with concepts such as the allocation of scarce resources, the roles of consumers, producers, and government in an economic system, the principles of supply and demand, and the role of prices in a market system. The third area, personal finance, may cover household budgeting, money management, saving and investing, and the use of credit. The saving and investing portion of such a course may offer discussion on various types of financial instruments, the relationship between risk and return, and the role of inflation, taxes and diversification in portfolio choice.[7] Finally, the area of consumer rights and responsibilities offers instruction on consumer protection laws and regulations, along with redress mechanisms available to consumers.

Thirty seven states have policies that encourage or require instruction in consumer education, or have

[6]See Bannister and Monsma (1982) for a complete classification of the various concepts that are included in the field of consumer education.

[7]See National Institute for Consumer Education (1994) for an example of a teaching guide for personal finance education.

had such policies in the past. Some policies merely "encourage" consumer education to be taught. Other states have mandates covering consumer education, although such mandates vary in scope, from requiring state education agencies to develop and distribute materials on consumer education to local districts to imposing requirements at the local level by requiring each district to offer some form of consumer education. Also, a number of states specifically mandate that each student receive instruction in a consumer education course; some of these states require a separate course, while others allow schools to integrate consumer education into existing courses.

The analysis in this paper will focus on the mandates that require all students to receive instruction in consumer education. Because they are universal, these types of mandates seem the most likely to affect behavior. Table 1 summarizes consumer education policies in the 50 states and the District of Columbia. The table indicates whether a state has adopted a consumer education policy, the first graduating class to be affected by the policy, and whether the policy mandates that all public school students receive instruction in consumer education, or specifically in personal finance education. The earliest policy was instituted by Nevada in 1957, and most of the policies were implemented in the 1970s.[8] Students are required to receive consumer education in 29 states, while in 14 states students specifically are required to receive instruction in personal finance.

This paper attempts to measure the effect of mandated consumer education policies on saving and wealth accumulation later in life. A potential problem with assessing the effect of mandates on saving behavior is that mandates may be endogenous, if states whose residents had a high preference for saving were

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[8]For more information on policies for each state, see Alexander (1979), Joint Council on Economic Education (1989) and the National Coalition for Consumer Education (1990). There are some discrepancies between sources, particularly between Alexander (1979) and later sources. These discrepancies may be due to differences in interpretations of survey questions or to policy changes after 1978. When possible, state school administrators were questioned about discrepancies. If discrepancies could not be reconciled, Alexander (1979) was used as the definitive source, on the grounds that more recent sources are less likely to have classified state policies correctly during the time period that is of greatest relevance to this study. There is some remaining ambiguity about the correct classification of Wisconsin, but our results are not particularly sensitive to the reclassification of this state. Policies also may have changed since 1990, but such changes would not affect the results in this paper because affected households would be too young to be included in the sample.

the same states that enacted mandates. This type of endogeneity seems unlikely for several reasons. Consumer education mandates seem to be an outgrowth of the broader consumer movement of the 1960s and 1970s that is most closely associated with Ralph Nader.[9] Few consumer education laws focused mainly on personal finance. More commonly, the laws were directed at all four of the areas mentioned above. Moreover, Mayer (1989) argues for the importance of sound tactical strategy by lobbying groups and the preferences of key legislators as keys for the passage of consumer laws, rather than public opinion more broadly defined.

When public opinion is important to enactment of consumer legislation, it is usually associated with outrage over a particular event that damaged consumers in some way.[10] The importance of lobbying groups as opposed to broad public opinion also was borne out in a survey of chief state school administrators (Scott, 1990). When asked "Which group is most likely to discuss consumer education initiatives?" 41 percent of the school administrators indicated education professionals such as teachers, school administrators, and their staffs; 39 percent indicated the business community; and only 10 percent indicated that parents or students were the most likely to discuss consumer education initiatives. There is little reason to believe that the preferences of politically active education professionals or business leaders are generally representative of household preferences in a given state. Further evidence is provided by Ford (1977), who finds that states that passed consumer education legislation were not statistically significantly different from other states in income, retail sales, or the proportion of residents who graduated from high school. Thus, it seems unlikely that passage of a consumer education policy in a particular state was correlated with the pre-mandate financial behavior of the state's households.[11] Nevertheless, the empirical analysis in this paper controls for

[9]See Bannister (1996) for a history of consumer education in the U.S., and Mayer (1989) for a history of the broader consumer movement.

[10]Conversations with consumer education experts in Illinois indicated that keys to enactment of its consumer education legislation were highly publicized fraud cases as well as the political skill of key legislators.

[11]For additional background on the passage and development of consumer education policies and programs, see Langrehr and Mason (1977), or Herrman (1982).

differences in pre-mandate saving behavior between states where a mandate was imposed and other states to check whether this intuition is borne out statistically.

3. Survey Data

To examine the effects of high school financial curriculum mandates, one requires a household survey data set with several characteristics. First, the data must be recent. Since most workers are on the steepest portions of their earnings trajectories during their 20s and early 30s, one generally observes relatively little saving (and relatively little divergence of saving across groups) prior to the mid-30s. As indicated in table 1, many states did not adopt mandates until the mid-1970s. An individual who was exposed to financial education in 1976 at the age of 16 would not have reached age 35 until 1995. Second, to assess exposure to financial education, the survey should solicit the state in which each individual attended high school. If mobility across states is sufficiently low, then current state of residence might be a reasonable proxy. Direct measures of exposure to financial education (based on appropriately worded questions) are also desirable. Third, the survey must collect information on financial choices (saving and wealth), as well as the usual array of economic and demographic data (e.g. earnings, age, and so forth).

Unfortunately, there is currently no publicly available data set that satisfies these criteria. Consider, for example, the Survey of Consumer Finances (SCF). When this project was conducted, data from the 1992 SCF were available, but the 1995 data had not yet been released. SCF respondents were not asked directly about exposure to financial education, and the survey did not solicit the state in which the respondent attended high school. While state of current residence is recorded in the survey, in practice this information is withheld from the public to ensure confidentiality. Furthermore, the use of current state in place of high school state would yield a poorer measure of exposure to mandates, and would thereby bias our analysis against the finding that mandates affect behavior. Similar problems arise for other data sources, such as the Survey of Income and Program Participation, and the Panel Study of Income Dynamics. Thus, to address the nexus of issues identified in section 1, it was necessary to collect new data.

The first author of this paper has directed an ongoing project, sponsored by Merrill Lynch, Inc., to monitor the adequacy of personal saving through annual household surveys (see Bernheim, 1996a). For the fall of 1995, the survey instrument was expanded to cover several new topics, including high school financial education.[12] Data were collected during the month of November, 1995 from a nationally representative sample of respondents between the ages of 30 and 49. This age-specific focus is appropriate for the current study, since these are the age groups that experienced the transition to financial curriculum mandates in many states. A total of 2,000 surveys were completed.[13] The survey gathered standard economic and demographic information, including household assets and liabilities,[14] earnings of respondent and spouse, total household income, various aspects of pension coverage,[15] employment status, gender, marital status, age, ethnic group, education, and household composition. It also solicited self-reported rates of saving,[16] as well as information on a variety of less standard topics, such as childhood influences of potential relevance to future financial decisions (e.g. parental attitudes toward saving, and the fraction of the respondent's high school class that attended college).

Most importantly, the Merrill Lynch survey specifically asked respondents to identify the state in which they attended high school,[17] and it solicited information concerning exposure to financial education.[18]

[12]The survey was designed in cooperation with the first author of this paper, and fielded for Merrill Lynch by the Luntz Research Companies.

[13]Respondents who terminated their interviews before completion of the survey are not included in this sample.

[14]Respondents were asked separately about the value of financial assets, houses, other real property, business interests, and debt.

