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## Evaluating the Impact of Financial Planners

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## Evaluating the Impact of Financial Planners

### Abstract

The growing complexity of investment decision-making has increased the potential value of advice from a financial professional. Past research provides only a very partial answer to the question of whether professional financial advice leads to better investment practices and results. In this chapter, we review findings in the existing research literature, detail 'best practice' research approaches for drawing confident conclusions regarding causal influences, and provide an example of such an approach using data from the 2007 Survey of Consumer Finances. Our results suggest that the benefits of financial advice may be overstated in the absence of methodological rigor.

### Disciplines

Economics

### Comments

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

# **The Market for Retirement Financial Advice**

EDITED BY

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## Chapter 8

### **Evaluating the Impact of Financial Planners**

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*Cathleen D. Zick and Robert N. Mayer*

Do professional financial planners improve their clients' financial well-being? The answer to this question is more important today than it was twenty-five years ago, because individuals now must shoulder increasing responsibility for their financial planning, and financial markets have become more complicated and volatile. As evidence of these trends, there were approximately 34 million shareholder accounts in the United States in 1985, and investors had their choice of 1,528 mutual funds. By 2010, there were 292 million shareholder accounts and 7,581 mutual funds (ICI, 2011).<sup>1</sup> Given the information and choice burdens of managing one's financial affairs, it is not surprising that roughly one-third of households report consulting a professional financial planner to help with saving, investments, and insurance (SunAmerica Financial Group, 2011; Twigg, 2011; Turner and Muir, 2013). But the challenges for consumers do not end here, because there are many different types of financial planners and a growing variety of titles, credentials, and certifications.<sup>2</sup>

Despite the importance of knowing about the conditions under which financial planners benefit their clients, to date only a handful of studies have evaluated the impact of financial planners. Moreover, prior studies are often plagued by design features that limit confidence in their conclusions. In this chapter, we detail 'best practice' approaches that can help an evaluator draw confident conclusions regarding whether financial planners improve their clients' financial well-being. We review existing literature in light of these best practices, and we provide an example of how the choice of evaluation design can affect a study's conclusions using the 2007 Survey of Consumer Finances (SCF).

#### **Evaluation designs**

A good evaluation of the impact of consulting with a professional financial planner involves insuring that three aspects of the study are sound: (a) the measurement of advice-seeking and its consequences, (b) the applicability

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of the evaluation's results to other settings, and (c) the confidence that observed relationships between seeking professional guidance and financial outcomes<sup>3</sup> are real (Langbein and Felbinger, 2006). We discuss each of these in turn as they apply to the evaluation of professional financial planner consultations.

### **Are the right concepts being measured?**

At first blush, it may seem that it is simple to measure whether someone has sought out a professional financial planner. Yet in practice, this is complicated. For instance, the researcher must decide whether consultation with an Accredited Financial Counselor or Certified Financial Planner should count the same as a session with a bank customer service specialist. Decisions must be made regarding whether to differentiate a commission-based planner from a fee-only planner, and whether a one-time consultation should count the same as repeated consultations. Likewise, the researcher must make decisions about what types of time frames (e.g., one year, ten years) should matter for planning sessions. Resolution of these conceptual issues has practical implications regarding how contact with a professional financial planner is to be measured.

In prior studies, researchers have often relied on a single dichotomous self-reported question about use of financial planners, broadly defined. Yet four studies are notable for improving on this typical approach. First, research in Germany has relied on brokerage houses and banks to measure use of professional financial advice (Bluethgen et al., 2008; Gerhardt and Hackethal, 2009; Hackethal et al., 2012; Hackethal and Inderst, 2013). Presumably these organizational records are more accurate than self-reports. Second, one of these German studies has compared the results of using an independent financial advisor to those from using a bank-affiliated advisor (Hackethal et al., 2012). The results of these studies are mixed. One concluded that financial advisors aid in diversification and matching actual investments with the contents of a predefined, ideal portfolio; the other study found little evidence that working with a professional financial advisor was beneficial to clients.

A 2010 Canadian study took advantage of longitudinal data to more precisely measure contact with a financial planner (IFIC, 2010). Specifically, consumers were asked in 2005 and 2009 whether they used a financial advisor. The empirical analysis compared those people who said yes in both years to those who said no in both years. This procedure has the advantage of eliminating people who had not consulted an advisor recently, as well as those who only recently began to work with one. That study revealed that advised households had higher investable assets in 2009 than non-advised

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households. This result held within each of five household income groupings, and the difference was more dramatic for households making less than \$100,000 per year compared to those earning more than this amount.

Despite the promising measurement practices of the studies just cited, existing research still does not do justice to the wide range of financial planners and the many factors that might differentiate their impacts on customers. The existing literature also fails to capture the nature of the planner–client relationship, especially its duration and the frequency of consultations. An equally thorny issue relates to the choice of measured outcomes. These should capture what financial planners are supposed to accomplish, but analysts disagree on this point. For instance, Collins (2010) distinguishes four roles that financial planners can play: technical expert, transactional agent, counselor, and coach. Each implies different criteria according to which the performance of a planner could be judged.

To give a flavor of the conceptual issues involved in selecting relevant outcomes, one should ask whether the benefits of using a planner are best measured in terms of dollars (e.g., account growth), time (e.g., time saved in the planning process), or psychological states (e.g., retirement confidence, peace of mind). Moreover, within financial results, it is unclear if a ‘good’ planner would be expected to help a person ‘beat the market,’ or help someone avoid major mistakes (e.g., by acting as a human ‘circuit breaker’ against behavioral biases). For example, Hackethal et al. (2012) conclude that planners play the role of ‘babysitter,’ that is, someone who allows competent people to use their time in other pursuits, as opposed to ‘psychiatrists’ (our term, not theirs) who provide the experience, expertise, and perspective that a consumer lacks.

If the benefits of working with a financial planner are primarily financial in nature, a stringent standard would be to generate higher risk-adjusted rates of return or greater wealth in the long run. Yet most researchers do not have the luxury of measuring exposure to financial advice at one point in time and account balances at a much later point in time. Moreover, in cross-sectional analyses, there is a danger of misinterpreting associations between using a financial planner and having greater than average wealth, since planners may help people become wealthier, but wealthier people may also have a greater propensity to seek financial advice.

Given the difficulty of using financial results (e.g., portfolio performance) as a measure of a planner’s potential value, many studies focus on ‘process’ factors that planners can influence in the short run (e.g., taking particular planning actions, diversifying investments, or setting aside an emergency fund). The implicit assumption is that consistently following the recommended financial planning process steps will improve long-term financial outcomes. Our own research finds that the act of estimating retirement financial needs is indeed associated with the accumulation of

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more retirement assets (Marsden et al., 2011). Yet evidence is scarce regarding the impact on wealth of planning steps such as setting goals and specific objectives, creating and implementing a financial plan, and monitoring results (Certified Financial Planner Board of Standards, 2009).

Partially because it is relatively easy to measure and partially because it is under the immediate control of clients and their advisors, diversification is the most common measure employed in studies of the impact of financial planners. Both diversification across and within asset classes can be examined. Bluethgen et al. (2008) do a particularly good job of measuring multiple aspects of diversification, including the use of mutual funds, geographical diversity of equity assets (versus home bias), and portfolio volatility.

