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Education and saving: The long-term effects of high school financial curriculum mandates

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Abstract

Over the last 40 years, a majority of states have adopted consumer education policies, and a sizable minority have mandated that high school students receive instruction on topics related to household financial decision-making. In this paper, we attempt to determine whether these mandates have had any effect on subsequent decisions. We exploit the variation in requirements both across states and over time to identify the effects of interest. The evidence indicates that mandates have raised both exposure to financial curricula and subsequent asset accumulation once exposed students reached adulthood. The estimated effects are gradual, probably due to implementation lags. © 2001 Elsevier Science B.V. All rights reserved.

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1. Introduction

Between 1957 and 1985, 29 states adopted legislation mandating some form of ‘consumer’ education in secondary schools. Fourteen states specifically required

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coverage of topics relevant to household financial decision-making, from budgeting, credit management, and balancing checkbooks to compound interest and other investment principles. The objective of these curriculum mandates was to equip students with practical decision-making skills that would prove useful in their adult lives.¹

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, previous studies suggest that consumer education has meaningful *short-term* effects on the knowledge, attitudes, and behavior of high school students.² If poor financial decisions result, at least to some degree, from a failure to appreciate economic vulnerabilities (Bernheim, 1994, 1995), then education may contribute pertinent knowledge and/or specific decision-making skills. Early exposure to financial concepts may increase comfort and familiarity with financial matters, thereby removing psychological barriers that impede proper decision-making. On the other hand, long-run effects of other targeted educational initiatives, such as the Head Start program, have been difficult to detect (see Currie and Thomas, 1995). Broad mandates may have little real influence on curriculum content, classroom treatment of critical financial topics may be cursory, teachers may have inappropriate or inadequate qualifications, and/or students may fail to take the material seriously.³ Of course, all of these considerations are speculative. Virtually nothing is known about the long-term behavioral effects of financial education in secondary schools.

Using a unique cross-sectional household survey fielded in the Fall of 1995, we attempt to determine whether the curricula arising from high school financial education mandates have affected adult financial decision-making. We focus on the level of saving. Given the widespread (though not universal) concern that the typical American saves too little (see e.g. Bernheim, 1991, 1996), we are primarily interested in the hypothesis that financial education increases saving. Our analysis exploits differences in policies across states and over time. In effect, we use the non-adopting states as a benchmark, and investigate whether the establishment of a

¹We present a concise summary of mandates in Section 2. See Alexander (1979), Brobeck and Cohart (1988), Highsmith (1989), and Scott (1990) for further details. To take one example, legislation in 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).

²See e.g. Peterson (1992), Soper and Brenneke (1981), Kohen and Kipps (1979), Langrehr (1979), and Fast et al. (1989). Boyce and Danes' (1998) study of the NEFE's High School Financial Planning Program is particularly pertinent since it documents significant short-term effects on money management.

³For information on the qualifications of the teachers who staff these courses, see Boyce and Danes (1998). The JumpStart Coalition's searchable database at <http://www.jumpstartcoalition.org/mdb/jssearch.cfm> contains detailed information on programs and curricula. The Commission on Saving and Investment in America (1995) also provides a listing of programs and sponsors. For an example of a teaching guide, see National Institute for Consumer Education (1994).

mandate coincided with a departure from this benchmark among the affected cohorts.

We begin our investigation by studying exposure to financial education. We find that 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 steadily with the passage of time. This indicates that the effects of mandates were 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. Likewise, for net wealth, there are systematic differences from the benchmark among those young enough to have been exposed, but not among those too old to have been exposed. We carefully consider and ultimately reject the possibility that these patterns reflect spurious correlations between the adoption of financial education mandates and the underlying tastes and interests of a state's population.

The current study is related to several strands of literature. One pertinent strand concerns the evaluation of policies to increase saving. There is, of course, an extensive literature on tax incentives.⁴

A smaller (but more closely related) literature investigates the relation between saving and education. Much of this is anecdotal and/or inconclusive (see, for example, Bernheim, 1991, or Central Council for Savings Promotion, 1981, for discussions of the post-War savings promotion campaign in Japan). Likewise, correlations between an individual's general level of educational attainment and his or her rate of saving (documented by Bernheim and Scholz, 1993, and by Hubbard et al., 1995) may be attributable to other related factors, such as rates of time preference. A more promising branch of this literature focuses on the behavioral effects of retirement education in the workplace (Bernheim and Garrett, 1996, Bayer et al., 1996, Bernheim, 1998, and Clark and Sylvester, 1998). 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).

This paper is also related to research on the returns to education (including schooling and training). A central issue in that literature concerns the likelihood that both wages and education are related to some underlying unobservable

⁴For recent surveys of this literature, see Bernheim (1997, 1999), Hubbard and Skinner (1996), Poterba et al. (1996), and Engen et al. (1996).

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. Parallel 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 examples of consumer education policies. Consumer education covers a wide range of topics (see Bannister and Monsma, 1982, for details). Alexander (1979) and Scott (1990) refer to four sub-fields of consumer education: Consumer decision-making, economics, personal finance, and consumer rights and responsibilities. The personal finance sub-field covers household budgeting, money management, saving and investing (including alternative financial instruments, the relationship between risk and return, the role of inflation, taxes, and diversification), and the use of credit (see Boyce and Danes, 1998, for further details).

Consumer education policies either encourage or mandate specific curricula. There are several varieties of mandates. Some require state education agencies to develop and distribute materials to local districts, some require districts to offer consumer education, and some require each student to receive instruction. In the latter case, some states require a separate course, while others allow schools to integrate consumer education into existing courses.

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. Nevada

instituted the earliest policy in 1957, and most of the policies were implemented in the 1970s. Thirty seven states have policies that encourage or require instruction in consumer education, or have had such policies in the past. Twenty nine states required instruction in consumer education, while fourteen states specifically required instruction in personal finance.⁵

Historically, the underlying predilection to save among states' residents appears to have played little if any role in the adoption of financial education mandates.⁶ Few consumer education policies focus exclusively or even primarily on personal finance. Most of these policies resulted from a broader consumer movement during the 1960s and 1970s.⁷ The features of consumer education policies typically reflect the views of particular interest groups, rather than broad public sentiment.⁸ According to Mayer (1989), the keys to the passage of consumer laws were the preferences of critical legislators and sound lobbying strategy, rather than public opinion more broadly defined. In cases where public opinion played a significant role, it was usually associated with outrage over an isolated event that caused a group of consumers to suffer in an immediate and observable way. Notably, states that passed consumer education legislation were not statistically significantly different from other states in terms of income, retail sales, or the proportion of residents who graduated from high school (Ford, 1977).

From conversations with consumer education activists, we have learned that mandates were rarely effective immediately upon passage. Delays typically resulted from the need to train teachers and develop curricula. For example, the

⁵For 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. The policies of a few states were difficult to classify. In particular, Tennessee and New Jersey required coursework in consumer education and required the development of financial education curricula, but do not appear to have required coursework specifically on financial topics. We classified these states as not requiring financial education. This was a conservative choice, since our results are generally stronger when we classify Tennessee and New Jersey as requiring financial education. There is also 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.

