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Asking For Help: Survey and Experimental Evidence on Financial Advice and Behavior Change

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Asking For Help: Survey and Experimental Evidence on Financial Advice and Behavior Change

Abstract

When do individuals actually improve their financial behavior in response to advice? Using survey data from current defined contribution (DC) plan holders in the RAND American Life Panel (ALP), we find little correlation between normatively-desirable behaviors and advice. Results from a hypothetical portfolio-allocation choice experiment using the ALP show that unsolicited advice has no causal effect on investment behavior, yet individuals who actively solicit advice ultimately improve performance, despite negative selection on financial ability. While expanding access to advice can have positive effects (particularly for the less financially literate), more extensive compulsory programs of financial counseling may be less effective.

Keywords

Financial advice, advisors, portfolio choice, defined-contribution plans, choice experiments

Disciplines

Economics

Comments

The published version of this Working Paper may be found in the 2013 publication: *The Market for Retirement Financial Advice*.

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The Market for Retirement Financial Advice

EDITED BY

Olivia S. Mitchell
and Kent Smetters

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Chapter 9

Asking for Help: Survey and Experimental Evidence on Financial Advice and Behavior Change

Angela A. Hung and Joanne K. Yoong

As US policymakers focus on the difficult problem of increasing access to unbiased financial advice in the context of self-directed retirement plans, a key question is whether implementing potentially costly reforms and regulations is likely to bring about the desired changes in behavior. This chapter addresses two research questions: do individuals actually improve their financial behavior in response to advice? And, if policymakers could enhance the availability of neutral financial advice, would participants actually seek and implement the advice given?

We present two complementary observational and experimental analyses of investors and advice, relevant to 401(k) plans. In observational analyses, we observe actual investment outcomes from real planholders, but our inferences about the effect of advice are limited by two problems—self-selection into advice, and reverse causality—that are not mutually exclusive. In experimental analyses, we are restricted to hypothetical investment choices, but we can eliminate both selection and reverse causality. By comparing and contrasting our results, we are able to draw on implications from both approaches.

For policymakers, our lessons about advice are mixed. One key implication is that having employers offer advice as an elective and ensuring employees' active decision-making are likely to result in significant take-up and some improvement of financial outcomes. Moreover, employees with low financial literacy are more likely to take advantage of these programs. Yet going further, to make the provision of advisory services mandatory for every employee, may be extremely costly and achieve little behavioral change. Furthermore, in some situations, policymakers have recommended compulsory financial counseling as a remedy; our results suggest this may not work unless recipients are prepared to take the advice. In general, motivation is extremely challenging.

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Background

Interactions between individual investors and financial advisors have changed considerably over the last few decades, as financial service providers have expanded their range of services and individuals have taken on greater responsibility for their own financial well-being. At year-end 2011, Americans held an estimated $9.4 trillion in employer-sponsored defined contribution (DC) plans and Individual Retirement Accounts (IRAs) (ICI, 2012). Yet research in behavioral finance suggests that when left to their own devices, investors often fail to make optimal investment decisions in their DC plans. Instead, they tend to use heuristics or simple decision rules to make their initial allocation decisions (Samuelson and Zeckhauser, 1988; Benartzi and Thaler, 2001; ICI, 2001; Hewitt Associates, 2004; Agnew and Szykman, 2005). Investing 'mistakes' and simple rules of thumb can have significant welfare implications, given that households may not invest according to optimal portfolio choice theory (Dominitz and Hung, 2008).[1] Some of these 'mistakes' may be attributed to individuals' lack of financial management skills: Lusardi and Mitchell (2006, 2007) argue that investing is a complex undertaking that requires consumers to gather, process, and project data on compound interest, risk diversification, and inflation, as well as to accumulate knowledge of the asset universe. Their findings suggest that most of the US population is not sufficiently financially literate to cope with the shifting burden of post-retirement planning to the individual.

In theory, financial advisors could ameliorate the negative consequences of differential financial literacy, improving returns and ensuring greater risk diversification among less sophisticated households (Hackethal et al., 2012; Hackethal and Inderst, 2013). Indeed, using advisors allows households to benefit from economies of scale in portfolio management and information acquisition, because advisors can spread these costs among their clients. But consumer advocates argue that investors who are unprepared to make sound decisions may also be most vulnerable to bad advice from affiliates of broker-dealers or investment companies who benefit from advising them to buy unsuitable products (Hung et al., 2008). Also, people who consult advisors but do not follow through might not benefit from good advice, as their knowledge may not translate into actual behavioral change.

Much attention has been paid to the pitfalls of bad investment advice. The theoretical and empirical economics research literature on investment advice has largely been concerned with moral hazard problems inherent in the advisor–advisee relationship (see, e.g., Liu, 2005; Inderst and Ottaviani, 2009; Yoong and Hung, 2009; Hackethal et al., 2012; Turner and Muir, 2013). Similarly, regulatory and legislative debate related to self-directed pension plans in the United States has focused on how best to mitigate the risk of exposing unsophisticated plan participants to manipulation while

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still allowing access to advice. The Pension Protection Act (PPA) of 2006 facilitated provision by granting exemptions to DC plan providers under level-fee compensation arrangements or for advice given by an unbiased computer model.

Nevertheless, and perhaps surprisingly, relatively little information exists about whether good investment advice actually works. Although regulators and legislators are deeply engaged in efforts to make financial advice more accessible to the everyday investor in a neutral setting, the practical benefit in terms of behavior change of achieving such a policy goal should not be regarded as a foregone conclusion. Indeed, there is remarkably little empirical evidence about individual responsiveness to financial advice outside an environment with moral hazard.

A long-standing literature on general advice-taking and receiving is rooted in psychology and organizational behavior. Findings on the propensity to seek advice are mixed and highly context dependent: studies find results that vary from resistance to advice-seeking, even if it is free (Gibbons, 2003), or nearly universal advice-seeking (Gino, 2008). Uncertainty about decisions, however, is found to predict advice-seeking (Gibbons et al., 2003). Although it is difficult to draw conclusions about when individuals seek advice, the research literature strongly suggests that people who do solicit advice are more likely to follow that advice, compared to those who receive unsolicited advice (Gibbons et al., 2003). Indeed, a robust finding is that individuals who receive advice by default tend to significantly discount it (Yaniv and Kleinberger, 2000; Yaniv, 2004a, 2004b; Bonaccio and Dalal, 2006). While explicitly solicited advice is perceived as helpful, unsolicited advice or imposed support is perceived as intrusive and can even lead to negative responses (Goldsmith and Fitch, 1997; Goldsmith, 2000; Deelstra et al., 2003). In a similar vein, Gino (2008) shows that individuals are significantly more receptive to advice they pay for, rather than advice they get for free.

Few empirical analyses specifically address the context of investment advice, particularly in a representative population. Much of the psychology-based evidence has been gathered in a laboratory using tasks unrelated to investment management. Moreover, it is difficult to extrapolate from the experimental laboratory-based literature on financial incentives, as the results are mixed: Sniezek and Van Swol (2001) and Sniezek et al. (2004) find that financial incentives decrease advice discounting, but, by contrast, Dalal (2001) finds the opposite. In the economics literature, evidence suggests that although investors often say they desire more advice, it is unclear how and when they implement the advice given (Helman et al., 2007). Furthermore, since investors actively choose whether to seek advice, correlations between actual behavior and advice may be the result of self-selection: people particularly prone to certain types of investing behavior

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may also be more likely to seek out advisors. Hackethal et al. (2012) find that self-selection largely explains better outcomes for advisees in the context of German internet brokerage accounts, and they suggest a theory of 'babysitters,' in which wealthy individuals outsource their financial management to others. Yet those online brokerage clients are likely to represent a population with experience and objectives that differ from the average US DC planholder. Kramer (2012) also finds that portfolio allocations of Dutch investors vary with advice, but performance does not. While some behavior in 401(k) plans such as trading activity has been found to be correlated with advice (Agnew, 2006), a causal relationship has not yet been well established.