Lacking a single conceptual way to measure the impact of financial planners, analysts often adopt a pragmatic stance. That is, they measure multiple outcomes, some of which cover long-term investment performance. Most studies, however, focus on short-term results or controllable elements of the financial planning process.<sup>4</sup>

### **Will the results be applicable to other settings?**

If the results of an evaluation are not generalizable to other settings, then they provide only limited insights about the effectiveness of financial planners. Two evaluation considerations are particularly relevant when assessing the generalizability of a study's design.

First, it is important to define the population of interest. A researcher may want to know the effectiveness of professional financial planners with respect to a relatively homogeneous group, such as individuals who work for the same employer or are customers at the same financial institution. Alternatively, a researcher may be interested in assessing the impact of professional financial planners on the general population or targeted subgroups (e.g., young adults, middle income households). The population of interest drives decisions about who should be in the study and how results can be extrapolated. For example, the findings from a study on individuals who invested their money with financial institution X should not be extrapolated to the population at large, as it may be that factors (e.g., education level) that led people to invest their money in institution X also influenced how they acted when given professional financial advice.

In practice, virtually all existing studies examining the potential effects of financial planners use idiosyncratic samples. First, one focuses on people who visited the website of a large financial services company (ING, 2010). Another US-based study examines only people with employer-provided 401(k) plans (Charles Schwab, 2007). A third uses survey data in which

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all of the respondents worked for one single US employer (Marsden et al., 2011). The most nationally representative survey relies on US data from the RAND American Life Panel (Hung and Yoong, 2013).<sup>5</sup> A study conducted in the Netherlands analyzes data from a single medium-sized bank (Kramer, 2012). Several German studies rely on data from a single online brokerage company and/or a single bank (Bluethgen et al., 2008; Jansen et al., 2008; Gerhardt and Hackethal, 2009; Hackethal et al., 2012; Hackethal and Inderst, 2013).

Second, once the population of interest has been determined, the researcher must decide whether data will be gathered from everyone or from a subset of the population. Cost considerations usually lead the evaluator to opt for a sample, but in such instances, every effort must be made to insure that the sample selected is representative of the population of interest. There is a well-established literature on how best to generate appropriate samples (Scheaffer et al., 2012) and insure reasonable survey cooperation rates (Groves and Couper, 1998). But, even if the overall sample does reflect the larger population of interest, item-specific non-response to questions can still create problems in representativeness, particularly in the case of potentially sensitive questions about financial matters (Riphahn and Serfling, 2003, 2005). Imputation methods can be used to deal with such non-response, but these are very technically sophisticated (Kennickell, 1998, 2011).

**Are the observed relationships credible between professional financial consultation and the subsequent outcomes?**

Evaluations of the impact of professional financial planners should seek to establish causality, meaning that the professional consultation led to improved financial practices and financial outcomes. Financial practices, such as goal setting and portfolio diversification, and financial outcomes, such as net financial holdings, are likely influenced by a myriad factors including economy-wide fluctuations in financial markets, individual life cycle stage, household income, risk tolerance, and random chance, as well as whether individuals have sought the counsel of professional financial planners. The evaluator wants to know the effect of seeking professional financial advice on the outcomes of interest, net of other random and systematic elements.

Random influences can be routinely controlled by imposing standard statistical testing techniques.<sup>6</sup> By contrast, netting out the effects of other systematic elements on financial outcomes is more complicated. Even studies using large samples can yield unconvincing results if they do not

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use the proper study design. Thus, simply comparing the financial well-being of people who use and do not use financial planners does not prove causality. For example, several industry-sponsored studies have concluded that professional financial planners are highly beneficial (Charles Schwab, 2007; FPA/Ameriprise, 2008; ING, 2010). One study claims (FPA/Ameriprise, 2008: 1): ‘Consumer confidence is near historic lows, yet one group is feeling optimistic and on track to meet their financial goals including retirement—people in a comprehensive financial planning relationship.’ Such simple comparisons, at a single point in time, without other controls, are not scientifically valid.

Drawing conclusions regarding causality is best done using a sophisticated evaluation design called a randomized field experiment (RFE). In an RFE, individuals are randomly assigned to treatment and control groups. Data on the financial variables of interest (e.g., financial plan development, implementation, and wealth) should be gathered for members of both groups at the outset. Then those assigned to the treatment group would be exposed to a professional financial planner’s advice (i.e., the treatment). Depending on how professional financial planning is measured, the treatment could include a range of activities (e.g., goal setting, determination of risk tolerance, account diversification recommendations), dissemination modes (e.g., face to face, online), and one or several exposures. After an appropriate lapse of time (e.g., six months, one year, five years), data on the financial variables of interest would be remeasured and the change in the financial variables for the treatment group would be compared to the change for the control group. Statistically significant differences in the treatment group’s financial outcomes when compared to the control group would be evidence of the impact of professional financial planning. Figure 8.1 depicts the structure of a well-designed RFE.<sup>7</sup>

Comparison of the treatment group’s outcomes to those of a control group insures that the evaluator can net out systematic factors affecting both groups. For example, historical events like the 2008 Great Recession should affect both groups equally. Likewise, if the study involves a significant follow-up period, the comparison to a control group insures that life stage changes in financial outcomes are netted out. For instance, by comparing the changes in the treatment group to those experienced by the control group, the evaluator would net out any changes in financial outcomes that are attributable to some households moving in to the empty nest stage when resources may be redirected from child-related expenditures toward investments. Finally, the inclusion of a control group surveyed twice insures that the evaluator can net out any effects of sensitizing participants to the purpose of the study. That is, if participants become aware of the importance of financial planning as a result of the baseline data collection, this can be netted out from the analysis by comparing

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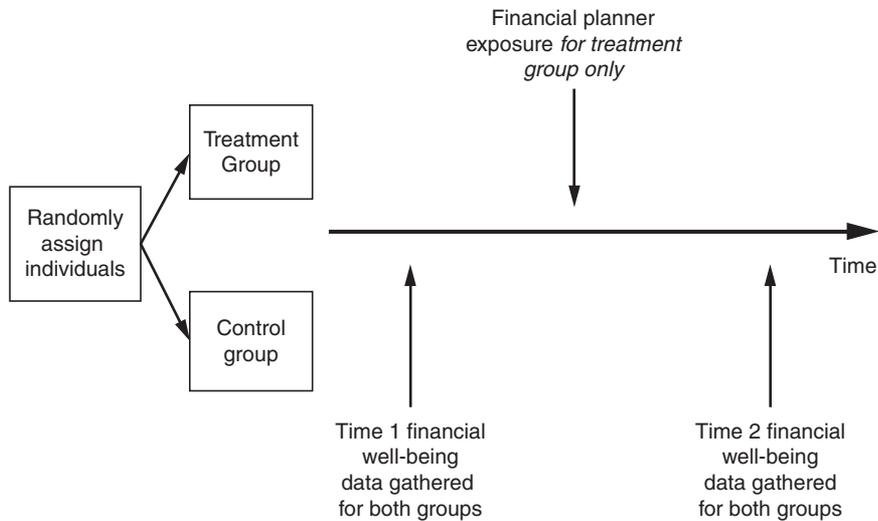


Figure 8.1 Randomized field experiment to assess the impact of consulting a professional financial planner

Source: Authors' analysis, adapted from Langbein and Felbinger (2006).

changes in the treatment group's financial outcomes to the changes in the control group's financial outcomes.

