Financial Conduct Authority

Research Note

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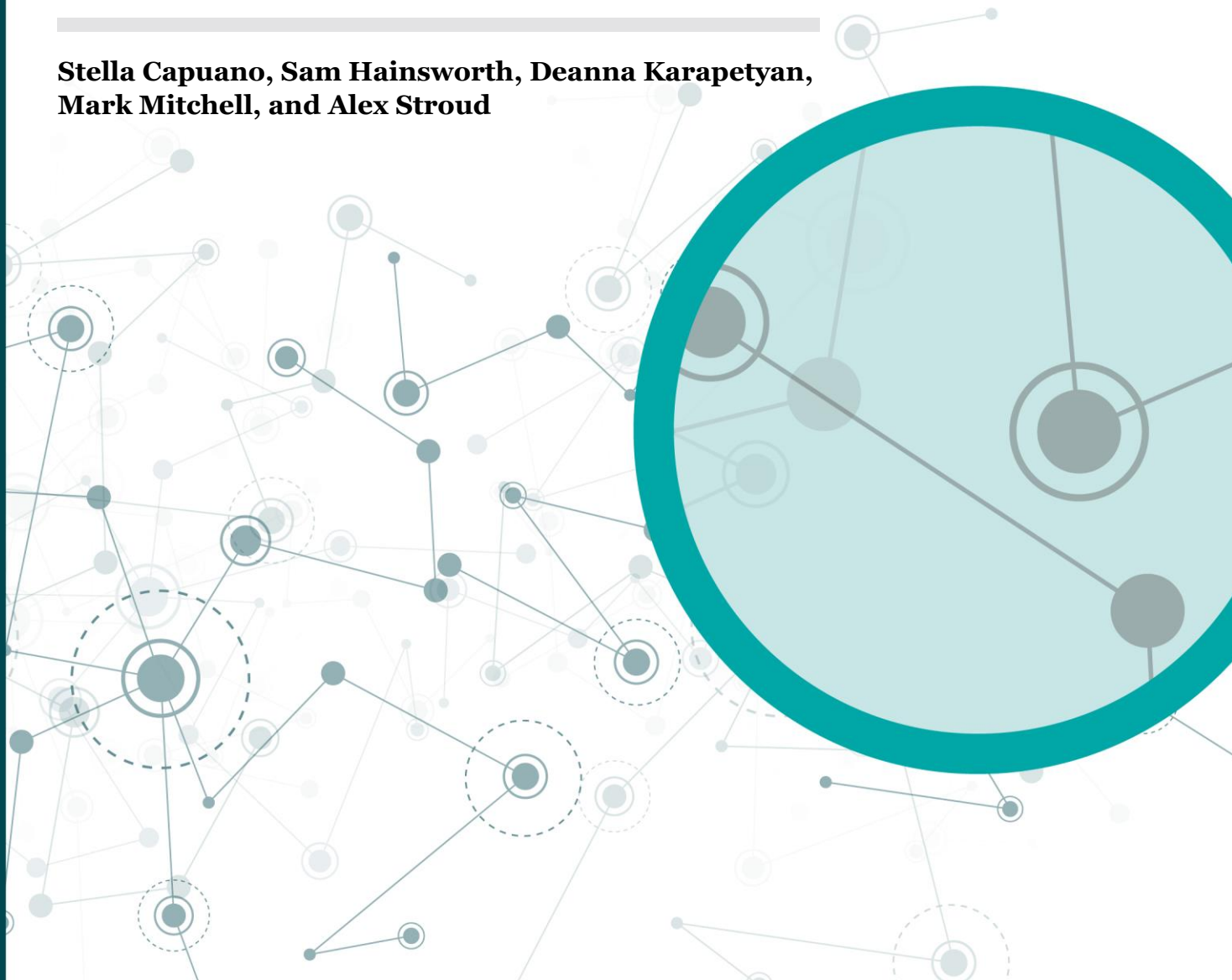
Bridging the advice gap: Estimating the relationship between financial advice and wealth

Stella Capuano, Sam Hainsworth, Deanna Karapetyan, Mark Mitchell, and Alex Stroud



FCA research notes in financial regulation

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Summary

Many people struggle to access affordable financial advice (Financial Lives Survey, 2022), an issue the FCA is seeking to address by increasing firms' ability to assist customers in financial decisions through the Advice Guidance Boundary Review (AGBR). To inform the AGBR, this research explores the characteristics of people who receive financial advice and estimates the wealth benefits associated with taking advice. It forms part of a wider programme of work supporting the design of effective financial support services that differ from existing financial advice. The AGBR is not seeking to increase the number of people getting existing forms of regulated financial advice. However, we estimate the value of existing advice because there are likely to be similarities with the benefits of new forms of support and because understanding the value of financial advice is important for determining a proportionate approach to policy reforms.

We find evidence that getting financial advice is associated with an increase in wealth of up to 10% in the years following advice, relative to those who did not receive advice. The relationship between advice and wealth fades and becomes increasingly uncertain over time. We also find that advice is likely more valuable for people receiving lump sum payments like inheritances, gifts, or one-off pension payments. Our analysis has several technical limitations, and so we are cautious about interpreting the size of this estimate and applying it to other forms of support.

In our analysis, we use data from the Office for National Statistics' (ONS) Wealth and Assets Survey, covering 2010 to 2020, to compare changes in wealth for people who received financial advice with those who did not, while accounting for measurable differences between the two groups. By comparing changes in wealth, we can estimate the impact of advice more rigorously than from a simple 'point-in-time' comparison of wealth across the two groups.

Our evidence suggests that removing barriers to advice and creating more inclusive advisory services may present an opportunity to enhance consumer financial resilience through improved financial outcomes. Related literature suggests financial advice could support people to make better decisions, fewer costly mistakes such as over allocating to cash, or less inefficient pension strategies. The FCA is seeking to help more consumers get support with financial decisions through the introduction of new financial advice services.

1 Overview

Purpose

This research is part of the FCA's work to promote growth and consumer financial resilience and strengthen the regulatory framework guiding financial support, including advice, in the UK. It aligns with the strategic objectives outlined in Discussion Paper DP23/5, which explores potential reforms to expand access to financial advice and support services (FCA, 2024).

Understanding the value of advice is important for determining a proportionate approach to reforms, ensuring that decisions are evidence based. This study estimates the wealth benefits of regulated financial advice, focussing on savings, investments, and pension accumulation - areas central to long term financial resilience (Xu & Zia, 2012) (Messy & Lewis, 2012).

Estimating the value of financial advice is complex. A person might get advice because they expect their wealth to increase rather than the advice causing the increase in wealth (i.e., reverse causality). In addition, a range of factors like financial literacy, marital status, and life events can impact both wealth and the likelihood someone seeks financial advice (i.e., selection bias). If not accounted for, estimates of the value of advice will not reflect its true value.

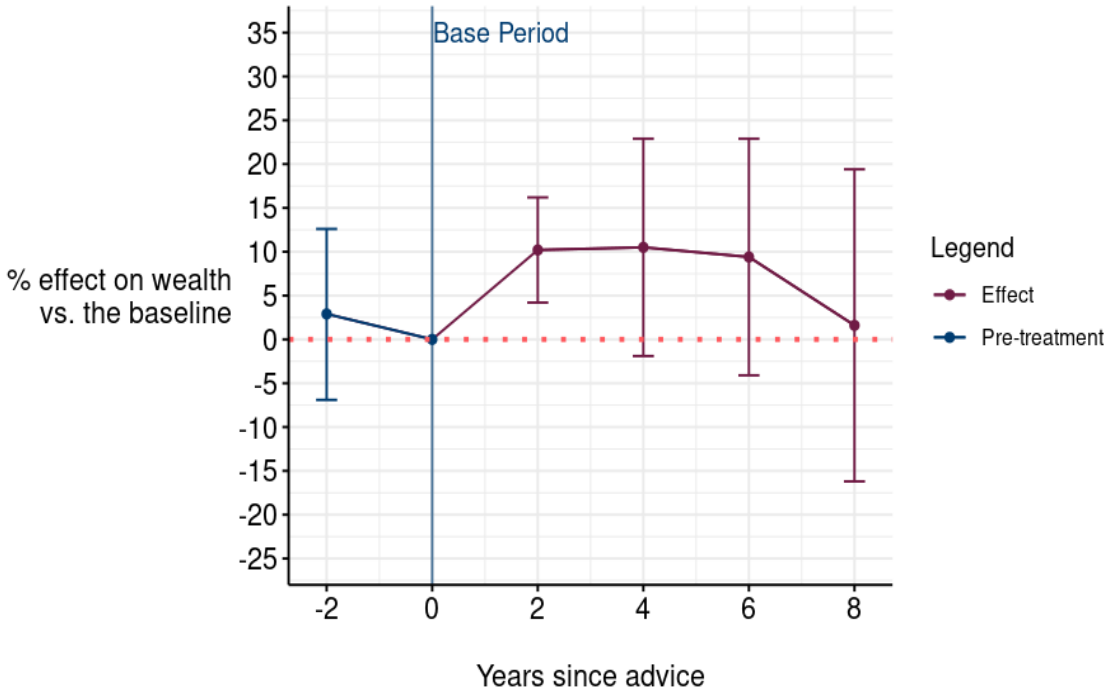
To our knowledge, our research is the first attempt at partially addressing both reverse causality and selection bias in estimating the value of advice. To do this, we use data on the same individuals over time (i.e., panel data) and compare changes in wealth for those who do and do not get advice (i.e., difference-in-differences or 'DiD'), while controlling for differences between the groups.

Key findings

- **Those who receive financial advice are demographically different to those who do not:** Compared to those who did not receive financial advice, individuals who receive financial advice are, on average, older, £274,000 wealthier, 15% more likely to hold a degree, 15% more likely to be married, and more likely to receive lump sum payments before and after receiving taking advice. We account for these differences using causal inference methods to improve the credibility of our estimates of the relationship between financial advice and wealth.
- **Receiving financial advice is associated with an increase in wealth in future periods, when compared to those who don't receive advice:** We estimate an increase in wealth of up to 10% in the years following financial advice, relative to those that do not receive it (See Figure 1 below).
- **However, this relationship diminishes over time, with estimates becoming smaller and more uncertain:** Uncertainty in our estimates, driven by variability in

outcomes, makes it difficult to draw conclusions on the long-term nature of this relationship. For example, some of the variability in outcomes could be driven by the inclusion of individuals receiving financial advice for pension decumulation decisions, which we are not able to identify in the data. People seeking advice for pension decumulation typically have an objective of sustainably reducing wealth, rather than wealth accumulation.

Figure 1. Financial advice has a positive association with wealth, but this relationship fades and becomes more uncertain over time:



Source: ONS, Wealth and Asset Survey, Wave 5 (2014-16).
Notes: This chart highlights the average difference in the change in wealth between those who do and do not get advice, in each period relative to the baseline. It shows that those who receieved advice experience a larger increase in wealth initially, but that effect dereases and becomes more uncertain over time.

- **Our results are sensitive to the inclusion of lump sum payments, such as inheritances:** Individuals who receive lump sum payments, such as inheritances or gifts, contribute substantially to the positive association observed between financial advice and wealth. When we exclude these individuals from our analysis, the relationship is not clearly different from zero (i.e., we identify no clear relationship between advice and wealth). It is intuitive that our estimates decrease, as the excluded group is expected to benefit from financial advice given the complex financial decisions that are often connected to receiving a one-off sum of money. However, it also demonstrates that some of the estimated benefit in our main results may be due to the receipt of one-off lump sums by this group, after receiving financial advice. We discuss in more detail in the robustness checks section of the report.

Policy implications

Our findings show that financial advice is accessed by wealthier, older individuals and is associated with positive changes in wealth following advice, when compared to those who do not receive it. This presents an opportunity to develop financial support services aimed at helping more consumers with their financial decisions. The literature (See Section 2) suggests that financial advice can help consumers avoid costly mistakes such as keeping excessive savings in cash, inefficient tax planning, or holding investments that do not align with their risk appetite. If underserved populations face similar challenges, then new forms of financial support which aim to tackle these issues will likely benefit consumers in similar ways.

Our estimates show that the wealth benefits from receiving advice may diminish over time. This finding may point to the need for ongoing or refreshed advice, or an improvement in the quality of advice to ensure it can have sustained wealth benefits over time. This diminishing relationship could also reflect the inherently complex nature of wealth. Wealth comprises various assets such as physical items, property, pensions, and financial investments, each responding differently to changes in market conditions and individual choices. For example, while an individual may initially implement recommendations from a financial advisor, subsequent market, or life events — such as changes in tax policy, financial gifts or inheritances, or unexpected health expenses — could lead to behaviour that deviates from the original advice or renders it obsolete. These varied and often unpredictable influences make it difficult to isolate the direct impact of advice, contributing to increased uncertainty in our long-term estimates.

Our analysis has limitations that should be considered when interpreting the results or applying them more broadly. Our findings are based on historical data, reflecting the quality of financial advice provided to specific individuals during the study period. The quality and impact of advice could change over time or vary for different demographic groups, which is not assessed in this analysis. Additionally, some limitations stem from the data and methods used, such as limited pre-advice data being available. Our analysis builds on the evidence base relating to the value of financial advice in the UK, but these limitations mean that extending these findings to different populations or new financial support services should be approached with caution.

This research forms part of a wider set of work that aims to support the design of new financial support services. Together with complementary demand estimation analysis and behavioural research, this work improves the evidence base used to support decision making regarding policy reforms.

Equality and diversity considerations

We have considered the equality and diversity issues that may arise from this research.

Overall, we do not consider that the proposals in this Research Note adversely impact any of the groups with protected characteristics i.e. age, disability, sex, marriage or civil partnership, pregnancy and maternity, race, religion and belief, sexual orientation and gender reassignment.

2 Introduction and Policy Context

Background and motivation

In 2012, the Retail Distribution Review (RDR) reshaped the UK financial advice market by changing how advice is delivered and paid for. The RDR sought to improve fairness and transparency by banning commission-based payments and shifting towards a fee-based model. While these changes were intended to enhance consumer protection, there have been concerns that the shift may have inadvertently affected access to advice, particularly for consumers with smaller portfolios or lower incomes.

Currently, financial advisory services in the UK fall into two main categories:

- **Financial guidance/information:** Information that includes the statement of facts and figures and is intended to educate consumers on financial matters without making specific recommendations tailored to their individual needs.
- **Regulated financial advice:** Regulated and fee-based advice that provides specific recommendations and opinion based on an individual's financial situation. This advice considers a person's overall financial goals, risk tolerance, personal circumstances, and often provide specific recommendations on financial products and strategies.