Study setting

The primary data collection instrument for our investment behavior analyses was a survey administered to 2,224 members of the RAND American Life Panel (ALP).[2] Our behavior survey was administered as wave MS73 of the ALP between May 5, 2009, and June 22, 2009. For a subset of 1,467 individuals, we can match our behavior survey to a previous module on financial literacy used to compute an index of measured financial literacy as well as an index of self-assessed financial literacy. The first index is computed from respondents' answers to questions related to basic numeracy skills and knowledge of investments, retirement plans, and insurance, while the second is based on respondents' own judgments about their abilities. Details of the construction of the indexes can be found in Hung et al. (2009).

Individuals from our panel who reported being enrolled in a current employer's DC plan were asked whether they had consulted a financial advisor for individual recommendations regarding their DC plan. Table 9.1 shows the descriptive statistics of sample size and weighted demographic composition for this group of 618 individuals.

The propensity to seek advice

In 2008, 18 percent[3] of employees in our sample with DC plans consulted an advisor. The breakdown by demographic composition shows that proportionally, more women and minorities consulted an advisor in 2008 regarding their DC plan. Older, more educated and wealthier individuals were also more likely to have consulted an advisor. We estimate a linear probability (LP) model with reported advice-seeking in 2008 as the binary outcome variable. In the LP model, the coefficients may be interpreted as the best linear predictors (BLP) of changes in the probability of the outcome associated with a unit change of each regressor.[4] Column 1 of

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Table 9.1 Summary statistics: American Life Panel (ALP) data

	Number of responses
Total ALP sample	2,224
Retired	498
Self-employed	185
Unemployed	287
Employer offers no retirement plan	293
Employer offers no DC plan or unknown	209
Employee is ineligible for plan	43
Employee is eligible but not enrolled	86
Missing/inconsistent status	5
Final sample: currently enrolled in DC	618
	Weighted %
Female	48.7
College degree	45.7
Married	65.4
Age $\leq$45	41.2
Annual family income (AFI) <$50,000	23.1
Black or Hispanic	18.7

Note: The sample consists of respondents to our ALP module who are current participants in DC plans (see text).

Source: Authors' computations; see text.

Table 9.2 shows that while the regression coefficients mirror the pattern observed in the summary statistics, among the various demographic characteristics, there are few statistically significant individual predictors of actual reported advice-seeking in 2008 apart from marital status.

The relationship between advice and reported DC plan behavior

Most DC planholders continued actively making contributions to their plans in 2008, and slightly more than half of plan assets were held in stock. But a large fraction of respondent portfolios featured at least one common investment mistake. Although respondents report that they most value advice for investing purposes, use of an advisor was not strongly related to investment portfolio quality.

When asked about the value of advice in a DC plan setting, most respondents (57 percent) placed the highest value on advice related to asset allocation. About one-third considered setting overall contribution goals most valuable, while only about one-quarter placed similar value on advice related to future planning such as tax and estate planning or decumulation.

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TABLE 9.2 Ordinary least squares estimates of the determinants of the propensity to seek advice

	(1)	(2)	(3)	(4)
Married	0.087**	0.036	0.035	0.071*
	(0.044)	(0.050)	(0.048)	(0.042)
Female	0.062	0.067	0.064	0.066
	(0.044)	(0.051)	(0.050)	(0.043)
Age <40	–0.020	0.059	0.059	–0.004
	(0.044)	(0.056)	(0.056)	(0.042)
AFI <$50,000	–0.039	–0.027	–0.029	–0.024
	(0.051)	(0.065)	(0.068)	(0.050)
Black or Hispanic	0.073	–0.014	–0.015	0.064
	(0.079)	(0.077)	(0.077)	(0.076)
College degree	0.024	0.012	0.015	0.012
	(0.043)	(0.051)	(0.047)	(0.043)
Measured financial literacy		–0.003		
		(0.034)		
Self-assessed financial literacy			–0.008	
			(0.033)	
Net plan losses (2008)				0.132**
				(0.055)
Constant	0.085*	0.108*	0.130	
	(0.048)	(0.056)	(0.113)	
N	590	450	450	590
R^2	0.02	0.01	0.01	0.04

Note: Standard errors are in parentheses. Statistical significance of differences are within the two categories, where * indicates significant at the 10 percent level, ** is significant at 5 percent, and *** is significant at 1 percent. See Table 9.1.

Source: Authors' calculations using the American Life Panel 2009; see text.

This suggests that many respondents look to advisors for specific tasks related to investment management, rather than larger retirement plan management issues. We therefore focus primarily on asset allocations throughout this work and also describe contributions behavior briefly below.

Table 9.3 shows that on average, 55 percent of DC plan assets are held in stock, 20 percent in bonds, and 20 percent in money market funds, with the remaining 4 percent in other assets. Women, Blacks, and Hispanics hold less stock, as do the less educated, older, and less wealthy respondents. Following Mottola and Utkus (2009), we diagnose portfolio 'mistakes' based on commonly accepted principles of investment. These 'mistakes' are defined as follows: (*a*) holding a zero balance in equities, (*b*) holding an equity balance of less than 40 percent (overly conservative), (*c*) holding more than 95 percent equity (overly aggressive), and (*d*) holding a portfolio that is 100 percent in a single asset class (under-diversified). More than half (56 percent) of respondents' portfolios are characterized by these

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TABLE 9.3 Portfolio allocation patterns of current defined contribution (DC) planholders

Panel A. Portfolio characteristics

	Stocks (%)	Bonds (%)	Money market (%)	Other (%)	*N*
Male	60.0	18.0	19.1	2.9	503
Female	49.9	24.1	21.8	4.2	503
No college degree	52.3	21.4	22.8	3.5	503
College degree	58.5	20.4	17.6	3.6	503
Age <45	59.1	19.6	17.8	3.4	503
Age ≥45	52.4	21.8	22.2	3.6	503
AFI <$50,000	48.5	19.2	28.1	4.1	503
AFI ≥$50,000	56.8	21.3	18.5	3.4	503
Black or Hispanic	52.0	16.8	28.2	3.0	503
Total	55.2	20.9	20.4	3.5	503
No advisor	55.5	20.4	20.4	3.7	478
Advisor	52.5	24.0	20.0	2.6	478

Panel B. 'Mistakes' in reported portfolio allocations

	Zero equity (%)	Under diversified (%)	Too aggressive (%)	Too conservative (%)	*N*
Male	6.4	25.6	22.9	26.2	503
Female	12.4	30.1	20.7	42.2	503
No college degree	11.4	34.0	24.8	40.1	503
College degree	6.8	20.5	18.4	26.7	503
Age <45	6.5	26.5	22.9	31.3	503
Age ≥45	11.3	28.6	21.1	35.7	503
AFI <$50,000	13.1	27.7	17.5	40.9	503
AFI ≥$50,000	8.3	27.7	22.9	32.1	503
Black or Hispanic	8.2	29.6	25.1	43.0	503
Total	9.3	27.7	21.8	33.9	503
No advisor	9.2	27.3	22.1	33.9	478
Advisor	7.8	23.6	17.1	36.6	478

Note: See Table 9.1.

Source: Authors' computations using the American Life Panel 2009.