Random assignment to the treatment and control groups insures that when comparisons across the two groups are made that the evaluator will not have results that are confounded by the possibility of more motivated or more capable individuals self-selecting into the group that receives professional financial planning. Nor will the evaluator have to adjust for the possibility that one group contains more high- or low-income individuals that could affect financial comparisons.

For these reasons, RFEs are the 'gold standard' for assessing the causal relationship between an intervention such as meeting with a professional financial planner and one or more outcomes. Yet since these are typically very expensive to implement and require extensive planning, they are rarely carried out in practice. Instead, study designs most often used to assess how professional financial planners affect financial well-being involve some sort of non-experimental research design.

Non-experiments take one of two approaches. One approach simply compares the outcome variables of interest for a single group of individuals before and after the exposure to the treatment. In such a setting, individuals serve as their own controls. For instance, financial wealth levels prior to meeting with a professional financial planner might be compared to

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peoples' financial wealth one year later. A handful of studies to date incorporate such a before-and-after comparison, including a study by Gerhard and Hackethal (2009), who analyze almost 600 German investors who had previously directed their own accounts but recently consulted an advisor. The researchers reported that switching to working with an adviser triggered higher trading activity, most likely as part of restructuring portfolios. Newly advised clients also increased their diversification and engaged in less risky trading.

Another analysis by Bhattacharya et al. (2012) found some minor benefits in terms of account diversification and performance from consulting a professional financial advisor, but in that study, subjects were a very idiosyncratic group. The authors examined the less than 5 percent of people who responded affirmatively to an opportunity to work with an advisor. As a result, these subjects represent a strongly self-selected group. Even then, most advised clients failed to follow the recommendations of the bank-provided advisors. Horn et al. (2009) also used the passage of time to compare advised and non-advised investors before and after a change in the tax laws relevant to investing. They noted that advised investors were less likely to fall victim to the new rules, but the authors could not rule out the possibility that people who were less prone to making tax errors were also more likely to choose to work with a financial planner.

A second non-experimental approach dominates existing research. Here, outcomes of interest are measured at a single point in time for two different sets of individuals, where one group has been exposed to the treatment, while the other has not. In our context, this might involve using cross-sectional survey data in which all respondents report on their financial wealth, but some respondents indicate that they have met with a financial planner while others say they did not.

Both non-experimental approaches make use of comparisons with control groups, but the absence of random assignment calls into question whether the two groups are similar on all relevant dimensions except for the treatment. As a result, analysts cannot rely on simple comparisons of means. Rather, researchers must control for variables that capture other systematic processes unrelated to the treatment that may affect the outcomes of interest. For instance, in the case of financial outcomes, these might include measures of education, household structure, and life cycle stage (to name a few). Omission of these potentially important covariates can lead to biased estimates of the causal relationship between professional financial planning and financial outcomes.

The absence of random assignment in non-experimental evaluations also creates concern about possible reverse causation, which could arise if people who seek professional financial planning do so because they have greater financial wealth initially. Alternatively, people with low levels of

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assets (or who experienced a recent drop) might seek professional help as a means of offsetting their past decisions. In either situation, uncertainty about the causal direction of the relationship between exposure to professional financial planning and financial outcomes leads to concerns about whether the estimate of the treatment effect on the outcome of interest may be biased.

Also, in the non-experimental evaluation design, confidence regarding estimated causal relationships requires the elimination of omitted variables and reverse causality bias. In essence, the evaluator must attempt to compensate for non-random assignment through the use of statistical modeling. When evaluating the impact of professional financial planning, confidence that the estimated relationship is truly causal is a function of two things: (a) the inclusion of covariates in the model that adequately capture the influence of other variables affecting financial well-being, and (b) the statistical allowance for the simultaneous relationship between seeking professional planning and financial well-being. A pioneering study in this field by Bluethgen et al. (2008) uses multiple regression analysis to examine the association between using a bank-based financial advisor and financial account characteristics. The authors control for a variety of individual attributes, including the investor's age, income, and risk attitude. The authors report that using an advisor appears to promote not only greater account diversification and closer adherence to predefined model portfolios but also higher expenses. Nevertheless, this study did not control for possible reverse causation.

An important methodological step forward was taken by Hackethal et al. (2012), who employed an instrumental variable approach to predict use of a financial advisor. First, they used regional-level data to approximate the geographic concentration of financial information, which they viewed as a substitute for the use of a professional financial planner. They then used the predicted likelihood of using an advisor in an analysis of various investor practices and portfolio outcomes. The authors uncovered little evidence that use of a financial professional was beneficial to clients.

Hung and Yoong (2013) set out to replicate the instrumental variable approach, but they elected to use two measures of financial literacy to help control for possible reverse causation between investor outcomes and use of an advisor. Their logic was that financial literacy could be a substitute or complement for advice and thus influenced the likelihood of seeking it. Therefore, to the extent that financial literacy helps drive the decision of whether to seek advice, controlling for literacy would help separate the impact of advice from the likelihood of seeking it. Hung and Yoong analyzed a variety of investment account features (account contributions and withdrawals, asset allocations, and variable investment 'mistakes'). They concluded that advice had little apparent influence. If anything,

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they found evidence of reverse causation, namely, that people who experienced declines in their account balances turned to advisors.

Research by Gerhardt and Hackethal (2009) exemplifies an alternative to the instrumental variables when identifying the influence of advisors on their clients. Rather than controlling on predicted use of financial advisors, these authors use a propensity score approach by which they statistically construct pairs of research subjects matched according to all observable characteristics except their reported use of a financial advisor. This procedure approximates random assignment in a true experimental design and thereby makes it less plausible that differences in investor outcomes are a function of anything other than use or non-use of professional advice, including differences in wealth. Using this approach, they found little evidence that professional advisors promoted better investment practices and outcomes for their clients.

A US-based study of employees of a large state university also used a propensity score approach, with mixed results (Marsden et al., 2011). Working with an advisor was linked to several important financial planning activities, including goal setting, calculation of retirement needs, retirement account diversification, use of supplemental retirement accounts, accumulation of emergency funds, positive behavioral responses to the recent economic crisis, and retirement confidence. Use of a financial advisor was not related, though, to self-reported retirement savings or short-term growth in retirement account asset values.

Two studies that most closely approximate the ideals of an RFE are a study by Kramer (2012) in the Netherlands, and a study by Hung and Yoong (2013) in the United States. The Dutch study is notable for its use of both cross-time and cross-group comparisons. Thus, Kramer compared the portfolios of previously self-directed investors after switching to being advised. Because this switch is relatively rare in real-world settings, the author analyzed only 228 investors during a four-year period (2003–7). Within a month of consulting an advisor, client portfolios not only evidenced extensive turnover in existing assets but investors also added capital to their accounts. To put these account changes into meaningful perspective, the author employed a matched-pair research design based on propensity scores. A final nicety of this study is the separation of the sample into larger and smaller investors, to account for the possibility that advisors put more effort into larger portfolios. In addition, the short-term effect of working with an advisor was an increase in account diversification, especially a rise in the use of stock mutual funds within the equity portion of the accounts. The author also compared the portfolio returns of early and late switchers over the 2003–7 period, finding that the accounts of early switchers performed slightly worse than late switchers. This result is apparently due to shifts from equities to bonds rather than differential

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performance of equity accounts. Despite the use of comparisons across time and investor group, this study did not account for the possibility of self-selection, namely, that people who switched from being self-directed to working with an advisor may have had characteristics that also influenced the outcome variables of interest.

