The Effect of Advisors' Incentives on Clients' Investments[*]

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Abstract

We use granular information from a Spanish investment firm to estimate the causal effect of financial advisors' compensation contracts on their clients' investments. Our identification exploits: (a) the fact that, for historical reasons, compensation contracts at our firm differed across mutual funds for the same advisor and across advisors for the same fund, and (b) the overhaul to the firm's compensation policy triggered by MiFID II, which resulted in within-advisor-fund plausibly exogenous variation in incentives. We find that clients' investments react markedly and swiftly to changes in their advisors' incentives. The effect is larger for new clients, for clients who trust their advisors more, and for clients with lower financial knowledge. We identify a dual mechanism underlying this effect: clients whose advisors experience a change in incentives bring more money into the fund portfolio and then direct this money into their advisors' preferred funds. We introduce our reduced-form estimates into a portfolio-choice model to quantify investors' utility loss due to the distortion in advice. We estimate losses ranging between 6% and 9%. The change in compensation policy triggered by MiFID II reduced these losses significantly.

1   Introduction

Many households use financial advisors to help them choose investment products.[1] In the last twenty years, governments around the world have introduced reforms to tightly regulate the relation between advisors and their clients, and the compensation structures that advisors are incentivised with.[2] A core assumption underlying these reforms is that advisors' compensation contracts, which are often based on sellers' commissions and favour certain products over others, introduce large distortions in their clients' investment choices. Parallel to the policy interest, a voluminous literature in economics and finance has been motivated by the conflicts of interest between advisors and their clients.

In this paper, we estimate the causal effect of advisors' incentives on their clients' investments. Despite the large body of work addressing incentive conflicts in investment advice, a precise account of this relation has proven elusive, for two major reasons. Firstly, it has been exceedingly difficult to observe individual advisors' compensation contracts and link them to their clients' investment decisions.[3] Yet, without linking contracts to individual investments, it is difficult to gain a complete picture of the policy-relevant effects of interest. It is impossible, for instance, to estimate elasticities (i.e. how much changes in advisor compensation translate into changes in their clients' investments), and therefore to use these elasticities to evaluate which types of clients will react more strongly to policy-induced changes in incentives. The lack of detailed micro data also makes it difficult to study the mechanisms through which clients adjust their investments in response to changes in advisor compensation. Lastly, linking contracts to investments is necessary to estimate the magnitude of the client utility loss associated with different contractual arrangements.

The second major obstacle to progress has been on the identification of causal effects. Contracts favouring certain products may attract advisors and/or clients with beliefs, abilities or preferences geared towards these products. The potential confluence of selection and treatment effects then makes it difficult to infer causality purely on the basis of correlations

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[1]Hung et al. (2008) report, for instance, that 73% of US individual investors had contacted a financial advisor before making investment decisions. In the EU, Chater et al. (2010) find that 58% of buyers of investment products report having been influenced by a financial advisor.

[2]For instance, the UK's 2013 Retail Distribution Review banned all advisors from receiving commissions from the sellers of financial products. In the US, the Dodd-Frank Act of 2010 extended the circumstances under which advisors are subject to the fiduciary standard, which typically requires them to be paid by the buyer (rather than the seller) of the financial product.

[3]Past research has often relied instead on audit studies measuring recommendations but not actual investment (Mullainathan et al. 2012, Anagol et al. 2017), or aggregate analyses at the product level (Bergstresser et al. 2009, Christoffersen et al. 2013, Del Guercio and Reuter 2014, Egan 2019). We discuss past research in more detail at the end of this section.

between compensation and investment, especially if these correlations are at an aggregate level.

We make progress on these challenges by exploiting the internal administrative records of a Spanish investment firm. Our firm manages a large number of active mutual funds, each associated with a different investment style and management fee charged to the client. These funds are marketed through a network of financial advisors who maintain exclusive long-term relations with their clients. Using the firm records, we can compute the investment stocks and flows by each client in each fund managed by the firm between 2015 and 2020. Critically, we can also measure the compensation received by the client's advisor from each fund, which in our firm takes the form of a trailer fee (i.e. a share of the fund's management fee that is charged every month to the client). To make progress on identification, we leverage a natural experiment triggered by the 2018 introduction of the set of European Union-wide regulations known as MiFID II. For historical reasons detailed in Section 2, the share received by advisors (and therefore the trailer fee) varied prior to 2018 both across advisors and (within advisors) across funds. In responding to MiFID II and with the objective of reducing conflicts of interest, the firm adopted in January 2018 a rigid compensation policy which equalised the shares both across advisors and across funds, thereby generating arguably exogenous within-advisor-fund time variation in trailer fees.[4] This variation forms the basis of a generalised differences-in-differences-in-differences (DiDiD) strategy which produces plausibly causal estimates.