⁶For additional background on the passage and development of consumer education policies and programs, see Langrehr and Mason (1977), or Herrman (1982).

⁷See Bannister (1996) for a history of consumer education in the US, and Mayer (1989) for a history of the broader consumer movement.

⁸In a survey of chief state school administrators, Scott (1990) found that educational professionals and members of the business community were far more likely than parents or students to press for consumer education.

Table 1
Consumer education policies by state^a

State	First graduating class affected ^b	Consumer education mandate? ^c	Personal finance mandate? ^c
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 ^d	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
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

Table 1. Continued

State	First graduating class affected ^b	Consumer education mandate? ^c	Personal finance mandate? ^c
Texas	1976	Yes (1979)	Yes (1979)
Utah	1978	Yes	No
Vermont	No policy		
Virginia	No policy		
Washington	No policy		
West Virginia ^d	1977	Yes (1983)	No
Wisconsin ^d	1974	Yes	Yes
Wyoming	No policy		
Number with policy:	37	29	14

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

^b 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.

^c In these columns, where a date appears in parentheses, this indicates the first class affected by the mandate (either consumer education or financial education). This may differ from the first class affected by the consumer education policy (listed in the first column), since some states adopted policies (e.g. curriculum recommendations) before they adopted mandates. Where no date appears in parentheses, the first class affected is the same as the first class affected by the general consumer education policy for that state.

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

state of Illinois ran teacher workshops for several years after legislators passed its mandate. Consumer education guidelines were not even issued until more than a year after adoption of the Illinois policy (Metcalf and Wetherington, 1969). State officials then set out to select pilot schools, with the object of developing and assessing model programs (Johnston, 1969). Thus, we would expect the effects of mandates, if any, to have been gradual rather than immediate.

3. Survey data

Our analysis is based on a unique cross-sectional household survey fielded in November, 1995.⁹ Respondents were between the ages of 30 and 49. Most presumably graduated from high school between 1964 and 1983, a period which

⁹The survey was designed in cooperation with the first author of this paper, and fielded for Merrill Lynch by Survey Communications, Inc. The original purpose of the survey was to monitor the adequacy of saving among members of the baby boom generation (see Bernheim, 1996). Unfortunately, existing public use data sets such as the Survey of Consumer Finances (SCF) and the Panel Study of Income Dynamics (PSID) do not contain data on the state in which the respondent attended high school, or on exposure to financial education curricula.

spans the transition to financial curriculum mandates in many states. A total of 2000 surveys were completed.¹⁰ The survey gathered standard economic and demographic information, including household earnings, total income, self-reported rates of saving, assets and liabilities,¹¹ pension coverage,¹² employment status, gender, marital status, age, ethnic group, education, and household composition. It also solicited information on childhood influences of potential relevance to future financial decisions. Most importantly, it asked respondents to identify the state in which they attended high school, and it solicited information concerning exposure to financial education.¹³

One potential concern 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 before posing sensitive questions. If a respondent declined to answer a quantitative question, he or she was asked to provide bracketed information, from which we imputed numerical values.¹⁴ Response rates were relatively high. For example, 74% (12%) of respondents provided numerical (bracketed) information on annual earnings.¹⁵

¹⁰Survey Communications, Inc. conducted the survey using a propriety CATI (Computer Assisted Telephone Interviewing) system. Sampling was based on automated generation and execution of random phone numbers, using an algorithm designed to ensure representativeness. Direct electronic data entry permitted automated control of skip patterns, and eliminated the possibility of transcription errors. Respondents who terminated their interviews before completion of the survey were deleted from the final sample. Information on the frequency of disconnects is not available.

¹¹Respondents were asked separately about the value of financial assets, houses, other real property, business interests, and debt.

¹²Respondents 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.

¹³If they lived in several states during high school, respondents were asked to name the state in which they lived the longest. Each respondents was also asked if, in high school, he or she took any courses dealing with household finances, consumer education, or economics. If the respondent answered “yes”, he or she was asked whether any of these courses specifically covered topics concerning personal finance. If the respondent again answered “yes”, he or she was asked whether his or her school required any of these courses. Issues concerning the accuracy of these recollections are considered below, where pertinent. The survey did not elicit similar information concerning spouses.

¹⁴For each bracketed answer, we imputed a 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.

¹⁵For the remaining 14% 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.

Table 2
Summary statistics

Variable and subsample	SCF, weighted ^a (benchmark)	Full sample	Covered by fin. ed. mandate	Not covered by fin. ed. mandate
Percent married	56.2	67.9	67.7	68.0
Percent single male	20.0	16.5	16.2	16.5
Percent single female	23.8	15.6	16.2	15.5
Percent white	75.1	80.4	75.3	81.0
Percent non-white	24.9	19.6	24.7	19.0
Percent no degree	9.9	5.9	4.5	6.1
Percent high school degree only	60.0	61.8	64.1	61.6
Percent college degree	30.1	32.3	31.3	32.3
Percent homeowners	62.6	71.7	66.2	72.3
Average age of respondent	39.4	39.6	34.7	40.1
Percent took consumer ed. course		42.0	53.4	40.1
Percent took consumer ed. course with financial topics		29.4	43.1	27.9
Percent required to take consumer ed. course with financial topics		11.4	21.2	10.4
Median household earnings	37,000	50,000	45,000	50,000
Median net worth	45,680	77,000	93,000	75,250
Earnings < \$25,000	6710	4550		
\$25,000 ≤ Earnings < \$50,000	39,290	43,500		
\$50,000 ≤ Earnings < \$75,000	91,500	105,000		
\$75,000 ≤ Earnings < \$100,000	144,050	176,580		
\$100,000 ≤ Earnings	321,400	345,000		
Married	69,400	106,500		
Single	19,300	31,500		

^a Statistics in this column refer to the subsample of the 1995 Survey of Consumer Finances for which respondents fell into the 30–49 age group.

The second column of Table 2 contains (unweighted) summary statistics for the full sample.¹⁶ For purposes of comparison and validation, the first column contains population-weighted summary statistics for the same age group (30 to 49 year olds) based on the contemporaneous (1995) Survey of Consumer Finances (SCF). There are some significant discrepancies. Single individuals (especially females),

¹⁶ Earnings are defined as income from employment or self-employment. Net worth equals the sum of financial assets (including defined contribution pension plan balances), home equity, other real property, and business interests, net of debt.

non-whites, and high school drop-outs are under-represented in the Merrill Lynch survey. Though earnings vary appropriately with marital status, gender, education, and age,¹⁷ median household earnings are roughly 35% higher than the benchmark (\$50,000 vs. \$37,000). Median net worth is roughly 64% higher than the benchmark (\$77,000 vs. \$47,000), and homeowners are significantly over-represented.