Hung and Yoong's analysis (2013) is the only one to date to employ random assignment of subjects. Unfortunately for our purposes, that study examined the impact of printed advice rather than interaction with advisers, and people allocated funds to hypothetical portfolios, not actual accounts. People were assigned to three groups: participants in the control group received information on fund expenses and past returns only, and the default treatment group automatically received advice on their allocation choices. This advice was based on a model of optimal portfolio allocation and was provided as (a) information only or (b) information plus feedback on chosen allocations. Finally, participants in the affirmative decision treatment group chose whether to receive investment advice. (There were also low and high return variations.) In a nutshell, the results for the default advice group approximated those of the control group, but the choices of people who deliberately chose advice were superior. Members of the latter group were less likely to make two serious investment mistakes: under-diversification and investing too conservatively. While random assignment should account for the possible effects of other participant characteristics, the authors reported that the findings were robust to multiple regressions that include experimental group membership along with a variety of covariate measures, including financial literacy.

In summary, recent scholarship has employed a variety of methodological techniques to explore the impact of professional financial planners on clients. The best models reduce the likelihood that observed connections are due to unmeasured variables or reverse causation. Absent a randomized experimental design, the use of propensity scores may be the best available method. To date, studies using propensity scores have been confined to investors with a relationship to a single bank, brokerage, or employer. In addition, these research subjects have tended to be wealthier than their respective national populations. Our use of propensity scoring below extends this line of research to a national sample, including individuals who fall below a country's median income level.

### Analyzing a national dataset

Measuring how much professional financial planners improve their clients' financial well-being is facilitated using data from the 2007 Survey of Consumer Finances (SCF), to demonstrate how evaluator choices can influence

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conclusions drawn from an evaluation. The SCF is a triennial survey sponsored by the Board of Governors of the US Federal Reserve and conducted by the National Opinion Research Center. It utilizes a dual-frame sample design with one frame focused on securing a sample that is representative of all US households while the other frame seeks to secure a sample of wealthy US households. In the 2007 SCF, there were 2,915 households in the former category, and 1,507 in the latter category (Kennickell, 2009).<sup>8</sup>

### Measuring concepts in the SCF

National surveys like the SCF have the advantage of including questions that have been tested for both measurement validity and reliability. Nevertheless, a researcher using the SCF may not always find that questions asked reflect all dimensions of the underlying concept that he would like to measure. Such is the case with regard to the measurement of professional financial planning, since the SCF includes a single question regarding information sources used when making savings and investment decisions. The SCF question reads as follows:

What sources of information do you (and your family) use to make decisions about saving and investments? Do you call around, read newspapers, magazines, material you get in the mail, use information from television, radio, the Internet or advertisements? Do you get advice from a friend, relative, lawyer, accountant, banker, broker, or financial planner? Or do you do something else?

Respondents can give multiple answers to the above questions. Here we code respondents who included ‘financial planner’ among their sources of information as consulting a professional financial planner. These households are used as our treatment group, while those who did not list ‘financial planner’ among their sources of information constitute the control group.

The SCF contains a great deal of detail regarding household financial holdings, allowing us several choices for how we measure financial outcomes. Here we select three: total household financial wealth, the proportion of all financial assets in equities, and diversification of financial assets as measured by the number of asset categories with positive balances that a household has. These three measures reflect elements of both financial plan implementation and outcomes, though they only implicitly measure the steps taken in developing a financial plan.

### Generalizing to a population

In the analyses below, we focus on households with working-age adults in the bottom 99 percent of the income distribution. To this end, we restrict

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the SCF sample to households where the head (defined as males in married couple households by SCF convention) is between the ages of 18 and 64. We also eliminate households with annual incomes of \$425,000 or more, as this is the approximate threshold for the top 1 percent that year (Luhby, 2011). To illustrate how the choice of target populations can affect conclusions drawn from an evaluation study, we divide the sample by median household income in 2007 and re-estimate our models for each subgroup.

As noted earlier, survey respondents are sometimes hesitant to answer what they perceive to be sensitive financial questions, so SCF staffers have developed imputation algorithms to deal with missing values. Imputations are generated five separate times for each missing value and stored in five replicates for each record. The replicates not only provide greater precision for the estimation of missing values, but their use also requires that researchers use programming macros to compute correct standard errors (Kennickell, 2009). To keep our analyses manageable, we follow the simpler strategy of undertaking separate estimation for each of the five implicate datasets (Hogarth et al., 2004). Our results do not differ markedly across the five implicate datasets, so we present estimates for dataset three here.<sup>9</sup>

### Insuring that observed relationships are credible

In what follows, we review three sets of estimates for the relationship between consulting a financial planner and our outcomes of interest, using the SCF. Bivariate estimates, presented first, do neither control for the influence of socio-demographic and economic factors that might affect the three financial outcomes of interest, nor do they allow for potential simultaneity of financial outcomes and seeking counsel from a professional financial planner. Second, we report ordinary least squares estimates that remove the systematic influence of socio-demographic and economic factors. Last, we report propensity score estimates that net out the socio-demographic and economic influences and also adjust for the potential simultaneity.

Descriptive information for the three outcome measures appears in Table 8.1, separately for households that do consult professional financial planners and those who do not. Here all observed mean differences are statistically significant ( $p < 0.05$ ): households that consult professional financial planners hold significantly more financial wealth, have a larger fraction of their wealth in stock equity, and have greater diversification of assets when compared to those households that do not consult professional financial planners. This holds both for the full SCF sample, and for the two income-stratified subsamples. In other words, using only bivariate

**166 The Market for Retirement Financial Advice**TABLE 8.1 Weighted means and *t*-tests for financial outcome variables

	Full sample ( <i>N</i> = 2,881)		<Median income ( <i>N</i> = 1,350)		≥Median income ( <i>N</i> = 1,531)	
	Consults planner ( <i>N</i> = 696)	Does not consult planner ( <i>N</i> = 2,185)	Consults planner ( <i>N</i> = 178)	Does not consult planner ( <i>N</i> = 1,172)	Consults planner ( <i>N</i> = 518)	Does not consult planner ( <i>N</i> = 1,013)
Total financial wealth (\$)	228,123	89,189	79,145	23,746	302,286	178,626
<i>t</i> -test	7.86**		2.84**		4.55**	
Proportion in equity (0–1.0)	0.35	0.21	0.24	0.12	0.41	0.33
<i>t</i> -test	10.44**		5.69**		4.62**	
Number of asset categories (1–8)	2.86	2.00	2.22	1.50	3.18	2.70
<i>t</i> -test	15.22**		8.84**		7.38**	

Note: \*\**p* < 0.05.

Source: Authors' calculations from the 2007 Survey of Consumer Finances.

comparisons, it appears that professional financial planners do help households adopt sound financial planning principles and create more financial wealth.