'mistakes.' Women tend to be more conservative, holding less equity, and they tend to be less well diversified. Indeed, more than 12 percent of female respondents hold no equity at all. Similarly, older, less wealthy, and less educated individuals hold no stocks, precluding longer-term asset growth. Table 9.3 also shows that individuals who use advisors invest less in stocks and more in bonds, and they also hold fewer assets outside the category of stocks, bonds, and money market funds. They tend not to be

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very aggressive, and they are less or equally likely to hold zero equities. Also, they are prone to being too conservative.

To examine the magnitude and significance of differences while controlling for potential demographic effects, we use ordinary least squares (OLS) regression to estimate a set of equations of the form:

$$Y_i = \alpha + \beta \text{ advice}_i + X_i'\delta + \epsilon \quad (9.1)$$

using alternative behaviors of interest as the outcome variables *Y*. In addition to a vector of demographic characteristics, *X*, we now include an indicator for advice as an explanatory variable. When the behavior of interest is a binary variable, the results are interpreted as an LP model, as before.

The multivariate regression (Table 9.4) yields results very similar to the trends in the summary tables, consistent with the finding that these demographic characteristics do not generally predict actual reported advice-seeking in 2008. Advice is not statistically significantly predictive of allocation levels or investment 'mistakes' within portfolios.

Contributions behavior also shows a mixed relationship to advice. In this sample, 88 percent of respondents eligible to contribute to their DC plan in 2007 and 2008 reported making a contribution and the average percentage contribution is above 7 percent, although this average is skewed by a small number of extremely high reported percentages. The median and modal value of the distribution is 5 percent. Twenty-two percent of respondents reported increasing their contributions since 2007, and 80 percent of those who were offered an employer match met the match amount. On the other hand, 9 percent reported either decreasing or stopping contributions in 2008, and 9.6 percent took an early withdrawal.[5] Both simple tabulations of the data and similar multivariate regression analysis reveal that individuals with advisors were somewhat more likely to make contributions in 2007 and 2008, and to meet their employer match. However, the data also show that those who received advice were more likely to have reduced their 2008 contribution relative to 2007.[6]

Does self-selection on financial literacy explain the advice–behavior relationship?

Researchers have argued that financial literacy is a key unobservable characteristic that often complicates analyses of advice, and vice versa. Theoretical arguments about the relationship between advice and behavior go in both directions. If financial literacy substitutes for advice and the least financially literate are more likely to take up advice, differences in observed behavior may understate the positive impact of advice. Conversely, if the

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TABLE 9.4 Ordinary least squares parameter estimates

Panel A. Empirical determinants of portfolio allocations by DC planholders

	(1) Stocks	(2) Bonds	(3) Money market	(4) Other
Consulted advisor in 2008	–2.226	4.233	–0.772	–1.234
	(5.424)	(3.311)	(3.840)	(1.190)
Married	3.065	–4.002	1.211	–0.274
	(5.469)	(3.871)	(4.839)	(1.461)
Female	–6.666	4.352	1.387	0.927
	(4.572)	(2.860)	(3.638)	(1.341)
Age <45	6.027	–1.365	–4.697	0.035
	(4.107)	(2.554)	(3.193)	(1.279)
AFI <$50,000	–2.846	–3.752	6.939	–0.340
	(5.437)	(3.610)	(5.080)	(1.703)
Black or Hispanic	–4.578	–6.041	10.673**	–0.055
	(6.070)	(3.739)	(5.364)	(2.028)
College degree	7.021*	–1.205	–5.999*	0.183
	(4.201)	(2.842)	(3.569)	(1.393)
Constant	51.766***	23.732***	21.045***	3.456*
	(7.254)	(5.134)	(6.423)	(1.852)
N	478	478	478	478
R^2	0.05	0.03	0.05	0.00

Panel B. 'Mistakes' in reported portfolio allocations

	(1) Zero equity	(2) Under-diversified	(3) Too aggressive	(4) Too conservative
Consulted	–0.012	–0.041	–0.057	0.007
advisor in 2008	(0.037)	(0.071)	(0.072)	(0.079)
Married	–0.015	0.030	0.038	–0.038
	(0.051)	(0.074)	(0.069)	(0.072)
Female	0.041	0.075	0.023	0.136**
	(0.037)	(0.062)	(0.061)	(0.061)
Age <45	–0.049	–0.010	0.025	–0.029
	(0.031)	(0.055)	(0.054)	(0.059)
AFI <$50,000	0.021	–0.005	–0.028	0.011
	(0.056)	(0.073)	(0.064)	(0.078)
Black or Hispanic	–0.051	–0.036	0.022	0.129
	(0.045)	(0.081)	(0.081)	(0.099)
College degree	–0.027	–0.101*	–0.047	–0.165***
	(0.036)	(0.058)	(0.055)	(0.058)
Constant	0.120*	0.277***	0.200**	0.373***
	(0.063)	(0.103)	(0.100)	(0.084)
N	478	478	478	478
R^2	0.02	0.02	0.01	0.06

Note: See Table 9.2.

Source: Authors' computations, using the American Life Panel 2009.

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most financially literate are more likely to take up advice, as proposed by Hackethal et al. (2012), differences in observed behavior may overstate this impact. Controlling for selection on observables using only formal education and experience as proxy variables for financial literacy may not adequately resolve this problem, as Dominitz et al. (2008) show that financial literacy has strong effects independent of both. Hackethal et al. (2012) go further by using an instrumental variables strategy to overcome this issue. In our study, we use measures of financial literacy to explicitly control for selection of this type, which is a major advantage. Our results suggest that self-selection on financial literacy plays no significant role.

To illustrate this, we first re-estimate the LP model from Table 9.2, but now we add financial literacy as an additional regressor (albeit on the smaller sample for which the financial literacy measure is available). Results in Table 9.5 provide no evidence of positive selection on financial literacy. Although having lower financial literacy is somewhat related to advice-seeking, the estimated relationship is very small and not significantly predictive. This is true of both measured financial literacy and self-assessed financial literacy. Further non-parametric analysis using a Lowess curve smoother shows a somewhat negative relationship between measured financial literacy and advice-seeking but the result is highly skewed by a few outliers; there is no discernible trend in the relationship between self-assessed financial literacy and advice-seeking. Consistent with this, re-estimating Equation (9.1) while controlling for financial literacy[7] in the regressions of behavioral outcomes on advice has little effect on the estimated coefficients, be they reported allocations or investment mistakes. Again, this result is robust to the use of both measured financial literacy and self-assessed financial literacy.

Discussion: potential reverse causality and selection on other unobservables

Our results show that although individuals believe that advice is important for investing, there appears to be no systematic, statistically significant relationship between advice and observed investment behavior. Moreover, individuals who consulted advisors were more likely to have reduced their contribution levels and were also more likely to continue making contributions. This apparently contradictory pattern of behavior suggests the presence of reverse causality: individuals who experienced unusual stress and negative plan performance may have turned to advisors.