Our empirical analysis proceeds in three steps. In the first step, we exploit the aforementioned natural experiment to estimate the effects of trailer fees on the investments of *existing* clients (i.e. clients that were active in the firm both before and after 2018). Using a panel of clients, funds, and months, our DiDiD model exploits time variation in fees while controlling for all pairwise interactions of the three panel dimensions (and therefore for selection effects). The baseline finding here is that a 10% increase in the advisor's trailer fee from a fund increases the client's investment stock in that fund by 4.9%. Heterogeneity analyses suggest that the effect is larger for clients who trust their advisors more (e.g. are more socially and geographically connected to them, and have been with them for longer) and self-report lower financial knowledge.

The credibility of the DiDiD design hinges on the assumption that no other factor

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[4]It is important to note that, because the management fees continued to differ across funds, equalising the shares did not equalise the trailer fees across funds. Therefore, the new policy did not create fully balanced incentives, and a conflict of interest remained (although we show that in a significantly weaker form). We detail the rationale for this policy in Section 2. We argue there also that no other change in the relation between advisors and clients was introduced discontinuously in January 2018.

correlated with the change in the trailer fee induced clients to alter their investment choices. To evaluate the validity of this assumption, we perform a placebo test using the fact that some advisors experienced no change in their trailer fees, and find no change in investments for their clients. Our second and most important falsification test interacts the change in trailer fees with a set of leads and lags around the introduction of the new policy. We find that the lead estimates exhibit a broadly flat trend prior to this introduction, which provides support for the identification strategy. The lagged estimates are informative in their own right, as they indicate that the adjustment of investment to the new incentives starts immediately and takes place over eighteen months.

Next, we investigate the mechanisms behind the above adjustment. We distinguish between three instances in which advisors could induce choices geared towards their high-incentive funds: (a) when clients are bringing new money into their portfolio of investments in the firm's funds, (b) when clients are taking money out, and (c) when clients are reallocating their capital across funds but within their overall fund portfolio. We find that the 2018 compensation policy affected investments through a dual mechanism. Firstly, clients whose advisors experienced a change in incentives brought more new money into their overall fund portfolio. Secondly, controlling for the overall amount of new money, clients disproportionally allocated it to the funds in which their advisors received a higher trailer fee than before 2018. On the other hand, clients do not reallocate existing investment across funds to suit the incentives of their advisors These findings allow us to identify the levers that advisors can use (and the constraints that they face) to induce their preferred investment choices.

The finding that the treatment effects of incentives are larger for new money suggests that the effects could be much larger for new clients (which, by definition, are bringing new money into the firm), relative to existing clients (which may or may not be bringing new money). In the second step of the empirical analysis, we therefore estimate treatment (as well as selection) effects for new clients. We start by investigating advisors' strategies to engage with new clients, and find that advisors rely heavily on social and geographic proximity to engage with new clients. For instance, the likelihood that a client of the firm and an advisor of the firm start a relation is 50% higher if they live 200 metres away from each other, relative to living one kilometre away. We then investigate the existence of selection effects of incentives and find evidence inconsistent with strong effects. Firstly, observable characteristics of new clients do not vary when the incentives of their advisors change. Secondly, we find that proximity remains as important after 2018, suggesting that advisors do not vary their engagements strategies after the change in incentives.

In terms of the treatment effects of incentives for new clients, we again use the natural experiment described above and estimate an elasticity between trailer fees and investments of 150%. The finding that the elasticity is three times larger for new clients than for existing clients supports the notion that advisors' influence is highest when the client is bringing new money into the portfolio. It also suggests that the aggregate effects of any policy-induced change in incentives is likely to be higher when advisor-client relations form and break more frequently in the economy.

In the third step of the empirical analysis, we propose and estimate a quantitative framework to measure the average client utility loss resulting from the misallocation of investments caused by advisors' incentives. Our starting point is a simple portfolio-choice model in which investors have mean-variance preferences over portfolio returns. We assume that advisors can influence client expectations about the returns of various funds, specifically by strategically communicating their information and inducing biases that are proportional to their trailer fees. Based on this distorted view, clients then optimise their portfolio composition. We again use the 2018 changes in incentives to identify from the data the parameters of the model and use these estimates to compute the average client utility loss, both prior to and following the 2018 change in compensation policy. Our quantitative exercise indicates that the average investor in our sample experienced prior to 2018 a utility loss ranging between 6% and 9%. Consistently with the objective of the firm, the 2018 change in compensation policy decreased these losses by between a quarter and a half. In line with the reduced-form treatment effects being lower for clients self-reporting higher financial knowledge, we find that these clients suffer much lower utility losses, both before and after 2018.