Table 2 also contains information on median net worth disaggregated by household earnings and marital status categories. Note that, once one conditions either on earnings or marital status, the gap between median net worth in the Merrill Lynch sample and the SCF closes substantially. The largest discrepancy for any earnings category (specifically, \$75,000 to \$100,000) is only 23%; in other earnings categories the gap is considerably smaller. Consequently, even though the Merrill Lynch survey oversampled certain groups, the distribution of wealth conditional upon earnings and demographics may nevertheless be reasonably representative.

To explore this possibility further, we weighted the observations in the Merrill Lynch sample to replicate the distribution of the population observed in the SCF across fifteen cells, defined by three marital status categories (married, single male, single female) and the five categories for household earnings used in the table. Discrepancies with the benchmark narrow considerably for most summary statistics. Median net worth falls from \$77,000 (unweighted) to \$47,000 (weighted), and the gap between median net worth for the two samples declines from 63% to less than 3% (\$47,000 vs. \$45,680). The distribution of net worth in the weighted Merrill Lynch sample is certainly not identical to the benchmark distribution. We continue to find discrepancies in the neighborhood of 10–15% within a number of population subgroups, and the variance of net worth is lower in the SCF (possibly reflecting a reduction in measurement error resulting from greater thoroughness). Nevertheless, these discrepancies are small enough to reassure us that the Merrill Lynch survey probably measures wealth reasonably well for those who were actually interviewed.

Based on their birth years and high school states, we find that roughly 10% of our respondents belonged to graduating classes covered by financial curriculum

¹⁷Median earnings are \$25,000 for females, \$38,000 for males, \$16,325 for high school drop-outs, \$25,000 for those with high school degrees but no education past high school, \$30,000 for those with high school degrees and some additional education (but no college degree), \$40,000 for those with college degrees, \$25,000 for blacks, \$28,000 for other non-whites, \$31,966 for whites, \$30,000 for those under 40, and \$32,000 for those 40 and over. The median value of total household earnings is \$59,809 for married couples, \$27,000 for single individuals, \$55,226 for homeowners, and \$32,000 for renters.

mandates.¹⁸ The third and fourth columns of Table 2 provide summary statistics separately for those who were and were not covered. Differences in marital status and education are minor. Non-whites account for a somewhat larger fraction of the population in states that enacted mandates. Since most mandates are relatively recent, covered respondents are considerably younger than non-covered respondents. As a result, their median earnings are lower, and they are less likely to own homes. Despite these differences, median net worth is considerably higher among covered respondents.

4. The impact of curriculum requirements on educational exposure

Curriculum mandates presumably cannot influence subsequent financial choices unless they increase exposure to financial concepts in the classroom. Yet there is no guarantee that this will occur. If school districts or individual schools require financial 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 financially-oriented curricula might simply choose not to comply with the mandate. The purpose of this section is to provide evidence on the link between curriculum requirements and educational exposure.

According to Table 2, among respondents belonging to graduating classes covered by financial curriculum mandates, 43% said that they took courses covering financial topics, and 21% said that they were required to do so. In comparison, of those not exposed to a curriculum mandate, only 28% said that they took courses covering financial topics, and only 10% said that they were required to do so. In addition, the likelihood of exposure to financial education within states with curriculum mandates increased as the mandates matured. Among those in the final three graduating classes not affected by a mandate, 28% said that they had taken courses dealing with personal finance. This figure rose to 36% for the first three graduating classes covered by a mandate, to 47% for the next three graduating classes, to 50% for all subsequent classes.¹⁹

¹⁸We assume that the respondent was exposed to the mandate if he or she is young enough to have been covered by the mandate (if any) for the state in which he or she attended high school. When no other information is available, we assume that the mandate applied to the first class entering its senior year 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% 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.

¹⁹43% of those exposed to mandates belonged to the first three graduating classes covered by the mandate, roughly 26% belonged to the next three classes, and roughly 31% belonged to subsequent classes.

Table 3

Probits explaining exposure to consumer and financial education^a

Independent variable	(1) Exposed to financial education	(2) Exposed to fin. ed., required	(3) Exposed to fin. ed., elective
State ever imposed mandate	0.0416 (0.0313)	0.0144 (0.0214)	0.0247 (0.0263)
Exposed to mandate	−0.0243 (0.0630)	−0.0330 (0.0387)	0.0080 (0.0530)
Years since mandate	0.0181 (0.0082)	0.0146 (0.0048)	0.0007 (0.0065)
Age	−0.0040 (0.0021)	−0.0014 (0.0015)	−0.0025 (0.0018)
Female	0.0806 (0.0224)	0.0383 (0.0154)	0.0403 (0.0186)
African American	0.0854 (0.0456)	0.140 (0.034)	−0.0545 (0.0366)
Other non-white	−0.0216 (0.0341)	−0.0041 (0.0236)	−0.0077 (0.0279)
Frugal parents	0.0217 (0.0243)	0.0078 (0.0167)	0.0132 (0.0203)
Percent class attending college	−0.0191 (0.0468)	0.0079 (0.0318)	−0.0234 (0.0392)
Observations	1726	1698	1698
χ^2	40.7	43.3	11.6

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

The first column of Table 3 contains estimates of a probit model explaining the likelihood of exposure to financial education (self-reported). To facilitate the interpretation, we transform the coefficients as follows: 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 (evaluated at sample means in both cases).

The equation contains two key explanatory variables, ‘exposed to mandate’ and ‘years since mandate.’ ‘Exposed to mandate’ is set equal to one if the respondent’s graduating class was covered by a financial curriculum mandate, and zero otherwise. If the imposition of a mandate leads to an immediate and substantial increase in consumer education, then the coefficient of this variable should be positive.²⁰ ‘Years since mandate’ measures the time elapsed between the imposi-

²⁰To the extent we sometimes incorrectly identify the first graduating class covered by the mandate, 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).

tion of the mandate and the year in which the mandate applied to the respondent. For example, if a respondent belongs to the fourth graduating class covered by the mandate, we set ‘years since mandate’ equal to 4. 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 (as schools train teachers or develop curricula to comply), then the coefficient of this variable should be positive.²¹

We also include a dummy variable indicating whether the state *ever* imposed a mandate (regardless of whether it covered the respondent). 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 impact 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.

Other explanatory variables include controls for the respondent’s age, gender, and ethnicity. Since exposure to financial education may in part reflect attitudes within the respondent’s family, we also include a variable measuring the respondent’s assessment of his or her parent’s degree of frugality.²² Finally, since curriculum content is probably related to school quality, we control for the percent of the respondent’s high school class that attended college (self-reported).

The estimates imply that, over time, mandates significantly increased the fraction of students taking courses covering personal finance topics. Though the typical mandate did not have a noticeable effect immediately upon adoption (the ‘exposed to mandate’ coefficient is small and statistically insignificant), it became more effective as it matured (the coefficient of ‘years since mandate’ is positive and highly significant). The other specifications in Table 3 explain the likelihood of exposure to, respectively, *required* courses and *elective* courses covering personal finance topics. Notably, the effect of mandates on exposure to financial education shows up only in the equation for required courses. This provides reassurance that the measured effect is not spurious. None of these findings are significantly affected by the inclusion of dummy variables for the respondent’s

²¹The 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 5 or 6 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.