[15]Respondents were asked to identify the type of pension (defined benefit, voluntary tax-deferred salary reduction plan, or other defined contribution), and to provide total assets for defined contribution plans.

[16]Each household was asked to estimate the fraction of take-home pay that it saved on its own behalf (not including reinvested capital income or employer contributions to retirement accounts).

[17]If they lived in more than one state during high school, they were asked to name the state in which they lived the longest.

[18]The survey did not elicit similar information concerning spouses.

This information was gathered in three steps. First, respondents were asked if, in high school, they took any courses dealing with household finances, consumer education, or economics. Those who answered "yes" were then asked whether any of these courses specifically covered topics that had to do with household finances, such as the use of budgets, credit, savings accounts, checking accounts, and so forth. Each respondent who answered "yes" to this second question was then asked whether any of these courses were required by his or her school. Issues concerning the accuracy of these recollections are considered below, where pertinent.

One potential problem is that the survey was administered by telephone. While telephone interviews are usually regarded as less reliable than face-to-face interviews, the survey was designed to achieve a high level of compliance and to assure accuracy. Questions were sequenced according to their degree of invasiveness. This permitted interviewers to establish credibility, to place respondents at ease, and to engage them in the survey process. Invasive questions concerning assets and earnings were deferred until later in the survey, and the most innocuous of these (for example, the household's estimated rate of saving) were placed before the most problematic ones (primarily those designed to elicit asset holdings). Bracketed information was solicited from those who were unable or refused to give numerical answers, and provided a basis for making imputations.[19] Response rates were relatively high; for example, 74 percent of respondents provided numerical information on annual earnings, and another 12 percent provided bracketed information.[20]

Tables 2 and 3 contain summary statistics based on the survey data. Since the reliability of telephone interviews is open to question, it is useful to compare these numbers to those drawn from other recognized data sources. For benchmarks, the reader is referred to a study by the Congressional Budget Office (1993), which tabulated statistics for 25-34 and 35-44 year olds, based on the 1989 Survey of Consumer Finances

[19]When an individual reported that the value of a variable fell within a given bracket, we imputed the value by taking the median of the variable for the subset of individuals who provided numerical answers within the same bracket. Within brackets, additional variables (e.g. demographics) had little predictive power, and therefore were not used to improve imputations.

[20]For the remaining 14 percent of the sample, we imputed earnings based on a regression of earnings on a third degree polynomial in age, years of education, gender, ethnicity dummies, marital status, and employment status.

and the 1990 Current Population Survey (see also Bernheim and Garrett, 1996, who compare the 1994 Merrill Lynch survey to these benchmarks). Generally, the data conform reasonably well to these benchmarks. The fraction of respondents who are married and the fraction who are homeowners are a bit high (by perhaps five or six percentage points), but this is understandable given that such households are probably relatively easy to reach by telephone. African Americans are also underrepresented in the survey. Once one accounts for these patterns of oversampling and undersampling, it becomes apparent that median income and wealth are of the right magnitudes. The wealth distribution also exhibits the usual thick upper tail. Wealth and earnings also vary in the appropriate manner across population subgroups.[21] It is particularly notable that the data display the well-documented chasm between the wealth of homeowners and renters. The similarity between these summary statistics and similar statistics based on recognized surveys provides considerable comfort concerning the validity of the Merrill Lynch data.

4. The Impact of Curriculum Requirements on Educational Exposure

It is hard to imagine that curriculum mandates could influence subsequent financial choices unless they increase exposure to financial concepts in the classroom. Yet there is no guarantee that mandates will have this effect. Obviously, if school districts or individual schools require consumer education to begin with, or if the vast majority of students take related courses as electives, then the imposition of a state mandate may add little more than window dressing. Alternatively, schools without consumer-oriented curricula might simply choose not to comply with the mandate. Even if the mandate is truly incremental and binding, its effects may not be immediate. Schools may take time to comply, or may initially comply in some nominal and ineffective way. In that case, one might not expect to observe changes in behavior among the oldest cohorts exposed to a mandate, even if the mandate was ultimately successful in altering financial

[21] In addition to the patterns displayed in the table, measured earnings also vary in the expected manner with education and ethnicity.

choices among younger cohorts.

In table 4, we estimate a series of probit models to study the probability of exposure to various forms of high school consumer education. There are four distinct dependent variables: whether the respondent took a high school course in consumer education (including any courses covering financial or economic topics), whether the respondent took a high school course covering household financial topics, whether the respondent was *required* to take a high school course covering household financial topics, and whether the respondent took, but was not required to take, a high school course covering household financial topics.

It is important to emphasize that our dependent variables are self-reported; hence, they measure *recollections* about exposure to various forms of education, rather than actual exposure. Certainly, some individuals may have forgotten that they have taken such a course, or think they remember taking such a course even when they have not. However, as long as the quality of the respondent's memory is not systematically correlated with the presence of a state mandate, our estimates should still reliably measure the existence and timing of effects on educational exposure, even though it may mismeasure magnitudes.

A high fraction of respondents (42 percent) indicated that they took a high school course in consumer education; 70 percent of those said that financial topics were covered, and 39 percent of this subset characterized the course as a requirement. Thus, just over 11 percent of respondents (39 percent of 70 percent of 42 percent) said that they were required to take courses covering financial topics.

There are two key explanatory variables in these regressions. The first is labeled "exposed to mandate," and the second is "years since mandate." "Exposed to mandate" is set equal to unity if the respondent is young enough to have been affected by the mandate (if any) for the state in which he or she attended high school.[22] If the imposition of a mandate leads to an immediate and substantial increase in

[22]When no other information is available, we assume that the mandate applied to the first class graduating after adoption. For individuals who did not finish high school, it is impossible to know whether they remained in school long enough to have been affected by a mandate. For this analysis, we assume that they were affected. As only 19 observations (less than 6 percent of those "exposed to mandate") fall into this category, the problem of misclassifying high school drop-outs is not very important as a practical matter.

consumer education, then the coefficient of this variable should be positive.[23] "Years since mandate" measures the time elapsed between the imposition of the mandate and the year in which the mandate applied to the respondent. For example, if a state adopted a mandate for 16 year olds in 1970, and if a respondent who attended high school in that state turned 16 in 1974, then "years since mandate" is set equal to 5 (1974 being the fifth year under the mandate). For individuals attending high school prior to the imposition of a mandate or in states without mandates, "years since mandate" is set equal to zero. If the effect of a mandate on educational exposure is gradual (e.g. as schools develop curricula to comply), then the coefficient of this variable should be positive.[24]

On the basis of other anecdotal and institutional information, we would expect the effects of mandates to be gradual rather than immediate. From conversations with consumer education activists, we have learned that implementation lags have been common, and have often resulted from the lack of qualified teachers. The state of Illinois ran teacher workshops for several years after legislators passed its mandate. Illinois did not even issue consumer education guidelines until more than a year after adoption (Metcalf and Wetherington, 1969); it then set out to select pilot schools, with the object of developing and assessing model programs (Johnston, 1969). This process suggests that, in practice, implementation lags may be protracted.

Both "exposed to mandate" and "years since mandate" are defined differently for different dependent variables. For "consumer education," we use generic consumer education mandates (including financial education); for "financial education," we use specific financial education mandates.

Based on their birth years and high school states, we find that roughly 15 percent of our respondents were exposed to general consumer education mandates, and that 10 percent were exposed to financial

[23]To the extent we sometimes incorrectly identify the first class affected (see previous footnote), the measured affect may appear to be somewhat gradual, even if the real effect is instantaneous. However, the mandate would still achieve something close to its maximal measured effect in short order (within a couple of years).