Despite the availability of these services, many consumers remain underserved. High costs for regulated financial advice, limited utility provided by basic financial guidance, mistrust, and the complexity of financial advice have led to the so-called "advice gap" - where individuals, particularly those with lower wealth, are unable to access potentially beneficial advice (Barnard, 2025). This advice gap is especially concerning in the context of pensions, savings, and investments, where poor financial decision making can significantly impact long term financial resilience. Previous attempts to expand advice by the FCA, such as Basic Advice (2004) and Streamlined Advice (2017), have faced limited uptake due to operational challenges, supply side barriers (such as high fixed costs) and consumer reluctance to pay upfront fees. Recent initiatives, like Core Investment Advice (2022), have struggled with industry concerns over viability and liability, highlighting persistent barriers to expanding affordable advice.

To address this, the Advice Guidance Boundary Review (AGBR) Discussion Paper (DP) and subsequent FCA Consultation Paper on Targeted Support for pensions, propose interventions designed to close the advice gap. The underlying assumption behind these proposed interventions is that support services, like financial advice, improve consumer outcomes. The primary motivation for this research is to assess whether regulated financial advice provides quantifiable wealth benefits to UK consumers. Estimating monetary values allow us to be proportionate when designing proposed interventions. Financial advice offers other benefits which are not assessed as part of this research.

Estimating the impact of financial advice on wealth is complex, as wealth is comprised of various components such as physical assets, property, pensions, and financial investments. Each of these may respond differently to advice. There are also various circumstances or events in an individual's life that can influence their ability to accumulate wealth. By examining who receives advice and its relationship with wealth, this research helps inform the FCA's approach to policy reform.

Global evidence on the value of financial advice

Studies from Switzerland and the US show that financial advice can help consumers manage their portfolios more effectively, reduce the impact of behavioural biases, and improve long term wealth outcomes (Hoechle, Ruenzi, Schaub, & Schmid, 2017) (Terrance & Finke, 2014). Research in Canada found that households receiving financial advice not only saved more but accumulated significantly more wealth over time compared to non-advised households (Montmarquette & Viennot-Briot, The Value of Financial Advice, 2012). Some factors contributing to these improved outcomes include higher savings rates, a higher allocation to non-cash investments, and disciplined behaviour during market downturns. However, the uptake of advice is not uniform across different populations, with lower income and less financially literate individuals less likely to seek advice due to barriers like cost and complexity (Fang, Hao, & Reyers, 2022).

Research from Germany supports the idea that solicited financial advice – where individuals actively seek advice – has a greater impact on improving financial outcomes, particularly for those with lower financial literacy. In contrast, unsolicited advice – where individuals are offered advice at random - often shows little effect (Bhattacharya, Hackethal, Kaesler, Loos, & Meyer, 2012).

Some studies quantify the value of financial advice. Russell Investments, an American investment firm, estimates that a financial advisor adds a net increase of 4.91% to a client's annual return in the U.S. (Russell Investments, 2022). Grable & Chatterjee (2014) estimate that investors with financial advisors lost 6.25% less during the financial crisis than those without advisors (Grable & Chatterjee, 2014). Another Canadian study found that those with an advisor for 15 years or more accumulated 173% more assets than they would have without an advisor (Montmarquette & Viennot-Briot, 2016).

However, differences in regulatory and market environments between countries limit the direct applicability of global findings to the UK. Variations in regulation, fee structures, and financial products influence both advisor behaviour and the investment opportunities available to consumers.

UK evidence on the value of financial advice

FCA data from the Financial Lives Survey 2022 finds of those with more than £100k in investable assets, 31% report receiving financial advice in the last year. Those who took regulated financial advice are more likely to be male (this differs from our findings, and we discuss why this may be the case in Section 4) and over the age of 55. Similar trends apply to those who received financial information or guidance (Financial Conduct Authority, 2022).

In the UK, the International Longevity Centre (ILC) UK, published a report estimating that those who received financial advice were on average £47,000 better off than those who did not, after a period of 8-15 years (International Longevity Centre UK, 2017). Research commissioned by Vanguard highlights that the value of financial advice lies in helping clients avoid costly mistakes. Vanguard estimates that clients with a financial advisor earned, on average, 3% higher net returns compared to those without an advisor (Vanguard, 2021). Specific to defined contribution pensions, research suggests that

receiving financial advice can positively impact wealth accumulation, investment performance, and retirement readiness (Byrne, 2007).

While these reports provide valuable insights, most of the existing analysis relies on controlling for observable characteristics, limiting their ability to address selection bias arising from unobservable factors. Selection bias refers to differences, both observed and unobserved, between groups that receive financial advice and those that do not. Without accounting for this bias, research may erroneously attribute benefits to financial advice where they were caused by some unobserved differences between groups (e.g., risk taking preferences).

Our study builds on this literature by employing econometric methods. More specifically we adopt a staggered difference-in-differences approach. This accounts for the fact that people receive advice in different periods in our data. This method accounts for observable and unobservable differences between groups that do not vary over time (e.g. initial wealth), by matching groups based on observable characteristics and comparing trends in wealth rather than absolute values. This approach improves the credibility of our estimates by reducing the impact of potential confounding variables and selection bias. Furter detail is found in Section 5, Research design.

3 Data: Wealth and Assets Survey

We use data from the Wealth and Assets Survey (WAS), which is a large, longitudinal household survey conducted by the Office for National Statistics (ONS) in Great Britain. The WAS collects comprehensive information on individuals' demographics, wealth (including savings, pensions, and assets) over seven waves, spanning from 2006 to 2020. To ensure consistency in our analysis, we focus on individuals with complete data, including both pre- and post-advice information over five of these waves (2010-2020). We are unable to use the data from earlier waves (i.e., before 2010) as the survey does not include questions on individuals' wealth outcomes. Therefore, despite the strengths of the WAS, some data limitations remain.

Financial advice in the WAS

The WAS collects data on 'professional financial advice' (hereafter 'financial advice') in three waves: Wave 1 (2006-2008), Wave 4 (2012-2014), and Wave 5 (2014-2016). Respondents are asked about 'any expert financial advice that you may have received in the last two years'. They are also asked whether this advice related to specific financial matters, for example investments, pensions, savings, etc.

Wealth in the WAS

Wealth in the WAS is measured comprehensively and includes four components: property wealth (value of primary residences and additional properties), physical wealth (tangible assets such as vehicles, jewellery, and collectibles), pension wealth (value of private pensions, both defined contribution and defined benefit schemes), and financial wealth (including savings, investments, and financial assets, minus liabilities). For more information about the survey design and data, please refer to the WAS user guides available on the ONS website.

In our research, we sum these four components to calculate total wealth. We focus on total wealth for the following reasons:

1. The WAS collects data on various types of financial advice, such as investments, pensions, savings, and life insurance. Some individuals in our sample reported receiving multiple forms of advice (e.g., both savings and pension advice). However, the survey does not provide information on the actions taken by respondents, therefore we cannot know whether the advice may impact wealth across a range of components. For example, whether savings advice focussed solely on cash savings, or on reorganising their broader asset portfolio, potentially impacting their property or pension wealth.
2. The sample size of advice takers is relatively small, so we do not disaggregate between types of advice.

3. A significant proportion of our sample has zero values for at least one wealth component across the waves analysed. Specifically, 42% of individuals have at least one wealth component recorded as zero. Focussing on individual wealth components with high variability wave-to-wave would introduce noise into our estimates and may not generate insightful results. Aggregating at the total wealth level mitigates this issue, as changes in the classification of wealth (e.g., between investments and property), balance out at the total wealth level.

Sample construction

Our sample construction involved the following key steps:

1. **Balanced panel**: We include individuals who have complete wealth data across the five waves covering 2010-2020, removing those who drop out of the survey or join in later waves. This ensures we have a consistent set of individuals across all waves. Attrition was non-random across key variables such as wealth and receipt of financial advice ('treatment'), therefore using an unbalanced panel would introduce biases and affect the reliability of our estimates. Wave 1 and 2 only included total household wealth, not individual wealth components, so these have been excluded.
2. **Exclusions based on advice type**: Our analysis focuses on those who received advice related to pensions, savings, investments, life insurance or protection, and major life events. Individuals who only received advice on mortgages or debt management are not included within our definition, as these forms of advice are less directly related to our policy interest.
3. **Excluding individuals with zero or negative wealth**: We restrict the sample to individuals with positive net wealth across all periods, as financial advice is unlikely to be relevant to those with no wealth to manage. This removes 30 individuals from our sample, which represents around a 1% reduction in our sample size. One extreme outlier with baseline wealth exceeding £80,000,000 was also removed from the sample.
4. **Pre-advice Data**: We focus on individuals who received advice for the first time in either Wave 4 (2012-2014) or Wave 5 (2014-2016). We exclude individuals who received advice in Wave 1, given we have no pre-advice data for this group.

Our final sample consists of 2,318 individuals, of whom 540 (23%) received advice, while 1,778 (77%) did not. This compares to the population weighted estimate of 17% of adults having received financial advice. It is worth noting that this differs from the (Financial Lives Survey, 2022), which estimates 8.3% of adults receive advice. We assume this is due to sampling differences across the studies. Section 4 describes the main characteristics of the data, scaled to population estimates.

Limitations of the data

Although the WAS provides rich and detailed data on wealth and financial advice, some data limitations remain. First, individual wealth data is not included in Waves 1 and 2 (only household wealth data), hence we do not use these waves in our analysis. This limits the

availability of pre-advice wealth data, particularly for individuals who received advice in Wave 4.

Second, waves 2, 3, 6 and 7 did not include questions about financial advice. If individuals received advice in periods when the question was not asked, but did not report it in subsequent waves, they would be incorrectly classified as not receiving advice. This contaminates the control group which can bias our estimates downwards.

Because survey participants were asked whether they received advice in the last two years, the exact timing of when advice was received within the two-year recall period is unclear. This uncertainty makes it difficult to precisely match the timing of advice to the timing of wealth estimates, complicating our ability to identify the immediate effects of financial advice. For example, if an individual says they received financial advice in Wave 4, this means they received it in the 2 years prior. Their Wave 4 wealth estimate will be used as the first post-advice period, with this being up to 2 years after receiving financial advice. The wealth estimate provided in Wave 3 will be their baseline wealth. In our analysis we control for the level of household wealth in the first period (i.e., Wave 1).

There are also measurement challenges. The financial advice and wealth data in the WAS is self-reported. This means it may be subject to measurement error (i.e., differences in the true value and the value measured). Additionally, there is no way to control for the quality of advice received, which may vary substantially. These issues make it more difficult to assess the true relationship between financial advice and wealth.

Finally, while we standardise wealth component definitions as much as possible, there are slight variations in the way wealth components like pensions and property are measured across different waves of the survey. These inconsistencies may affect the precision of our wealth estimates over time.

4 Descriptive analysis

Our descriptive analysis reveals significant differences between individuals who received financial advice and those who did not. Using population weighted WAS data from Wave 5 (the most recent year with data on receiving financial advice), we observe that people who receive financial advice tend to be wealthier, older, more educated, and with a greater share of their wealth held in pensions and property. In contrast, individuals who did not receive financial advice have lower wealth and more of their wealth held in physical assets. These differences persist within our estimation sample, where advice takers appear wealthier and older than advice takers in the general population (see Annex 2 for more details on the estimation sample). These differences inform our research design and determines how broadly we can apply our findings.

Population level descriptive analysis

We use population weights to estimate characteristics representative of the population of Great Britain. This weighted approach allows us to draw broader conclusions about individuals who receive advice relative to the general population.

Individuals who received financial advice are significantly wealthier, more likely to be married, and more likely to hold a degree. Those who receive financial advice are more likely to be female, differing from the 2022 Financial Lives Survey where males are more likely to receive advice. This discrepancy may be explained by changes in participation trends over time since our estimates are from 2014 to 2016. This finding may also be driven by differences in sampling and survey methodologies. For instance, if one survey allows any household member to respond while the other requires the primary financial decision maker, it may skew results based on gender roles within households.

Those who received financial advice have a mean total wealth of £655,933 compared to £210,805 for those who did not (see Table 1 below). Among those who received financial advice, 68% were aged 45 or above and 39% have a degree. In comparison, for those who did not receive financial advice, only 45% were aged 45 or above and 24% have a degree. Differences between groups, notably with regards to total wealth, highlight the need to control for observable characteristics in our research design (see Section 5, Research design). In our analysis we use a statistical matching technique to account for differences in observable characteristics.

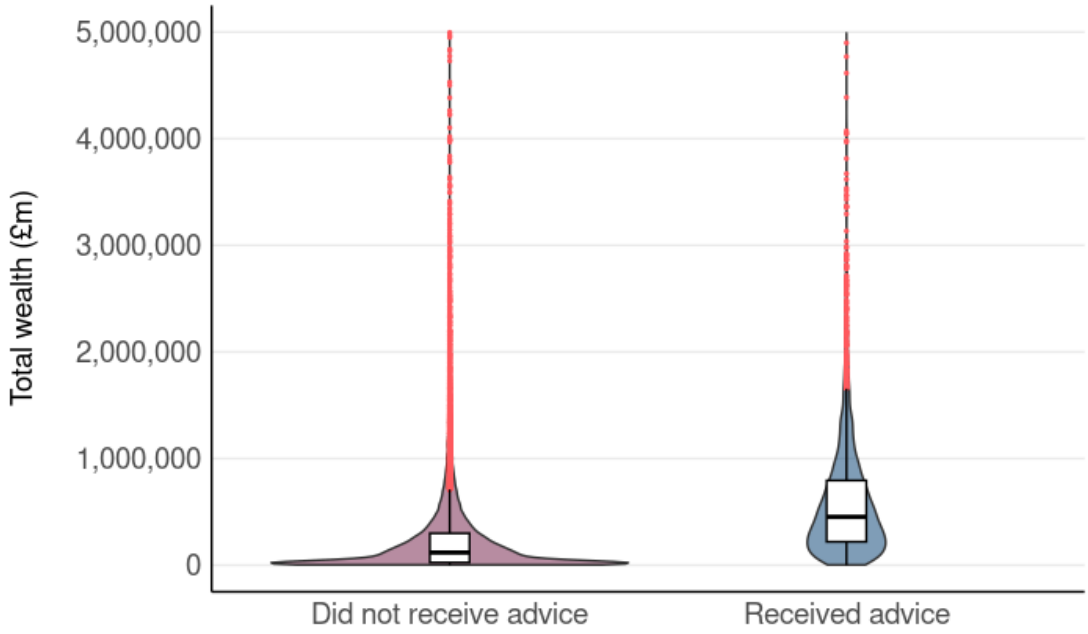
Table 1. Individuals that receive financial advice are significantly wealthier, older, and have higher financial literacy.