The conclusion above must be tempered by the recognition that other systematic processes affect the decisions households might make regarding stock equity, portfolio diversification, and ultimately, financial wealth accumulation. For example, individuals who are more highly educated are more likely to understand the importance of asset diversification. Likewise, those who have minor children in the home may find it more challenging to build financial wealth because of the need to spend income on child-related goods and services. To capture the impact of consulting with a professional financial planner while also simultaneously accounting for these other socio-demographic and economic processes that affect a household's financial interests, we utilize multivariate (OLS) regressions (independent variables are defined in Appendix Table 8.A1).

Tables 8.2–8.4 report our multivariate OLS estimates for the three outcomes of interest, for both the full sample and the income-stratified subsamples. Several features of these results relative to the bivariate results are worth noting. Strikingly, the estimated impact of consulting with a professional financial planner declines by more than 50 percent for each of the three outcomes. In the case of the full sample, for example, the estimate of a financial planner's contribution to total financial wealth falls from \$138,934 (bivariate) to \$66,182 (OLS), controlling for the socio-demographic and

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TABLE 8.A1 Weighted means for ordinary least squares regression covariates

Variable name	Definition	Full sample	<Median income	≥Median income
Age	Age of the household head measured in years (head defined by SCF to be the husband in married couple households)	43.03	41.21	45.04
Education	Head's education measured by years of formal schooling (head defined by SCF to be the husband in married couple households)	13.50	12.63	14.46
Female	Interviews are done with the person in the household who self-identifies as the most knowledgeable about financial matters (1 = female, 0 = male)	0.54	0.58	0.50
Marital status	1 = Household head is currently married, 0 = otherwise	0.61	0.42	0.81
Number of children	Number of minor children in the home	1.01	0.97	1.06
Non-Hispanic Black	1 = Non-Hispanic Black household head, 0 = otherwise	0.14	0.19	0.09
Hispanic	1 = Hispanic household head, 0 = otherwise	0.11	0.14	0.07
Wage income	Annual wage income measured in \$10,000s	5.34	2.18	8.83
DC pension plan	1 = Head and/or spouse have a DC pension plan with current employer(s), 0 = otherwise	0.42	0.23	0.62
DB pension plan	1 = Head and/or spouse have a DB pension plan with current employer(s), 0 = otherwise	0.22	0.13	0.32
DC and DB pension plan	1 = Head and spouse have both DC and DB pension plans with current employer(s), 0 = otherwise	0.11	0.03	0.20

Source: Authors' calculations from the 2007 Survey of Consumer Finance.

economic factors hypothesized to affect financial wealth. Correspondingly, the stock equity effect falls from 14 to 6 percent, while the diversification effect falls from 86 to 39 percent.

Comparisons across the income-stratified subsamples also reveal sizable differences in the estimated financial planner effects. In the case of total financial wealth, the estimates indicate that consulting a financial planner appears to boost higher-income households' financial wealth by \$76,739, while such consultation has a smaller effect on lower-income households' financial wealth, raising it by \$39,488 holding other factors constant. Financial planner consultation appears to have a larger marginal effect on the fraction of wealth held in stock equity for lower-income households (7 percent) compared to higher-income households (4 percent). Likewise,

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TABLE 8.2 Ordinary least squares parameter estimates of total financial wealth<sup>a</sup>

Independent variables	Full sample	<Median income	≥Median income
	Coefficient	Coefficient	Coefficient
Consults financial planner	66,182.00 (3.88**)	39,488.00 (1.99**)	76,739.00 (3.01**)
Age <sup>b</sup>	6,385.75 (10.53**)	1,927.39 (3.31**)	11,561.00 (9.80**)
Age-squared <sup>b</sup>	243.44 (4.64**)	42.83 (0.90)	359.69 (3.15**)
Education	17,676.00 (5.71**)	6,207.86 (2.12**)	35,708.00 (5.96**)
Female	-39,569.00 (-2.85**)	-10,725.00 (-0.77)	-46,341.00 (-1.95)
Marital Status	-3,468.22 (-0.22)	12,196.00 (0.81)	-28,962.00 (-0.92)
Number of children	5,722.47 (0.91)	1,402.57 (0.24)	6,209.15 (0.54)
Non-Hispanic Black <sup>c</sup>	-39,352.00 (-1.97)	-17,310.00 (-0.99)	-98,753.00 (-2.38**)
Hispanic <sup>c</sup>	-4,305.89 (-0.19)	-11,112.00 (-0.53)	-12,559.00 (-0.28)
Wage income (\$10,000s)	190.71 (11.75**)	28.22 (0.60)	172.97 (7.73**)
DC pension plan	-8,600.09 (-0.50)	22,238.00 (1.23)	-10,173.00 (-0.34)
DB pension plan	-60,088.00 (-2.53**)	12,093.00 (0.51)	-96,691.00 (-2.32**)
DC and DB pension plan	12,344.00 (0.37)	-20,118.00 (-0.42)	20,132.00 (0.39)
Adjusted- $R^2$	0.15	0.02	0.16
F-statistic	39.55**	2.61**	23.24

<sup>a</sup> Regressions use SCF final weights.

<sup>b</sup> Age centered on the mean age of the household heads so as to avoid multicollinearity between age and age-squared (Glantz and Slinker, 1990).

<sup>c</sup> The omitted racial/ethnic category in this sequence of dummy variables is households where the head is non-Black and non-Hispanic.

Notes: *t*-statistics in parentheses. \*\* $p < 0.05$ .

Source: Authors' calculations from the 2007 Survey of Consumer Finances.

the marginal diversification effects are larger for lower-income households (43 percent) than higher-income households (32 percent). Thus, the OLS models provide a more nuanced picture of professional financial planners' impacts compared to the bivariate results. While still statistically significant, the planner effects are more modest for all three outcomes and the effects also vary markedly by income group. These findings illustrate the importance of controlling for other factors

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TABLE 8.3 Ordinary least squares parameter estimates of proportion stock equity<sup>a</sup>

Independent variables	Full sample	<Median income	≥Median income
	Coefficient	Coefficient	Coefficient
Consults financial planner	0.06 (4.69**)	0.07 (3.53**)	0.04 (2.76**)
Age <sup>b</sup>	0.00 (5.09**)	0.00 (4.06**)	0.00 (3.19**)
Age-squared <sup>b</sup>	0.00 (-2.53**)	0.00 (-0.11)	0.00 (-3.17**)
Education	0.02 (6.82**)	0.01 (3.44**)	0.02 (4.28**)
Female	-0.04 (-3.56**)	-0.01 (-0.82)	-0.06 (-4.12**)
Marital status	0.00 (0.06)	0.00 (-0.21)	-0.03 (-1.40)
Number of children	-0.01 (-2.64**)	-0.01 (-2.29**)	-0.01 (-1.77)
Non-Hispanic Black <sup>c</sup>	-0.07 (-5.06**)	-0.08 (-4.43**)	-0.05 (-2.03**)
Hispanic <sup>c</sup>	-0.06 (-3.30**)	-0.06 (-2.69**)	-0.06 (-2.20**)
Wage income (\$10,000s)	0.01 (5.90**)	0.01 (2.19**)	0.00 (3.09**)
DC pension plan	0.19 (15.09**)	0.21 (11.71**)	0.14 (7.64**)
DB pension plan	0.00 (0.17)	-0.02 (-0.67)	-0.01 (-0.28)
DC and DB pension plan	0.01 (0.27)	0.09 (1.97)	0.01 (0.16)
Adjusted- $R^2$	0.27	0.22	0.14
F-statistic	81.43**	30.15**	20.02**

<sup>a</sup> Regressions use SCF final weights.