²²‘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%, 32%, and 35% (a small fraction of respondents declined to answer the question). Although parents’ frugality plays no significant role in the regressions of Table 3, it is highly correlated both with current saving, and with other pertinent childhood experiences (such as ownership of a bank account).

state of high school attendance, the respondent's current state, or both (results omitted).²³ Results for consumer education curriculum mandates (omitted) are similar.

Note also that there is essentially no effect associated with the dummy variable indicating whether the respondent's high school state ever imposed a mandate. This implies that, before a given state adopted a financial curriculum mandate, exposure to pertinent courses was roughly the same as in states that never adopted mandates. Equivalently, 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 specific legislators, rather than from widespread interest and/or concern among the general populace.

To investigate this hypothesis further, we reestimated specification (1) with one additional variable: 'years before mandate' (defined symmetrically to 'years after mandate'). The coefficient of this variable was small (-0.0056) and insignificant at conventional levels of statistical confidence ($\sigma=0.0064$). None of the other coefficients were appreciably affected. 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 not correlated with the general public's interest in financial education.

The preceding analysis concerns *recollections* about exposure to financial education rather than actual exposure. If recollections are imperfect, magnitudes may be mismeasured. However, as long as the quality of the respondent's memory is not systematically correlated with the presence of a state mandate, our results still shed light on the existence and timing of effects on educational exposure.

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). Before discussing specific findings, we address four general issues.

The first issue concerns the choice of an estimation technique. Survey data on net worth and self-reported rates of saving typically have skewed distributions with extreme outliers in thick upper tails. We prefer to use estimation strategies

²³Current state is the same as high school state for roughly 70% of respondents.

that moderate the influence of these outliers, for two reasons. First, extreme outliers may result from measurement error processes with non-standard properties (e.g. an incorrect number of zeros, or a whimsical answer). Second, the relationship between the dependent and independent variables may be systematically different for households with extreme values of the dependent variable. For example, the effect of financial education on saving may be negligible for households that inherit large estates from relatives, even if it is substantial for ordinary households.

In this paper, we use two estimation 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, where we suspect measurement error is more prevalent and behavioral responses less representative. For the second technique, we convert the dependent variables to population percentiles (equivalently, population ranks) before fitting OLS regressions. The coefficients in the resulting equations are robust with respect to outliers. Moreover, they are easily interpreted: 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 limiting the influence of outliers. The ones that we have explored yield similar results.²⁴ For the reasons mentioned above, we do not report OLS regression results for specifications in which the untransformed values of net worth and saving rates are used as dependent variables. In practice, this approach also yields qualitatively similar results, but in a number of cases the estimates are simply too imprecise to support reliable inferences.

The second general issue concerns the choice of independent variables measuring exposure to financial education. Three alternatives are available: (1) variables summarizing the applicability of mandates ('exposed to mandate' and 'years since mandate'), (2) self-reported information on actual educational exposure, and (3) self-reported information on required courses. We adopt the first of these alternatives.²⁵ Our estimates therefore reflect the composite effects of mandates on course content, availability, and enrollment, and of education on behavior. Since some students would take pertinent courses in the absence of mandates, and since some might not take such courses even in the presence of mandates, the effect of subjecting a student to a mandate is necessarily smaller than the effect of enrolling the student in a pertinent course (the 'treatment').

The second and third alternatives mentioned above are problematic. Correlations

²⁴We have explored two alternative approaches. One involves iterative 'robust' regression techniques that weight observations based on absolute deviations obtained from the previous iteration's regression. Another involves truncating the dependent variable (i.e. 'trimming' the upper tail), and estimating a tobit model to correct for censoring.

²⁵Results based on self-reported educational exposure are in most cases similar to those discussed in Sections 5.1 and 5.2, but are not reported due to the concerns mentioned in the text.

between financial behavior and exposure to pertinent curricula may be spurious if voluntary decisions to enroll in financial education courses are related to underlying tastes and/or interests. Similar concerns arise even if one uses self-reported information on course *requirements*. Respondents' memories of high school courses are probably selective or otherwise imperfect. Individuals with greater interest in financial topics may be more likely to remember financially oriented courses. In addition, school and/or district curriculum policies may be more sensitive to (and therefore more correlated with) parental concerns than are state policies.

If, as we have argued, state financial curriculum mandates are largely unrelated to the tastes and interests of the general populace, then one could attempt to solve these problems by using our mandate variables as instruments for self-reported education exposure. We do not adopt this approach for two reasons. First, courses are not homogeneous. If schools gradually develop and revise curricula subsequent to the imposition of a mandate, then course quality may improve over time. 'Years since mandate' captures this effect. If we use this variable only to predict enrollment, we lose potentially important information. Second, self-reported measures of exposure are probably subject to non-classical measurement error. For example, forgetting a course is probably more common than falsely remembering one. In such cases, instrumental variables estimators may be subject to bias.²⁶

The third general issue concerns policy focus. In this paper, 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, and therefore should not be expected to affect, saving and investment choices. 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.

The fourth general issue concerns interpretation. With self-reported financial data, it is difficult to determine whether financial education affects behavior, or simply the way that people answer questions about behavior. For example, those exposed to financial education may be more likely to think that they should have saved more, and to report their intentions rather than their actual behavior. This possibility strikes us as unlikely. The pertinent courses typically teach concepts and processes, and generally do not advise students on how much they will need to save as adults. We doubt that students would retain such specific information even if it were provided (rather, we conjecture that the link to behavior arises instead from increased comfort with financial transactions and concepts). We nevertheless acknowledge our inability to formulate a formal test that would distinguish between effects on behavior and on reporting.

²⁶In general, instrumental variable results are inconclusive, in that the key coefficients tend to have large standard errors.

5.1. Rates of saving

To promote the application of consistent measurement principles across observations, the survey instrument asks respondents to think of saving as unspent take-home pay plus voluntary deferrals (e.g. employee contributions to 401(k)s). This definition excludes some economically legitimate components of saving, such as reinvested capital income (including capital gains), employers' pension contributions, and social security accruals. It is nevertheless a natural definition for most respondents, many of whom would have difficulty quantifying the omitted components.

Saving rates are available for roughly 95% of the sample. This high response rate probably reflects two factors: first, the question is not particularly invasive; second, most individuals tend to think about saving as a fraction of earnings, rather than as a dollar amount (Bernheim, 1995). The median rate of saving is 10%, and the interquartile range runs from 3% to 15%. Roughly 17% of respondents say that they save nothing, and no respondent reports a negative rate. Though some households undoubtedly dissave in the broad economic sense, the absence of negative rates is consistent with the survey's narrow definition of saving. Roughly 4% of the sample reports saving more than 30% of earnings, and nine respondents report rates of saving greater than 50% (the maximum is 75%). This distribution seems somewhat high given the observed levels of accumulated assets. Nevertheless, at a minimum, the data do appear to contain meaningful ordinal information. The correlation between self-reported rates of saving and net worth 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). Since we do not have either true panel data or a detailed log of household spending, this is the best measure of flow saving available.