A growing body of evidence has documented that retail investors often display lower financial literacy than is necessary to make competent investment decisions (Lusardi and Mitchell 2011, 2014; Hastings et al. 2013). Because of this, regulators have typically targeted advisor compensation while continuing to encourage access to financial advice. This paper provides the first comprehensive view of the effect of incentives on the advisor/client relation.

Related Literature Our most direct contributions are to the literature studying conflicts of interest in investment advice (Inderst and Ottaviani, 2012a). Early US-based work measures the performance of broker-recommended portfolios relative to more readily observable benchmarks (Bergstresser et al. 2009, Christoffersen et al. 2013, Del Guercio and Reuter 2014). Parallel studies use European account-level data and compare the returns of advised clients and self-directed clients (Hackethal et al. 2010, Hackethal et al. 2012, Hoechle et

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al. 2018). These papers show that broker-sold investments perform worse than direct-sold investments, which is consistent with advisor recommendations being both performance-decreasing and conflicted.[5] However, the across-fund and/or across-client comparisons in these papers do not account for other factors correlated with the presence of advice which might be influencing (or compensating clients for their) investment choices.[6] Furthermore, limitations in data have not allowed the existing literature to tackle a wider set of questions underlying the relations between advisors and clients.

We contribute to this literature in four ways. Firstly, we overcome the identification limitations above by using granular data and a credible identification strategy, which allow us to estimate and compare elasticities, that is to measure how much the investment *of the same client with the same advisor* changes as incentives change.[7] These elasticities are important inputs for any policy debate seeking to reduce misalignments of incentives while maintaining access to professional advice (Campbell et al. 2011, Inderst and Ottaviani 2012b). The finding that the elasticities are larger for new clients than for existing clients is important in this respect, as it predicts that the impact of any policy affecting incentives will depend on the turnover of relationships in the industry (Gurun et al., 2021).

A second advantage of our granular data is that we can examine the mechanisms through which advisors induce investment distortions. We find that advisors experiencing a change in incentives both encourage additional investment and then direct new money (but not old money) to their preferred funds. To the best of our knowledge, we are the first to discover this dual mechanism as the one underlying investment distortions.

Our third contribution is to examine empirically the formation of relations between advisors and clients, including studying whether this formation is affected by the advisor's incentives (for theoretical work, see Inderst and Ottaviani 2009). While existing work has

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[5]Supporting evidence is provided by audit studies in which trained actors meet with advisors under a variety of existing (fabricated) portfolios (Mullainathan et al. 2012, Anagol et al. 2017). The main drawback of audit studies is that they do not measure the extent to which a client presented with certain advice chooses to follow it.

[6]For instance, advised clients might display lower financial literacy and perform even worse in the absence of advice (Calcagno and Monticone 2015; see however Chalmers and Reuter 2020). A second confounding factor is that financial products could also differ across unobservable dimensions, including in terms of giving clients access to additional products or services (Hortaçsu and Syverson 2004, Egan 2019). Egan (2019) overcomes this limitation by studying the special case of strictly dominated convertible bonds. Another limitation of comparing returns across clients is that advised portfolios might perform worse because advisors are not good at selecting investments, even for themselves (Foerster et al. 2017, Linnainmaa et al. 2021).

[7]In recent work using data on client-advisor matches, Foerster et al. (2017) and Linnainmaa et al. (2021) show that advisors' preferences and beliefs (as proxied by their personal investments) have a causal effect on the investments of their clients. We replicate this result and evaluate in our setting the relative magnitudes of the 'advisor-own' effect and the 'incentive' effect.

studied the propensity to follow advice within an existing relationship (Bhattacharya et al. 2012, Hoechle et al. 2018, Stolper and Walter 2019), we are the first to examine empirically drivers of the emergence of the relationship.

The existing literature has quantified potential utility losses of advised clients by focusing on strictly dominated products (Egan, 2019) or comparing measures of return and risk, *averaged* across all advised and non-advised clients (Hackethal et al. 2010, Hoechle et al. 2018). However, advisors could decrease welfare by matching clients with products that (while not worse on average) do not fit their individual risk preferences. Our last contribution is to provide a novel quantitative framework to estimate client preferences and use these to measure the client utility loss generated by the misalignment of incentives, in terms of both directing clients to inferior products and matching clients with the wrong products. We estimate losses between 6% and 9%, which suggests that clients can experience significant decreases in welfare even in the absence of financial fraud (Dimmock et al. 2018, Egan et al. 2019).

Investment advice is just one of the many settings in which customers are (at least partially) uninformed about the quality of the service bought, relative to expert sellers. Therefore, our paper contributes to the wider economics literature on credence goods (Darby and Karni 1973, Dulleck and Kerschbamer 2006). A core premise of this literature is that the magnitude of the information asymmetry will determine the size of the distortion in the client's decision. Recent tests of this prediction involve either laboratory experiments (Schneider et al., 2016) or audit studies measuring advice but not clients' decisions (Balafoutas et al. 2013, Anagol et al. 2017). Our finding that more knowledgeable clients display smaller distortions provides supportive evidence in a natural field setting with meaningful client decisions.