To explore this possibility, we also examined year-end plan balances in 2008 as well as net changes in plan balances between 2007 and 2008. Our

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TABLE 9.5 Portfolio allocations and financial literacy: ordinary least squares estimates for current DC planholders

Panel A. Reported portfolio allocations and financial literacy (current DC planholders)

	(1) Stocks	(2) Bonds	(3) Money market	(4) Other
Demographic controls only				
Consulted advisor in 2008	0.692	2.285	–2.519	–0.016
	(6.948)	(4.125)	(4.808)	(0.045)
Financial literacy controls:				
Specification I:				
Consulted advisor in 2008	0.639	2.285	–2.463	0.639
	(6.938)	(4.122)	(4.790)	(6.938)
Measured financial literacy	–1.313	0.004	1.379	–1.313
	(3.773)	(2.486)	(3.531)	(3.773)
Specification II:				
Consulted advisor in 2008	0.756	2.299	–2.567	–0.017
	(6.920)	(4.096)	(4.797)	(0.045)
Self-assessed financial literacy	1.086	0.245	–0.807	–0.010
	(3.365)	(2.008)	(2.487)	(0.025)

Panel B. 'Mistakes' in reported portfolio allocations and financial literacy

	(1) Zero equity	(2) Under-diversified	(3) Too aggressive	(4) Too conservative
Demographic controls only				
Consulted advisor in 2008	–0.016	–0.025	–0.024	–0.027
	(0.045)	(0.088)	(0.090)	(0.094)
Financial literacy controls:				
Specification I:				
Consulted advisor in 2008	–0.015	–0.027	–0.028	–0.028
	(0.045)	(0.088)	(0.089)	(0.094)
Measured financial literacy	0.039	–0.055	–0.103**	–0.024
	(0.037)	(0.053)	(0.046)	(0.050)
Specification II:				
Consulted advisor in 2008	–0.017	–0.027	–0.026	–0.029
	(0.045)	(0.086)	(0.088)	(0.094)
Self-assessed financial literacy	–0.010	–0.046	–0.035	–0.033
	(0.025)	(0.045)	(0.046)	(0.041)
N	360	360	360	360

Note: Other demographic controls are also included but estimates not shown. See also Table 9.2.

Source: Authors' calculations from the American Life Panel 2009.

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results (not detailed here) show that, indeed, individuals who experienced net plan losses were more likely to consult advisors, but that even accounting for financial literacy and income levels, consulting an advisor in 2008 had a marginally significant and positive effect on overall plan balances (and the point estimate is sizable). This reinforces the possibility that seeking advice may actually ultimately help in preserving wealth, but negative events tend to influence advice-seeking (rather than vice versa).

Another complication in establishing causality between advice and behavior is the possibility of selection on a wide variety of unobservable factors other than financial literacy. ALP respondents report diverse reasons for not consulting an advisor (individuals were allowed to indicate only one response). Thirty-seven percent of individuals felt they could make their own decisions (in other words, saw themselves as financially literate enough to forego advice), while 39 percent also cited the availability of various substitutes for professional advice, either from friends or family or other sources such as the Internet. A significant minority (one-quarter) did not do so because of financial constraints. This heterogeneity is consistent with our previous results, in which financial literacy (or the lack thereof) is correlated with advice-seeking but not an overwhelmingly dominant explanatory factor.

Experimental evidence on advice and behavior

In the case of survey data, it is not possible for us to rule out either reverse causality or selection on unobservables.[8] Without a plausibly exogenous and predictive source of variation in advice-seeking and given the issues described above, we cannot cleanly identify the causal impact of receiving advice on behavior. We therefore turn to an experimental analysis of advice and behavior. Given the focus on investment advice uncovered in the survey data, our experiment was designed to test the effect of receiving investment advice on portfolio allocation. While we are restricted to an analysis of hypothetical outcomes, we do have two key advantages: the advice provided is uniform, and reverse causality cannot arise. We designed a multi-stage randomized experiment in which participants were presented with a hypothetical portfolio allocation task. Participants were presented with six investment options: a money market fund, a bond market index fund, an S&P 500 index fund, a small cap value index fund, a REIT index fund, and a global equity index fund. Participants received basic information on the funds, namely fees and returns, and were then allocated a hypothetical portfolio among the funds.

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Choice treatment: defaults and affirmative decisions

Participants were randomly assigned to either a control group or one of two experimental conditions. In all conditions, participants were informed that they would be asked to allocate an investment portfolio. The control group received no further information or support before performing the task. In one treatment, which we termed the *default treatment*, all participants received advice regarding optimal portfolio allocation. In the other treatment, the *affirmative decision treatment*, participants were given a choice and received advice only if they asked for it. These experiments were designed to allow us to study the effects of solicited versus unsolicited advice as well as self-selection into advice.

Financial environment treatment: low/high past returns

Previous research has shown that individual investor responses are very sensitive to reported past performance (Sirri and Tufano, 1998; Zheng, 1999). To see whether advice can mitigate this sensitivity, we varied the historical returns shown in the portfolio allocation task. For a randomly selected half of all respondents (the *low-returns treatment*), we presented returns for the various asset classes representative of typical fund performance over the last year, while in the other half (the *high-returns treatment*), we presented returns representative of performance over the last five years, which were significantly less negative.

Advice presentation

We presented advice about normatively desirable investing rules first proposed by Mottola and Utkus (2009). These rules are based on commonly accepted principles of investment as follows: (*a*) a zero balance in equities is not recommended, (*b*) an equity balance of less than 40 percent is considered overly conservative, (*c*) holding more than 95 percent equity is considered overly aggressive, and (*d*) a portfolio that is 100 percent in a single asset class may be under-diversified. In this analysis, we focus on the general results of advice, rather than the format, although we note that half the participants who received advice (whether by default or by choice) randomly received the *rules treatment*, in which participants were presented with the set of simple investing rules or guidelines in table form. The remaining half was assigned to the *portfolio checkup treatment*. We designed an interactive mechanism that provided feedback to participants after they enter a suggested allocation. A Portfolio Checkup Tool evaluates the allocation and compares it to the set of rules. A 'Green' signal was given if the

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portfolio did not violate any guidelines, while a 'Yellow' or 'Red' signal was given if the portfolio's allocation went against less or more stringent rules. Figure 9.1 shows samples of the task description for the control group and treatment groups.

Experimental sample and summary statistics

All 2,224 panel members of the ALP, regardless of plan status, were invited to participate in the experiment; 2,070 respondents completed the experiment. Table 9.6 shows the sample after accounting for missing responses, as well as the demographic composition for the final sample. Note that for the experimental analyses, we do not apply population weights to the analyses. Figure 9.2 shows a full schematic representation of the randomized experiment and probability of assignment for each treatment group.[9] For the present analysis, we focus attention on the choice treatments,[10] and a discussion of the other randomized treatments is held for future work. As a check on the randomization, we tabulated the number of individuals and summary statistics for the sample of the choice treatment groups, to identify any remaining differences across groups that need to be acknowledged and accounted for in later analysis. Results suggest that the randomization achieves a reasonable balance across the treatment groups in terms of observables, with two possible exceptions: firstly, a skew toward younger individuals in the 'affirmative decision' treatment versus the 'default' treatment and second, a skew toward DC plan enrollees in the control treatment versus the advice treatments. We therefore include appropriate demographic controls in our multivariate regression analysis.

Who chooses advice? Self-selection and financial literacy

About 65 percent of individuals in the 'affirmative decision' treatment group elected to receive advice. Accordingly, two observations are worth noting: first, not all individuals chose to receive advice and second, individuals did not appear to be randomly choosing to receive advice or not (as 50 percent lies outside the 95 percent confidence interval). The relative demographic composition of those who did solicit advice is quite different from those who did not. In the 'affirmative decision' treatment, there were clear and significant differences along age and wealth—those who chose advice were more likely to be older and wealthier than those who did not. For the subsample with financial literacy data, we also test for differences on financial literacy. Unlike the observational data, however, the bottom panel of Table 9.6 also shows strong evidence of selection on financial literacy. Those who chose advice were significantly less financially literate.

In this section, we have some questions for you about possible investments in mutual funds. A mutual fund is a form of investment that pools money from many investors and invests their money in stocks, bonds, and/or other securities.

Suppose you were offered the following selection of mutual funds for investing your money in a retirement savings account(s). Below is a table that provides a brief description of the mutual funds, showing the annual fee charged by each fund and the annual rate of return on each fund over the past 5 years.
Suppose you have six options in which to invest.