Our estimation strategy identifies the effects of mandates from cross-state variation in saving rate differences across ages. This approach is akin to the familiar procedure of estimating 'differences-in-differences' across states and time; here, the respondent's age fills the role usually played by time. By relating rates of saving to 'years since mandate' rather than to 'exposed to mandate,' we allow the magnitude of the estimated effect to depend upon the amount of time elapsed since adoption. In most specifications, we control for age effects with a single linear term, rather than with higher-order terms or a complete set of age dummies. Likewise, we typically control for differences between the circumstances of individuals from states with and without mandates (including differences arising from variation in other public policies) with our 'state ever imposed mandate' variable, rather than with a complete set of state dummies. As we will see, these simplifications do little or no violence to the data.

We motivate our basic specification as follows. Fig. 1 depicts hypothetical cross-sectional relations between rates of saving and birth year for two 'bench-

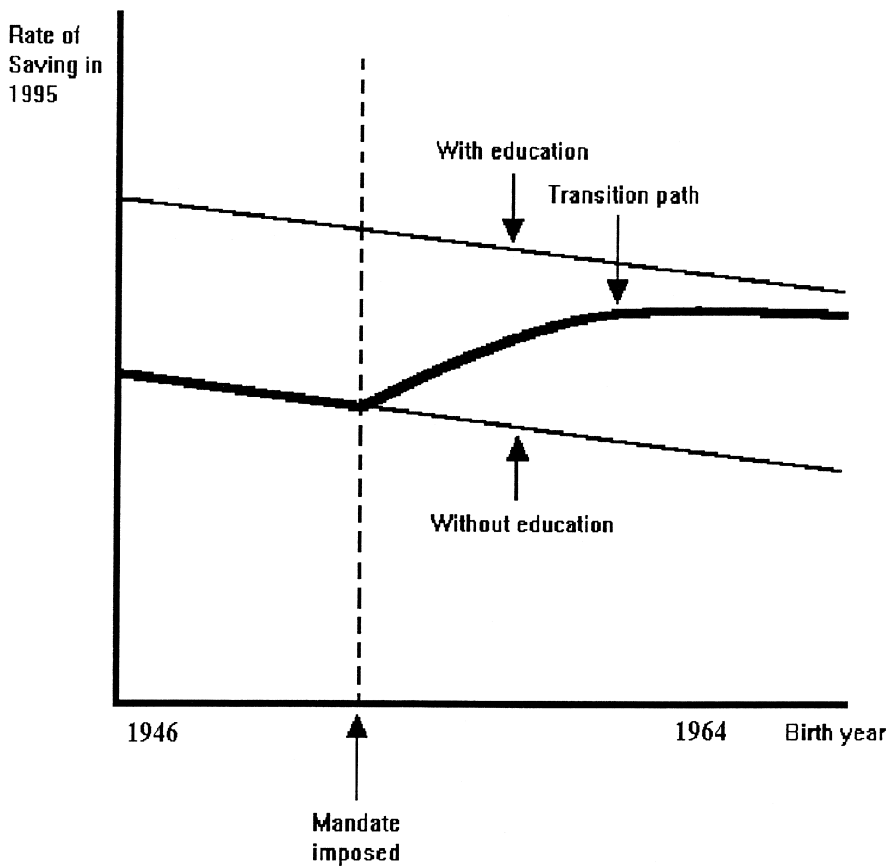


Fig. 1. Hypothesized pattern for rates of saving.

mark' groups: those who were exposed to financial education, and those who were not. Both lines slope slightly downwards, reflecting the empirical tendency for rates of saving to increase slowly with age.²⁷ What kind of cross-sectional saving rate profile would one expect to observe for a state that imposed a mandate affecting only younger members of the sample? For simplicity, imagine that no student received financial education prior to the mandate. For those too old to have been covered by the mandate, the profile should coincide with the lower benchmark. Recall that mandates cause enrollments to rise gradually, rather than discontinuously. One would therefore expect average rates of saving among younger cohorts to diverge smoothly from the lower benchmark subsequent to

²⁷This assumed tendency is consistent with the estimates reported later in this section.

Table 4
Regressions explaining percentage rates of saving^a

Independent variable	(1) Median regression	(2) Percentile rank, OLS	(3) Median regression	(4) Percentile rank, OLS	(5) Median regression	(6) Percentile rank, OLS
Constant	3.33 (1.88)	40.2 (4.8)	3.68 (1.94)	38.5 (4.8)	3.61 (1.96)	38.3 (4.8)
State ever imposed mandate	−0.491 (0.523)	−1.25 (1.59)	−0.235 (0.562)	−1.42 (1.59)	−0.180 (0.559)	−1.54 (1.59)
Years since mandate	0.300 (0.088)	0.803 (0.352)	0.289 (0.100)	0.785 (0.352)		
Years since mandate * parents not frugal					0.369 (0.114)	1.29 (0.44)
Years since mandate * frugal parents					−0.006 (0.214)	0.083 (0.505)
Frugal parents			1.73 (0.53)	4.46 (1.39)	1.95 (0.55)	5.16 (1.44)
Married	0.476 (0.689)	3.18 (1.40)	0.621 (0.706)	3.12 (1.40)	0.527 (0.660)	3.10 (1.40)
College educated	1.82 (0.49)	9.28 (1.36)	1.77 (0.51)	8.57 (1.38)	1.89 (0.54)	8.54 (1.37)
Age	0.0467 (0.0434)	0.172 (0.117)	0.0304 (0.0475)	0.191 (0.117)	0.0319 (0.0479)	0.192 (0.117)
Total earnings/10 ⁵	4.00 (0.97)	5.65 (1.04)	3.49 (0.94)	5.52 (1.04)	3.38 (0.97)	5.46 (1.04)

^a Standard errors are in parentheses. For median regressions, standard errors are bootstrapped based on 1000 replications. All regressions are based on 1869 observations.

adoption, converging eventually to the upper benchmark (see the curve labeled ‘transition path’ in Fig. 1). Provided that we control properly for ‘years since mandate,’ we do not expect to find any relation between saving rates and ‘exposed to mandate.’ As we will see, the data are consistent with this prediction.

The first two columns of Table 4 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. According to Eq. (1), self-reported saving rates were 1.5 percentage points higher for those entering the affected high school grade 5 years after the imposition of a mandate, than for those who were not exposed to a mandate. According to Eq. (2), the saving rates of individuals from the first group tend to be roughly 4.15 percentage points higher in the population distribution than the saving rates of individuals in the second group. Other findings in columns (1) and (2) are unsurprising.²⁸

²⁸In some instances, one must interpret the coefficients with care. For example, the small age coefficient may at first seem inconsistent with the standard observation that rates of saving rise with age. Recall, however, that the regression controls for earnings. Saving rates need not rise with age if earnings remain constant.