Another important question in the credence goods literature is whether the potentially long-term nature of the relation between buyer and seller can help to reduce seller misbehaviour (Dulleck et al. 2011, Kerschbamer and Sutter 2017). In our setting relations are often long-lasting, advisors' trailer fees are payable for as long as the client maintains the investment, and changing recommendations might conflict with previous advice and undermine advisors' credibility. Despite these seemingly promising features, we find large changes in investments when incentives change, which casts strong doubt on the ability of long-term horizons to eliminate the misalignment of incentives.[8] Related to this, we find that proxies for the level of trust between advisor and client predict the size of the investment distortion.

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[8] One potential explanation is that, even ex post, clients find it difficult to evaluate the quality of the advice received (Mailath and Samuelson, 2001).

While existing empirical literature has not emphasised the mediating role of trust (for theoretical work, see Gennaioli et al. 2015), we propose trust as an additional variable potentially affecting distortions in credence goods provisions.

Plan Section 2 describes the institutional setting. Section 3 introduces and briefly discusses the data. In Section 4 we present some preliminary descriptive evidence and discuss the potential effects underlying that evidence. In Section 5, we study treatment effects of incentives on existing clients. In Section 6 we examine selection and treatment effects on new clients. In Section 7 we present a quantitative framework to estimate the client utility loss resulting from the misalignment of incentives. Section 8 discusses external validity and concludes.

2 Institutional Setting

Firm Products and Revenues The firm actively manages a large number of mutual funds (the *internal funds*), on behalf of the participants in these funds. These funds are associated with a variety of investment styles, and include both equity funds and balanced funds. In addition, the firm provides brokerage services. Therefore, the financial products that clients can acquire through the firm include the internal funds, and also products managed by other firms (the *external products*), such as stocks, bonds, investment funds, futures, options, etc. In practice, most clients devote the overwhelming majority of their capital to investing in internal funds.[9] Because of this, most of the analysis in the paper is limited to the study of internal funds, although we incorporate external funds into the utility losses calculations of Section 7.

The firm's compensation on the internal funds is charged monthly and is proportional to the size of the investment held in the fund by the client.[10] This proportion is called the *management fee.* Management fees differ across the funds in our sample, with a typical value of an annual 1.5% and the highest fee being more than twice the lowest fee.[11] The

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[9]Figure A4 displays the evolution of the percentage of the investment in external funds, relative to the total amount of investment. The average is around 8%, and the time trend is slightly positive.

[10]Clients using the firm as a broker to transact external products compensate the firm with a one-off *brokerage fee*, at the time of the transaction. Advisors receive a share of this fee, a share that in our sample period is constant across products, advisors and time. Naturally, clients purchasing products such as investment funds may also pay periodically to the external asset management firms.

[11]The management fees in our firm are independent of the fund's return and the only cost paid by clients. There are no front-end loads (a fee paid by investors upon purchase of the financial product) or back-end loads (a fee paid upon sale).

management fees did not vary across clients or advisors and remained constant throughout our sample period.

Advisors   The firm markets both its internal funds and its brokerage services through a network of financial advisors. These advisors are self-employed but have an exclusive contractual arrangement with the firm. The role of these advisors is to solicit clients, inform them of potential investment opportunities, manage their orders and keep them regularly informed of their portfolio's performance. Advisors are licensed to sell individual securities and derivatives, but they are not subject to a fiduciary duty and cannot engage in discretionary trading on behalf of their clients.[12] An internal company rule specifies that clients first signing with the firm through an advisor will not be transferred to a different advisor, unless the original advisor ceases to work for the firm.

Contracts   Throughout our sample period, advisors were not paid any base wage. Instead, advisors were paid a share of the management fee that the firm extracted monthly from the client, when the client maintained their investment in an internal fund. Importantly, however, prior to 2018 the actual value of this share differed *both across advisors and across funds*. A major factor determining the value of the share for an advisor/fund combination was whether the advisor had been hired prior to 2010.

   Consider a typical advisor hired before 2010. Prior to 2018, this advisor would receive a relatively high share in around two thirds of the funds, a lower share in a quarter of the funds and an even lower share in the remaining funds.[13] By contrast, a typical advisor hired after 2010 would receive the same share regardless of the fund where their clients invested their money. Interestingly, the share received by the pre-2010 advisors was higher in some funds and lower in other funds, relative to the constant-across-funds share received by the post-2010 advisors. Figure 1 provides an illustration of the variation in contractual arrangements in our sample.