Fund Choices	Fees	5 Year Return
Money Market Fund	0.21%	3.28%
Total Bond Market Index Fund	0.20%	4.56%
S&P 500 Index Fund	0.18%	−2.29%
Small Cap Value Index Fund	0.23%	−0.76%
REIT Index Fund	0.21%	0.77%
Global Equity Index Fund	0.72%	−0.24%

On the next screen, we'll ask you what percentage of your money you would like to allocate to each fund. Would you like to get some general advice while making these choices?

○ Yes

○ No

Please indicate the percentage of your portfolio that you would like to hold in each of the following funds.

My Portfolio Choices	%	Fees	5 Year Return
Money Market Fund		0.21%	3.28%
Total Bond Market Index Fund		0.20%	4.56%
S&P 500 Index Fund		0.18%	−2.29%
Small Cap Value Index Fund		0.23%	−0.76%
REIT Index Fund		0.21%	0.77%
Global Equity Index Fund		0.72%	−0.24%
% Total			

General Advice for Investing
1) A zero balance in equities is not recommended.
2) An equity balance of less than 40% is considered overly conservative.
3) Holding more than 95% equity is considered overly aggressive.
4) A portfolio that is 100% in a single asset class may be underdiversified.

When you're satisfied with your final choices click "Next" to move on.

Figure 9.1 Survey questions of ALP modules: Panel A. Screen shot of task description: high returns + affirmative decision advice treatment; Panel B. Screen shot of task: rules treatment; Panel C. Screen shot of task: portfolio checkup treatment

Source: RAND American Life Panel.

Please indicate the percentage of your portfolio that you would like to hold in each of the following funds. Then click the "Evaluate my portfolio allocation" button and we'll then give you some feedback on your choices based on standard financial advice.

My Portfolio Choices	%	Fees	5 Year Return
Money Market Fund	100	0.21%	3.28%
Total Bond Market Index Fund	0	0.20%	4.56%
S&P 500 Index Fund	0	0.18%	−2.29%
Small Cap Value Index Fund	0	0.23%	−0.76%
REIT Index Fund	0	0.21%	0.77%
Global Equity Index Fund	0	0.72%	−0.24%

Based on your choices, the Portfolio Checkup Tool suggests

Portfolio Checkup Tool	
[redacted]	You have zero equities in your portfolio - research suggests most people benefit from holding some equity allocation

If you want to change your choices you can update your allocations before submitting your final answer. Click the "Evaluate my portfolio allocation" button to get new feedback from the Portfolio Checkup Tool. When you are finished, click "Next" to move on.

Figure 9.1 Continued

Interestingly, the difference is more statistically significant for self-assessed financial literacy, rather than measured financial literacy.

Table 9.7 reports the determinants of the probability of advice-seeking for the 'affirmative decision' group estimated using the LP model used in Table 9.2. Column 1 includes as regressors the demographic characteristics from the survey data analysis, as well as an indicator for having a DC plan and an indicator for being in the low-returns treatment as these may independently affect the propensity to seek advice. Columns 2 and 3 add the financial literacy measures. Our results show that the age effect on the propensity to seek advice becomes insignificant once we account for financial literacy, but the wealth effect remains significant. This is remarkable, as the advice is free and the incentives for the hypothetical task have no relationship to actual wealth. We also note that financial literacy is strongly significant (regardless of whether measured or self-assessed financial literacy is used) regardless of the other demographic controls. Nevertheless, the relatively low fit indicates that a large amount of the variation in advice-seeking in the experiment is still not explained by observable characteristics, even with the inclusion of the financial literacy measures.

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Table 9.6 Summary statistics on the ALP experimental sample

	Unweighted %						
Married	66.5						
Female	57.3						
Age <45	31.8						
AFI <$50,000	39.2						
Black or Hispanic	9.5						
College degree	45.2						
Currently enrolled in employer DC plan	29.0						
	Affirmative decision: chose advice (%)	*N*	Affirmative decision: chose no advice (%)	*N*	*t*-test of equality (*p*-value)	Default group (unsolicited advice) (%)	*N*
Married	70.26	548	65.76	295	0.18	65.29	801
Female	59.49	548	54.58	295	0.17	57.80	801
Age <45	30.66	548	40.68	295	0.00***	30.34	801
AFI <$50,000	33.39	548	42.71	295	0.01**	41.32	801
Black or Hispanic	8.94	548	10.17	295	0.56	10.36	801
College degree	45.26	548	43.73	295	0.67	46.82	801
Currently enrolled in employer DC plan	31.39	548	27.46	295	0.24	26.09	801
Measured financial literacy	0.23	406	0.36	178	0.09*	0.24	569
Self-assessed financial literacy	2.64	406	2.99	178	0.00***	2.66	569

Note: See Table 9.2.

Source: Authors' calculations using the American Life Panel 2009. See text.

The impact of advice on behavior

Having established that in the absence of reverse causality and financial constraints, negative self-selection on financial literacy is likely to occur, we next move on to analyze the impact of advice on investment behavior. Our next goals are to (*a*) establish whether advice itself has an effect, (*b*) understand if investors are likely to behave differently toward advice when it is presented as an affirmative choice rather than as a default, and (*c*) gain insight into the relative importance of selection versus the actual impact of advice in observed real-world behavior, where advice is typically a choice variable. These questions are of first-order importance when considering the likely impact of policy alternatives such as making advice more freely available, or instituting compulsory financial counseling.

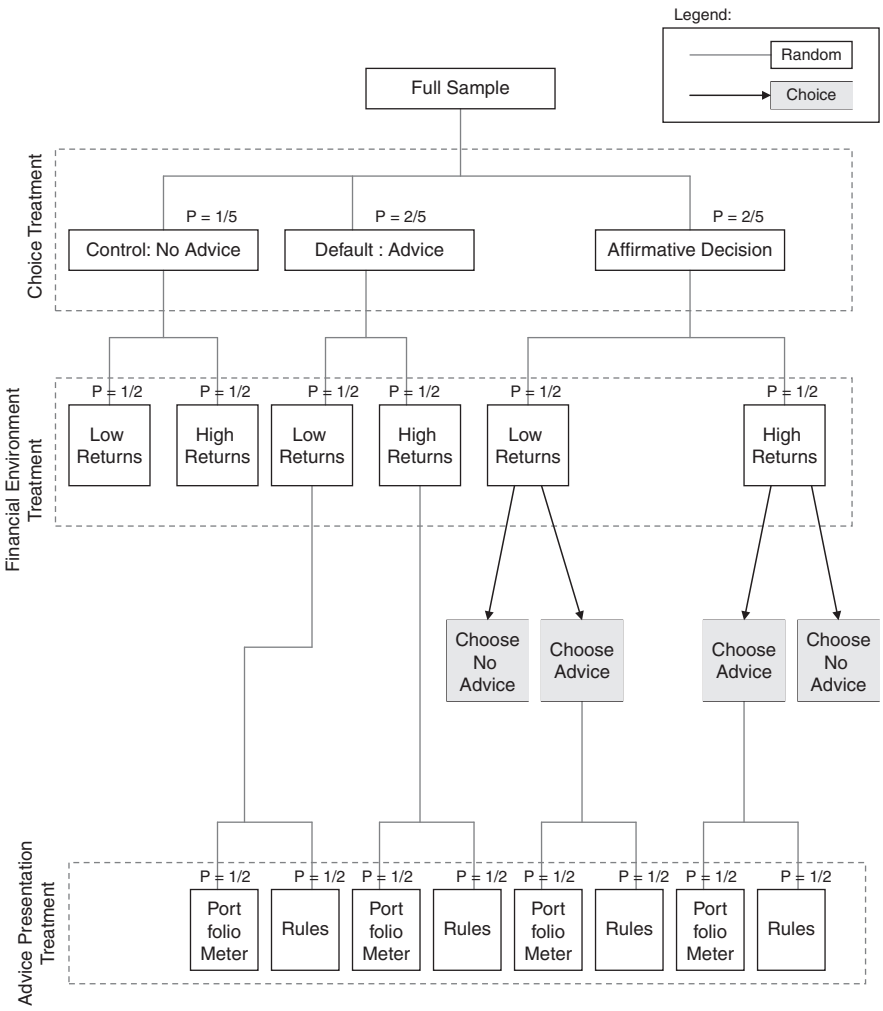

Figure 9.2 Experimental design schematic

Source: Authors' formulation; see text.