Notably, the coefficients of ‘state ever imposed mandate’ are statistically insignificant. This implies that systematic differences in saving rates across states do not appear until after mandates are imposed. As in Section 4, our estimates are largely unaffected by the inclusion of a ‘years before mandate’ variable, the estimated coefficient of which is consistently insignificant.²⁹ These findings provide additional evidence 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.

Eqs. (3) and (4) of Table 4 are identical to (1) and (2), except that they also include the ‘frugal parents’ dummy variable discussed in Section 4. 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 raises an intriguing possibility. Suppose that the children of frugal parents save more because they receive some form of financial instruction at home. For such children, formal financial education might be redundant, and therefore relatively ineffective. To investigate this possibility, we estimate a specification (Eqs. (5) and (6)) containing interactions between ‘years since mandate’ and two dummy variables indicating, respectively, whether the respondent’s parents saved more than average (‘frugal parents’) or not more than average (‘parents not frugal’). The effects of curriculum mandates on respondents whose parents were not frugal are substantially larger than the ‘average’ effects in Eqs. (1) through (4). Moreover, as predicted, there is no indication that the children of frugal parents materially altered their behavior in response to curriculum mandates.

Next we explore the robustness of our results, treating Eqs. (5) and (6) of Table 4 as our preferred specifications. For Eqs. (1) and (2) in Table 5, we add the variable ‘exposed to mandate’ to test for an immediate effect. As expected, the coefficients for this variable are statistically insignificant,³⁰ and the estimated effects of the key explanatory variable (years since mandate * parents not frugal) change relatively little. We obtain similar results (omitted) when we interact ‘exposed to mandate’ with ‘frugal parents’ and ‘parents not frugal.’ For Eqs. (3) and (4), we abandon the assumption of linearity in the ‘years since mandate’ variable, and estimate separate effects on individuals exposed within the first 3 years after the mandate, within the fourth through sixth years after the mandate, and more than 6 years after the mandate.³¹ Given that only 10% of our sample was

²⁹When ‘years before mandate’ is added to Eq. (1), its coefficient is 0.014 with a standard error of 0.103; when it is added to Eq. (2), its coefficient is 0.101 with a standard error of 0.328.

³⁰This 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.

³¹An alternative approach would be to include higher order terms in ‘years since mandate.’ Unfortunately, one cannot learn much about the curvature of the function through this approach in practice since the coefficients of the individual terms tend to be statistically insignificant.

Table 5
Supplemental regressions explaining percentage rates of saving^a

Independent variable	(1) Median	(2) % Rank	(3) Median	(4) % Rank	(5) Median	(6) % Rank	(7) Median	(8) % Rank
Constant	3.71 (1.89)	39.4 (4.9)	3.69 (2.07)	39.4 (4.9)	40.9 (110)	-119 (301)	State effects	State effects
State ever imposed mandate	-0.313 (0.633)	-0.72 (1.74)	-0.163 (0.593)	-0.83 (1.67)	-0.160 (0.584)	-1.46 (1.59)		
Exposed to mandate	0.33 (1.40)	-4.19 (3.67)						
Years since mandate * parents not frugal	0.351 (0.170)	1.68 (0.56)			0.353 (0.099)	1.27 (0.44)	0.354 (0.193)	1.03 (0.55)
1-3 years since mandate * parents not frugal			-0.46 (1.67)	-2.33 (4.17)				
4-6 years since mandate * parents not frugal			2.87 (1.21)	5.67 (5.42)				
7 or more years since mandate * parents not frugal			4.27 (1.84)	12.0 (5.0)				
Years since mandate * frugal parents	-0.052 (0.256)	0.423 (0.587)	-0.015 (0.210)	0.007 (0.509)	-0.184 (0.205)	0.059 (0.510)	0.124 (0.237)	-0.117 (0.590)
Frugal parents	1.92 (0.55)	5.22 (1.44)	1.95 (0.57)	4.93 (1.45)	2.12 (0.56)	5.14 (1.44)	1.18 (0.54)	5.18 (1.45)
Married	0.599 (0.661)	3.09 (1.40)	0.428 (0.692)	3.02 (1.40)	0.557 (0.646)	3.13 (1.40)	-0.160 (0.576)	2.91 (1.42)
College educated	1.86 (0.51)	8.52 (1.37)	1.89 (0.51)	8.50 (1.38)	1.84 (0.49)	8.56 (1.38)	2.29 (0.53)	8.63 (1.40)
Age	0.0296 (0.0466)	0.164 (0.120)	0.0301 (0.0486)	0.167 (0.119)	-2.23 (8.43)	12.9 (23.0)	0.0751 (0.0424)	0.202 (0.123)
Age ²					0.0415 (0.212)	-0.337 (0.583)		
Age ³ /100					-0.022 (0.177)	0.294 (0.487)		
Total earnings/10 ⁵	3.40 (0.96)	5.43 (1.04)	3.45 (0.97)	5.47 (1.04)	3.53 (0.93)	5.47 (1.04)	3.65 (0.93)	5.20 (1.07)

^a Standard errors are in parentheses. For median regressions, standard errors are bootstrapped based on 1000 replications. All regressions are based on 1869 observations.

exposed to a mandate, we necessarily lose considerable precision by subdividing this group. However, the results are still instructive. We find no effect for those exposed within the first 3 years, a substantial effect among those exposed within the next 3 years, and an even larger effect on those exposed in later years. The close correspondence between these estimates and the predicted pattern is striking. For Eqs. (5) and (6), we augment our controls for the baseline variation in saving rates across age by adding age-squared and age-cubed. The coefficients of these variables are individually and jointly insignificant, and other coefficients change very little. We reach identical conclusions even when we add a complete set of age dummies (omitted). For Eqs. (7) and (8), we augment our controls for baseline variation in saving rates across states by adding a collection of dummy variables indicating the state in which the respondent attended high school. The key coefficients (of years since mandate * parents not frugal) do not change much, though precision declines. Still, the associated coefficient is significant at the 93% level in the median regression, and at the 94% level in the percentile rank regression. The state constants are jointly insignificant for both equations, so we are comfortable omitting them.

To some extent, the basic patterns that drive the results in Tables 4 and 5 are visible in the raw data. For rates of saving, average percentile rankings rise from 0.524 among those exposed to mandates within 3 years of adoption, to 0.556 among those exposed within 4–6 years of adoption, to 0.621 among those exposed more than 6 years after adoption. This pattern is almost entirely attributable to individuals whose parents were not frugal (the corresponding percentile rankings are 0.488, 0.568, and 0.650, compared with 0.584, 0.536, and 0.579 among those with frugal parents). Since average age and earnings tend to be lower among those exposed to mandates further from the date of adoption, one would expect to find the opposite pattern if financial education had no effect on behavior.

The average percentile ranking among those not exposed to mandates is actually the same as among those exposed with 4–6 years of adoption (0.556).³² Although this may seem contrary to our results, it occurs because those not exposed to mandates are on average nearly 6 years older, and consequently their median incomes are roughly 12% higher, than those exposed to mandates. All else equal, one would expect them to save at significantly higher rates. The regression results adjust for these differences in age and income.