   The reason for the differential treatment of advisors was as follows. The firm's management decided in 2010 to simplify its compensation policy, and move to a single share applying to all funds. However, it proved very difficult to renegotiate contracts with existing

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[12]Advisors subject to a fiduciary duty are legally bound to act in their clients' best interests. Advisors not subject to a fiduciary duty typically only have to fulfill a 'suitability' obligation. The advisors in our firm resemble the US 'brokers-dealers' more than the US 'registered investment advisors'. We follow existing work in referring to them as 'financial advisors' (see Egan et al. 2019 and Gurun et al. 2021).

[13]The actual values of these shares cannot be disclosed for confidentiality reasons. We can not disclose the number of funds in our dataset either.

advisors. As a result, the constant share applied only to newly hired advisors. Thus, advisors with different contracts worked side by side, depending largely on whether they had joined the firm before or after 2010.[14] This disparity prevailed until the introduction of MiFID II in January 2018.[15]

MiFID II The Markets in Financial Instruments Directive II (MiFID II) is a comprehensive set of regulatory reforms introduced by the European Union with the core objective of strengthening investor protection. In terms of advisor incentives, MiFID II continued to allow their compensation to depend on the revenue generated, although it now encouraged firms to provide more balanced incentives across products. It was the introduction of MiFID II that prompted the firm to modify its compensation policy in January 2018, in the way that we describe in the next subsection.

In addition to its implications for incentives, MiFID II contained additional requirements regarding advisory services. These were: (a) increased transparency of charges, (b) the requirement that all advisors acquire approved qualifications within a four-year period, (c) the requirement to keep records of all communications with clients, and (d) formal surveys, to be completed by customers, attesting to the suitability of the advice provided.[16] Importantly for our purposes, these provisions were typically *not* introduced by the firm to discontinuously coincide with the January 2018 change in compensation policy. We discuss the timing of these provisions in more detail in Section 5. There, we also perform a placebo exercise to alleviate any remaining concerns that it might be these provisions, rather than the change in compensation policy, which might be generating our baseline effects.

The January 2018 Change in Advisor Incentives MiFID II provided the firm with the impetus to renegotiate existing contracts and homogenise its compensation policy. A core objective of this change was to decrease conflicts of interest between advisors and clients. Specifically, starting in January 2018 all advisors received a share of the firm's revenue that was the same both across advisors and across funds. The pre-2018 heterogeneity in shares

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[14]In addition to the 2010 change, other changes were introduced throughout the years. These changes were relatively minor in that they involved only small adjustments in the shares of some funds. The main difference in contracts is between advisors hired before and after 2010.

[15]The fact that the treatment and control groups in the empirical strategy of Section 5 are linked with the advisor's cohort raises the potential concern that clients of different advisor cohorts might have been differentially changing their investment strategies already prior to January 2018. We show in Figure 5 that this was not the case. We also show there a discontinuous change coinciding with early 2018.

[16]MiFID II also required a percentage of the agents' compensation to be based on qualitative components. The firm complied by subjecting advisors to a small deduction in their compensation if they consistently failed to adopt any steps necessary to meet the additional provisions of MiFID II.

implied that a typical post-2010 advisor experienced no change at all in their compensation structure. On the other hand, for pre-2010 advisors the share increased in some funds while decreasing in others. The change in compensation policy therefore generated time variation in shares *within advisor/fund.* We illustrate this time variation in shares in Figure 1.

To capture the change on financial incentives, we define the trailer fee as the percentage of client c's investment in fund j that is paid in month t to their advisor a. We can then write the revenue that the investment of a client in a fund generates for their advisor as:[17]

$$AdvisorRevenue_{cjt} = \underbrace{Share_{a(c)jt} \times ManagementFee_j}_{TrailerFee_{a(c)jt}} \times Investment_{cjt}$$

The trailer fee is the main independent variable in our study, as it most directly captures an advisor's financial incentive to encourage their client to invest in a fund. It is equivalent to the 'piece rate' in incentive theory (Gibbons, 1987).

Figure 1 displays an illustration of how the cross-sectional and time variation in shares translates into variation in trailer fees. In Figure 2, we display the actual distribution of trailer fees paid from different funds to different advisor types. In the top panel, we display the pre-2018 distribution for a typical advisor hired before 2010. In the bottom panel we display the pre-2018 distribution for a typical advisor hired after 2010. This is also the distribution for all advisors after the change in 2018.

Four features of the time variation in trailer fees are important to emphasise. Firstly, there was no change in disbursements from the clients' perspective, as the management fees remained constant throughout the sample period. Therefore, any change in client investments coinciding with the change in advisor compensation *cannot* be attributed to a change in the fees paid by the client.

Secondly, as with the advisor's share, the trailer fee increased in January 2018 for some advisor/fund combinations, but decreased for others. Specifically, pre-2010 advisors became better paid in some funds and worse paid in other funds.