<sup>b</sup> Age centered on the mean age of the household heads so as to avoid multicollinearity between age and age-squared (Glantz and Slinker, 1990).

<sup>c</sup> The omitted racial/ethnic category in this sequence of dummy variables is households where the head is non-Black and non-Hispanic.

Notes: *t*-statistics in parentheses. \*\* $p < 0.05$ .

Source: Authors' calculations from the 2007 Survey of Consumer Finances.

that may influence financial outcomes, and also the relevance of focusing on the target population when selecting a sample for empirical work.

As noted above, OLS models do not incorporate a correction for reverse causation. Yet, the possibility exists, for example, that those with more financial wealth may be more motivated to seek professional advice in order to protect that wealth. In our data, statistical tests for reverse

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TABLE 8.4 Ordinary least squares parameter estimates of the number of asset categories<sup>a</sup>

Independent variables	Full sample	<Median income	≥Median income
	Coefficient	Coefficient	Coefficient
Consults financial planner	0.39 (8.34**)	0.43 (6.27**)	0.32 (5.26**)
Age <sup>b</sup>	0.02 (12.42**)	0.02 (8.81**)	0.02 (7.54**)
Age-squared <sup>b</sup>	0.00 (4.44**)	0.00 (4.46**)	0.00 (3.21**)
Education	0.12 (14.33**)	0.09 (9.05**)	0.11 (7.86**)
Female	-0.04 (-1.00)	0.02 (0.45)	-0.10 (-1.79)
Marital status	0.10 (2.41**)	0.03 (0.57)	-0.11 (-1.51)
Number of children	0.01 (0.34)	0.01 (0.35)	0.02 (0.58)
Non-Hispanic Black <sup>c</sup>	-0.33 (-6.15**)	-0.37 (-6.20**)	-0.12 (-1.25)
Hispanic <sup>c</sup>	-0.31 (-4.87**)	-0.34 (-4.68**)	-0.28 (-2.58**)
Wage income (\$10,000s)	0.04 (9.87**)	0.07 (4.24**)	0.02 (4.53**)
DC pension plan	0.80 (17.26**)	0.91 (14.53**)	0.53 (7.52**)
DB pension plan	0.16 (2.46**)	0.08 (0.93)	0.07 (0.65)
DC and DB pension plan	-0.20 (-2.25**)	-0.06 (-0.37)	-0.12 (-0.95)
Adjusted- $R^2$	0.42	0.38	0.20
F-statistic	164.92	63.54**	30.84**

<sup>a</sup> Regressions use SCF final weights.

<sup>b</sup> Age centered on the mean age of the household heads so as to avoid multicollinearity between age and age-squared (Glantz and Slinker, 1990).

<sup>c</sup> The omitted racial/ethnic category in this sequence of dummy variables is households where the head is non-Black and non-Hispanic.

Notes: *t*-statistics in parentheses. \*\* $p < 0.05$ .

Source: Authors' calculations from the 2007 Survey of Consumer Finances.

causation (Baum et al., 2003) suggest that it exists.<sup>10</sup> Here we correct for this problem using a propensity score approach because, unlike structural equation modeling, the propensity score approach is not limited by the functional form that is chosen. It is also appealing because individuals in the treated sample who have no counterfactual in the control sample are dropped (Black and Smith, 2004; Gibson-Davis and Foster, 2006).<sup>11</sup>

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In what follows, we develop a propensity score that adjusts for the bias caused by reverse causation by creating matches between members of the treatment and control groups rather than through a random assignment process used in true experiments (Rosenbaum and Rubin, 1983, 1984). We first estimate a logistic regression in which the dependent variable indicates whether the respondent consults with a financial planner. Independent variables in this equation include all factors that might affect the decision to meet with a financial planner, as well as those factors that might affect the substantive outcomes of interest (i.e., total financial wealth, proportion in stock equity, and diversification of assets). We use responses to attitudinal questions regarding financial risk, financial luck, spending, and retirement confidence as indicators of factors that affect only the likelihood of meeting with a financial planner. We also include the standard socio-demographic and economic variables described above as factors that could influence both the likelihood of consulting a financial planner and our substantive outcomes of interest. Using the first stage logit estimates (results presented in Appendix Table 8.A2), we generate predicted probabilities of meeting with a financial planner. These then are used to match treatment households to controls.<sup>12</sup>

Figure 8.2 compares the outcome of interest—the estimated incremental effect of consulting a financial planner on total financial wealth—across all three estimation approaches. Here we show that the bivariate model suggests that consultation with a professional financial planner is associated with significantly more financial wealth (for the full sample as well as both the lower- and higher-income subsamples). Adjusted for socio-demographic and economic characteristics, the OLS estimate of the net effect of consulting a professional financial planner is reduced. After using the propensity score adjustment, the impact of the professional financial planner on financial wealth becomes statistically insignificant (for the full and the two subsamples).

Turning to the proportion of total financial wealth held in stocks, Figure 8.3 again shows that simple bivariate comparisons provide the largest estimate of financial planner effects. Controlling for socio-demographic and economic covariates reduces the effect size. And adjusting for simultaneity reduces estimated effects even further (for both the full and the lower-income subsamples). Interestingly, now we see that consultation with a professional financial planner leads to a significantly higher equity proportion for lower-income households, but it has no significant effect for higher-income households.

Finally, Figure 8.4 shows how the estimates differ when the outcome of interest is the number of different financial assets held. Again, in all cases, the bivariate estimates of consulting a professional financial planner are largest, followed by the OLS estimates, and the propensity score estimates

TABLE 8.A2 Logistic regression parameter estimates of the first stage of the propensity score analysis: dependent variable is 'consults a financial planner'

Independent variables	Full sample			<Median income sample			≥Median income sample		
	Coefficient	Odds ratio	95% CI	Coefficient	Odds ratio	95% CI	Coefficient	Odds ratio	95% CI
Head age <sup>a</sup>	-0.02	0.98	0.98-0.99	-0.01	0.99	0.98-1.00	-0.02	0.98	0.97-0.99
Head age-squared <sup>a</sup>	0.00	1.00	1.00-1.00	0.00	1.00	1.00-1.00	0.00	1.00	1.00-1.00
Head education	-0.13	0.88	0.84-0.92	-0.13	0.88	0.81-0.95	-0.11	0.89	0.84-0.95
Female	-0.20	0.82	0.67-0.99	-0.16	0.85	0.60-1.20	-0.24	0.78	0.62-0.99
Marital status	-0.23	0.80	0.64-0.99	-0.13	0.88	0.61-1.27	-0.15	0.86	0.63-1.18
Number of children	0.01	1.01	0.93-1.11	0.07	1.07	0.91-1.26	-0.01	0.99	0.89-1.11
Non-Hispanic Black <sup>b</sup>	0.30	1.35	0.97-1.88	0.00	1.00	0.64-1.56	0.56	1.75	1.04-2.93
Hispanic <sup>b</sup>	0.22	1.25	0.84-1.86	0.06	1.06	0.57-1.97	0.23	1.26	0.74-2.13
Wage income (\$10,000s)	-0.01	0.99	0.98-1.01	0.00	1.01	0.93-1.09	0.00	1.00	0.99-1.02
DC pension plan	-0.27	0.77	0.61-0.96	-0.15	0.86	0.56-1.32	-0.28	0.76	0.57-1.01
DB pension plan	-0.30	0.74	0.54-1.01	-0.33	0.72	0.43-1.21	-0.33	0.72	0.48-1.07
DC and DB pension plan	0.14	1.15	0.76-1.74	-0.25	0.78	0.30-2.04	0.24	1.27	0.77-2.09
Takes no financial risks (1 = yes)	1.03	2.81	2.17-3.66	0.78	2.17	1.51-3.13	1.24	3.45	2.30-5.18
Compared to others, lucky in financial affairs (1 = strongly agree or somewhat agree)	-0.33	0.72	0.58-0.89	-0.04	0.96	0.67-1.38	-0.46	0.63	0.48-0.83