[24]The most natural relation between educational exposure and "years since mandate" would be non-linear. However, since most of the individuals in this sample were educated within five or six years of a mandate, the relation may be approximately linear over the relevant range. In practice, a linear function adequately summarized the patterns in the data.

education mandates. Of those exposed to general consumer education mandates, roughly 44 percent were exposed within the first three years after the imposition of the mandate, roughly 30 percent were exposed within the next three years, and roughly 26 percent were exposed more than six years after the mandate. The figures for exposure to financial mandates are similar (43 percent, 26 percent, and 31 percent, respectively).

Notably, similar fractions of respondents (11 percent and 10 percent, respectively) say they were required to take courses covering financial topics, and were exposed to financial education mandates. The correlation between these two variables is, however, imperfect. Of those not exposed to financial education mandates, 28 percent said that they took courses covering financial topics, and 10 percent said that they were required to do so; of those exposed to financial education mandates, 43 percent said that they took courses covering financial topics, and 21 percent said that they were required to do so. Likewise, of those not exposed to general consumer education mandates, 41 percent said that they took consumer education courses, while of those exposed to general consumer education mandates, 51 percent said that they took such courses.

For each of our four dependent variables, we estimate two models, which are distinguished by the inclusion or exclusion of state-specific constants. Here, "state" refers to the state in which the respondent attended high school, rather than the current state of residence.[25] When state-specific constants are omitted, we include a dummy variable indicating whether the state *ever* imposed a mandate (regardless of whether the particular respondent was affected by it). In both specifications, our object is to remove spurious correlation between mandates and educational exposure. Such correlations might arise if mandates were more (or less) likely to be adopted in states where financial education was more common to begin with. In effect, we identify the effects of mandates by asking whether the change in exposure for states that adopted mandates (measured by comparing individuals who were young enough to be affected with individuals who were too old to be affected) was larger than the change in exposure for states that did not adopt mandates.

[25]Current state is the same as high school state for roughly 70 percent of respondents. The inclusion of dummy variables for state of current residence (in addition to high school state) has very little effect on the results – we omit the associated estimates for the sake of brevity.

Note that the number of observations varies from specification to specification in table 4. This occurs for two reasons. First, response rates differ for each of our dependent variables. Second, for each dependent variable, there are a few state constants that predict responses perfectly; hence, the inclusion of state constants necessitates the exclusion of the corresponding observations.

To facilitate the interpretation of our results, we report probit coefficients that are transformed to measure the effects of the explanatory variables on the probability of educational exposure, evaluated at sample means. In particular, for continuous variables, we report the derivative of the probability of exposure; for dummy variables, we report the discrete change in probability associated with a discrete change in the variable. Our results indicate that the effects of mandates have been significant, but gradual rather than immediate. In particular, the "exposed to mandate" coefficients are uniformly small, usually negative, and statistically insignificant. This indicates the absence of an immediate impact on educational exposure. However, in equations (1) through (6), the coefficients of "years since mandate" are positive, and generally significant at very high levels of confidence. Thus, over time, mandates appear to have significantly increased the fraction of individuals taking generic consumer education courses, as well as the fraction taking courses covering household financial topics. The coefficients of "years since mandate" are estimated with the greatest precision when the dependent variable indicates whether the individual was *required* to take a course covering financial topics. This provides us with some reassurance that recollections about requirements are reasonably reliable. Likewise, the corresponding coefficients are essentially zero in equations (7) and (8), which explain whether the respondent took, but was not required to take such a course.

Actually, it is somewhat surprising that mandates do not *reduce* the fraction of individuals who say that they took, but were not required to take, courses covering financial topics. Obviously, if the mandate was universally effective, it would reduce this fraction to zero. There are at least two possible explanations for the absence of significant negative coefficients. First, some respondents may recollect incorrectly that they took a course as an elective, even though it was required. Second, if students are given a variety of choices for fulfilling a curriculum requirement, they may regard their particular choice as an elective.

It is particularly interesting to note that, in the odd-numbered equations (those omitting state dummies), there is essentially no effect associated with the dummy variables that measure whether the respondent's high school state ever imposed a mandate. The significance of this finding is perhaps best-illustrated through an example. Imagine that state A imposed a mandate in 1970, while state B never imposed a mandate. Respondents W and X both attended high school in state A. However, respondent W is 35 years old in 1995 (and was therefore exposed to the mandate), while respondent X is 45 (and was therefore not exposed). Respondents Y and Z both attended high school in state B; Y is 35 years old in 1995, while Z is 45. In essence, we have found that X and Z (neither of whom were affected by mandates) were equally likely to have taken pertinent courses. That is, before adoption of the mandate in state A, exposure to consumer education was about the same as in state B. However, we have also found that respondent W was considerably more likely to have taken pertinent courses than respondent Y. Thus, educational exposure in the two states did not diverge until after one of them adopted a mandate -- and then it did so gradually.

The preceding finding suggests that mandates were no more (or less) likely to be adopted in states where consumer/financial education was already common. This is consistent with the view that mandates arise from political activism on the part of narrow interest groups, or from the idiosyncratic interests and concerns of legislators, rather than from a political consensus that reflects the preexisting tastes and inclinations of the general populace (recall the discussion of exogeneity in section 2). To investigate this hypothesis further, we reestimated our specifications with one additional variable: "years before mandate" (the definition of which is symmetric to that of "years after mandate"). The coefficients of this variable were usually small and uniformly insignificant at conventional levels of statistical confidence. Thus, there is no indication that school districts were increasingly requiring these kinds of courses, or that students were increasingly enrolling in pertinent elective courses, prior to the adoption of a state mandate. Since a change in public attitudes would most likely show up first in district-level mandates and voluntary enrollments, and only later (after a delay) in state-wide legislation, this finding provides considerable reassurance that the imposition of a mandate is exogenous with respect to the general public's interest in financial education.

Other findings are also of interest. Women and African Americans were far more likely to receive consumer/financial education. The first of these patterns may reflect the gender-specific incidence of courses in "home economics," while the second suggests that there may have been greater emphasis on practical skills at schools serving lower income and/or African American communities. Aside from African Americans, there appears to be little difference in exposure to financial education between whites and other minorities. The negative coefficients for age, though generally insignificant, hint that consumer/financial education may be on the rise generally, independent of mandates (see especially equations (3) and (4)). Alternatively, older respondents may simply be more likely to forget that they have taken these courses. Finally, the respondent's assessment of his or her parent's degree of frugality is unrelated to educational exposure, including (somewhat surprisingly) electives.[26]

5. The Impact of Financial Curriculum Requirements on Adult Behavior

In this section, we study the relations between state financial education mandates and measures of adult financial behavior, including self-reported rates of saving (a flow variable) and accumulated net worth (a stock variable). Results are contained in tables 5 and 6. Before discussing specific findings, it is important to address three general issues.

The first general issue concerns the choice of estimation technique. As is well-known, the distributions of wealth and self-reported rates of saving are highly skewed. Since it is not at all clear that one should be interested in studying the means of these distributions (which tend to be driven by the upper tails),

[26]For these regressions, "frugal parents" is a dummy variable, and is set equal to unity if the respondent reported that his or her parents were above-average savers. The fractions of the sample classifying their parents as above average savers, average savers, and below average savers were, respectively, 30 percent, 32 percent, and 35 percent (a small fraction of respondents declined to answer the question). Although parents' frugality plays no significant role in the regressions of table 4, it is highly correlated both with current saving, and with other pertinent childhood experiences (such as ownership of a bank account).

standard OLS regression is not a particularly attractive procedure.[27] In this paper, we use two alternative techniques. The first is median regression (with bootstrapped standard errors). By studying medians, one describes financial behavior at the center of the population distribution, rather than in the upper tail. For the second technique, dependent variables are converted to population percentiles (equivalently, population ranks) before fitting OLS regressions.[28] The coefficients in the resulting equations are robust with respect to outliers, and they have natural interpretations: they describe the effects of changes in the independent variables on the respondent's position in the distribution of the dependent variable. There are, of course, other techniques for dealing with the skewness of the distributions for accumulated assets and rates of saving. One alternative is to use iterative "robust" regression techniques that weight observations based on absolute deviations obtained from the previous iteration's regression. This procedure generally yields results similar to those presented in the text.[29]

A second general issue concerns the choice of independent variables. Specifically, when estimating behavioral equations, which variables should we use to summarize exposure to financial education? In tables 5 and 6, we opt for variables that are based on high school state, year of birth, and the timing of state mandates ("mandate" variables). There are at least two other alternatives: (1) use self-reported information on educational exposure, (2) use self-reported information on educational exposure, instrumenting with the

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[27]In practice, OLS estimates are often qualitatively similar to those presented in subsequent sections, but in many cases they are simply too imprecise to support reliable inferences.