	Received financial advice	Did not receive financial advice	T-statistic (P-value)
Total observations	3,538,290	40,337,278	NA
Median Annual Employment Income	£7,000	£7,800	NA
Median total wealth	£420,199	£80,013	NA
Mean total wealth	£655,933	£210,805	27.17 (0.00)
Female (%)	53	47	8.65 (0.00)
Aged 45+ (%)	68	45	16.08 (0.00)
Married (%)	72	64	2.36 (0.01)
Degree (%)	39	24	14.92 (0.00)
Financial Literacy (proxied by whether individuals report regularly reading up on financial matters) (%)	30	16	23.64 (0.00)

Source: ONS, Wealth and Asset Survey, Wave 5 (2014-2016). Estimated using population weights from the WAS. The T-statistics in the last column are generally above 5, meaning we reject the null hypothesis that there isn't a statistically significant relationship between the observable characteristic and the treatment variable (i.e. recieving finanical advice).

There is significant variation in wealth across individuals, both for those who did and did not receive financial advice (see Figure 2). This makes estimating the relationship between financial advice and wealth challenging.

Figure 2. Individuals that receive financial advice are wealthier on average, but wealth is varied across both groups:

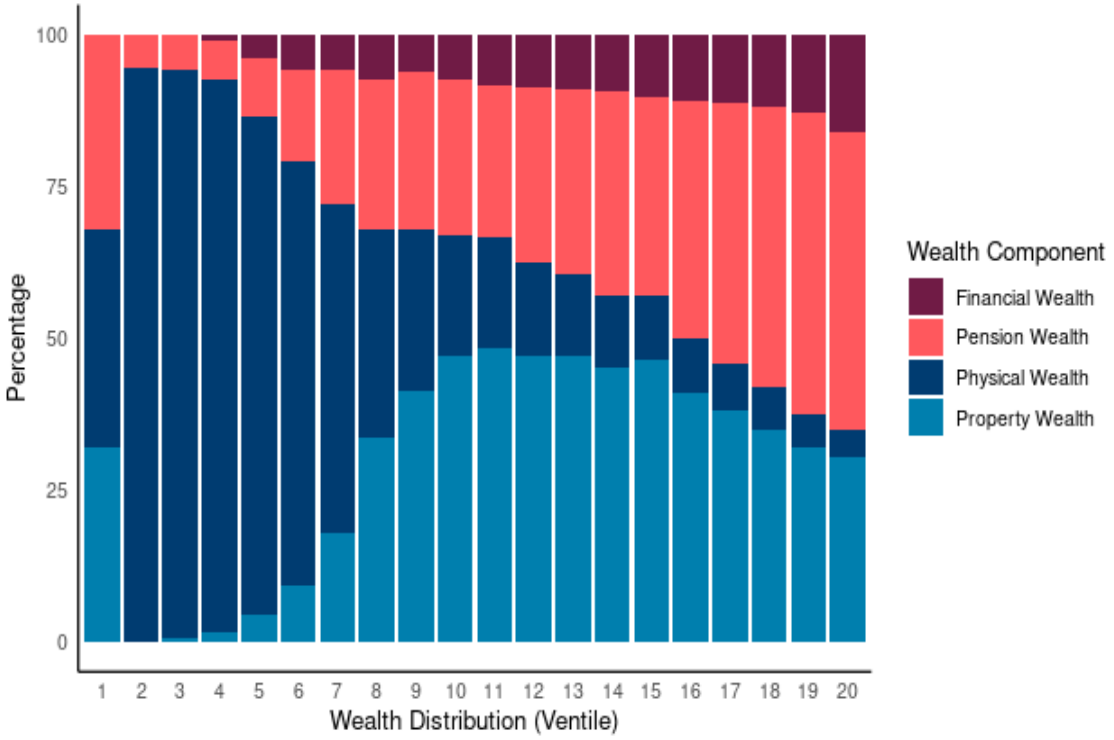


Source: ONS, Wealth and Asset Survey, Wave 5 (2014-16).

Notes: For presentational purposes, the Y axis has been cut at £5m. The box plot shows the interquartile range and the median wealth level for each group. Those who receive advice are generally wealthier (blue area) versus those that did not receive advice (maroon area), and there are many individuals who are very wealthy in both groups (red dots). For those who did not receive advice, we observe a clustering of wealth just above £0.

Figures 3 and 4 below demonstrate that the composition of wealth differs between those that did and did not receive advice. Advice takers have lower proportions of physical wealth at the lower end of the wealth distribution, which is replaced by property, financial and pension wealth. At the higher end of the wealth distribution, the composition is more similar.

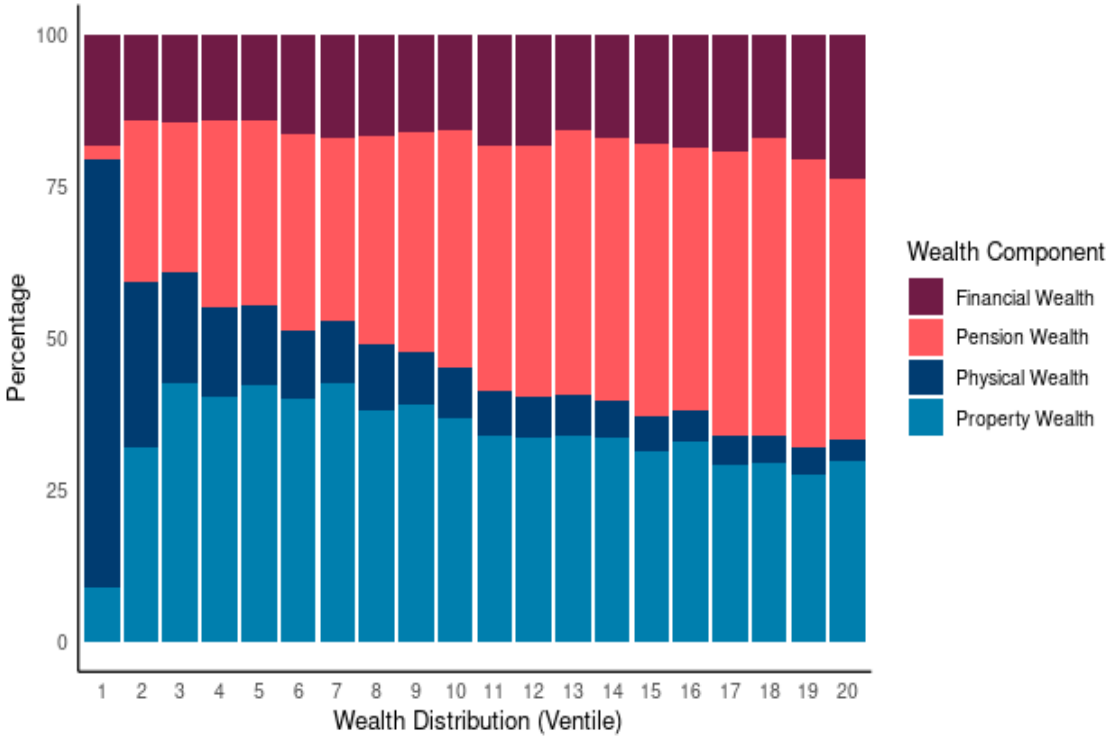
Figure 3. For those that did not receive advice, the lower half of the wealth distribution have a high proportion of physical wealth:



Source: ONS, Wealth and Asset Survey, Wave 5 data (2014-16).

Notes: The data relates to individuals that have positive wealth and did not receive finanical advice. This chart shows the average proportion of total wealth each wealth component represents, for each ventile of the wealth distribution. For example, the 20th wealth distribution ventile on the X axis represents the composition of wealth for those in the highest 5% of the wealth distribution. We replace negative wealth values with 0 to remove the impact of individuals in debt. However, due to a significant number of negative wealth values at the lowest ventile (i.e. a number of individuals at the lowest end of the distribution are in debt), the breakdown is not accurate for this ventile.

Figure 2. For those that received advice, pension and property wealth accounts for most of the wealth across the distribution.



Source: ONS, Wealth and Asset Survey, Wave 5 data (2014-16).
Notes: The data relates to individuals in our sample that have positive wealth and received financial advice. This chart shows the average proportion of total wealth each wealth component represents, for each ventile of the wealth distribution. For example, the 20th wealth distribution ventile on the X axis represents the composition of wealth for those in the highest 5% of the wealth distribution. We replace negative values with 0. However, due to a significant number of negative values at the lowest ventile (i.e., several individuals at the lowest end of the distribution are in debt), the breakdown is not accurate for this ventile.

In conclusion, the level of wealth, the composition of wealth and demographic characteristics vary dramatically across the population (and our estimation sample, see Annex 2). These differences mean our analysis focuses on a group of individuals that are not representative of the population. Those who receive financial advice are significantly wealthier and hold a higher proportion of their wealth in financial, property and pension wealth. Whilst some of this difference might be attributable to advice, it is unlikely that all of it is. This highlights the importance of controlling for observable characteristics and being clear on limitations when generalising results. More information on our methodological approach, and how we consider these differences, can be found in the Research Design, section 5.

5 Research design

Because the WAS collects data from the same individuals before and after taking advice, we can use a difference-in-difference (DiD) approach. DiD compares the changes in outcomes over time, between a group that received treatment (financial advice in our setting) and a control group (did not receive advice). As individuals receive advice at different points in time, we apply a 'staggered' design, following the approach proposed by Callaway and Sant'Anna (2020) (hereafter 'CSA').

The CSA method – and DiD more generally - is a causal inference method. This method supports estimating the average impact of a policy or an intervention on those affected (i.e., the 'average treatment effect on the treated', or 'ATT'). Under certain assumptions, DiD can eliminate biased estimates that arise because the treatment was not randomly assigned (unlike Randomised Control Trials, where participants are randomly assigned a treatment and groups, quasi-experimental designs rely on naturally occurring treatment assignments). For reasons we discuss in this section, we cannot eliminate biases in our comparisons between those who did and did not receive financial advice. Instead, we use the CSA method to limit their impact and make comparisons between groups as credible as possible.

Motivation for staggered DiD

We have two groups of individuals who receive financial advice (two treatment groups): Group 4 who received it for the first time between 2012-14; and Group 5 who received it between 2014-2016. Figure 5 below shows the staggered nature of the take up of financial advice.

Figure 5. Staggered take up of advice across time periods:

Notes: This figure shows that the uptake of financial advice is not measured in the first two waves of the WAS, between 2006 and 2010, but that it is measured in waves 4 and 5, between 2012 and 2016. It shows how we categorise individuals into two groups: group 4 who reported receiving advice in wave 4, between 2012-1014; and group 5, who reported receiving advice in wave 5, between 2014-

2016. It also shows that we do not observe whether people received advice in waves 3 (2010-2012), 6 (2016-2018), or 7 (2018-2020.)

In recent years, various studies (Goodman-Bacon, 2021) (Sun & Abraham, 2021) (Borusyak, Jaravel, & Spiess, 2024) (de Chaisemartin & D'Haultfœuille, 2020) have shown how using two-way fixed effects (TWFE) or event studies estimators when treatment is staggered involve making invalid comparisons between groups and weighting group-specific treatment effects incorrectly. More specifically, (Goodman-Bacon, 2021) show that there are two main issues with a standard TWFE DiD model that groups all post-advice periods together and compares them with all pre-treatment periods to estimate an overall average effect. First, when there are differential impacts of a treatment across groups, the overall averages are biased because the model does not correctly weight group-level effects into an overall average. Second, in a TWFE model outcomes from groups treated earlier are used as a control for those treated later.

The CSA approach allows us to overcome these limitations by, broadly speaking, (a) estimating the relationship between financial advice and wealth for each treatment group (e.g. those that received advice in Wave 4 are one group and Wave 5 are a different group) in each post-advice time period; and (b) averaging them to show the relationship between financial advice and wealth changes over time, while considering differences in timing and the size of the groups.

Our approach attempts to mitigate the effects of two types of selection bias:

1. **Observable selection**: differences between those who did and did not receive financial advice that we observe in our data, for example their age.
2. **Time invariant unobservable characteristics that may impact selection:** differences we cannot observe that do not vary over time, (e.g., location of birth) but are associated with the decision to receive financial advice.

Our study builds on existing evidence of the impact of financial advice on wealth, which mostly account for observable characteristics and usually rely on point-in-time comparisons between individuals who did and did not receive financial advice.

Our DiD setup

In addition to our two treatment groups, we establish two control groups:

- **Never Treated Group (NT):** This group consists of individuals who, as far as we can tell from the data, did not receive financial advice at any point during the entire study period. We use this group as a comparison for both groups 4 and 5. However, it is important to note that financial advice was not consistently asked about in all waves. So, there is a possibility that some individuals in this group may have received advice during those waves, but it was not captured.
- **Not Yet Treated Group (NYT):** For Group 5, we also use individuals in group 4 before they received financial advice as a comparison group. This is a useful control because we might expect those who go on to receive advice to be most like those who have already received it (we discuss the assumptions underpinning our

application at the end of this section). Because group 4 is the first group to be treated, we do not have an equivalent NYT group for them.

We define the baseline periods for each treatment group as the last period wealth was measured before they received financial advice: For Group 4, we use Wave 3 (2010-2012) and for Group 5 we use Wave 4 (2012-2014).

Applying the CSA Method

In this section we outline the CSA method. We focus on presenting the method in terms of potential outcomes, rather than providing the details of the actual DiD estimator we use. CSA is based on calculating 'group-time' ATT for each treatment group - in our case Group 4 and Group 5 - in each post-advice period. These group-time effects capture the effect of financial advice over time for each group based on when they received advice (We use the doubly robust covariate adjustment. The doubly robust method is consistent if either the propensity score model or the regression is mis-specified, making it doubly robust to misspecification).

For each treatment group (g), the group-time ATT in time *t* is defined as:

$$ATT(g,t) = E[Y_t(g) - Y_t(0)|G = g]$$

Where:

- ***G*** denotes the group, which we define as the first period individuals are treated. In our case g = 4,5.
- $Y_t(g)$ denotes the *observed* outcome (wealth) at time t for those in group g.
- $Y_t(0)$ denotes the *unobserved* counterfactual outcome - wealth for those in group g at time t if they had not received advice. Because it is unobserved, this is the outcome we proxy with our control groups.
- In practice, each of these group-time ATTs is calculated by taking the difference in outcomes between the baseline period and time *t* for each group (see above).

Handling Wealth Data: Transformations and Adjustments

We use log-transformed wealth as our outcome variable. This approach estimates the percentage change in wealth associated with receiving financial advice. Since wealth grows through compounding mechanisms (i.e., investment returns), relative changes are more meaningful than absolute changes. Taking logs also mitigates the effect of extreme values in wealth, which could skew the results.