When things increase in value, more likely to spend money (1 = strongly disagree or somewhat disagree)	-0.09	0.91	0.76-1.10	-0.39	0.67	0.47-0.96	0.03	1.03	0.82-1.29
Household retirement income will be enough to maintain standard of living, satisfactory, or very satisfactory (1 = yes)	-0.04	0.96	0.80-1.16	-0.35	0.71	0.50-0.99	0.11	1.12	0.89-1.41
In planning saving/spending, most important time horizon is 5+ years (1 = yes)	-0.27	0.76	0.63-0.92	-0.41	0.66	0.47-0.94	-0.16	0.85	0.68-1.07
$\chi^2$	359.39**			89.51**					127.96**

<sup>a</sup> Age centered on the mean age of the household heads so as to avoid multicollinearity between age and age-squared (Glantz and Slinker, 1990).

<sup>b</sup> The omitted racial/ethnic category in this sequence of dummy variables is households where the head is non-Black and non-Hispanic.

*Note.* \*\*\* $p < 0.05$ .

*Source:* Authors' calculations from the 2007 Survey of Consumer Finances.

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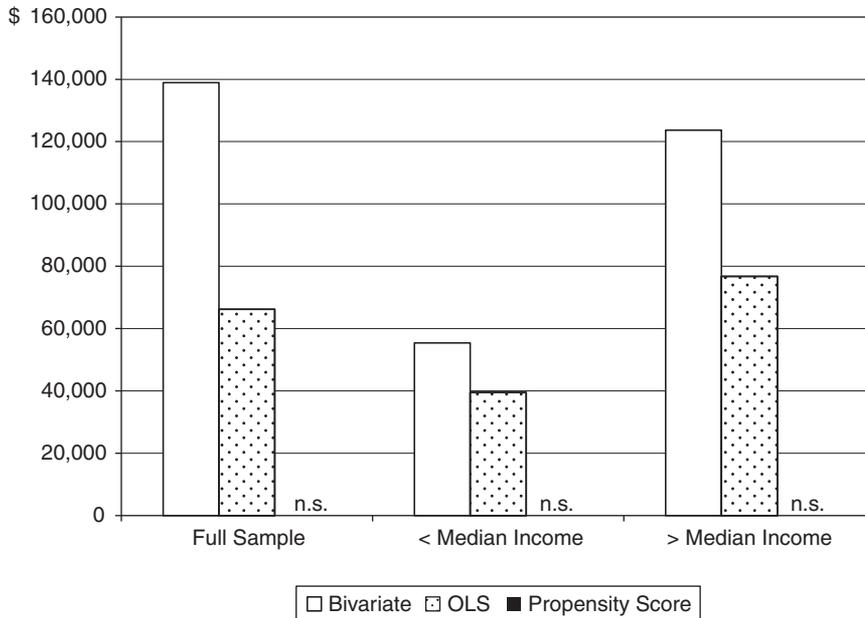


Figure 8.2 Estimated incremental effect of consulting a financial planner on total financial wealth

Note: n.s. = not statistically significant at  $p < 0.05$ .

Source: Authors' calculations from the 2007 Survey of Consumer Finances (Kennickell, 2009).

are smallest. But, unlike the above, now we find that consulting a professional financial planner produces greater diversification.

## Conclusion

This chapter describes how different methodologies can alter the estimated impact of financial professionals on advice recipients. We now know with reasonable certainty that simply comparing people who do and do not use professional financial advice paints an overly rosy picture of the benefits of working with a financial planner. More careful research that addresses self-selection, other confounding variables, and reverse causation shows smaller benefits of working with a financial planner. Moreover, the main effect is to enhance account diversification, as shown by our SCF analyses.

Yet knowledge of the impact of professional financial advice remains in its infancy, and future research can profitably proceed in three directions.

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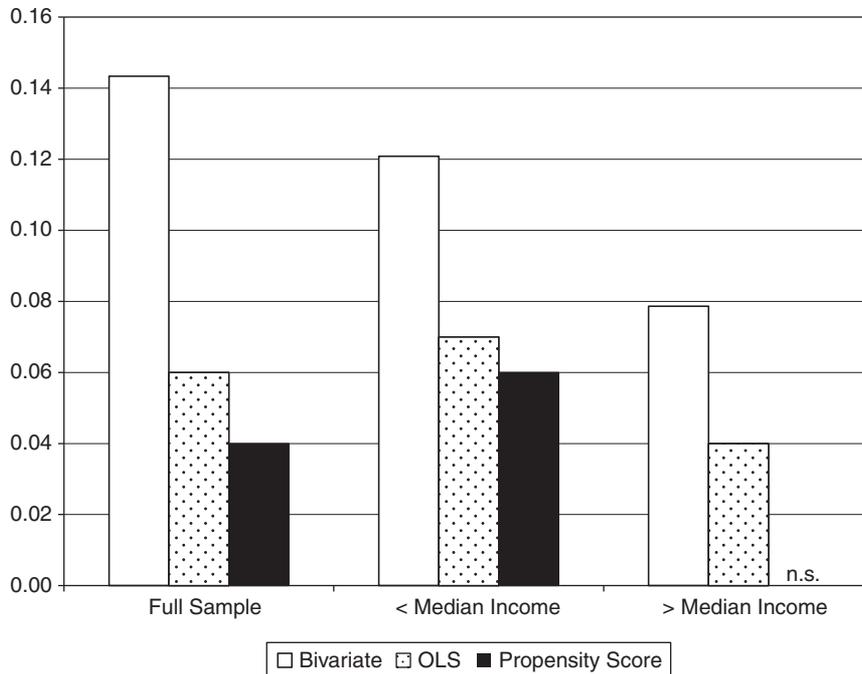


Figure 8.3 Estimated incremental effect of consulting a financial planner on proportion of total financial wealth held in stocks

Note: n.s. = not statistically significant at  $p < 0.05$ .

Source: Authors' calculations from the 2007 Survey of Consumer Finances (Kennickell, 2009).

First, to date no study has carefully distinguished different types of financial professionals, even though they vary enormously in expertise, access to investment products, and financial incentives. Some studies lump all types of financial planners into a single category, while others examine a single type of advisor employed by a single financial institution. Much remains to be learned about the impact of different types of financial professionals.