The average effect of default versus optional advice on behavior

With the randomized controlled trial design, we obtain unbiased estimates of the effects of a treatment by simply comparing mean outcomes of interest between treatment and control groups. We first describe respondents' portfolio allocations as well as the investment 'mistakes' explicitly addressed by the rules, comparing both the default treatment group

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TABLE 9.7 Determinants of the propensity to seek advice: affirmative decision treatment (ordinary least squares estimates)

	(1)	(2)	(3)
Married	0.030	0.008	0.017
	(0.038)	(0.045)	(0.045)
Female	0.065*	0.025	–0.007
	(0.034)	(0.041)	(0.041)
Age <45	–0.104***	–0.031	–0.038
	(0.035)	(0.046)	(0.045)
AFI <$50,000	–0.077**	–0.092*	–0.092**
	(0.039)	(0.047)	(0.046)
Black or Hispanic	0.010	–0.020	–0.016
	(0.058)	(0.071)	(0.071)
College degree	–0.005	–0.057	–0.025
	(0.034)	(0.043)	(0.042)
Has DC plan	0.036	0.054	0.064
	(0.037)	(0.043)	(0.042)
Low-returns treatment	–0.007	0.032	0.029
	(0.033)	(0.038)	(0.038)
Measured financial literacy index		–0.045*	
		(0.027)	
Self-assessed financial literacy index			–0.089***
			(0.022)
Constant	0.650***	0.723***	0.952***
	(0.053)	(0.061)	(0.084)
N	843	584	584
R^2	0.02	0.02	0.05

Note: See Table 9.2.

Source: Authors' calculations using the American Life Panel 2009. See text.

(with unsolicited advice) and the affirmative decision group (where advice is optional), to the control group (with no advice). For each treatment group, we test the hypotheses that the group sample means are equal to the sample mean of the control (i.e., there is zero treatment effect for that group). Note that for the affirmative decision group, the mean includes outcomes for individuals who both chose and did not choose advice. The difference between treatment and control thus reflects the overall effect of being administered the affirmative decision treatment, regardless of the actual choice.

Table 9.8 shows clearly that the mean values of all outcomes for the default group are not significantly different from the control group. Unsolicited advice, it appears, may have no effect on behavior. In the affirmative decision group, on the other hand, we find that respondents are significantly less likely to commit two 'mistakes'—under-diversification and being too conservative. This implies that the affirmative decision

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TABLE 9.8 Experimental results: comparisons of means

	Sample means		*t*-test of equality of means (*p*-value)		
	Control (no advice %)	Default group (unsolicited advice %)	Affirmative decision group (optional advice %)	Default = control	Affirmative decision = control
% Allocation					
Stocks	25.6	25.2	28.1	0.81	0.12
Bonds	29.9	29.9	29.3	0.97	0.65
Money market	38.2	39.2	37.1	0.54	0.48
Other	5.9	5.7	5.5	0.31	0.17
Mistakes					
Zero equity	37.6	37.1	34.1	0.87	0.22
Under-diversified	13.2	10.4	9.6	0.14	0.06*
Too aggressive	1.4	1.5	1.7	0.90	0.73
Too conservative	65.5	65.3	59.6	0.94	0.04**

	Sample means		*t*-test of equality of means (*p*-value)		
	Chose advice (%)	Did not choose advice (%)	Chose advice = did not choose advice	Chose advice = default	Did not choose advice = control
% Allocation					
Stocks	29.5	25.5	0.04**	0.00***	0.96
Bonds	29.8	28.4	0.36	0.96	0.40
Money market	34.5	41.9	0.00***	0.00***	0.11
Other	6.2	4.3	0.00***	0.28	0.00***
Mistakes					
Zero equity	27.6	46.1	0.00***	0.00***	0.02**
Under-diversified	4.4	19.3	0.00***	0.00***	0.02**
Too aggressive	1.1	2.7	0.08	0.53	0.21
Too conservative	56.2	65.8	0.01**	0.00***	0.94

Note: See Table 9.2.

Source: Authors' calculations using the American Life Panel 2009. See text.

treatment has a positive average effect on behavior for the group as a whole.

In light of the slightly differing demographic composition between the treatment groups, we run an OLS regression on the whole sample to estimate the following equation:

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$$Y_i = \alpha + \beta_d \text{ default}_i + \beta_a \text{ affirmative}_i + X_i'\delta + \varepsilon, \quad (9.2)$$

where 'default' and 'affirmative' are now treatment dummies, and we control for the observables vector *X*. In the terminology of program evaluation, we can think of the default experimental group as being enrolled in a compulsory program of free advice, and the affirmative decision group as a group which is enrolled in a program which simply offers advice for free. The β coefficients give the treatment effects of being exposed to the program, or the *intent-to-treat* estimate (which for mandatory, full-compliance programs similar to the default treatment is the same as the actual program effect).

Table 9.9 shows the results on portfolio quality using the 'mistake' indicators as outcome variables. The default treatment has no significant effect,

TABLE 9.9 Intent-to-treat effects on portfolio quality: all choice treatments (ordinary least squares estimates)

	(1) Zero equity	(2) Under-diversified	(3) Too aggressive	(4) Too conservative
Default treatment	–0.000	–0.026	0.002	–0.002
	(0.029)	(0.018)	(0.007)	(0.028)
Affirmative decision treatment	–0.030	–0.034*	0.002	–0.056**
	(0.028)	(0.018)	(0.007)	(0.028)
Married	–0.012	0.001	–0.003	0.005
	(0.024)	(0.015)	(0.006)	(0.024)
Female	0.072***	–0.002	–0.005	0.100***
	(0.021)	(0.014)	(0.006)	(0.021)
Age <45	–0.044*	0.000	0.014**	–0.096***
	(0.023)	(0.015)	(0.006)	(0.023)
AFI <$50,000	0.062**	0.010	–0.008	0.049**
	(0.024)	(0.016)	(0.006)	(0.024)
Black or Hispanic	–0.064*	–0.012	–0.007	–0.032
	(0.036)	(0.024)	(0.009)	(0.036)
College degree	–0.070***	–0.027*	–0.006	–0.041*
	(0.022)	(0.014)	(0.006)	(0.022)
Has DC plan	–0.058***	0.042***	0.008	–0.102***
	(0.021)	(0.014)	(0.005)	(0.021)
Low-returns treatment	–0.001	0.028*	0.016***	–0.060**
	(0.024)	(0.015)	(0.006)	(0.024)
Constant	0.394***	0.112***	0.012	0.694***
	(0.039)	(0.025)	(0.010)	(0.039)
N	2,070	2,070	2,070	2,070
R^2	0.03	0.01	0.01	0.05

Note: See Table 9.2.

Source: Authors' calculations using the American Life Panel 2009. See text.