[28]This amounts to taking a specific nonlinear monotonic transformation of the dependent variable. One could, of course, take other nonlinear monotonic transformations that similarly condense the tail of the distribution. As an example, one could take the log of the dependent variable. The selection of a particular non-linear function is, of necessity, somewhat arbitrary (unless, for example, one knows on the basis of other information that the error term is log normal). We prefer percentile ranks in part because the resulting coefficient estimates have a natural interpretation. For logs, one has the additional complication that some values of the dependent variables are zero or negative.

[29]Another approach would be to truncate the dependent variable (i.e. "trim" the upper tail), and estimate a tobit model to correct for censoring. There are, however, several problems with this approach. First, the truncation point is arbitrary. Second, the underlying statistical justification is, at best, murky: presumably, one has in mind that the center of the distribution and the upper tail are generated by different processes, but this reasoning is informal. Third, it is not clear why one is intrinsically interested in the mean of the truncated distribution. We have, nevertheless, explored the use of this procedure. Provided that one sets the truncation point at some moderate multiple of the median, one obtains results similar to those presented in subsequent sections (indeed, they are often somewhat stronger).

mandate variables. Both of these alternatives strike us as problematic.

Consider first the use of self-reported information on educational exposure.[30] In the absence of mandates, the decision to enroll in a financial education course is endogenous, and potentially related to underlying tastes and/or interests. Consequently, correlations between financial behavior and exposure to pertinent curricula may be spurious. Even if one uses self-reported information on course *requirements*, similar problems may arise. Requirements at the school or district level may reflect the preferences and inclinations of those who live in the local community, including the students' parents. There is also some indication in the data that respondents' memories of high school courses are selective or otherwise imperfect. A spurious correlation between self-reported requirements and behavior could arise if, for example, individuals with greater interest in financial topics are more likely to remember financially oriented courses.

As we have discussed, the imposition of a state financial curriculum mandate does not appear to be driven by changes in the tastes or interests of the general populace -- for our purposes, these mandates are plausibly exogenous. This suggests that one might attempt to estimate equations explaining behavior as a function of self-reported exposure and/or requirements, instrumenting with the mandate variables. There are several problems with this approach. First, courses are not homogeneous. If schools gradually develop and improve curricula subsequent to the imposition of a mandate, then the quality of courses may improve over time. If so, then variables such as "years since mandate" may proxy for quality. If this variable is used only to predict enrollment, then potentially important information is lost. Second, self-reported measures of exposure are probably subject to non-classical measurement error. For example, individuals are probably more likely to forget courses they took than falsely remember courses they didn't take. In such cases, even instrumental variables estimators may be subject to bias.[31]

[30]Results based on self-reported educational exposure are in most cases similar to those discussed in sections 6 and 7, but are not reported due to the concerns mentioned in the text.

[31]In general, instrumental variable results are inconclusive, in that the key coefficients tend to have large standard errors.

By estimating equations that explain financial behavior directly as a function of the mandate variables, we avoid the various problems that plague these alternative approaches. In essence, our strategy is to search for direct evidence that state financial curriculum mandates affected (or did not affect) the adult behavior of those who were subject to the mandates.

The third general issue concerns policy focus. In particular, when studying behavior, which kinds of curriculum mandates should we consider? Throughout this section, we study the behavioral effects of policies mandating that students actually take courses covering financial topics. Other kinds of consumer education courses are not directly related to saving and investment choices, and therefore should not be expected to affect behavior in these areas. Not surprisingly, results based on a broader class of consumer education mandates (omitted) are significantly weaker than those presented here. Unfortunately, the data are not sufficiently rich to distinguish between the effects of different kinds of policies at a finer level.

A. Rates of Saving

The use of self-reported saving rates raises important issues of interpretation. To some extent, these rates are suspect because they do not necessarily reflect the consistent application of appropriate economic principles. For example, one individual may think of saving as the portion of his paycheck (take-home pay) that he refrains from spending, while another may count her employer's contribution to her defined contribution pension account, or even some portion of reinvested capital income. We have attempted to minimize this problem by stating the survey question in a way that defines saving as unspent take-home pay plus voluntary deferrals (e.g. employee contributions to 401(k)s). Certainly, this is not the only conceivable measure (and perhaps not even the best measure) of saving; however, we suspect that it is the most intuitive notion of flow saving for most respondents.

Notably, response rates to questions about rates of saving are extremely high (roughly 95 percent), particularly relative to questions about assets. This probably results from a combination of factors.

Presumably, most individuals regard questions about saving rates as less invasive than questions about dollar amounts. In addition, survey evidence suggests that -- for their own decision-making purposes -- most individuals already think about saving as a fraction of income, rather than as a dollar amount (Bernheim, 1995). One practical consequence is that our saving rate regressions use a much larger fraction of the data sample than our net worth regressions.

The median self-reported rate of saving is 10 percent, and the interquartile range runs from 3 percent to 15 percent. Roughly 17 percent of respondents say that they save nothing, and no respondent reports a negative rate ("dissaving"). Though some households undoubtably dissave in the broad economic sense, the truncation of the observed distribution at zero is consistent with the notion that this variable measures unspent take-home pay plus voluntary deferrals. Roughly 4 percent of the sample reports saving more than 30 percent of earnings, and nine respondents report rates of saving greater than 50 percent. The highest reported rate of saving in the sample is 75 percent. Based on asset accumulation profiles (e.g. Bernheim, Lemke, and Scholz, 1997), one would tend to regard this distribution as a bit on the high side, and one might therefore be inclined to regard the associated cardinal information with a grain of salt. Nevertheless, at a minimum, the data do appear to contain meaningful ordinal information. The correlation between self-reported rates of saving and net worth (where available) is highly statistically significant, and these rates exhibit the expected correlations with variables such as 401(k) eligibility and education, even controlling for wealth (see Bernheim and Garrett, 1996). Finally, absent either a true panel data or a detailed log of household spending, self-reported rates of saving are the only available measures of flow saving.

Results for saving rates (in percentages) are contained in table 5. For the most part, these regressions are designed to measure the effects of "years since mandate." To remove systematic differences between the circumstances of individuals from states with and without mandates (including differences arising from variation in other public policies), we control for whether the respondent's state ever imposed a mandate, as well as for a short list of critical socio-economic characteristics. With two exceptions, these regressions do not control for "exposed to mandate." In figure 1, we motivate this practice by illustrating the likely effect of a financial curriculum mandate on rates of saving. The figure assumes that mandates are effective and that education increases saving. In the figure, we plot hypothetical cross-sectional data on rates of saving against birth year. The figure includes two "benchmark" cases: the cross-sectional savings rate profile for individuals who were not exposed to financial education, and the cross-sectional savings rate profile for individuals who were exposed. As we have drawn the figure, both lines slope slightly downwards, reflecting the empirical tendency for rates of saving to increase slowly with age.[32]

Now suppose that a state imposed an educational mandate at a particular time. For simplicity, suppose that no student received financial education prior to the mandate. What kind of cross-sectional saving rate profile would one expect to observe? For those who are too old to have been exposed to the mandate, the profile will coincide with the lower benchmark. For those who are young enough to have been exposed to the mandate, average rates of saving will be higher. However, one would not expect average rates of saving to jump immediately to the higher benchmark, for two reasons: first, as we have seen in section 4, the likelihood of exposure to financial education increased gradually, rather than discontinuously, after the imposition of mandates; second, as discussed above, curriculum quality may have improved gradually subsequent to the imposition of a mandate. Consequently, one would expect to observe a smooth departure from the lower benchmark, and a gradual convergence to the higher benchmark, with "years since mandate" (see the curve labeled "transition path" in the figure). To the extent we control properly for "years since mandate," there is no reason to expect a separate effect from "exposed to mandate." As we will see, the data are consistent with this prediction.