The interpretation of the resulting coefficients in our DiD model is the percentage change in total wealth associated with having received financial advice, relative to those that do not receive advice.

Dynamic and Overall Aggregation of ATT

Once the group-time ATTs are computed, they can be aggregated to obtain the overall effect of financial advice. We aggregate the group-time effects in two ways:

(1) **Dynamic Treatment Effects by 'Length of Exposure'**. We aggregate the group-time ATTs based on how much time has elapsed since individuals received financial advice (i.e., length of exposure). This is the most economically relevant aggregation in our setting, given we are interested in the potential impact of advice over time.

This supports analysis of whether the benefits of advice persist, grow, or diminish. We calculate a weighted average of the group-time ATTs, with weights determined by the proportion of individuals in each group at each length of exposure. Because they receive treatment at different points in time, the same length of exposure will occur at different calendar times for each group. For example, when we calculate the combined ATT 'one period after receiving advice', we use Group 4's ATT in 2012-14 and group 5's ATT in 2014-16. The average ATT at a specific length of exposure is expressed as:

$$\mathrm{ATT}^{e} = \sum_{g} w_{g}^{e} \cdot \mathrm{ATT}_{g}^{e}$$

Where:

- e is the exposure length.
- W_g^e is the weight for each group g based on the proportion of individuals at that exposure length.
- ATT_g^e is the ATT for group g at exposure length e

(2) **Overall ATT**. We aggregate these dynamic effects to estimate a single, overall ATT across all lengths of exposure. This gives us an overall measure of the relationship between financial advice and wealth. The overall ATT is calculated as a weighted average of the dynamic ATTs, with weights determined by the proportion of individuals at each length of exposure. This overall ATT can be expressed as:

$$\text{Overall ATT} = \sum_{e} w_{e} \cdot \mathrm{ATT}^{e}$$

Where:

- $\boldsymbol{w_e}$ is the weight for each length of exposure.

This aggregation helps provide a summary measure across the entire study period.

In Section 4, we outlined how those who received financial advice in the WAS are, on average, older and wealthier than those who did not. When estimating group-time, dynamic, and overall ATTs described above we account for these differences by controlling for a range of relevant characteristics. To limit notation, we did not include these controls in equation above. The relevant characteristics we include as covariates in our analysis are gender, age, marital status, education, financial literacy, initial wealth, initial income, and dummy variables representing whether they received a lump sum payment pre- or post-advice. These variables are informed by the relevant literature. Our descriptive analysis also shows that many of these variables are correlated with the decision to receive financial advice.

Main assumptions of the CSA DiD

The validity of CSA relies on several assumptions, the most critical being the assumption of 'conditional parallel trends' (CPT). CPT requires that, absent the treatment, and after accounting for observable differences between the groups, those who did and did not receive financial advice would have experienced, on average, similar trends in wealth over time.

One potential concern is whether changes in wealth prior to receiving advice might differ systematically between the treatment and control groups, which could challenge the credibility of the parallel trends' assumption. Specifically, we must assume that CPT holds for both the Never Treated (NT) and Not Yet Treated (NYT) groups when compared to the treatment groups.

- **Never Treated Group (NT)**: We use this group as comparison for both Group 4 and Group 5. However, because this group never received financial advice during the study period, individuals in this group might disproportionately represent individuals with stagnating or declining wealth**.** If this is true, the NT group might not be a valid counterfactual for the treatment groups, introducing an upwards bias in our estimates.

- **Not Yet Treated Group (NYT)**: For Group 5, we also use Group 4's pre-advice wealth trajectory as a comparison. The rationale for using the NYT is that this group should be more similar to the treatment group given that they both eventually receive financial advice. This similarity mitigates the influence of unobservable selection bias.

Due to data limitations, we cannot verify the CPT assumption. Specifically, the study lacks sufficient pre-treatment time periods to observe trends in wealth before advice is received. This constraint limits our ability to test whether the treatment and control groups were on parallel trajectories, conditional on observable characteristics, prior to the intervention.

To partially mitigate this limitation, we include wealth and income levels from the initial Wave (wave 1 of the WAS) as control variables in our models. While this does not fully address concerns about differences in pre-treatment trends, it helps account for initial wealth and income differences that might influence subsequent trajectories.

We also assume individuals did not significantly alter their financial behaviour in anticipation of receiving advice. However, certain events such as lump sum payments (i.e., any one-off payment, such as gifts or inheritance) may present a complication, especially if they are received after treatment. For example, if someone expects an inheritance or pension payout, they may delay receiving advice or adjust their financial decisions beforehand, distorting our baseline wealth comparisons. This would artificially suppress or inflate wealth in the pre-treatment period, leading to biased estimates associated with financial advice.

To partially address this limitation, we incorporate pre- and post-advice lump sum dummies into our model. This mitigates distortions caused by these payments. However, this only controls for the existence of lump sum payments, not their size, meaning that wealth changes driven by large lump sums may still bias results. Lump sum payments are an issue if they occur after treatment and are non-random across groups, due to a potential violation of the CPT assumption. We discuss this further in the results section.

In this setting, we recognise that unobserved time varying differences between treatment and comparison groups may still bias our results. For instance, changes in risk tolerance over time, which we cannot measure directly, might affect how individuals accumulate wealth. Similarly, changes in income or employment status might affect wealth and further challenge the CPT assumption. While we adjust for observable differences, the potential

presence of both unobserved and observed time varying factors remains a limitation that could influence the credibility of our results.

In summary, while the CSA method helps mitigate some biases, our design still faces limitations related to selection bias and endogeneity. We address these limitations and their implications for our results in Section 7.

6 Results

We estimate an increase in wealth of up to 10% in the years after receiving advice, compared to those that do not receive it. However, there is uncertainty in the long-term nature of this relationship. When we exclude individuals who receive any lump sum payments (e.g., lump sum pension payments, inheritances, and gifts) the relationship is not clearly different from zero, however when only removing those with lump sum payments post-advice our estimates remain positive with similar conclusions to our main results. This is discussed in more detail in the robustness checks section of the report.

Variable results and relatively small sample sizes limit our statistical power. Additionally, there is notable variation between treatment groups: the estimates for Group 4 are noisy and imprecise, while Group 5 shows more consistently positive results. Without an intuitive rationale for this disparity, it is likely due to sample differences influencing the outcomes.

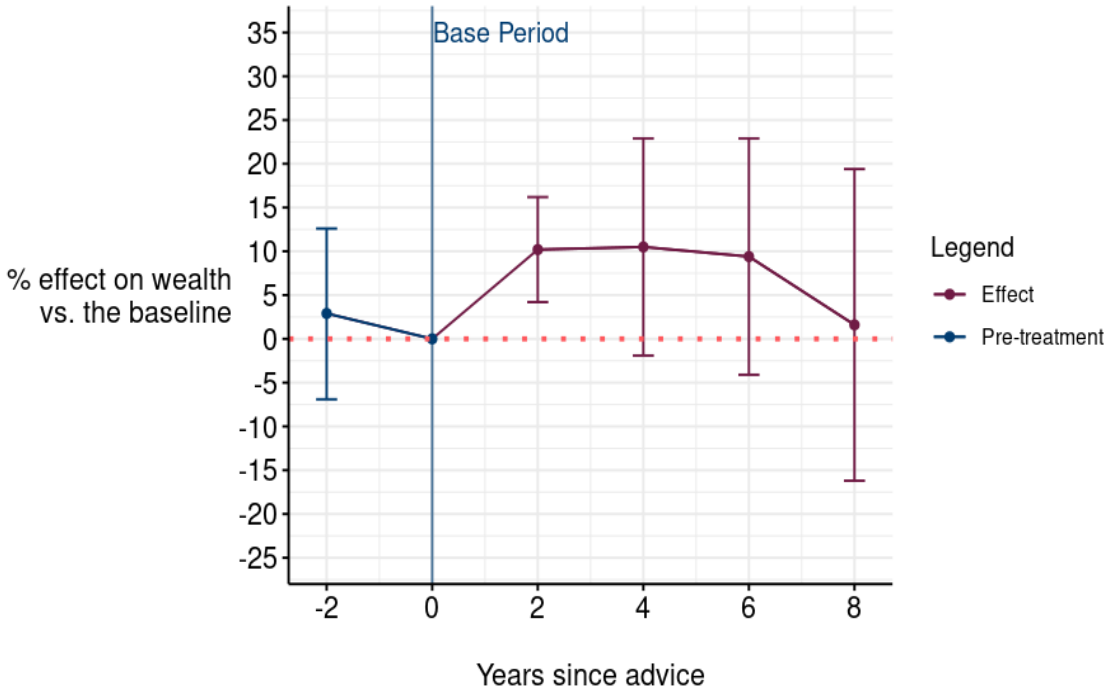
The finding of a positive association between financial advice and wealth is consistent across a range of robustness checks, but we advise caution when interpreting the estimated magnitudes or applying to other contexts. Overall, while these results illustrate a positive relationship between receiving financial advice and accumulating wealth, they also outline the difficulty in estimating relationships in such a complex setting.

Financial advice and wealth

Figure 6 below graphically demonstrates the point estimates and 95% confidence intervals for the relationship between financial advice and wealth, based on the number of years since receiving advice. Table 8 (see Annex 3) presents the numerical values. There are three key features that stand out:

1. All the post-advice point estimates are positive, ranging from 1.6% to 10.2%. The first post-advice period suggests that after two years, wealth is 10.2% higher among those who receive financial advice, relative to those who do not.
2. Whilst all point estimates are positive, only the estimate two years post-advice is statistically significant. The other estimates have confidence intervals crossing zero, demonstrating uncertainty and variability especially in longer-term results.
3. The eight-year point estimate is substantially lower (1.6%), and the 95% confidence interval is substantially wider than all other estimates. This result is driven by those who received treatment in Wave 4. There is essentially no difference in the change in total wealth between individuals treated in wave 4 and individuals who did not receive advice at the eight-year point (see figure 7 below). This relationship could be partly driven by the inclusion of pensions decumulation advice within the group of advice takers. The objective of decumulation advice is a sustainable withdrawal rate during retirement rather than wealth accumulation, therefore inclusion of these groups may bias our longer-term estimates downwards.

Figure 6. Financial advice has a positive association with wealth, but this relationship fades and becomes more uncertain over time:



Source: ONS, Wealth and Asset Survey.

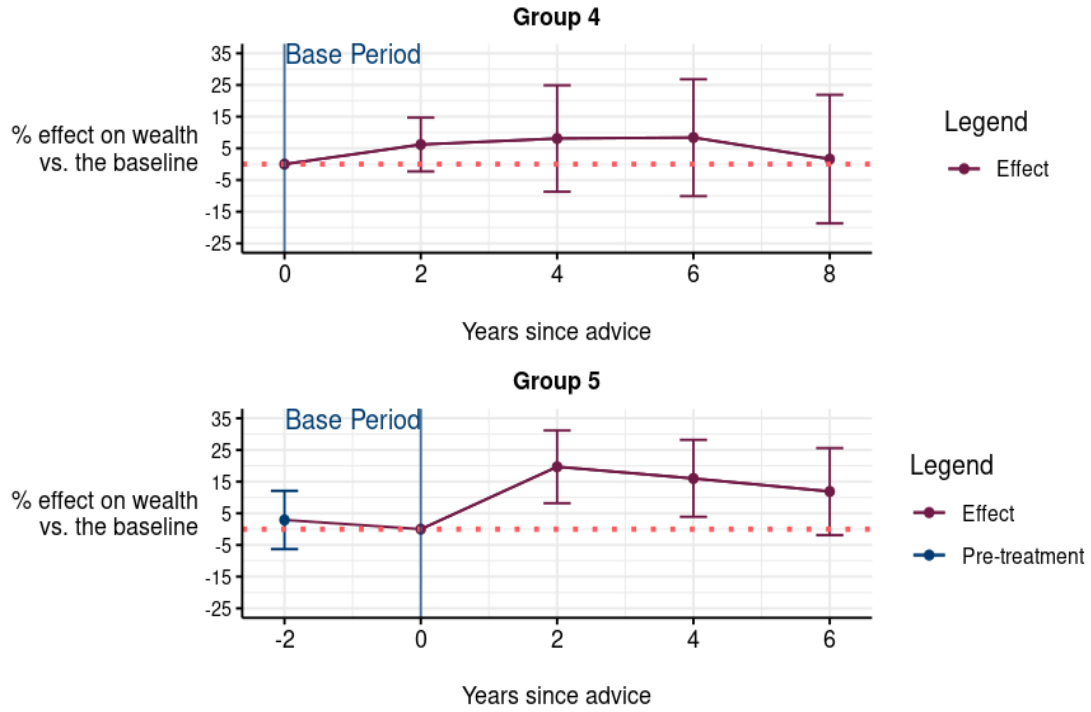
Notes: This chart highlights the average difference in the change in wealth between those who do and do not get advice, in each period relative to the baseline. It shows that those who received advice experience a larger increase in wealth initially, but that effect decreases and becomes more uncertain over time. These results represent the aggregate impact (i.e., the average combined impact for Group 4 and 5, weighted according to their relative share of the sample).

We interpret these findings as an upper estimate of the relationship between financial advice and wealth, recognising that the post-advice lump sum dummy variable only partially adjusts for one-time payments received after financial advice. We address the details of this adjustment in the robustness checks sub-section of the report.

The estimates in Figure 6 mask underlying differences across our two treatment groups. Figure 7 demonstrates the relationship between financial advice and wealth for each treatment group in each post-advice period, while Table 9 (see Annex 3) displays the numerical values. We note two features of the estimates:

1. The point estimates for Group 5 are consistently higher and more precise than those for Group 4. For Group 5, the estimates range from 11.9% to 19.7%; whereas for Group 4 they range from 0% to 8.4%.
2. The point estimates for both Groups first increase, then level off or decline over time. This is particularly true for group 5, for whom the DiD estimate declines from 19.7% to 11.9%. This pattern is less pronounced for group 4, whose estimates decrease from 6.2% to 1.6%.

Figure 7: The relationship between financial advice and wealth varies across treatment groups but remains positive:



Source: ONS, Wealth and Asset Survey.

Notes: This chart highlights the average difference in the change in wealth between those who do and do not get advice in groups 4 and 5, in each period relative to the baseline. For group 4, it shows that there is a small but imprecise effect in each period after receiving advice. For group 5, it shows that there is a larger increase in wealth initially, but that effect dereases and becomes more uncertain over time.

There is no clear rationale for this relationship to differ across these groups. Both groups opted to receive financial advice at different points in time, meaning that macroeconomic performance or other external factors may cause variations in wealth between groups. However, as far as we are aware, no intrinsic differences between the 2012-14 and 2014-16 periods would cause the relationship between advice and wealth to differ systematically across the groups. For instance, the Retail Distribution Review tool effect at the end of 2012 and therefore we do not believe this would cause significant impacts across groups.