5.2. Net worth

Response rates are significantly lower for net worth than for rates of saving. Only 55% of the sample provided sufficient information to construct net worth.

³²Note that the average percentile rank for the entire population exceeds 0.5. This results from two considerations: first, we define the percentile rank as the fraction of the sample with *weakly* lower rates of saving; second, 16.9% of the sample reports the lowest rate of saving (zero). Hence, the lowest percentile rank in the sample is 0.169.

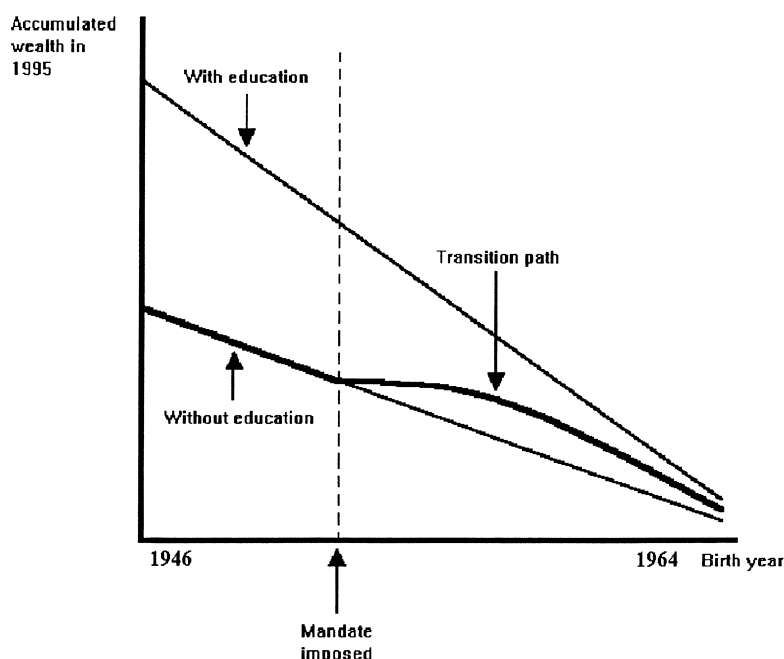


Fig. 2. Hypothesized pattern for accumulated wealth.

For this reason alone, one would 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.³³

Our basic estimation strategy is the same as for saving rates, except that we generally control for ‘exposed to mandate,’ rather than ‘years since mandate.’ We motivate this change as follows. Fig. 2 depicts hypothetical cross-sectional relations between accumulated wealth and birth year. As in Fig. 1, we include two ‘benchmark’ cases, as well as a transition path. The analysis is the same as in the case of saving rates, with one important exception: cross-sectional wealth profiles are rather steeply downward sloped, and converge towards zero for respondents with relatively recent birth years. Even if the transition path converges to the upper benchmark, the extent to which it diverges from the lower benchmark *cannot* increase monotonically with ‘years since mandate.’ Consequently, we 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

³³ 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.

Table 6
Regressions explaining ratio of net worth to earnings^a

Independent variable	(1) Median regression	(2) Percentile rank, OLS	(3) Median regression	(4) Percentile rank, OLS	(5) Median regression	(6) Percentile rank, OLS
Constant	–2.04 (0.73)	9.35 (7.24)	–1.99 (0.67)	5.80 (7.27)	–2.12 (0.71)	5.31 (7.26)
State ever imposed mandate	–0.210 (0.321)	–1.59 (2.65)	–0.305 (0.272)	–2.04 (2.64)	–0.321 (0.279)	–2.13 (2.63)
Exposed to mandate	1.03 (0.46)	9.48 (4.05)	1.16 (0.45)	9.34 (4.04)		
Exposed to mandate * parents not frugal					1.24 (0.50)	14.1 (4.7)
Exposed to mandate * frugal parents					0.325 (0.804)	1.91 (5.64)
Frugal parents			0.692 (0.274)	7.16 (2.07)	0.749 (0.279)	8.51 (2.19)
Married	–0.160 (0.264)	–2.05 (2.11)	–0.248 (0.266)	–1.90 (2.10)	–0.159 (0.268)	–1.87 (2.10)
College educated	0.041 (0.200)	3.19 (2.03)	–0.116 (0.206)	2.19 (2.04)	–0.129 (0.204)	2.36 (2.04)
Age	0.0835 (0.0182)	0.943 (0.178)	0.0821 (0.0169)	0.992 (0.178)	0.0847 (0.0175)	0.995 (0.177)
Total earnings/10 ⁵	0.771 (0.428)	5.31 (1.67)	0.793 (0.429)	5.08 (1.66)	0.692 (0.439)	4.96 (1.66)

^a Standard errors are in parentheses. For median regressions, standard errors are bootstrapped based on 1000 replications. All regressions are based on 910 observations.

by the mandate (i.e. by controlling for ‘exposed to mandate’). For reasons discussed below, we take the latter approach.

We present results in Tables 6 and 7. Each of the reported regressions explains the ratio of wealth to earnings, rather than the level of wealth. This choice reflects our expectation that the effects of changes in explanatory variables vary with the household’s resources; proportionality to earnings is intended as an approximation.³⁴ Unfortunately, the wealth-to-earnings ratio is not defined for households with zero earnings, and low values of earnings generate extreme outliers. We handle these problems by dropping observations with household earnings below \$20,000 (40% of the median). Failure to impose any earnings threshold inflates our standard errors substantially; however, our results are not particularly sensitive to smaller variations in the exclusion criterion.

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. Eq. (1) implies that net

³⁴ 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.

Table 7
Supplemental regressions explaining ratio of net worth to earnings^a

Independent variable	(1) Median	(2) % Rank	(3) Median	(4) % Rank	(5) Median	(6) % Rank	(7) Median	(8) % Rank
Constant	-1.99 (0.71)	5.62 (7.26)	-1.99 (0.70)	5.36 (7.26)	56.7 (47.7)	868 (443)	State effects	State effects
State ever imposed mandate	-0.299 (0.298)	-2.11 (2.63)	-0.298 (0.266)	-2.13 (2.63)	-0.168 (0.293)	-1.86 (2.64)		
Exposed to mandate	1.43 (0.88)	20.0 (6.2)			1.23 (0.49)	13.1 (4.8)	0.690 (0.538)	9.76 (5.21)
* parents not frugal								
1-3 years since mandate			1.30 (1.01)	16.3 (6.5)				
* parents not frugal								
4-6 years since mandate			1.73 (1.59)	21.0 (8.3)				
* parents not frugal								
7 or more years since mandate			1.02 (0.80)	6.09 (7.19)				
* parents not frugal								
Exposed to mandate	0.329 (0.912)	7.39 (6.78)	0.294 (0.820)	1.90 (5.64)	0.421 (0.833)	1.13 (5.67)	0.135 (0.756)	-1.47 (5.90)
* frugal parents								
Years since mandate	-0.034 (0.101)	-1.07 (0.74)						
Frugal parents	0.759 (0.282)	8.47 (2.19)	0.763 (0.273)	8.50 (2.19)	0.697 (0.291)	8.64 (2.19)	0.516 (0.275)	7.69 (2.23)
Married	-0.205 (0.264)	-1.84 (2.10)	-0.202 (0.267)	-1.85 (2.10)	-0.182 (0.270)	-1.81 (2.10)	-0.301 (0.257)	-2.47 (2.13)
College educated	-0.113 (0.211)	2.51 (2.04)	-0.112 (0.209)	2.41 (2.04)	-0.164 (0.212)	2.04 (2.04)	0.015 (0.226)	2.03 (2.10)
Age	0.0793 (0.0177)	0.985 (0.177)	0.0795 (0.0173)	0.993 (0.177)	-4.30 (3.65)	-64.4 (34.0)	0.0775 (0.0172)	0.916 (0.182)
Age ²					0.108 (0.092)	1.63 (0.86)		
Age ³ /100					-0.0868 (0.0767)	-1.34 (0.72)		
Total earnings/10 ⁵	0.896 (0.414)	4.97 (1.66)	0.874 (0.416)	4.97 (1.66)	0.893 (0.402)	5.00 (1.66)	0.714 (0.424)	4.76 (1.70)