The first two columns of table 5 summarize our basic findings. Note that the coefficient of "years since mandate" is positive and statistically significant in both the median regression and the percentile rank regression. From equation (1), one infers that (self-reported) saving rates were roughly 1.5 percentage points

[32]This assumed tendency is consistent with the estimates reported later in this section.

higher for those entering the affected high school grade five years after the imposition of a mandate, than for those who were not exposed to a mandate. From equation (2), one infers that the saving rates of individuals from the first group tend to be roughly 4.75 percentage points higher in the population distribution than the saving rates of individuals in the second group.

The inclusion of "state ever imposed mandate" implies that the central finding (above) is, in effect, based on differences in behavioral changes.[33] Our strategy is akin to taking "differences-in-differences," except that we allow the magnitude of this second difference to depend upon the amount of time elapsed since adoption of the mandate. Notably, however, the coefficients of "state ever imposed mandate" are statistically insignificant. This implies that there are no significant, systematic differences between the saving rates of individuals who attended high school in states that never imposed mandates, and the saving rates of individuals of the same ages who attended high school in states that eventually imposed mandates, but who were too old to be affected. In other words, systematic differences in saving rates across states do not appear until after mandates are imposed. As in section 4, we have refined these estimates by also including a variable measuring "years before mandate" (not included in the table), and its coefficient is consistently insignificant. These findings provide additional reassurance that the imposition of a mandate is not correlated either with generally prevailing inclinations to save within a state, or with pre-existing trends in these inclinations. In other words, though our analysis is akin to taking differences-in-differences, there is no indication that simple differences (between exposed and unexposed individuals of similar ages) are misleading.

Other findings in columns (1) and (2) are unsurprising. Self-reported rates of saving rise significantly with education and earnings. There is some evidence that married couples save at higher rates than single individuals, and that rates of saving rise moderately with age.

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[33]The difference in saving rates between two individuals attending high school in the same state, one before and the other after the mandate, is compared to the difference in saving rates between two individuals in another state that never imposed a mandate.

Equations (3) and (4) of table 5 are identical to (1) and (2), except that they also include the "frugal parents" dummy variable discussed in section 4, which is set equal to unity if the respondent characterized his or her parents as having saved more than average.[34] While the inclusion of this variable does not alter any of our central findings, its coefficients are economically large and highly significant statistically. This pattern is consistent with the view that many individuals acquire some of their attitudes towards saving as children, from their parents. It also raises the following intriguing possibility: if the children of frugal parents save more because they learn the basic elements of household saving at home, then the impact of high school financial education may vary significantly and systematically across the population. In particular, one might expect to find a much smaller effect on the behavior of individuals who had frugal parents than on the behavior of individuals whose parents were not frugal, since the formal educational curriculum was more likely to be redundant for the former than for the latter.

We investigate this possibility in equations (5) and (6), where we omit "years since mandate," substituting interactions between "years since mandate" and two dummy variables, one indicating whether the respondent's parents saved more than average ("frugal parents"), and another indicating whether the respondent's parents were average or below average savers ("parents not frugal"). The results strongly support the prediction outlined in the preceding paragraph. The measured effects of curriculum mandates on respondents whose parents were not frugal are substantially larger than the "average" effects measured in equations (1) through (4). Moreover, there is no indication that the children of frugal parents materially altered their behavior in response to curriculum mandates.

The final four columns of table 5 investigate the robustness of our results with respect to changes in specification. For equations (7) and (8), we add the variable "exposed to mandate" to test whether the empirical relationship actually has the shape depicted in figure 1. As expected, the estimated coefficients for

[34]As mentioned previously, 30 percent of respondents placed their parents in this category.

this variable are statistically insignificant.[35] The coefficients of the key explanatory variable (years since mandate * parents not frugal) change relatively little, and despite some loss of precision, they remain statistically significant at conventional levels.

For equations (9) and (10), we add a collection of dummy variables indicating the state in which the respondent attended high school. This, of course, requires us to remove "state ever imposed mandate." The key coefficients (of years since mandate * parents not frugal) are not greatly changed, though once again precision is sacrificed. Still, the associated coefficient is significant at the 93 percent level in the median regression, and at the 94 percent level in the percentile rank regression. Notably, the state constants are jointly insignificant for both equations.[36]

We have performed a variety of other robustness checks that are not reported in this table. Our central findings are not affected by the inclusion of a wider range of socio-economic controls (e.g. ethnicity, gender of single individuals, employment status, etc.), higher order terms in age and earnings, or dummy variables for state of current residence (as opposed to high school state). Since "years since mandate" is correlated with age, we are particularly reassured by the robustness of our results with respect to the inclusion of higher order terms in age. We looked for possible interactions between financial education and other variables such as gender and income, but did not detect any robust patterns.

We also attempted to examine in greater detail the functional relation between saving rates and "years since mandate." Based on figure 1, one would expect to observe asymptotic convergence to a new profile, rather than linear divergence from the baseline profile. Indeed, our results become somewhat stronger when we truncate "years since mandate" at ten years. More generally, however, the data are simply not sufficiently informative to reveal much about the shape of the transition path. This is not surprising, in that

[35]This finding suggests that the observed change in behavior are not attributable to high-profile lobbying efforts (which may have raised the visibility of financial issues), or to other public or private sector activities that may have coincided with the adoption of the mandate.

[36]For the median regression, $F(47,1815) = 1.12$; for the percentile rank regression, $F(47,1815) = 1.15$.

only 10 percent of respondents were exposed to mandates, and in that "years since mandate" exceeds 6 years for only about one-quarter of this group. The inclusion of higher order terms in "years since mandate" generally renders the individual coefficients statistically insignificant, so that one cannot learn much about curvature. Similarly, if one partitions the set of exposed respondents into three groups (exposed within 3 years of mandate, exposed within 4 to 6 years of mandate, and exposed more than 6 years after mandate), the individual coefficients give the appearance of an approximately linear relationship, but their standard errors are too large to draw this inference with much confidence.[37]

B. Net Worth

Table 6 contains results for statistical specifications explaining net worth. Before discussing specific results, several general comments are in order.

First, response rates are significantly lower for net worth than for rates of saving. Only 55 percent of the sample gave sufficiently complete answers to the full array of questions on assets to permit construction of net worth. For this reason alone, one might not expect to identify the effects of curriculum mandates with the same level of precision as with rates of saving. Low response rates also raise concerns about possible sample selection biases.[38]

Second, the explanatory variable in each of the reported regressions is the ratio of wealth to earnings, rather than the level of wealth. The logic of this choice is that the effects of changes in explanatory variables almost certainly vary with the household's resources; proportionality to earnings is intended as an approximation.[39] The difficulty with this dependent variable is that it is not defined for households with zero

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[37]The coefficients for "more than six years since mandate" are significant in both the median regression and the percentile rank regression, and the coefficient for "4 to 6 years since mandate" is significant in the median regression.

[38]We have made no attempt to correct for potential sample selection bias. In large part, this reflects our inability to identify a variable that is plausibly related to the inclination to report wealth, but unrelated to the inclination to accumulate wealth.