Nonetheless, the two treatment groups do differ in observable ways, most notably their wealth and financial literacy (see Table 2). On average, Group 4 is wealthier, has higher income, and is more financially literate than Group 5. While we adjust for these covariates (see Section 5) when estimating the relationship in Figures 6 and 7, this method balances observable characteristics between each treatment group and the control group *rather than* between the two treatment groups directly. However, due to using the Not Yet Treated (NYT) as a control group, the estimates for the two treatment groups may not be fully comparable.

Table 2: Characteristics of the estimation sample, by treatment group.

	Group 4	Group 5	T-statistic
Total observations	379	161	NA
Median Annual Employment Income	£0	£0	NA
Median total wealth (Wave 3)	£474,192	£359,670	NA
Median total wealth (Wave 4)	£552,708	£409,475	NA
Mean total wealth (Wave 3)	£633,093	£517,222	-2.2
Mean total wealth (Wave 4)	£737,032	£562,847	-2.7
Mean total wealth (baseline data)	£633,092	£562,847	-1.26
Female (%)	57	59	0.43
Aged 45+ (%)	88	83	-1.62
Married (%)	73	76	0.59
Degree (%)	38	41	0.60
Financial Literacy (%)	80	66	-3.53

Source: ONS, Wealth and Asset Survey

Notes: Baseline data (i.e., total wealth in Wave 3 for those treated in Wave 4 and total wealth in Wave 4 for those treated in Wave 5). Median annual employment income is equal to £0 in both groups which reflects the large proportion of individuals in the sample who are in retirement.

When it comes to the average estimates in Figure 6, the cross-group differences are consequential, because the Group 4 sample is over two times the size of Group 5 (see Table 3). This means that Group 4's estimates contribute more to the averages than Group 5's, pulling down the average estimates and increasing the noise associated with them.

When we aggregate across all post-advice periods, our overall estimate (like the Average Treatment effect on the Treated, ATT) is 7.8%. This value serves as a summary measure of the relationship between financial advice and wealth over the entire study period.

Overall, we consider the results to show evidence of a positive association between receiving financial advice and changes in wealth, relative to those that do not receive advice.

Robustness checks

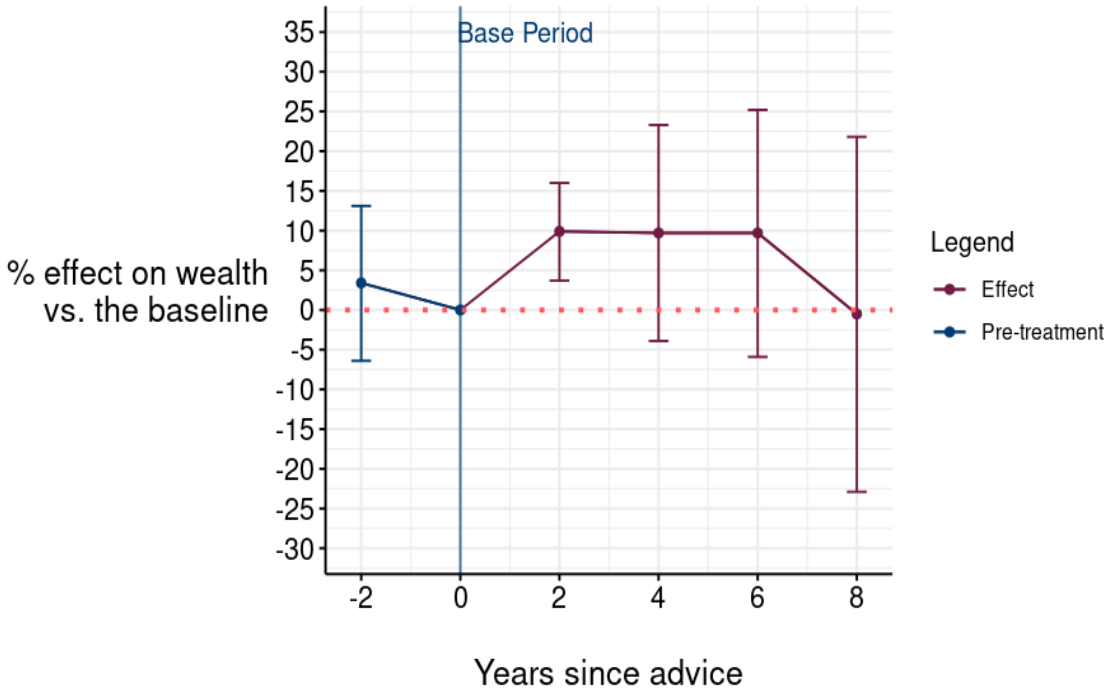
Several factors could be driving the positive relationships shown in Figures 6 and 7. For example, there are large wealth outliers, individuals with large returns due to low levels of initial wealth, and irregular lump sum payments post-advice that could all influence these estimates. We check the robustness of our results to these issues.

Restricting wealth

Individuals at either end of the wealth distribution could have a disproportionate impact on our results. For example, extremely high wealth individuals might be the only individuals achieving returns, pulling up the average. On the other hand, individuals with extremely low levels of wealth can experience large percentage returns from relatively small increases in wealth.

Overall, our findings are similar if we impose reasonable wealth floors or ceilings on our sample. For example, excluding anyone with wealth of more than £5 million in any wave (this excludes eleven individuals or 2.9% of Group 4 and three or 1.9% of Group 5), the point estimates in Figure 8 decrease slightly compared to the main results, ranging from -0.5% to 9.9%. The pattern remains similar over time, showing an initially increasing relationship that declines slightly and becomes more uncertain over time. The aggregate estimate is 6.4%, slightly lower than the main result (7.8%). The overall estimate is not statistically significant, as the 95% confidence interval includes zero.

Figure 8: Restricting wealth to less than £5m does not change the relationship between financial advice and wealth:



Source: ONS, Wealth and Asset Survey, Waves 3 to 7.

Notes: This chart highlights the average difference in the change in wealth between those who do and do not get advice, in each period relative to the baseline, restircting the sample to indidivuals with wealth less than £5 million. It shows that those with wealth less than £5 million who received advice experience a larger increase in wealth initially, but that effect decreases and becomes more uncertain over time. These results represent the aggregate impact (i.e., the average combined impact for Group 4 and 5, weighted according to their relative share of the sample).

If we exclude all individuals with wealth below £100,000 (this excludes 44 individuals or 11.6% of the individuals in Group 4 and 25 or 15.5% of the individuals from Group 5), our point estimates decrease to between -2.2% and 9.5% and display a similar pattern over

time to our main findings (see Annex 2). Based on this analysis, we conclude that our results are not being driven by wealth outliers.

Excluding lump sum payments

Life events happen. Whether it is being made redundant, receiving inheritance or insurance payouts, or generous gifts from friends or family. There are instances of all of these (and more) life events in our sample. However, in implementing the CSA estimator, we assume Conditional Parallel Trends and no anticipatory behaviour (See Section 5, Research design section).

In certain circumstances, individuals receiving lump sum payments due to life events might violate these assumptions. Two such circumstances are problematic:

1. **Lump sums are more common among the treated group after they receive financial advice**. Part of the difference-in-difference we measure will be the result of our treated group (i.e., those who receive advice) receiving a lump sum payment, rather than changes in wealth based on the advice they received. The fact lump sums are not random across those who did and did not receive advice means that in the absence of treatment, the two groups may not have had the same trend in wealth. In our main results we control for differences in the prevalence of receiving a lump sum post-advice, but do not control for differences in the size of these lump sums.
2. **Individuals receive financial advice because they anticipate the arrival of a lump sum payment**. This is particularly problematic if individuals altered their savings or spending behaviour in advance of receiving advice. This type of 'anticipation effect' can be common in non-experimental settings (Ashenfelter, 1978), and means that baseline wealth for those who received advice could be biased. This is because our estimates would capture a change in behaviour that results from the 'anticipation effect', not just the relationship with financial advice.

Table 3 shows basic summary statistics across our treatment and control groups who received a gift, inheritance, or lump sum pension payment in any post-advice period.

Table 3: A higher proportion of individuals that took financial advice also received one of the following lump sum payments:

	Inheritance (n)	Gift (n)	Pension (n)	Inheritance Mean (£)	Pension Mean (£)	Gift Mean (£)
Treatment Group	166 (31%)	81 (15%)	68 (13%)	171,163	3,739	782
Control Group	349 (20%)	213 (12%)	114 (6%)	76,250	1,280	496

Source: ONS, Wealth and Asset Survey.

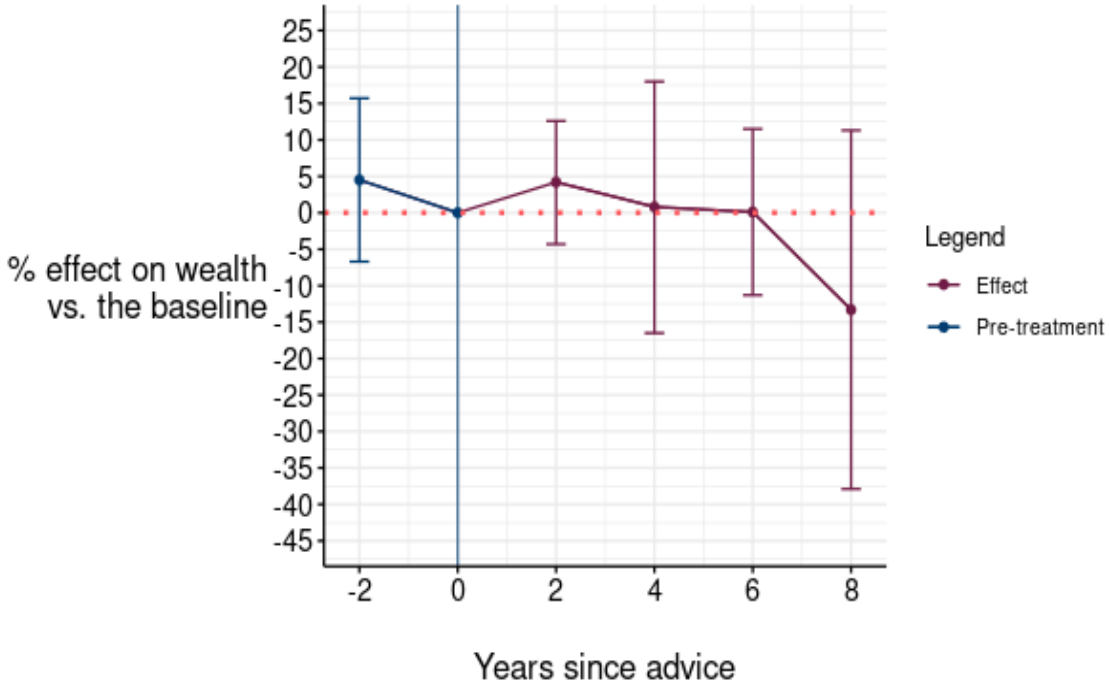
A large proportion of all groups received a lump sum income in a post-advice period. Among the treatment group, 34% received a lump sum payment, while only 25% received a lump

sum in the control group. To explore the potential impact of lump sums on our findings, we exclude individuals that receive a lump sum payment and re-run our results.

We have not excluded individuals who received a gambling win, life insurance payout, accident insurance payout, other insurance payout, redundancy payout or other irregular lump sum. These events are generally not predictable, meaning we are not concerned about the 'anticipation effect' here and we might expect these to be almost random across treated and control groups.

Figure 9 shows that adjusting our sample in this way means the difference between the groups is not clearly different from zero. By removing individuals with a lump sum payment post-advice, we exclude a group that we would intuitively expect to benefit from advice, as they are likely to have investable assets to manage. Therefore, it makes sense for our estimates to decrease substantially once these individuals are removed. However, this result demonstrates that the wealth benefits associated with receiving advice are predominantly driven by individuals who receive lump sum payments. When only removing those with post-advice lump sums from the sample, we find a positive estimate in the initial period which substantiates the argument that those receiving lump sums benefit from financial advice. This has important implications that we discuss in more detail in Section 7.

Figure 9: There is not a statistically significant association between financial advice and wealth once you remove individuals who received lump sum payments after treatment.



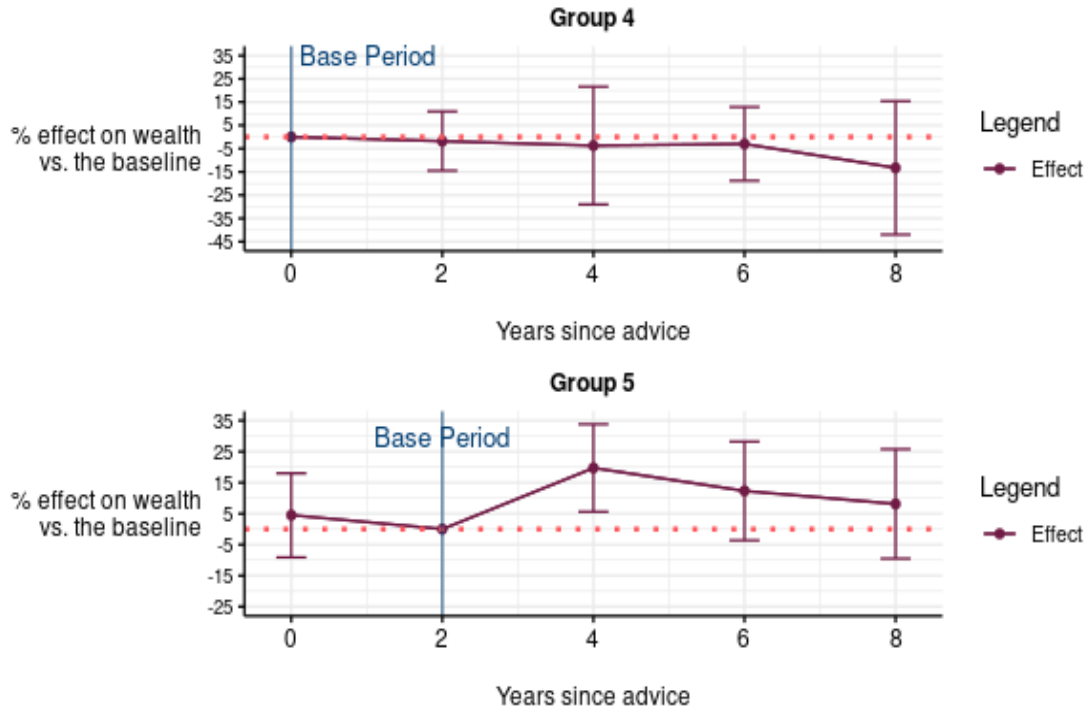
Source: ONS, Wealth and Asset Survey
Notes: This chart highlights the average difference in the change in wealth between those who do and do not get advice, in each period relative to the baseline, excluding those who received a lump sum payment after they got advice. It shows there is no statistical difference in the change in wealth between those who did and did not receive advice in each period when limiting the sample in this way. These results represent the aggregate impact (i.e., the average combined impact for Group 4 and 5, weighted according to their relative share of the sample).