Second, structural equations and propensity score approaches are useful steps in learning more about the effects of professional financial advice, but these methods are no substitute for an RFE. Though these are expensive to conduct, RFEs eliminate the possibility of self-selection and reverse causation. Moreover, randomized experiments can recruit people who, under real-world conditions, would rarely elect to consult a financial planner but who might gain the most from professional advice—people with below-average incomes and/or in the early stages of their work life.

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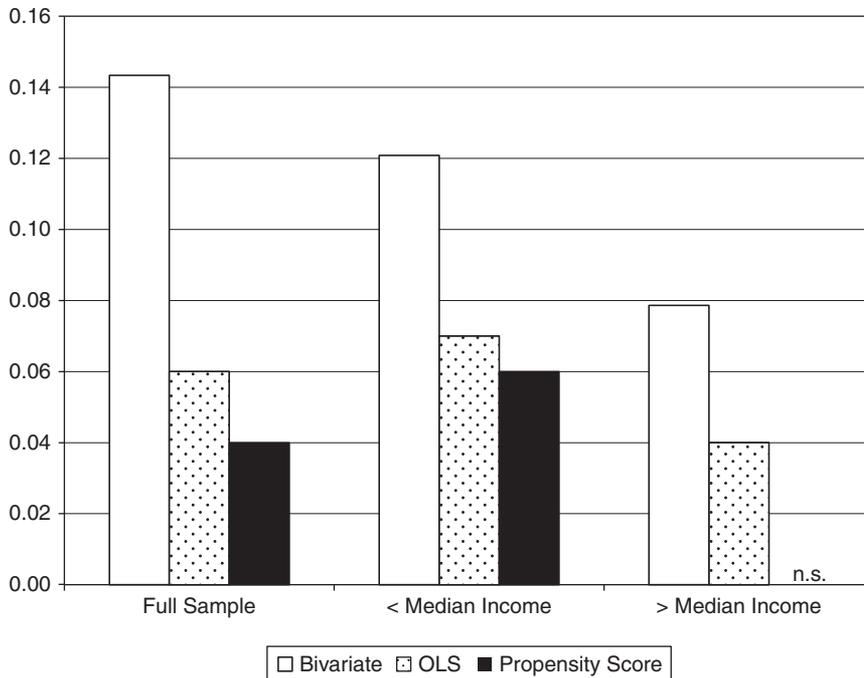


Figure 8.4 Estimated incremental effect of consulting a financial planner on the number of different types of financial assets held

Note: n.s. = not statistically significant at  $p < 0.05$ .

Source: Authors' calculations from the 2007 Survey of Consumer Finances (Kennickell, 2009).

Third, in the future, we need studies of longer duration than those currently available. It is not surprising that the short-term effect of consulting a financial planner is to diversify a person's portfolio or rebalance it toward a particular risk profile. In light of the short-term and often ongoing financial costs of working with a financial professional, some may not find this compelling evidence of the value of professional advice. Results showing that advised investors feel more confident about their financial futures than do-it-yourself investors (as comforting as such findings may be to existing clients) are also unlikely to move people toward employing financial professionals. It would be more useful to demonstrate that working with an advisor or planner boosts long-term rates of return and levels of asset accumulation. Given short-term fluctuations in financial markets, what we need is evidence on account performance over the medium- or long-term, to allow planners and researchers alike to drop the word 'potential' in the phrase: 'potential benefits of professional financial advice.'

## Endnotes

1. The growth in investor numbers and options has been fueled, in part, by the dramatic shift from defined benefit (DB) to defined contribution (DC) pension plans over the same time period. In 1985, 38 percent of Americans whose employer provided a pension plan were in a DB plan only, 35 percent were in a combination DB/DC plan, and 29 percent were in a DC 401(k) plan only. By 2008, those percentages had shifted to 7, 26, and 67 percent, respectively (EBRI, 2010: figure 2).
2. The Certified Financial Planning Board lists twenty-three different financial services credential designations (see <http://www.cfp.net/learn/knowledge-base.asp?id=15>).
3. Throughout this chapter we use the term 'financial outcomes' as a summary term for financial practices and wealth-related results that may be linked to seeking the counsel of a professional financial planner.
4. Once conceptual decisions about advising exposure and key outcomes are made, the next step is to insure that these measures are operationalized in ways that have little random measurement error. That is, the evaluator must insure that the data sources used (e.g., survey questions or administrative records) have minimal measurement error. Such measurement error may be particularly problematic in the case of surveys containing potentially complicated and/or sensitive financial questions. Fortunately, there is a literature that details the best practices for asking financial questions in a way that minimizes measurement reliability concerns (Avery et al., 1988; Juster and Smith, 1997; Kennickell and Starr-McCluer, 1997; Duncan and Petersen, 2001; Hurd et al., 2003).
5. Although the RAND American Life Panel survey is conducted via the Internet, the Panel has special arrangements to facilitate the participation of people with limited Internet access.
6. The influence of random elements can lead to erroneous conclusions in two ways. First, chance variation can lead to the conclusion that a professional financial planner had an impact on a financial outcome when s/he actually did not. The risk of making this error (i.e., Type I error) can be minimized by setting a low alpha level (i.e.,  $<0.05$ ) for statistical significance which will vary by sample size. Second, chance variation can lead to the conclusion that professional finance counsel had no impact on a financial outcome when it actually did (i.e., Type II error). The likelihood of making this second type of error is a function of the size of the sample, the magnitude of the effect size in the population associated with having sought professional financial counsel, and the type of statistical test used. Generally speaking, larger samples, larger effect sizes, and multivariate statistical tests all reduce the likelihood of making this second type of error.
7. In theory, RFEs should have four groups to control for measurement effects. In practice, this is very rarely done.

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8. Sample weights are used when presenting descriptive statistics for the 2007 SCF because of the unusual sample design and to correct for systematic refusals to participate in the SCF. The decision to weight multivariate analyses is the subject of some debate in the literature (Lindamood et al., 2007); in the current chapter, no weights were used.
9. Other sets of estimates are available from the authors on request.
10. The test involves estimating the reduced form equation in which financial planner consultation is the dependent variable. The residuals from this equation are then included as an additional regressor in the structural equations estimating each of the three financial outcomes. The resulting Durbin-Wu-Hausman  $F$ -statistic generated from this second equation is a measure of endogeneity. For the current application, that  $F$ -statistic is 5.88 ( $p = 0.02$ ) for total financial wealth, 15.37 ( $p < 0.00$ ) for stock equity, and 42.85 ( $p < 0.00$ ) for asset diversification, suggesting that there is ample evidence that consulting a financial planner and each of the three outcomes of interest are endogenous.
11. Rosenbaum and Rubin (1983, 1984) proposed the use of the propensity score method to address simultaneity problems by balancing members of a so-called treatment group (i.e., individuals who consult a financial planner) with specific members of a so-called control group (i.e., individuals who have NOT met with a financial planner) with regard to their covariates.
12. Several matching methods have been used in the literature (see Gibson-Davis and Foster, 2006). Given that there is no consensus on the best matching method, we use a radius caliper matching technique that makes use of all members of the control group within a 0.01 radius of the treatment observation. After the matching,  $t$ -tests are conducted to ascertain if statistically significant differences exist between the treatments and the controls.

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