[39]One apparently natural alternative would be to use the log of net worth. Unfortunately, this raises further difficulties, since net worth is either zero or negative for a non-trivial fraction of the sample.

earnings. Moreover, low values of earnings generate extreme outliers, as well as extreme sensitivity to small changes in earnings. We handle these problems by dropping observations for which earnings fall below $20,000 (40 percent of median household earnings). Failure to impose any earnings threshold inflates our standard errors substantially; however, our results are not particularly sensitive to smaller variations in the specific value of the threshold.

Third, our specifications for net worth (table 6) differ from our specifications for saving rates (table 5) in that we typically control for "exposed to mandate," rather than "years since mandate." Figure 2 motivates this choice. In the figure, we plot hypothetical cross-sectional data on accumulated wealth against birth year. As in figure 1, we include two "benchmark" cases, as well as a transition path. The analysis is essentially the same as in the case of saving rates, with one important exception. For accumulated wealth, one expects to find cross-sectional profiles that are rather steeply downward sloped, and that converge towards zero for respondents with relatively recent birth years. This implies that the two benchmark cases tend to converge towards the right hand side of the diagram.

What does this imply for our estimation strategy? Once again, our estimator amounts to comparing the transition path with the lower benchmark. Because the benchmarks converge towards the right side of the figure, we do *not* expect the divergence between these lines to increase monotonically with "years since mandate." The relation is necessarily more complex. In light of this prediction, one must either specify a flexible functional form for the effects of "years since mandate," or settle for measuring the average effect on those who were young enough to be affected by the mandate (i.e. by controlling for "exposed to mandate"). For reasons discussed below, we take the latter approach.

The first two columns of table 6 summarize our basic findings. Note that the coefficient of "exposed to mandate" is positive and statistically significant in both the median regression and the percentile rank regression. From equation (1), one infers that net worth is higher by roughly one-year's worth of earnings for the typical individual who was exposed to a mandate. From equation (2), one infers that the net-worth-to-earnings ratios of those who were exposed to mandates are roughly 9 percentage points higher in the population distribution than the net-worth-to-earnings ratios of those who were not exposed. As in table 5, the coefficients of "state ever imposed mandate" are insignificant, indicating that, prior to the imposition of mandates, there are generally no systematic difference between states that eventually imposed mandates, and states that did not.

Other findings in columns (1) and (2) are not surprising. Unlike saving rates, the ratio of wealth-to-earnings does not appear to rise sharply with education. This discrepancy may, however, be attributable to fact that college educated individuals have saved at higher rates for fewer years -- significant differences in accumulated wealth based on education may show up for older workers. Wealth also increases much more sharply with age than does the rate of saving, but this discrepancy is an inevitable consequence of the accumulation process. Like rates of saving, the ratio of wealth-to-earnings appears to rise with earnings.

In equations (3) and (4), we add our control for frugal parents. While the key coefficients are largely unaffected, we again find evidence that respondent's savings behavior is strongly correlated with their perceptions of their parents' behavior. Equations (5) and (6) measure the effects of curriculum mandates separately for individuals whose parents saved more than average, and for individuals whose parents did not save more than average. Again we find that the effect is concentrated in the second group. The consistency of this pattern across regressions for rates of saving and for net worth provides considerable support to the view that financial education at school is a close substitute for financial education at home, and is largely redundant when parents communicate the basic elements of household saving.

In equations (7) and (8), we add the variable "years since mandate" to see if we can glean further information about the shape of the transition path illustrated in figure 2. The coefficients of this variable are slightly negative and statistically insignificant. This is not surprising since, as shown in figure 2, the two benchmark profiles must converge as "years since mandate" rises. Our other findings are largely unaffected, except that we estimate the coefficients of the "exposed to mandate" interactions with somewhat less precision. Further attempts to identify the shape of the transition path were largely unsuccessful, probably owing (as in section 5.A) to the relatively small fraction of respondents who were exposed. In this instance the problem is even more severe since the usable data sample is only half as large.

In equations (9) and (10), we add dummy variables for the respondent's high school state, again dropping the variable "state ever imposed mandate." This reduces the size of the key coefficients (of "exposed to mandate * frugal parents"). For the median regression, this coefficient is no longer significant at conventional levels,[40] though its economic magnitude is still substantial. For the percentile rank regression, the coefficient remains significant at the 94 percent level of confidence. In contrast to our findings for rates of saving, the state constants are jointly significant in both regressions. The weakening of our results does not come as a surprise, given that the state constants consume degrees of freedom that are particularly scarce in the context of net worth. To the extent the state constants proxy for effects that are orthogonal to the variables of interest, their omission may improve precision without introducing a systematic bias.

As in section 5A, we performed a variety of other robustness checks that are not reported in the table. Our central findings are not affected by the inclusion of a wider range of socio-economic controls, higher order terms in age and earnings, or dummy variable for state of current residence. No robust patterns identifying interactions between education and other characteristics were detected.

6. Conclusions

In this study, we have provided the first systematic evidence on the long-term behavioral effects of high school financial curriculum mandates. Our findings are consistent with the view that mandates are uncorrelated with preexisting inclinations to offer, require, and take courses that cover financial topics. We also find that mandates significantly increase exposure to financial education, and ultimately elevate the rates

[40]One can only be 80 percent confident that the coefficient is different from zero.

at which individuals save and accumulate wealth during their adult lives. These results contribute to the growing body of evidence that education may be a powerful tool for stimulating personal saving.

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Figure 1: Hypothesized Pattern for Rates of Saving