Again, these overall results mask some interesting cross group differences in the impact of excluding post-advice lump sums. Figure 10 shows that Group 4's estimates become small or negative with 95% confidence intervals comfortably covering zero, whereas Group 5's estimates are extremely similar to Figure 7 without the exclusion.

We also run robustness checks that match treatment and control groups on the value of their post post-treatment lump sum. The results are qualitatively similar to our main results, however the first period ATT reduces from ~10.5% to ~ 8.8%.

Our robustness checks highlight that our results are not driven by wealth outliers; however, they are sensitive to the exclusion of individuals who receive a post-advice lump sum payment. We also ran additional tests, including running the model without any controls, and the results from this model as well as the results from our other checks can be found in Annex 1 (figure 17). In the next section, we discuss the policy implications of these findings for the Advice Guidance Boundary Review and discuss the limitations that exist in our analysis.

Figure 10: The exclusion of individuals with lump sum payments after treatment affects Group 4 estimates more substantially:



Source: ONS, Wealth and Asset Survey
Notes: This chart highlights the average difference in the change in wealth between those who do and do not get advice in groups 4 and 5, in each period relative to the baseline, excluding those who received a lump sum payment after they got advice. For group 4, it shows there is no statistical difference in the change in wealth that between those who did and did not receive advice in each period when limiting the sample in this way. For group 5, it shows that there is a larger increase in wealth initially, but that effect decreases and becomes more uncertain over time.

7 Conclusions and discussion

Our research reveals evidence of a positive association between financial advice and wealth, particularly in the initial years following advice. However, the relationship appears to fade over time and become more uncertain. In part, this uncertainty highlights the difficulty in estimating relationships in such a complex setting. This complexity means it is difficult to draw conclusions on the long-term relationship between financial advice and wealth.

We estimate up to a 10% increase in wealth in the years following advice, relative to those who did not get advice. The 10% approximate estimate includes individuals who received lump sum payments (e.g., gifts, inheritances, pension payments), but this relationship is not clearly different from zero once we exclude this group. However, when only removing those with lump-sum payments post-advice we continue to find a positive association. This demonstrates that our findings are substantially influenced by individuals that receive lump sum payments and that those who receive lump sums are likely to benefit from advice.

Our study builds on previous research by using causal inference techniques to increase the credibility and robustness of estimates for the wealth benefits from receiving financial advice. However, we interpret the magnitudes of our estimates cautiously. Given we find a positive influence of regulated financial advice on wealth over a range of robustness checks, we are confident in the evidence of a positive influence of regulated financial advice on wealth.

Limitations

The research has several limitations, primarily due to the observational nature of the data and challenges associated with the complexity of wealth accumulation and financial advice. Because individuals voluntarily choose to receive financial advice, selection bias remains a potential issue. While our method controls for observable and unobservable differences that are constant over time, selection effects could still arise from unobserved and observed time varying factors.

We use data on a sample of individuals receiving financial advice between 2012-2016. This means that our results reflect the quality of financial advice at that time, for the group of individuals in our sample. The wealth benefits of advice may vary over time and for different demographic groups. These differences are not reflected in our results.

Wealth itself is complex, comprising multiple assets and liabilities that fluctuate with individual choices and market conditions, which complicates the identification of a clear causal relationship. We cannot observe the specific financial decisions of individuals within our data, and other factors such as life events, location, and social support also play roles in wealth outcomes. Furthermore, financial advice is specific to an individual’s circumstances and their financial objectives might not always include growing their wealth. One example of this is pensions decumulation advice, which we are unable to differentiate from other forms of pension’s advice in our analysis. Therefore, our

estimates may be biased downwards given people in this circumstance are included in our treatment group.

Our results are specific to the group of individuals that received advice, who we know are wealthier and older than those that do not receive advice. This means that generalising empirical results to other populations and different forms of advice will be uncertain. Care should be taken and using techniques such as scenario analysis, breakeven analysis and using other avenues of research to determine the similarities between the support offered and the target audience will help to make appropriate comparisons.

Two underlying assumptions of the difference-in-differences approach are potentially violated in our setting. The receival of lump sum payments post-advice means the conditional parallel trends (CPT) assumption is potentially violated in our main results. Further, we might expect individuals to display anticipatory behaviour before receiving advice.

Our results may be influenced downwards by assuming that individuals did not take advice in periods where questions on financial advice were not asked. For example, our non-treated group may include some individuals that took advice in subsequent periods (e.g., Wave 6 or 7). If we assume that advice may have a positive influence on wealth, this could increase the wealth of our control group, thereby reducing the estimates associated with receiving advice. Further, the baseline period for those that received advice in Wave 4 (i.e., Wave 3) does not include a question on advice. This may also downwards bias our estimates, because baseline wealth for both the control group and treated group may include individuals that previously received advice.

Some of these limitations are a direct consequence of the data we use; however, others are inherent in analysing the complex mechanisms that determine wealth and financial decision making. We believe our results build on previous studies, and using advanced econometric techniques brings greater credibility to our findings.

Policy implications

Our findings suggest that financial advice positively supports wealth accumulation, especially in the initial years after advice is received. Our empirical findings reflect the qualitative insights from the literature which suggest financial advice helps consumers to avoid costly mistakes. Examples of these costly mistakes include inefficient tax planning, excessive cash holdings, or a non-diversified wealth portfolio. The short-term benefits of receiving advice could reflect consumers taking informed decisions that avoid these issues, providing an initial boost to wealth.

The fading, and increasing uncertainty, of the relationship between financial advice and wealth over time may have multiple causes. Firstly, this could reflect some of the limitations in our analysis. Alternatively, it may reflect challenges in isolating the longer-term value of advice or issues with longer-term value when only receiving one-off advice. For example, one-off advice may become obsolete or out of date following a change of circumstances, such as a change in tax policy. Without updated advice, this could lead to suboptimal investment behaviour, which may be reflected in smaller long-term estimates of the benefit of financial advice on wealth. Further research to explore the difference in short- and long-term value of financial advice would be beneficial.

This research provides evidence that engaging with financial advice benefits consumers financially, indicating that broadening access to support services will benefit currently underserved consumers. This assumption is reasonable given people that are likely to benefit from other financial support services are also likely to make the same costly mistakes that the literature suggests financial advice can address. These mistakes include excessive cash holdings, inefficient tax planning and misaligned investments with risk preferences. Combining the empirical results from this study with qualitative research looking at whether underserved groups make similar costly mistakes can ensure that new forms of support are designed to deliver similar benefits to a wider group of consumers. For instance, consumer research conducted by the FCA suggests individuals would benefit from support that goes beyond the provision of information or guidance.

Our results show that advice can be particularly beneficial to individuals that receive lump sum payments. This is demonstrated by non-significant relationship between financial advice and wealth once these groups are excluded from the sample (see Figure 9). We believe that individuals who receive inheritances (20%), gifts (12%) and pension payouts (6%) but do not currently take financial advice, are good candidates for being likely to benefit from receiving financial support. There may be barriers such as financial literacy, lack of information on financial advice, the cost of advice, or behavioural issues such as inertia that currently prevent these individuals from accessing advice. New forms of financial support should look to address these barriers.

Our analysis demonstrates the complexity of wealth accumulation and the large number of influences on an individual's wealth. These complexities lead to nuanced findings over time and sensitivity to factors like lump sum payments. These results indicate that policy responses should address the diverse financial situations and needs of various consumer groups and consider that suitable financial support will vary as circumstances change.

The challenges encountered in this research offer insights for future analysis. While some limitations may be addressed through improved data or analytical methods, others will persist due to the nature of self-selection into voluntary financial advice services. We will use our experience from conducting this analysis to ensure that future work are robust and limit biases in results, where possible. This includes accounting for lump sum payments, using analytical methods that reduce bias, and in the approach to survey design. This is part of our commitment to understanding what works and improving our evidence-based policy making.

This research forms one component of a broader FCA-led research initiative, which also includes demand estimation and consumer behaviour studies. Each of these complementary efforts offers distinct insights – together, they can create a more detailed picture of the demand for financial advice and the mechanisms through which consumers derive benefits from advice. Combined findings from this initiative will equip policymakers to design financial support services that promote financial resilience for a broader spectrum of the population. By making financial support accessible, relevant, and effective, policymakers can help bridge the advice gap and empower consumers to make informed financial decisions.

Annex 1: Additional figures and tables

Additional descriptive analysis

We undertake descriptive analysis on our estimation sample (i.e., the group of individuals that we use in our results). Those that receive financial advice are significantly wealthier, older, more likely to be married and have a degree. The most significant differences show up in total wealth. Mean total wealth for those that received financial advice is £612,149 compared to £303,049 for those that did not receive financial advice. Individuals in our sample are, on average, far wealthier, older, and more educated than in the population.

Table 4: Characteristics of the estimation sample.

	Received financial advice	Did not receive financial advice	T-statistic
Total observations	540	1778	NA
Median Annual Employment Income	£0	£0	NA
Median total wealth	£459,061	£184,059	NA
Mean total wealth	£612,149	£303,049	13.15
Female (%)	58	46	4.81
Aged 45+ (%)	87	73	6.75
Married (%)	74	69	2.25
Degree (%)	39	23	7.32
Financial Literacy (%)	76	65	4.68

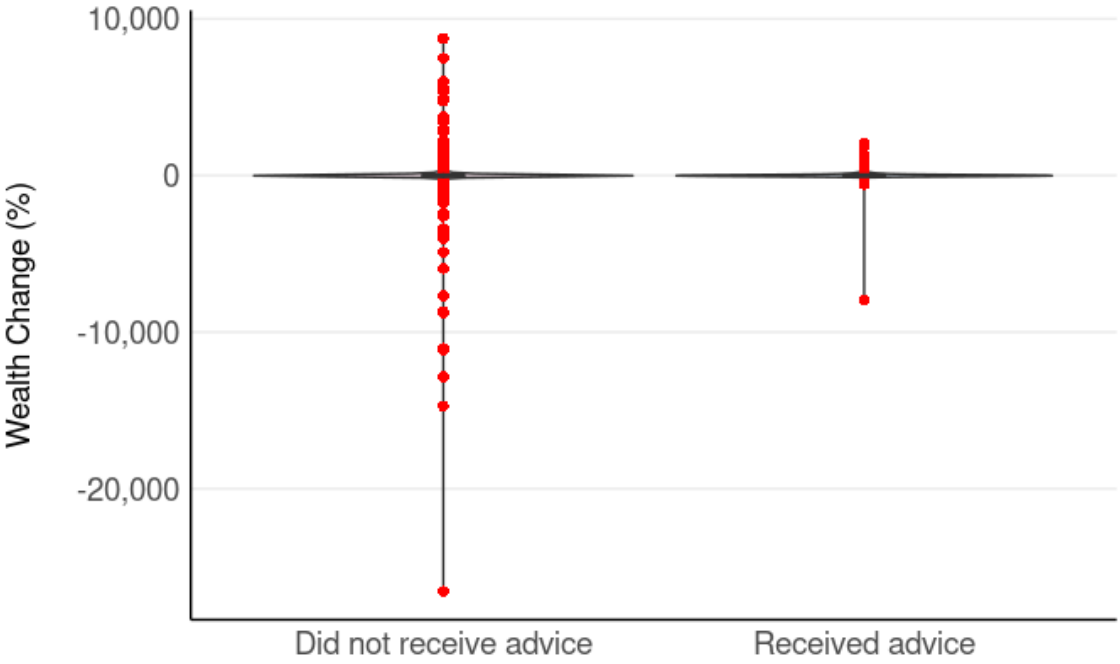
Source: ONS, Wealth and Asset Survey, baseline data(i.e. total wealth in Wave 3 for those treated in Wave 4 and total wealth Wave 4 data for those treated in Wave 5).

How does wealth change over time?

There is significant variation in the extent to which wealth changes overtime for the individuals in our sample. The mean period-to-period change in wealth for individuals that did not receive financial advice was 728% between 2010-2020 - this represents a significant increase in wealth. The mean change in wealth is influenced by extreme outliers. For instance, over this period, in the most extreme case total wealth increases by over 1,000,000%. To put this in perspective, filtering the sample to individuals with wealth changes below 5000% reduces the mean change to 15.5% - highlighting the impact of extreme outliers. In addition, this is evidenced by the fact that the median period-to-period change in wealth for individuals that did not receive financial advice (before filtering the sample to individuals with wealth changes below 5000%) is just 7.6%.

Moving to those that received financial advice, we see that they are less influenced by extreme outliers. The mean period-to-period change in wealth is 28.4%. However, there are still extreme cases. The max change in wealth was 35,970%. The median change in wealth was 4.1%.

Figure 11: There are large changes in wealth across the treatment and control group:

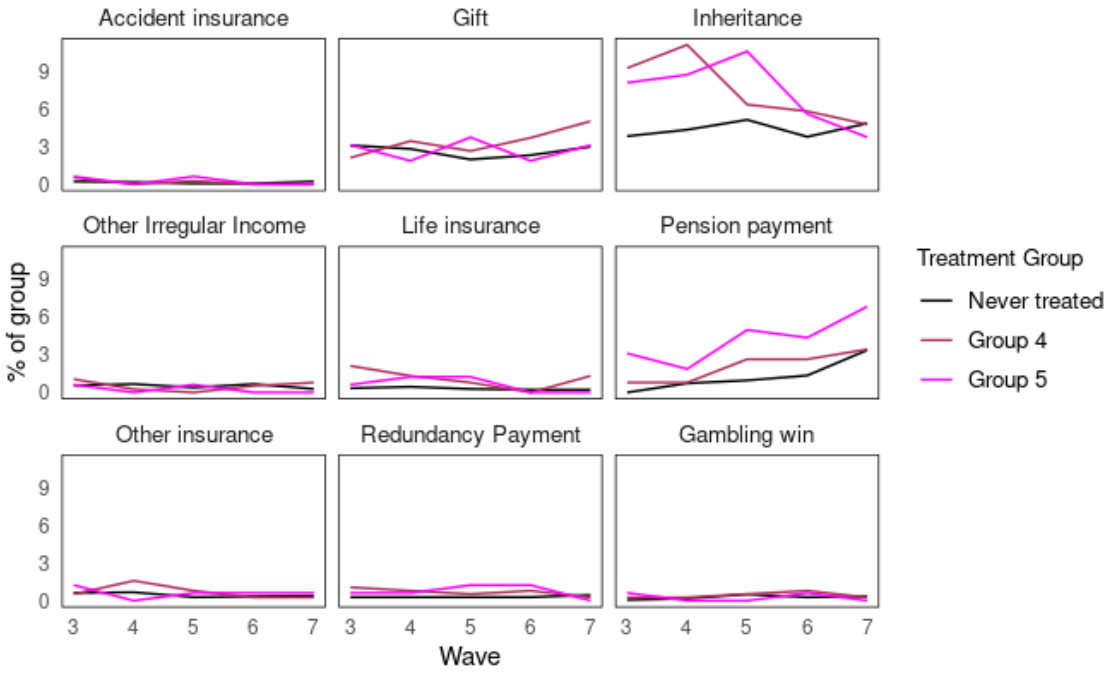


Source: ONS, Wealth and Asset Survey. For presentational purposed the period-to-period percentage change in wealth has been cut at 10,000, meaning observations above 10,000 are not presented in this figure.