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while the previously noted affirmative decision treatment effects are robust to the inclusion of the demographic controls as well as controls for DC plan ownership and the low-returns treatment. For the subsample of individuals with financial literacy data, we also re-estimate Equation (9.2) using both measured and self-assessed financial literacy measures. When we control for financial literacy, we find that the overall effects in the affirmative decision are reduced, but there is still a positive and significant effect in reducing over-conservatism. More generally, in line with the message of the advice, we find that individuals who follow advice should allocate a significant part of their portfolio to stocks, but not more than 95 percent. Additional regression analysis shows there is no significant average effect on stockholding in either treatment, although the point estimates are consistently positive for the affirmative decision treatment and negative for the default treatment, whether or not we control for financial literacy.[11]

These results establish that unsolicited advice has no average effect, but that offering advice as a choice may indeed positively affect overall investment behavior. In the default treatment group, we explore the possibility of heterogeneous treatment effects that might justify the provision of advice even when it is not asked for. In the affirmative decision group, we next turn to the estimation of the actual effects of treatment on the treated, and analyze the implications of self-selection on other unobservable characteristics.

Are there heterogeneous treatment effects in the default treatment group?

Although in the default group, we find no strong average effects, it is reasonable to speculate that perhaps there are smaller subgroups of interest that do respond to such unsolicited advice and that may be targeted separately. In particular, policymakers may consider targeting such interventions to groups of individuals that have lower skills. One conclusion from the survey results might well be that, since the less financially literate are not seeking out advice on their own for reasons that may include financial constraints, and may also be prone to making mistakes, giving free advice as a default may help them. Our results show some support for this idea, but the evidence is not strong.

We focus on individuals in the default and control groups and we re-estimate Equation (9.2) with the default treatment dummy interacted with a measure that reflects skill levels (college education, age, or the financial literacy measures). In this specification, a significant coefficient on the interaction term suggests a differential (additive) treatment effect for that group. For this analysis, we characterize individuals who lie below the median value for each financial index as having 'low financial literacy' in order to generate an indicator of low financial literacy. Results show no

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clear trends: there are statistically significant differential impacts for the young or the less financially literate, although the point estimates suggest that giving out advice has a salutary impact on the less-financially literate respondents across all the portfolio quality metrics. Overall, however, in the default group, there is no overwhelming compelling case for making free counseling compulsory (we note however other studies, such as Hastings and Mitchell (2011), suggest that altering the format of advice can make a difference in low-literacy groups: the interactive portfolio meter treatment can have a greater effect than the non-interactive rules treatment).

Treatment effects vs. selection in the affirmative decision group

Within the affirmative decision group, we find extremely large behavioral differences between those who choose to receive advice and those who do not, implying that both treatment effects and selection are present. Our previous results rule out the most immediately intuitive type of selection, positive selection on financial literacy. Instead, the findings point to self-selection on other unobservables such as motivation or interest. At the same time, in line with the intent-to-treat analysis above, we also find positive average treatment effects on the treated—the advice itself does alter the behavior of the recipients. Yet the magnitude of the actual impact is small relative to the difference due to self-selection on unobservables, which serves as a cautionary note for those evaluating such programs with observational data alone.

Returning to the main results in Table 9.8, we find that recipients and non-recipients in the affirmative decision group differ significantly with respect to portfolio allocation and portfolio quality. On all four quality metrics, advice recipients perform significantly better than non-recipients: they are less likely to hold zero equity, be under-diversified, or rated as too conservative. Recipients are also not simply increasing risk exposure across the board, as they are also less likely to be too aggressive. While advice recipients in the affirmative decision group outperform advice recipients in the default group, those who do not receive advice do worse or no different compared to the control group (who also received no advice). The latter observation implies that some of these differences may be due to self-selection along some dimension that also influences task performance.

One way to explain the better performance by advice recipients might be that individuals who are more financially literate are also more likely to seek advice, but our earlier analysis shows that advice recipients self-select negatively on financial literacy. In the absence of any advice effect, if financial literacy was the primary source of selection, we would expect advice recipients to perform worse, not better, on the task. We therefore

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re-estimate Equation (9.1) using only the affirmative decision treatment group with a control vector that includes and excludes financial literacy measures. For portfolio quality metrics, we find a large significant association between better performance and advice, which is robust to the vector of regular demographics as well as the inclusion of either measure of financial literacy (Table 9.10).

If we assume that this set of controls resolves the selection problem, our estimates would imply very large effects of advice on behavior. Thus, individuals who choose to receive advice are about 18–25 percent less likely to have zero equities in their portfolios, or to be under-diversified. They are also about 10 percent less likely to be over-conservative, a result which contrasts dramatically to the zero effect of delivering the same advice automatically. However, we cannot rule out selection on other unobservable characteristics. To try to understand how much of this association is likely due to selection effects, we next estimate the average impact of treatment on the treated and compare it to these differences in behavior.

We note that the analysis in the preceding section implies that the advice does indeed have an effect, although it remains to be seen whether this effect can explain the whole observed difference. If advice had no impact

TABLE 9.10 Portfolio quality, advice, and financial literacy: affirmative decision treatment (ordinary least squares estimates)

	(1) Zero equity	(2) Under-diversified	(3) Too aggressive	(4) Too conservative
Demographic controls only				
Chose advice	−0.227***	−0.169***	−0.022**	−0.108**
	(0.041)	(0.025)	(0.010)	(0.043)
Financial literacy added:				
Specification I:				
Chose advice	−0.237***	−0.174***	−0.022**	−0.118***
	(0.041)	(0.025)	(0.011)	(0.043)
Measured financial literacy	−0.089***	−0.047***	0.003	−0.095***
	(0.027)	(0.016)	(0.007)	(0.028)
Specification II:				
Chose advice	−0.262***	−0.182***	−0.020*	−0.139***
	(0.041)	(0.025)	(0.011)	(0.043)
Self-assessed financial	−0.112***	−0.041***	0.006	−0.100***
literacy	(0.022)	(0.013)	(0.006)	(0.023)
N	584	584	584	584

Note: Other demographics from Table 9.9 included but not shown. See Table 9.2.

Source: Authors' calculations using the American Life Panel 2009. See text.

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on behavior for anyone in both groups but simply acted as a sorting device, we would expect *on average* behavior in both the (randomly assigned) affirmative decision and control groups to be equal. Yet the significant intent-to-treat effect implies that, on average, the affirmative decision group is either less or equally likely to commit mistakes than the control group.

The intent-to-treat estimate is simply the average effect of treatment for the whole affirmative decision group; it therefore pools together both individuals who received and did not receive advice. We are interested in the average effect of treatment on the treated, or the effect of advice on those who actually took it up. An estimate of the average effect of treatment on the treated is simply the intent-to-treat estimate divided by the actual take-up rate, or, in this case, the average treatment effect for the whole affirmative decision group, divided by the fraction of respondents in the group who chose advice. In a multivariate regression framework, this is equivalent to re-estimating Equation (9.1) using instrumental variables regression on both treatment and controls, where assignment to the treatment group is the instrumental variable.

Results in Table 9.11 (with and without financial literacy) are relatively modest. In this case, an individual who chooses advice is 8–9 percent less

TABLE 9.11 Estimates of the average effect of treatment on those treated on portfolio quality and financial literacy: affirmative decision treatment vs. controls (IV regression: instrument for advice = assignment to affirmative decision treatment)

	(1) Zero equity	(2) Under-diversified	(3) Too aggressive	(4) Too conservative
Demographic controls only				
Chose advice	−0.055	−0.036	−0.002	−0.092*
	(0.047)	(0.029)	(0.012)	(0.048)
Financial literacy added:				
Specification I:				
Chose advice	−0.058	−0.038	−0.002	−0.095**
	(0.047)	(0.029)	(0.012)	(0.047)
Measured financial literacy	−0.099***	−0.049***	−0.003	−0.120***
	(0.022)	(0.014)	(0.006)	(0.022)
Specification II:				
Chose advice	−0.047	−0.033	−0.002	−0.083*
	(0.046)	(0.029)	(0.012)	(0.047)
Self-assessed financial literacy	−0.101***	−0.036***	0.006	−0.105***
	(0.018)	(0.011)	(0.005)	(0.018)
N	897	897	897	897

Note: Other demographics in Table 9.9 included but not shown. See Table 9.2.