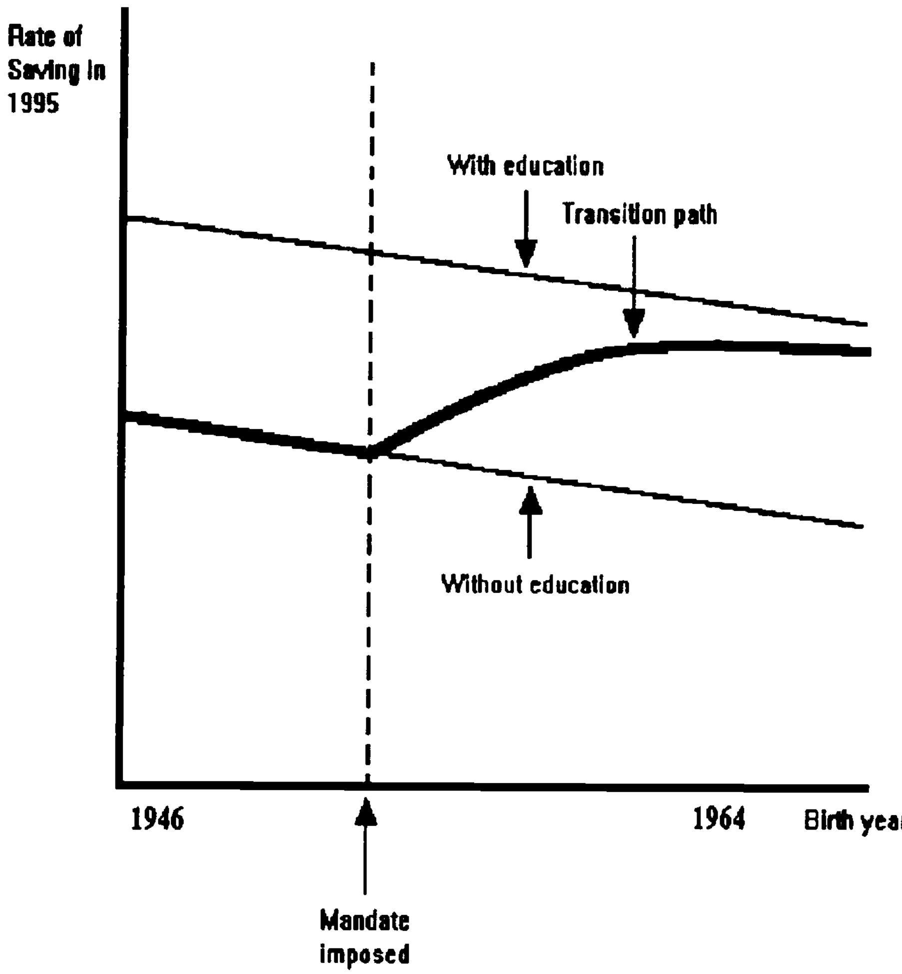

Figure 2: Hypothesized Pattern for Accumulated Wealth

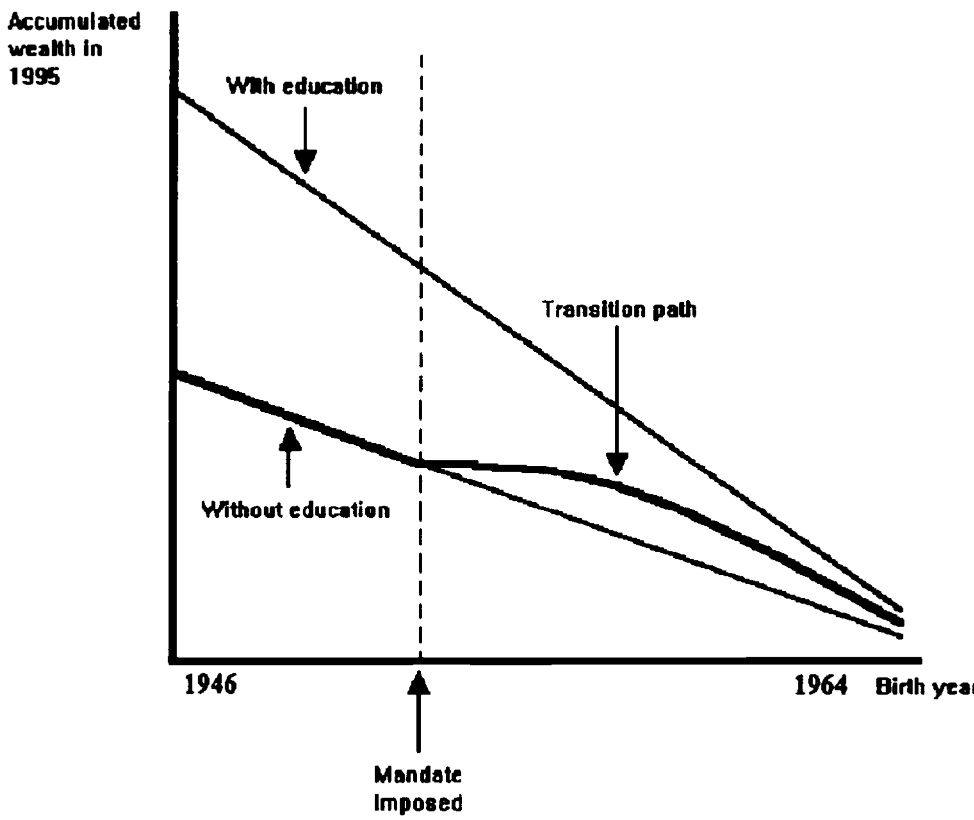

Table 1: Consumer Education Policies by State

State	First graduating class affected*	Consumer education mandate?	Personal finance mandate?
Alabama	1976	Yes	No
Alaska	1964	No	No
Arizona	1972	Yes	No
Arkansas	1977	Yes (1988)	No
California	1975	Yes (1989)	No
Colorado	No policy		
Connecticut	No policy		
Delaware	1976	Yes	Yes
District of Columbia	No policy		
Florida	1975	Yes	Yes
Georgia	1977	Yes	Yes
Hawaii	1973	Yes	Yes
Idaho	1978	Yes	Yes
Illinois	1968	Yes	Yes
Indiana	1976	No	No
Iowa	1976	Yes	No
Kansas	1977	No	No
Kentucky**	1975	Yes	No
Louisiana	1978	Yes	No
Maine	No policy		
Maryland	1975	No	No
Massachusetts	No policy		
Michigan	1980	No	No
Minnesota	No policy		
Mississippi	1982	Yes	No
Missouri	No policy		
Montana	1972	No	No
Nebraska	No policy		
Nevada	1957	Yes	Yes
New Hampshire	1985	Yes	No
New Jersey	1976	Yes	No
New Mexico	1979	Yes	Yes

State	First graduating class affected*	Consumer education mandate?	Personal finance mandate?
New York	1979	Yes (1986)	No
North Carolina	1978	Yes	Yes
North Dakota	No policy		
Ohio	1970	No	No
Oklahoma	1978	Yes	Yes
Oregon	1973	Yes	Yes
Pennsylvania	1978	No	No
Rhode Island	1969	Yes (1985)	No
South Carolina	1982	Yes	Yes
South Dakota	No policy		
Tennessee	1975	Yes	No
Texas	1976	Yes (1979)	Yes (1979)
Utah	1978	Yes	No
Vermont	No policy		
Virginia	No policy		
Washington	No policy		
West Virginia**	1977	Yes (1983)	No
Wisconsin**	1974	Yes	Yes
Wyoming	No policy		
Number with policy:	37	29	14

* If no information on implementation date was available, the first class affected was assumed to be the class graduating in the year after the policy was approved.

** Kentucky withdrew its consumer education policy in 1984, West Virginia withdrew its mandate in 1989, and Wisconsin withdrew its mandate in 1983.

Note: Unless noted, the first class affected by mandate is the same as the first class affected by policy.

Source: Alexander (1979), Joint Council on Economic Education (1989), and National Coalition for Consumer Education (1990).

Table 2: General Characteristics of the Sample

Characteristic	
Percent married	67.9%
Percent female respondent	50.0%
Percent homeowner	71.7%
Percent African American	6.9%
Percent other non-white	12.7%
Percent high school degree, no college degree	61.8%
Percent college degree	32.3%
Average age - respondent	39.6
Average age - spouse	40.2
Percent took consumer ed. course	42.0%
Percent took consumer ed. course with financial topics	29.4%
Percent required to take consumer ed. course with financial topics	11.4%

Table 3: Summary Statistics for Earnings and Wealth

Variable and Subsample	Median	First Quartile	Third Quartile
Household earnings			
All households	50,000	30,000	71,400
Unmarried individuals	27,000	15,000	40,000
Married couples	59,800	43,500	80,300
Homeowner	55,200	38,000	80,000
Non-homeowner	32,000	18,000	50,500
Total net worth			
All households	77,000	8,500	209,500
Unmarried individuals	31,500	0	124,000
Married couples	106,500	27,500	233,000
Homeowner	129,000	48,000	272,800
Non-homeowner	2,000	-3,600	35,000
Non-housing wealth			
All households	25,000	-500	120,000
Unmarried individuals	7,400	-2,000	71,000
Married couples	40,000	0	148,000
Homeowner	46,000	800	173,500
Non-homeowner	2,000	-3,600	35,000