Lump sum payments

We explore the relationship between lump sum payments (e.g., inheritances, gifts, and gambling) and wealth. We are interested in the extent to which they are responsible for the large outliers in our sample and how they differ across groups. The following graph plots the proportion of individuals, in each group, that receive a lump sum payment (across payment type). Apart from the 'other irregular income' and 'gambling' (which is more mixed), individuals that did not receive advice have a smaller proportion of individuals in receipt of lump sum payments.

Figure 12: A higher proportion of individuals in the treatment group receive irregular lump sums after treatment:



Source: ONS, Wealth and Asset Survey. This chart highlights the proportion of individuals that receive lump sum payments across waves.

Those that received advice have a relatively high proportion of individuals with gifts, inheritance payouts, and pension payouts. In Wave 7, the proportion of individuals receiving gift payments peaked at 4.4% for those that received advice, received gift payments compared to 2.98% for individuals that did not receive advice. In Wave 4, over 10% of those that received advice received inheritance payments. This compares to the control group, which has less than 6% of individuals receiving inheritance between periods 3-7. With regards to Pension payouts, 4.4% of the 'advice' group received a pension payout in Wave 7. This compares to the group of individuals that did not receive advice, where 3.4% received a pension payout.

Table 5: The Significant Proportion of our Sample Received Lump sum Payments After Seeking Financial Advice

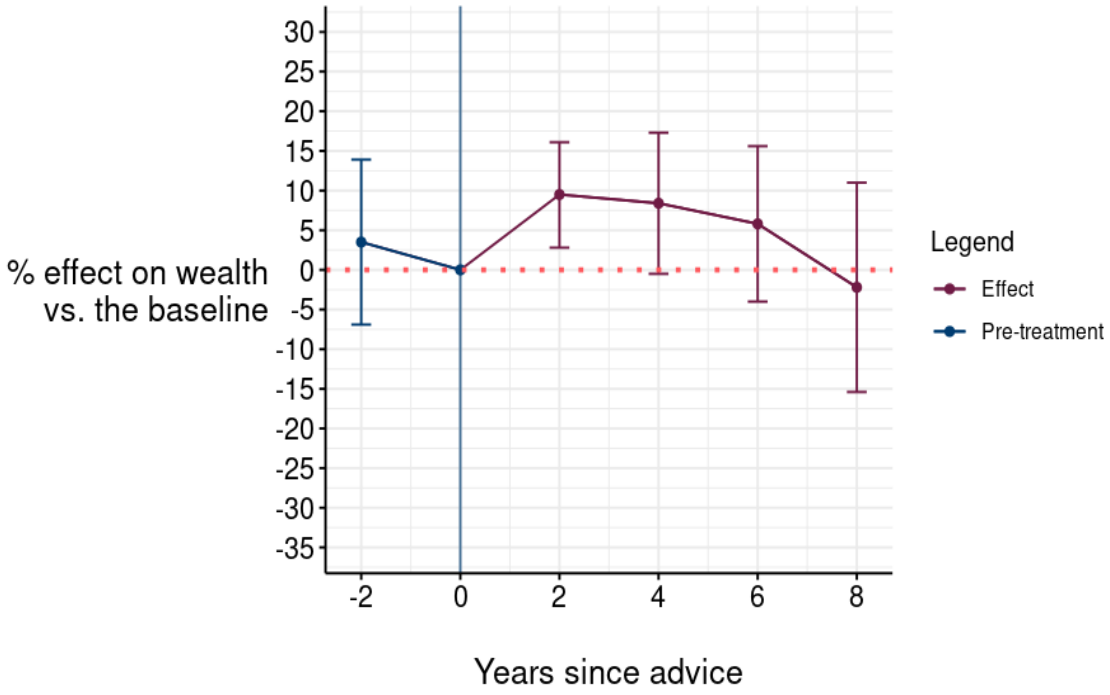
First Treated	Number of individuals with a lump sum payment before financial advice (n)	Proportion of total sample (%)	Number of individuals with a lump sum payment following financial advice (n)	Proportion of total sample (%)
Never treated	122	7	444	25
Treated in Wave 4	45	11	129	34
Treated in Wave 5	39	20	54	34

Figure 13: Individuals that receive financial advice receive higher value gifts, inheritance, and pension payments:

Source: ONS, Wealth and Asset Survey. This chart plots the mean value of lump sum payments by treatment group and control group over time.

Figure 13 plots the mean value of lump sum payments across waves and groups. It is clear from the graph that individuals in group 4, on average, received greater inheritance payments. In Wave 4, mean inheritance for group 4 was just under £150,000, which compares to around £60,000 and £30,000 for group 5 and the control group', Wave 5 for all groups. In Wave 5, the mean 'pension payout' for group 4 stood just under £100,000, around £60,000 for group 5 and around £25,000 for the control group.

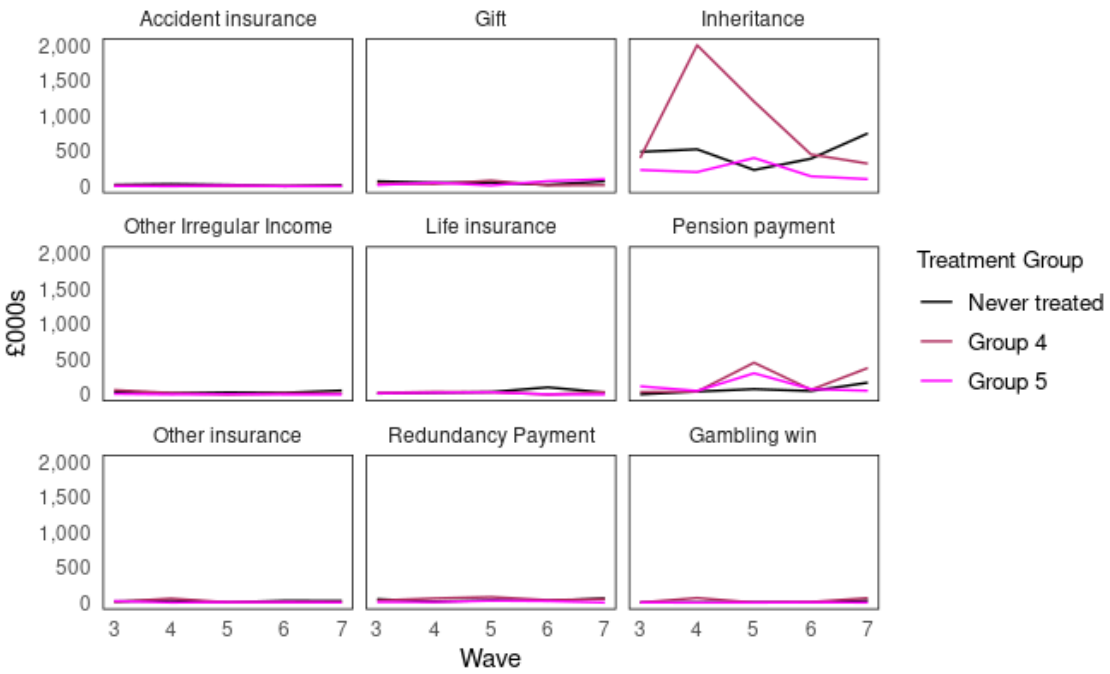
Figure 14: Removing Individuals with total wealth below £100,000 does not alter our findings.



Source: ONS, Wealth and Asset Survey.

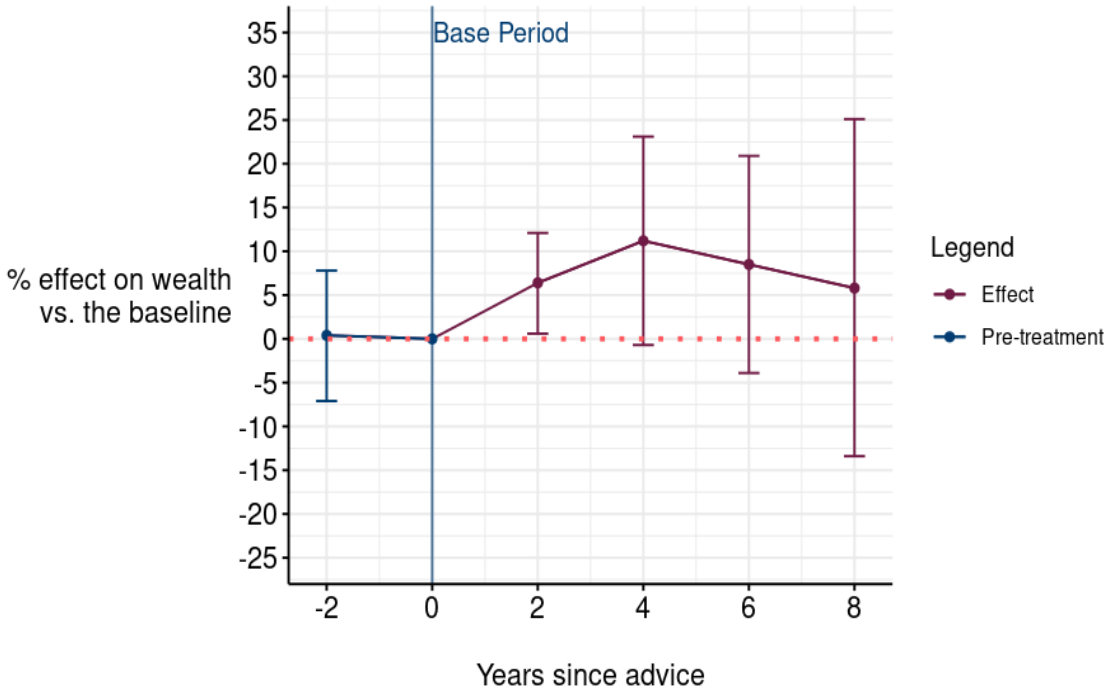
The chart below plots the largest (or maximum) payment received by an individual across all groups and lump sum payment types. The largest payments fall into 'Inheritance' and 'Pension Payout', which algins with the graph above on mean lump sum payouts above. The largest inheritance payment of around £2m occurs in Wave 4 and in Group 4. The largest payments for group 5 and control group are much smaller at around £500,000. Similarly, an individual in group 4 had the largest pension payout, which was around £400,000.

Figure 15: The largest Payouts Fall into the Inheritance and Pension Categories



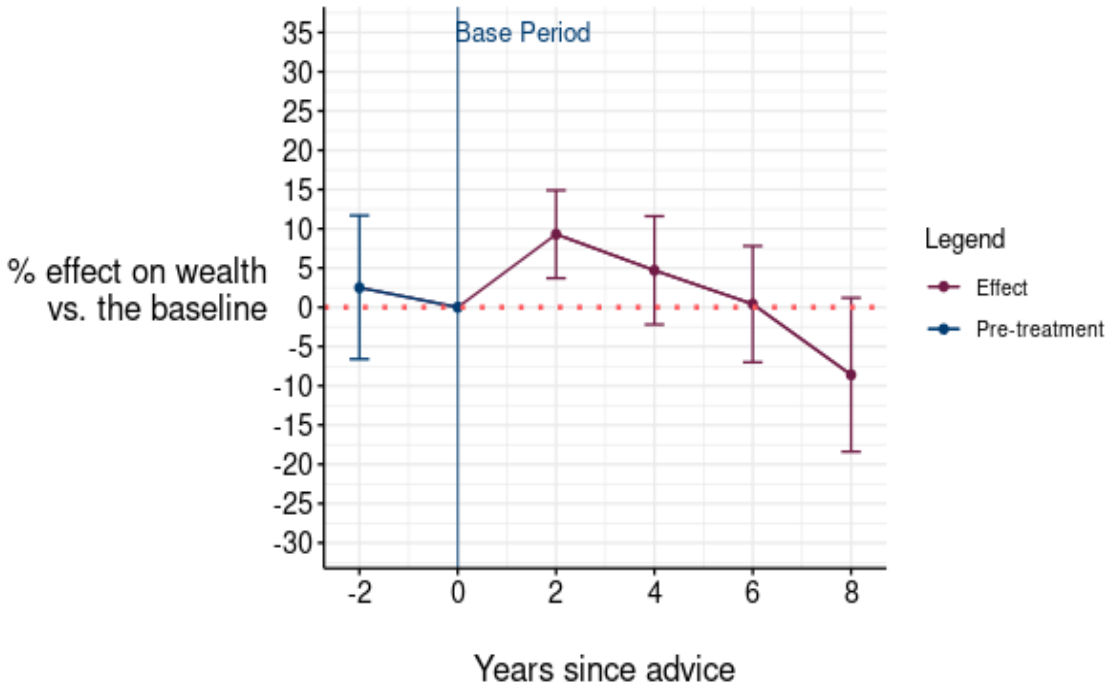
Source: ONS, Wealth and Asset Survey. This chart plots the maximum lump sum payment by treatment group and control group.

Figure 16: Restricting our sample to wealth changes below 200% in each wave does not significantly alter our results:



Source: ONS, Wealth and Asset Survey. This chart highlights the difference in difference results for our sample of individuals with wealth changes below 200% across Waves 3 and 7.