^a Standard errors are in parentheses. For median regressions, standard errors are bootstrapped based on 1000 replications. All regressions are based on 910 observations.

worth is higher by roughly one-year's worth of earnings for the typical individual who was exposed to a mandate. Eq. (2) implies that the net-worth-to-earnings ratios of those who were exposed to mandates are more than 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 plausible.³⁵

In Eqs. (3) and (4), we add our control for frugal parents. While the key coefficients are largely unaffected, we again find evidence that respondents' savings behavior is strongly correlated with their perceptions of their parents' behavior. Eqs. (5) and (6) measure the effects of curriculum mandates separately for individuals whose parents did and 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 net worth provides considerable support to the view that financial education at school is a close substitute for financial education at home.

Next we explore the robustness of our results, treating Eqs. (5) and (6) of Table 6 as our preferred specification. In Eqs. (1) and (2) of Table 7, we add the 'years since mandate' variable. Its coefficients are slightly negative and statistically insignificant. This is not surprising in light of our comments concerning Fig. 2. Our other findings are largely unaffected, except that we estimate the coefficients of the 'exposed to mandate' interactions with somewhat less precision. We obtain similar results (omitted) when we interact 'years since mandate' with 'frugal parents' and 'parents not frugal.' For Eqs. (3) and (4), we estimate separate effects on individuals exposed within the first 3 years after the mandate, within the fourth through sixth years after the mandate, and more than 6 years after the mandate. The associated loss of precision is more severe than in section 5.1 since we start with roughly half as many observations. Though one cannot reject the hypothesis

³⁵Note that education and marriage have weaker effects on the wealth-to-earnings ratio than on saving rates. The weaker effect of education is plausible. Even if those with more education save higher fractions of earnings, they may accumulate less wealth by any given age. College grads often start out with less wealth because of educational loans. Education also reduces years in the labor force and increases the growth rate of earnings. Consequently, holding current earnings constant, those with more education have typically earned less (cumulatively) in the past. The weaker effect is also plausible for married couples. Among those recently married, accumulated wealth primarily reflects choices made as single individuals. Saving rates may rise sharply with marriage due to a pure marital status effect and due to an earnings effect (marriage combines spouses' earnings, which increases rates of saving), but the impact on assets is gradual. Since our sample is relatively young, these effects may be large.

that the three coefficients are identical in either regression, the results nevertheless exhibit the predicted hump-shaped pattern (recall Fig. 2).³⁶ For Eqs. (5) and (6), we augment our controls for the baseline variation in saving rates across age by adding age-squared and age-cubed. Note that there is practically no change in the other coefficients. With a complete set of age dummies (omitted), the median regression results are a bit weaker – the key coefficient (of ‘exposed to mandate*parents not frugal’) is significant at the 94% confidence level – but the percentile rank results are essentially unchanged. For Eqs. (7) and (8), we augment our controls for baseline variation in saving rates across states by adding a collection of dummy variables indicating the state in which the respondent attended high school. In both regressions, the state constants are jointly significant ($F(47,855)=1.48$ and $F(47,855)=1.45$, respectively). The key coefficient is smaller, but its economic magnitude is still substantial. It remains significant for the percentile rank regression at the 94% level of confidence, but for the median regression, one can be only 80% confident that it is different from zero. Since the state constants consume degrees of freedom that are particularly scarce in the context of net worth, this weakening of statistical confidence is not surprising.

As in Section 5.1, the basic patterns that drive the results in Tables 6 and 7 are to some extent visible in the raw data. The median wealth-to-earnings ratio is 2.12 (average percentile ranking of 0.522) among those exposed to mandates, compared with 1.67 (average percentile ranking of 0.498) among those not exposed. These figures are particularly striking in light of the fact that those exposed are, on average, nearly 6 years younger than those not exposed. To illustrate, confining attention to individuals attending high school in states that never imposed mandates, the median wealth-to-earnings ratio is 1.33 (average percentile ranking of 0.454) among 34 to 40 year olds, and 1.88 (average percentile ranking of 0.542) among 41 to 46 year olds. The differences between the exposed and unexposed groups are almost entirely attributable to those whose parents were not frugal. For this group, the median wealth-to-earnings ratio is 2.15 (average percentile ranking of 0.535) among those exposed to mandates, compared with 1.48 (average percentile ranking of 0.474) among those not exposed. In contrast, for respondents with frugal parents, the median wealth-to-earnings ratio is 1.79 (average percentile ranking of 0.500) among those exposed to mandates, compared with 2.31 (average percentile ranking of 0.559) among those not exposed.

³⁶The existence of an effect on net worth among those exposed within 3 years of adoption seems somewhat inconsistent with the absence of an effect on rates of saving for this same group (see Table 5). It is therefore worth emphasizing that the results in Table 5 do not permit one to reject the hypothesis that exposure increases rates of saving among this group.

5.3. Additional findings

We have performed a variety of robustness checks that are not reported in Tables 4–7. Our central findings are not affected by the inclusion of a wider range of socio-economic controls, higher order terms in earnings, or dummy variables for state of current residence. We looked for possible interactions between financial education and other characteristics such as gender, ethnicity, education, and income, but did not detect any robust patterns. We restricted the sample by dropping individuals who attended high school in states that never adopted financial curriculum mandates. Results for saving rates were largely unchanged. For the wealth-to-earnings ratio, percentile rank regression also yielded similar results, while the key coefficients for the median regression resembled those obtained when state constants were included. We found that we obtained stronger results in the saving rate regressions when we truncated ‘years since mandate’ at 10 years. This suggests that mandates may achieve their full effect within a 10-year time frame. There is some indication that exposure to financial curriculum mandates is associated with a higher probability that a household has positive net worth (the estimated effect is significant at the 90% level of confidence). Though we attempted to determine whether financial education affects some classes of assets more than others, our estimates were inconclusive.

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 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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