Source: Authors' calculations using the American Life Panel 2009. See text.

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likely to be over-conservative, but other effects are far more muted: the effects on under-diversification range from being 5 percent less likely to nothing significant across the various specifications. Compared to the estimates in Table 9.10, the magnitude of the actual treatment effects suggests that a sizable part of the gap between advice recipients and non–recipients is due to self-selection, and that this selection occurs on performance-related unobservables other than financial literacy.

Conclusion

Using experimental methods to try to better understand the causal relationship between advice and behavior, we report on a hypothetical choice experiment in which participants are asked to perform a portfolio allocation task. This means we can only analyze hypothetical outcomes rather than real plan outcomes, but there are several advantages to the experimental analysis. First, the advice provided is completely uniform in content. Second, the issue of reverse causality does not arise. Respondents are randomly assigned to one of three study arms. The first arm is a *control* group, in which the task is presented to respondents without any advice. Respondents in the second arm, the *default treatment* group, all receive the same financial advice. In the third arm, the *affirmative decision* group, respondents are given the choice of whether to receive the advice. Only those who choose advice receive it. Comparing the three groups allows us to study the effects of advice given as matter of course, versus the effects of advice given as a result of requiring an active rather than a passive decision.

Results demonstrate that unsolicited advice has no effect on investment behavior: in terms of behavioral outcomes, individuals who are simply given advice disregard it almost completely. When advice is optional, individuals with low financial literacy are more likely to seek it out. In spite of this negative selection on ability, individuals who actively solicit advice do perform better. Solicited advice thus appears to have more of an effect than unsolicited advice, although the magnitude of self-selection effects can overshadow actual treatment effects.

In some situations, policymakers may find mandatory counseling an attractive remedy. Our results suggest this is not likely to work, however, if the target population is not inherently prepared to take advice, even if it is truly lacking necessary skills. We do know that when employers offer advice as an elective benefit and ensuring employees' active decision-making, this can result in significant take-up and some improvement of financial outcomes, and employees with low financial literacy are more likely to take advantage of these programs. At the same time, policymakers should be realistic about the effects of such programs. One cannot

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overestimate the impact of voluntary advice programs, since observed differences between recipients and non-recipients are likely to be influenced by selection. Ex-post evaluations are particularly likely to be subject to such biases, even when researchers have access to data on seemingly key variables such as financial skills and education.

Our findings point to a challenging problem. Building financial literacy can improve outcomes, but other unobservable factors such as inherent motivation are also highly performance-relevant and do not appear to be not perfectly correlated with financial ability. Hence, if motivation and other underlying factors remain unaffected, increased advice and other support tools may not only raise average outcomes but also increase the disparities between individuals who are self-motivated and those who are not. Accordingly, transitioning from knowledge to actual behavior change requires advice and educational materials designed to engage rather than simply inform the consumer. Future work will investigate whether an engaging presentation format in addition to knowledge content can independently promote behavioral change.

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Endnotes

1. Their approach is similar in spirit to Calvet et al. (2007), who assess the welfare costs of household investment 'mistakes' in Sweden, focusing on under-diversification of risky assets and non-participation in risky asset markets.
2. The ALP is an Internet panel of respondents aged 18 and over. Respondents in the panel either use their own computer to log on to the Internet or a Web TV, which allows them to access the Internet using their television and a telephone line. The technology allows respondents who did not have previous Internet access to participate in the panel. ALP members are recruited from among individuals aged 18 and older who respond to the monthly Survey of Consumers conducted by the University of Michigan's Survey Research Center. The monthly survey produces, among other measures, the widely used Index of Consumer Sentiment and Index of Consumer Expectations. On joining, respondents to the

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ALP complete a separate survey about individual demographic, work history, and other household information, which they are prompted to update each time they log in to a new module. This provides a series of self-reported demographic characteristics of interest, including birth date, gender, education, ethnicity, occupation, state of residence, and income. The ALP population as a whole tends to have more education and income than the broader US population; there are two main reasons for this sample selection. First, the Michigan respondents tend to have more education than the population at large, as described by Census data. Second, the great majority of ALP members have their own Internet access. Americans with Internet access tend to have more education and income than the broader population. As such, for survey data analysis, we apply population weights to all survey response. For the experimental data analysis, the data remain unweighted.

3. It should be noted that twenty-five respondents were erroneously omitted from the survey sample for the question about advice received in 2008; in addition, three individuals gave no response. The maximum and minimum possible bounds for the true full-sample statistic are 17–22 percent, computed using the extreme assumptions of 0–100 percent take-up for the omitted group. We note that the low overall use of advice for the rest of the sample suggests that the true sample mean is likely to be at the lower end of this range.
4. Throughout this chapter, we use linear probability models for simplicity. In robustness checks, probit regressions delivered qualitatively and quantitatively similar results.
5. We also note that of the forty-one individuals reporting making early withdrawals, seven did not report their contributions activity. However, only two individuals reported also stopping their contributions, and five individuals made lower contributions. On the other hand, twenty-three individuals report continuing the same (positive) average contribution from the previous year, and five people actually increased their contribution.
6. As individuals with missing data were in the group reporting early withdrawals, the latter statistics are the most sensitive to assumptions about the missing values. In the sample without missing data, individuals with advice were more likely to report early withdrawals. Assuming the upper bound of one for all missing, this is clearly even more pronounced; assuming a lower bound of zero for all missing, we find that, unsurprisingly, this result is reversed. While no conclusive inference can be made, given that prevalence of advice in the group of early withdrawals without missing data is close to the sample average, true early withdrawal behavior is not likely to be very different across those with and without advisors.
7. A detailed analysis of the impact of financial literacy itself on these and other related outcomes is found in Hung et al. (2009).
8. In their study of German investors, Hackethal et al. (2012) used an instrumental variables strategy to identify the impact of advice, employing regional statistics for number of bank branches per capita, voter participation, log income, and

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fraction of population with a college degree as instruments for use of a financial advisor. As ALP respondents report their current state of residence, we collected analogous data for the United States and replicated this strategy using state-level averages for the number of financial advisors per capita in 2005–6, log 2006 median income, the fraction of population above age 25 with a college degree in 2009, and voter participation rates for the 2008 general election. However, the first-stage regression (not reported here) with these instruments is extremely weak (F-statistic < 2). We conclude that in the United States, unlike Germany, local geographical variation in the supply of advisors is not a strong predictor for advice-seeking, and that instrumental variables regression is not a valid strategy.

9. We drop 16 observations in which the individuals did not complete the survey and a further 138 responses which were missing or invalid. Cross-tabulations and chi-squared tests indicate that the missing responses are not correlated with the choice treatments.
10. The randomization for the other treatments is conducted orthogonally, and so it should not affect the results of the analysis which essentially compares means across this set of randomized treatments.
11. A Kolmogorov–Smirnov test of equality of distributions does not reject the null of no difference between default and control ($p = 0.996$). It also does not reject the null of no difference between affirmative decision and control ($p = 0.144$), but this result is considerably more marginal. As an aside, both financial literacy measures strongly and significantly predict behavior independent of the treatments, in a direction consistent with the advice: the more literate are more likely to hold stock and less likely to commit mistakes.

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