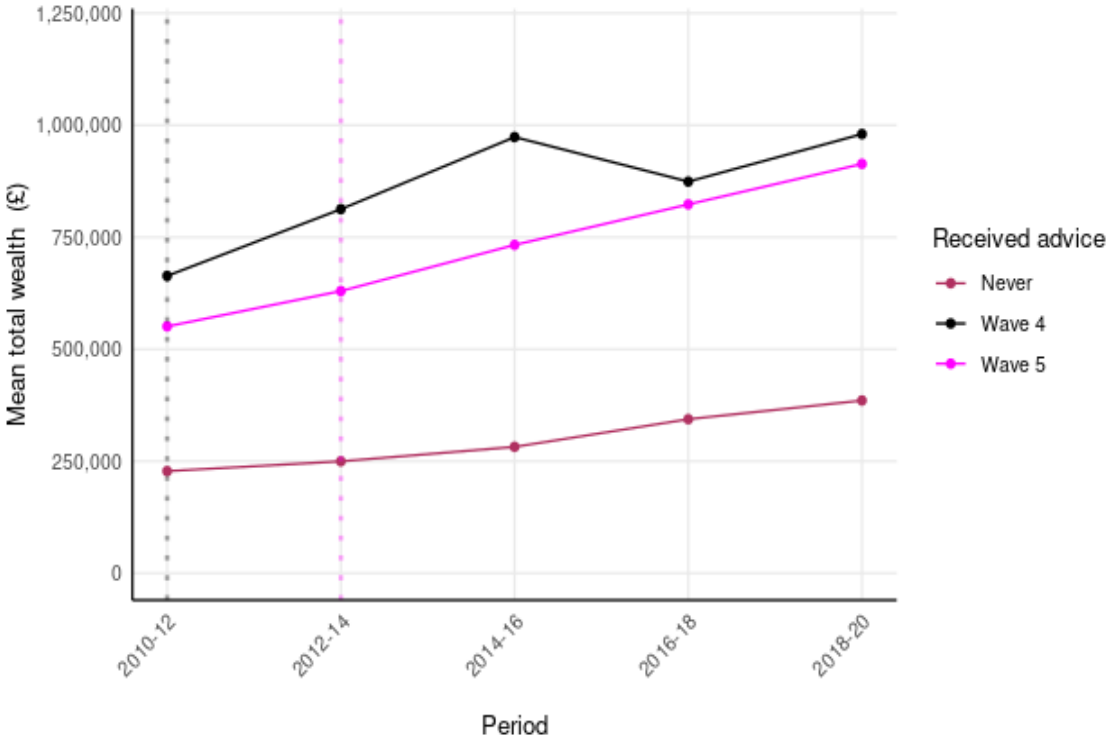
Figure 17. The effect of financial advice on wealth reduces over time if we remove all controls from our model.



Source: ONS, Wealth and Asset Survey. This chart highlights the difference in difference results when we run our model without any covariates.

Annex 2: Estimation sample

Figure 18. Wealth differences exist across treatment groups:

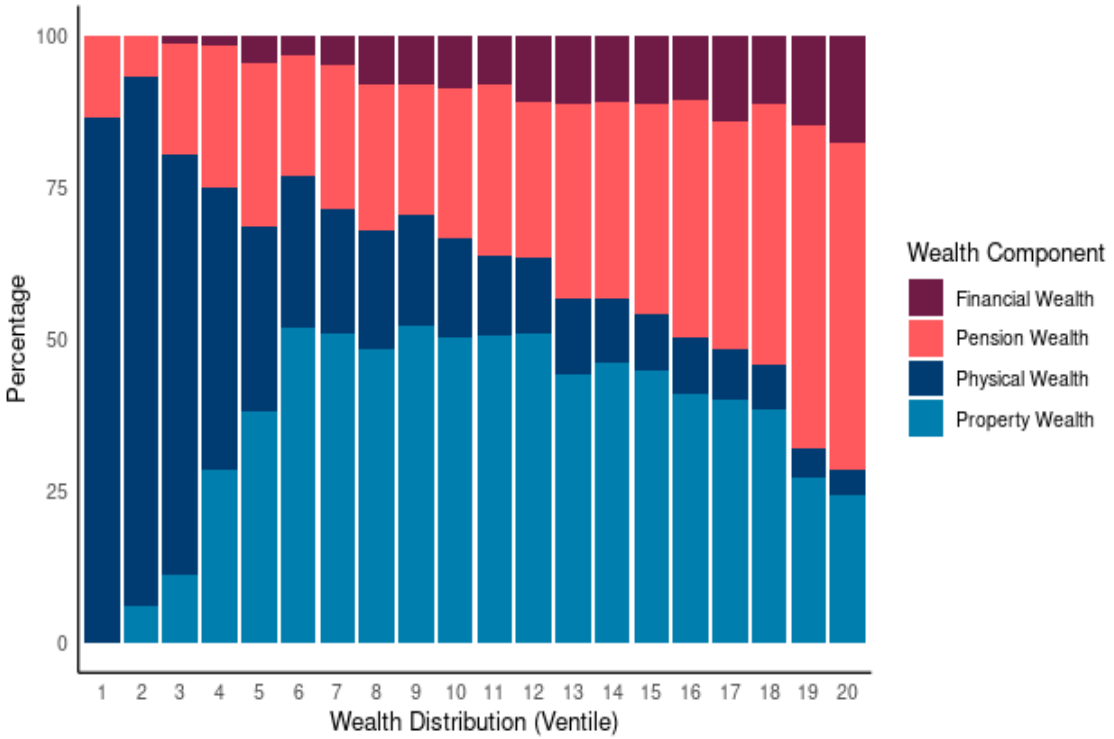


Source: ONS, Wealth and Asset Survey, Waves 3 to 7.
Notes: In this chart, mean total wealth is plotted across the 5 waves ranging from 2010 to 2020. The dotted horizontal lines represent the two treatment periods. The black dotted line highlights when treatment group 4 received financial advice and the pink dotted line highlights when treatment group 5 received advice.

Figure 18 above highlights the importance of controlling for individual wealth. Individuals that received financial advice were, on average, consistently and significantly wealthier than those who did not receive financial advice. There are also differences between the two treatment groups. Treatment group 4 are wealthier than group 5 but the gap between the two groups narrows in the last two periods.

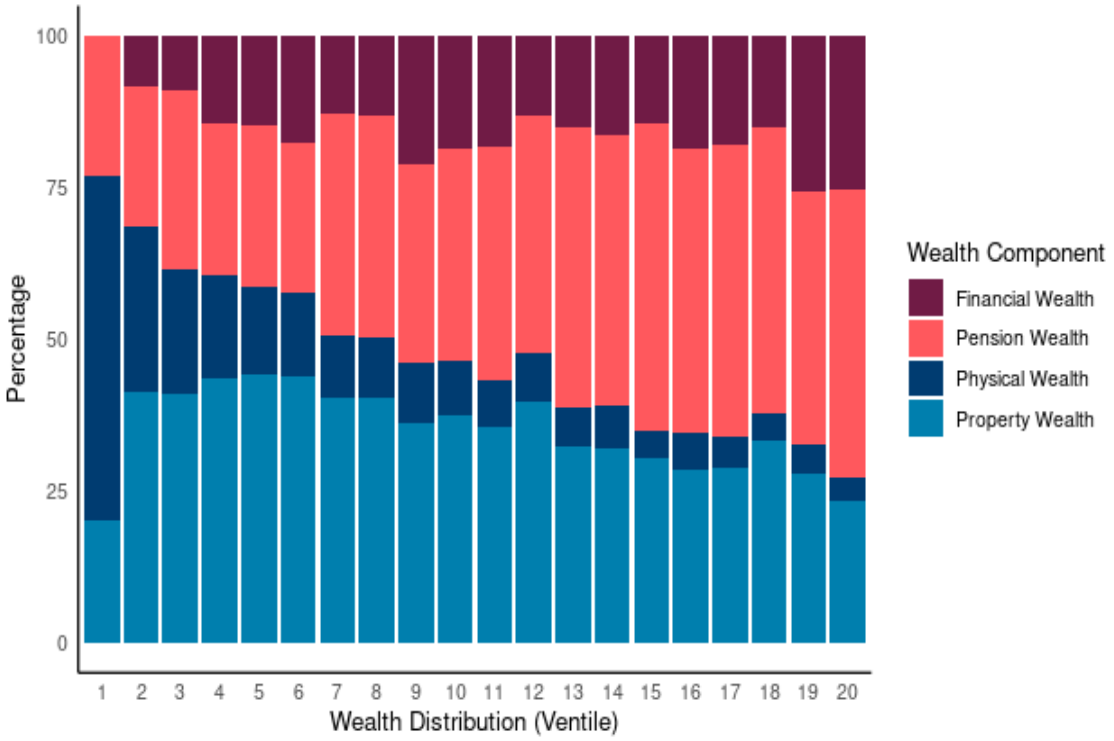
In Section 4, we analyse the difference between those who receive advice and those who do not receive advice using population weights. For Section 4, we use Wave 5 data. In this section we look at differences across groups in our estimation sample and use baseline data (Wave 3 for Group 4, Wave 4 for Group 5) for the comparison. This supports the decisions regarding including covariates in our model. For the group that do not receive financial advice, the composition of wealth in the estimation sample and the population are similar. For those that do receive advice, our estimation sample has a smaller proportion of wealth in physical assets at the lower end of the wealth distribution. At the higher end of the wealth distribution, those that receive advice are similar across our sample and the wider population.

Figure 19. Those who do not receive financial advice have a higher proportion of wealth in physical assets at the lower end of the wealth distribution:



Source: ONS, Wealth and Asset Survey, baseline data (i.e. total wealth in Wave 3 for those treated in Wave 4 and total wealth Wave 4 data for those treated in Wave 5). The data relates to individuals in our sample that have positive wealth and never received finanical advice. This means we replace negative wealth values with 0.

Figure 20. Individuals who receive financial advice have a higher proportion of financial and pension wealth:



Source: ONS, Wealth and Asset Survey, baseline data (i.e. total wealth in Wave 3 for those treated in Wave 4 and total wealth Wave 4 data for those treated in Wave 5). The data relates to individuals in our sample that have positive wealth and received finanical advice. This means we replace negative wealth values with 0.

Annex 3: Model outputs

Table 5: t-tests of the difference in average observable characteristics between individuals

variable	estimate	std. error	t statistic	p value
income	3 993***	855	4.6681	0.0000
wealth	309 100***	23 511	13.1472	0.0000
sex	0.1175***	0.0244	4.8083	0.0000
married	0.0506**	0.0225	2.2546	0.0243
age	0.1402***	0.0208	6.7518	0.0000
degree	0.1579***	0.0216	7.3215	0.0000
financial literacy	0.1072***	0.0229	4.6803	0.0000

*****p < 0.01; **p < 0.05; *p < 0.1**

Table 6: t-tests of the difference in average observable characteristics between individuals in the population that did and did not seek financial advice.

variable	estimate	std. error	t statistic	p value
income	2 764***	452	6.1146	0.0000
wealth	538 571***	19 819	27.1749	0.0000
sex	0.1081***	0.0125	8.6452	0.0000
married	0.0273**	0.0116	2.3566	0.0185
age	0.1874***	0.0117	16.0829	0.0000
degree	0.1647***	0.0110	14.9214	0.0000
financial literacy	0.1783***	0.0075	23.6442	0.0000

*****p < 0.01; **p < 0.05; * <0.1**

Table 7: Group Balance Test

variable	estimate	std. error	t statistic	p value
income	-1 899	2 077	-0.9141	0.3611
wealth	-70 246	55 760	-1.2598	0.2083
sex	0.0201	0.0466	0.4325	0.6655
married	0.0243	0.0413	0.5874	0.5572
age	-0.0516	0.0318	-1.6243	0.1049
degree	0.0274	0.0460	0.5950	0.5521
financial literacy	-0.1411	0.0399	-3.5326	0.0004

*****p < 0.01; **p < 0.05; * <0.1**

Table 8: Main estimation Results

Time (years)	Estimate (%)	Std. error	CI lower	CI Upper
-2	2.9	3.8	-6.4	12.1
0	0			
2	10.2	2.6	4.0	16.5
4	10.5	5.2	-2.2	23.1
6	9.4	5.6	-4.1	22.9
8	1.6	7.5	-16.5	19.7
Overall ATT	7.9	4.7	-3.5	19.4

Notes:

Table 9: Main Estimation, Group 4

Time (years)	Estimate (%)	Std. error	CI lower	CI Upper
-2	NA	NA	NA	NA
0	0.00	NA	NA	NA
2	6.2	3.3	-2.6	14.9
4	8.1	6.5	-9.2	25.4
6	8.4	7.1	-10.3	27
8	1.6	7.4	-18	21.2

Notes:

Table 10: Main Estimation Results, Group 5

Time (years)	Estimate (%)	Std. error	CI lower	CI Upper
-2	2.9	3.7	-6.8	12.5
0	0.0	NA	NA	NA
2	19.7	4.1	8.7	30.6
6	16.0	4.8	3.3	28.82
4	11.9	5.1	-1.6	25.3

Notes:

Table 11: Restricting wealth to less than £5m

Time (years)	Estimate (%)	Std. error	CI lower	CI Upper
-2	3.4	3.7	-5.5	12.2
0				
2	9.9	2.6	3.6	16.2
4	9.7	5.1	-2.6	21.9
6	9.7	6.3	-5.5	24.9
8	-0.5	8.7	-2.1	20.4
Overall ATT	7.2	5.0	-4.8	19.2

Notes:

Table 12: Removing Individuals with lump-sum payments.

Time (years)	Estimate (%)	Std. error	CI lower	CI Upper
-2	4.5	5.4	-8.6	17.5
0				
2	4.2	3.6	-4.5	12.9
4	0.8	7.7	-17.9	19.4
6	0.1	5.1	-12.2	12.4
8	-13.3	1.5	-40.8	14.2

Notes:

Table 13: Restricting to less than 200% changes.

Time (years)	Estimate (%)	Std. error	CI lower	CI Upper
-2	-0.4	3.1	-7.0	7.8
0				
2	6.4	2.3	0.8	11.9
4	11.2	5.2	-1.3	23.7
6	8.5	5.1	-3.9	20.9

8	5.8	8.2	-13.8	25.5
Overall ATT	8.0	4.9	-3.8	19.7

Notes:

Table 14: Removing individuals with wealth below £100,000.

Time (years)	Estimate (%)	Std. error	CI lower	CI Upper
-2	3.5	3.9	-6.1	13.1
0				
2	9.5	2.5	3.3	15.6
4	8.4	3.7	-0.9	17.7
6	5.8	3.9	-4.0	15.6
8	-2.2	5.4	-15.7	11.3
Overall ATT	5.4	3.3	-2.9	13.6

Notes:

Table 15: Model without controls

Time (years)	Estimate (%)	Std. error	CI lower	CI Upper
-2	2.5	3.7	-6.6	11.7
0				
2	9.3	2.3	3.7	14.9
4	4.7	2.8	-2.2	11.6
6	0.4	3.0	-7.0	7.8
8	-8.6	4.0	-18.4	0.1
Overall ATT	1.4	2.6	-4.9	7.8

Notes:

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