Thirdly, the fact that in 2018 the share became constant across both advisors and funds meant that trailer fees continued to differ across funds (due to the differences in management fees), although they ceased to differ across advisors. Therefore, the post-2018 incentives

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[17]Throughout the paper, we choose the intermediate subscripts to reflect the variation in the variable and the final subscripts to uniquely define a row in our dataset. In our baseline dataset, a row is uniquely defined by a client/fund/time combination. The variable $Investment_{cjt}$ varies at this level, hence its subscripts. The variable $Share_{a(c)jt}$ varies at the advisor/fund/time level, but an advisor is uniquely determined by the client's identity, hence $a(c)jt$.

continued to be misaligned, as they continued to favour certain funds over others.[18]

Our last point relates to timing. Advisors were obviously aware of the forthcoming introduction of the MiFID II regulations and likely expected it to affect their incentive contracts. However, the specific form that the new policy took (i.e. equalising the shares across all funds and advisors) was not determined by the firm and communicated to advisors until the autumn of 2017. Because of this, we can regard the change in compensation policy as largely unanticipated, in that advisors could have foreseen its specific details no more than a quarter prior to January 2018. Consistently with this, we find in Figure 5 below (and discuss in Section 5) that the observed change in clients' investments broadly coincided with the introduction of the new compensation policy.

3   Data

The data in this paper was made available to us under a strict confidentiality agreement. We observe the contracts of the 165 financial advisors working in the firm in the summer of 2017 (see Panel A Table 1 for summary statistics). 29% of advisors had acquired an approved financial advisor qualification, such as CISI (Chartered Institute for Securities and Investment) and EFPA (European Financial Planning Association) by December 2020. The average advisor joined the firm in 2007, that is eight years prior to the start of the baseline dataset. 46% of advisors joined before 2010, and therefore had shares that varied across funds before 2018.

We match all advisors to their clients and measure the average investment of each client in each fund at a monthly level, between January 2015 and December 2020. Our focus is on the firm's general purpose mutual funds, and we exclude from the sample 173 clients who are either advisors themselves or are close relatives of advisors. Our baseline sample includes a maximum of 6,133 clients. On average, each advisor serves 41 clients.

Panel B Table 1 provides client summary statistics. As part of MiFID II, clients have to fill a 'Know Your Customer' questionnaire that is used to evaluate the suitability of the products purchased. We use these answers to measure the financial education, professional

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[18]Nevertheless, the new policy made incentives better aligned for the pre-2010 advisors. Two pieces of evidence support the notion of a better alignment. The first one is based on examining a simple measure of alignment: the angular distance between the existing vector of trailer fees and a hypothetical vector of balanced incentives where all trailer fees coincide. We compute this distance both before and after January 2018 and find that, using this simple alignment metric, the incentives became more balanced for the pre-2010 advisors. There was obviously no change for the post-2010 advisors. The second piece of evidence can be found in Section 7, where we estimate the parameters of a portfolio-choice model and find that the average client utility loss induced by advisors' incentives decreased after 2018.

links to finance and financial knowledge of the client. We also generate a dummy variable for whether the client self-reports an income in the top two brackets of the questionnaire (i.e. above €60,000). Unfortunately, the questionnaires are available only for around half of the clients. The average client joined the firm in 2007.

Our baseline dataset is a panel dataset at the active client/fund/month level. To be active in a month a client must maintain a positive investment in at least one of the internal funds. The most salient finding from Panel C Table 1 is that the average active client invests in only 19% of the funds. Median investment in a fund is therefore zero, while the mean is around €4,000. There is significant variation in trailer fees, with the mean being around one percentage point and the standard deviation of the trailer fee in the baseline dataset being an annual .26%.

4   Descriptive Evidence and Discussion of Potential Effects

In this section, we compute simple correlations between clients' investments and their advisors' incentives, discuss the potential effects underlying these correlations, and provide an exploratory analysis to evaluate the relative importance of some of these effects. The broad conclusion from this section is that the correlations are sizeable and are likely to mostly reflect causal (treatment) effects.

Naïve Evidence   To study simple 'naïve' correlations we use the 2015-2017 period, in which there was no time variation in trailer fees and advisors with different contracts worked alongside each other. We start with the baseline dataset, take only the observations in the 2015-2017 period, and collapse across the time dimension to create a single observation for each client/fund. The resulting dataset provides a cross-sectional view of fees and investment in the period before the 2018 change to the compensation policy. We estimate the following equation:

$$Investment_{cj} = \alpha Pre18TrailerFee_{a(c)j} + \beta_c + \gamma_j + \nu_{cj} \tag{1}$$

where $Investment_{cj}$ is the (averaged over the 2015-2017 months in which the client is active) investment of client c in fund j, $Pre18TrailerFee_{a(c)j}$ is the trailer fee received by advisor a of client c in fund j prior to 2018, and β_c and γ_j are client and fund fixed effects.