Table 4: Exposure to Consumer and Financial Education, Probits

Independent Variable	(1) Exposed to consumer education	(2) Exposed to consumer education	(3) Exposed to financial education	(4) Exposed to financial education	(5) Exposed to fin. ed., required	(6) Exposed to fin. ed., required	(7) Exposed to fin. ed., elective	(8) Exposed to fin. ed., elective
State constants	No	Yes	No	Yes	No	Yes	No	Yes
State ever imposed mandate	-0.0127 (0.0263)		0.0403 (0.0312)		0.0139 (0.0214)		0.0238 (0.0262)	
Exposed to mandate	-0.0567 (0.0562)	-0.0605 (0.0620)	-0.0227 (0.063)	-0.0277 (0.0655)	-0.0326 (0.0388)	-0.0506 (0.0399)	0.0090 (0.0530)	0.0189 (0.0561)
Years since mandate	0.0271 (0.0088)	0.0310 (0.0107)	0.0181 (0.0082)	0.0240 (0.0104)	0.0146 (0.0048)	0.0230 (0.0070)	0.0007 (0.0065)	0.0024 (0.0085)
Age	-0.0028 (0.0024)	-0.0019 (0.0026)	-0.0039 (0.0021)	-0.0037 (0.0023)	-0.0014 (0.0015)	-0.0011 (0.0016)	-0.0024 (0.0018)	-0.0019 (0.0019)
Female	0.0889 (0.0239)	0.0949 (0.0245)	0.0810 (0.0224)	0.0867 (0.0228)	0.0384 (0.0154)	0.0429 (0.0161)	0.0405 (0.0186)	0.0420 (0.0188)
African American	0.146 (0.048)	0.147 (0.051)	0.0857 (0.0456)	0.102 (0.049)	0.140 (0.034)	0.132 (0.037)	-0.0544 (0.0366)	-0.0339 (0.0395)
Other non-white	-0.0133 (0.0369)	-0.0042 (0.0384)	-0.0213 (0.0341)	-0.0107 (0.0357)	-0.0040 (0.0236)	0.0075 (0.0255)	-0.0075 (0.0279)	-0.0080 (0.0287)
Frugal parents	0.0158 (0.0260)	0.0124 (0.0268)	0.0212 (0.0243)	0.0238 (0.0248)	0.0076 (0.0167)	0.067 (0.0175)	0.0129 (0.0202)	0.0157 (0.0205)
Percent class attending college	0.0157 (0.0497)	0.0589 (0.0524)	-0.0198 (0.0468)	0.0187 (0.0490)	0.0076 (0.0318)	0.0240 (0.0341)	-0.0238 (0.0391)	-0.0065 (0.0406)
Observations	1751	1744	1727	1721	1699	1593	1699	1671
χ^2	44.0	137.1	40.7	127.7	43.3	82.2	11.5	82.5

Coefficients represent probability changes (derivatives for continuous variables, changes for discrete variables). Standard errors are in parentheses.

Table 5: Percentage Rates of Saving

Independent Variable	(1) Median regression	(2) Percentile rank, OLS	(3) Median regression	(4) Percentile rank, OLS	(5) Median regression	(6) Percentile rank, OLS	(7) Median regression	(8) Percentile rank, OLS	(9) Median regression	(10) Percentile rank, OLS
Constant	3.23	40.0	3.66	38.3	3.55	38.1	3.62	39.2	State	State
	(1.87)	(4.8)	(1.85)	(4.8)	(1.86)	(4.8)	(2.00)	(4.9)	constants	constants
State ever imposed mandate	-0.520	-1.32	-0.330	-1.51	-0.255	-1.63	-0.314	-0.84		
	(0.504)	(1.59)	(0.546)	(1.58)	(0.601)	(1.58)	(0.619)	(1.74)		
Exposed to mandate							0.35	-4.03		
							(1.49)	(3.66)		
Years since mandate	0.303	0.815	0.299	0.799						
	(0.092)	(0.352)	(0.104)	(0.351)						
Years since mandate * parents not frugal					0.374	1.30	0.348	1.68	0.352	1.04
					(0.117)	(0.44)	(0.172)	(0.56)	(0.190)	(0.55)
Years since mandate * frugal parents					0.012	0.100	-0.054	0.428	0.105	-0.110
					(0.204)	(0.505)	(0.255)	(0.587)	(0.221)	(0.589)
Frugal parents			1.69	4.40	1.95	5.11	1.89	5.16	1.30	5.18
			(0.52)	(1.39)	(0.57)	(1.44)	(0.54)	(1.44)	(0.51)	(1.45)
Married	0.435	3.16	0.581	3.09	0.518	3.08	0.575	3.06	-0.073	2.93
	(0.689)	(1.40)	(0.688)	(1.40)	(0.683)	(1.40)	(0.648)	(1.40)	(0.597)	(1.42)
College educated	1.85	9.30	1.79	8.60	1.88	8.57	1.82	8.55	2.25	8.63
	(0.52)	(1.36)	(0.54)	(1.38)	(0.51)	(1.37)	(0.52)	(1.37)	(0.56)	(1.40)
Age/10	0.498	0.177	0.307	0.196	0.345	0.198	0.323	0.170	0.622	0.202
	(0.427)	(0.117)	(0.458)	(0.117)	(0.455)	(0.117)	(0.496)	(0.120)	(0.450)	(0.123)
Total earnings/10^5	4.00	5.65	3.52	5.52	3.36	5.46	3.43	5.43	3.53	5.19
	(0.95)	(1.04)	(0.93)	(1.04)	(0.927)	(1.04)	(1.03)	(1.04)	(0.97)	(1.07)
Observations	1870	1870	1870	1870	1870	1870	1870	1870	1870	1870

Standard errors are in parentheses. For median regressions, standard errors are bootstrapped based on 500 replications.

Table 6: Ratio of Net Worth to Earnings

Independent Variable	(1) Median regression	(2) Percentile rank, OLS	(3) Median regression	(4) Percentile rank, OLS	(5) Median regression	(6) Percentile rank, OLS	(7) Median regression	(8) Percentile rank, OLS	(9) Median regression	(10) Percentile rank, OLS
Constant	-2.00 (0.71)	9.96 (7.22)	-1.97 (0.68)	6.28 (7.25)	-2.09 (0.69)	5.76 (7.25)	-1.99 (0.73)	6.08 (7.25)	State constants	State constants
State ever imposed mandate	-0.205 (0.341)	-1.29 (2.64)	-0.297 (0.316)	-1.79 (2.63)	-0.316 (0.303)	-1.88 (2.62)	-0.266 (0.306)	-1.86 (2.62)		
Exposed to mandate	1.02 (0.475)	9.09 (4.05)	1.15 (0.50)	9.00 (4.03)						
Exposed to mandate * parents not frugal					1.26 (0.55)	13.8 (4.7)	1.41 (0.87)	19.7 (6.2)	0.690 (0.517)	9.78 (5.16)
Exposed to mandate * frugal parents					0.272 (0.861)	1.51 (5.63)	0.265 (0.946)	7.00 (6.77)	0.135 (0.723)	-1.45 (5.87)
Years since mandate							-0.036 (0.103)	-1.07 (0.74)		
Frugal parents			0.692 (0.266)	7.27 (2.07)	0.781 (0.280)	8.63 (2.19)	0.766 (0.296)	8.60 (2.18)	0.516 (0.264)	7.69 (2.23)
Married	-0.159 (0.283)	-1.99 (2.11)	-0.262 (0.276)	-1.85 (2.10)	-0.125 (0.274)	-1.82 (2.10)	-0.168 (0.255)	-1.79 (2.10)	-0.301 (0.257)	-2.46 (2.13)
College educated	0.038 (0.218)	3.13 (2.03)	-0.109 (0.213)	2.12 (2.04)	-0.127 (0.216)	2.30 (2.04)	-0.143 (0.213)	2.45 (2.04)	0.015 (0.224)	2.04 (2.10)
Age/10	0.824 (0.180)	9.27 (1.78)	0.814 (1.65)	9.79 (1.77)	0.841 (0.172)	9.83 (1.77)	0.296 (0.179)	9.73 (1.77)	0.775 (0.161)	9.16 (1.82)
Total earnings/10^5	0.771 (0.426)	5.30 (1.67)	0.811 (0.442)	5.07 (1.66)	0.632 (0.442)	4.95 (1.66)	0.857 (0.415)	4.97 (1.66)	0.714 (0.414)	4.76 (1.70)
Observations	911	911	911	911	911	911	911	911	911	911

Standard errors are in parentheses. For median regressions, standard errors are bootstrapped based on 500 replications.