In Figure 3A, we plot the relation between investments and trailer fees, net of client and fund fixed effects. To aid visual analysis, we average the investment across all observations

within trailer fee bins of .02 in size. A positive relation can be easily discerned from the (residuals of the) raw data. The linear regression (1) is displayed and has an estimate in which a standard deviation increase in the trailer fee (i.e. .26) is associated with an increase in investment of around €2,000, which is around half of the average investment in a fund (see Panel C Table 1). This is clearly a very sizeable correlation.

Selection and Treatment There are three candidate explanations for the large association in Figure 3A. An initial distinction is between *treatment effects* and *selection effects*. In our setting, treatment effects occur when advisors direct investments towards their own high-incentive funds, holding constant their clients' preferences or beliefs. Throughout the paper, we refer to these treatment effects as generating 'distortions'.[19]

Selection effects appear instead when advisors' incentives happen to be correlated with the preferences/beliefs of the clients that these advisors have matched with. It is useful to further subdivide selection effects into two types: *across-advisor selection* and *within-advisor selection*. *Across-advisor selection* arises when the set of trailer fees that the firm offers to prospective new advisors affects the type of professionals recruited. For instance, a contract with a relatively high trailer fee on an Asian-focused fund may make it more likely that an advisor optimistic about Asian markets joins the firm, and that advisor may then match with clients who are themselves bullish on Asia. Across-advisor selection is shaped by the incentives at the time that an advisor joins the firm and therefore persists for a given advisor even if incentives change.

Within-advisor selection refers instead to the notion that, once an advisor has been hired, a change in incentives could prompt them to seek clients with different underlying preferences/beliefs. For instance, an advisor suddenly receiving a high trailer fee on an Asian fund may start searching for clients who are predisposed to investing in Asian markets.

Econometrically, *across-advisor selection* is time-invariant holding constant the identity of the advisor, while *within-advisor selection* is time-variant. This implies that any empirical strategy that controls for advisor/fund fixed effects eliminates across-advisor selection. If the empirical specification controls for client/fund fixed effects (which subsume advisor/fund fixed effects), then both across-advisor and within-advisor selection are eliminated.[20]

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[19]Our rationale for the use of this term is that portfolio choices will display a weaker link with the preferences or beliefs of clients, when they are at least partially affected by advisors' incentives. The portfolio allocations will therefore be 'distorted' away from those maximising client utility given client preferences. In Section 7, we estimate the resulting client utility loss with the help of a mean-variance portfolio choice framework.

[20]The discussion in this subsection assumes that client investment preferences and/or beliefs are time-

Suggestive Evidence that Treatment Effects are Important   We can form an initial view on whether treatment effects explain at all the sizeable Figure 3A association by estimating equation (1) again on *the same group of clients* (i.e. those active in the 2015-2017 period), but this time using their 2018-2020 investments.

The reason that this exercise is informative is as follows. Remember that after 2018 all advisors had the same contract, so there was no across-advisor variation in trailer fees. We can, however, still correlate the 2018-2020 investment of a client with the trailer fee that their advisor had *prior to 2018*. If selection effects dominate and treatment effects are negligible, we should find that the correlation between a client investment and their advisor's pre-2018 trailer fee is as strong after 2018 as before 2018, given that the identity of clients is unchanged.[21] If instead treatment effects are important, the correlation after 2018 should be weaker, as advisors direct the investments *of the same clients* towards their current incentives and therefore away from their pre-2018 incentives.

We find in Figure 3B that the correlation is much reduced in size (i.e. the estimated coefficient has dropped by half), relative to the correlation in Figure 3A. In other words, the unchanged set of clients exhibits a weaker relation with the incentives of their advisors in the period after these incentives no longer apply.[22] We interpret this weaker relation as suggestive evidence that, after 2018, advisors directed their clients' investments in the direction of their current incentives, regardless of client preferences. We confirm this interpretation when we estimate treatment effects for existing clients more formally in Section 5.

Suggestive Evidence that Across-Advisor Selection Effects are not Important
We now perform a similar exercise to evaluate the likely empirical relevance of the across-advisor selection mechanism. We start with the baseline dataset and keep only the clients joining the firm for the first time at some point in the 2018-2020 period. For these new

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invariant. In practice, clients may change their preferences and/or beliefs over time, for instance as they become older. In the presence of time-variant preferences, the empirical model (2) in Section 5 identifies the causal effect of incentives under the assumption that any change in preferences is uncorrelated with the change in incentives. We are explicit about this in Section 5, where we mention that the identification assumption is that no trend or contemporaneous shock caused existing clients to modify their investments across funds in a way that is correlated with the January 2018 within advisor/fund change in trailer fees. The evidence in Figure 5 below, which we discuss in Section 5, provides support for this assumption.

[21] A second reason why we might find an equally strong correlation after 2018 is the scenario in which investment decisions display extreme inertia, and clients rarely vary their investment choices following their initial allocations.

[22] The comparison between Figures 3A and 3B is only suggestive because we are, for instance, not taking into account client attrition. While all the clients in Figure 3B meet the condition of having been active at some point in the 2015-2017 period, it may be that some 2015-2017 active clients have left the firm prior to 2018. The empirical strategy in Section 5 accounts for this by using within client-fund variation in the associated advisor's trailer fee.

clients, we further keep only their initial investments, that is, their average investments during their first quarter with the firm. In Figure 4B we plot the initial investments of the clients joining in the 2018-2020 period against the trailer fees that their advisors had prior to 2018, both after controlling for client and fund fixed effects.[23]

The rationale for this exercise is as follows. Because the pre-2018 trailer fees do not apply in the 2018-2020 period, neither the treatment mechanism nor the within-advisor selection mechanism predict a positive correlation for this subsample of new clients (once we control for the post-2018 trailer fees). The across-advisor selection mechanism however predicts a positive relation, as the pre-2018 trailer fees were the ones on offer at the time that the advisor joined the firm. If advisors with different preferences, beliefs or skills join the firm as a result of the trailer fees on offer, we should find that these initial trailer fees predict the investments of the advisors' new clients, even after the fees have ceased to be applicable.[24]

In Figure 4B we find that the investments of new clients are uncorrelated with the trailer fees that their advisors had in the past (but do not have anymore).[25] Specifically, the coefficient is negative and non-statistically significant. We interpret this finding as suggestive evidence that across-advisor selection does not appear to be an empirically important mechanism in our setting.

Discussion   To summarise this section, we find strong cross-sectional correlations between clients' investments and the trailer fees of their advisors. Descriptive evidence suggests that these correlations reflect, at least partially, treatment effects. We also find that the first alternative explanation, across-advisor selection, does not appear to be economically sizeable in our setting. We have not yet examined descriptively the potential relevance of the second alternative explanation (i.e. within-advisor selection). To anticipate our results,

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[23]A reminder that, in the post-2018 period, the trailer fees vary across funds but not across advisors. Therefore, controlling for fund fixed effects perfectly controls for the post-2018 trailer fee. Therefore, in this regression, we are using the cross-sectional variation in trailer fees across advisor/fund from the pre-2018 period, while controlling for the trailer fees in the post-2018 period.

[24]A second reason why we could find a positive relation between the investments of new clients and their past (but not current) trailer fees is that advisors accumulate human capital that is specific to the recommendation of specific funds. For instance, an advisor who had recommended and therefore followed an Asian fund for many years (because their incentives were geared towards that particular fund) may continue to recommend that fund on the basis of the previous accumulated knowledge. The evidence in Figure 4B is also inconsistent with this effect.

[25]In Figure 4A we repeat this exercise using the clients joining the firm in the 2015-2017 period, for which treatment and both selection effects predict a positive relation. We find here a sizeable correlation, similar in magnitude to the one in Figure 3A. The similarity between Figures 3A and 4A suggests that the absence of a correlation in Figure 3B is not due to the focus on new clients or the limitation of the sample to the first quarter of the new client.

we will find in Section 6 evidence inconsistent with within-advisor selection. Specifically, we will find that advisors do not match with a higher number or different types of clients (in terms of their observables) as their incentives change.

5   Treatment Effects on Existing Clients

In this section, we estimate the causal effect of trailer fees on investments. Our focus in this section is on the subset of clients joining the firm before the 2018 change to the compensation scheme, and remaining with the firm for at least one month after that. Econometrically, a major advantage of studying these 'existing' clients is that we can follow the same client/fund combination over time (therefore controlling for both types of selection), and analyse how its associated investment varies with the advisor's trailer fee. We do this by using the baseline client/fund/month dataset and exploiting the 2018 change in compensation policy as a source of plausibly exogenous variation in trailer fees.

Baseline Empirical Strategy   The equation of interest is a generalised differences-in-differences-in-differences (DiDiD) equation with continuous treatment:

$$Investment_{cjt} = \lambda LogTrailerFee_{a(c)jt} + \eta_{ct} + \kappa_{jt} + \mu_{cj} + \epsilon_{cjt}, \tag{2}$$

where $Investment_{cjt}$ is the stock of investment by client c in fund j in month t, and $LogTrailerFee_{a(c)jt}$ is the (log of the) trailer fee received by advisor a of client c from fund j in month t.[26]

Panel C Table 1 shows that the investment variable takes value zero in a high proportion of observations, while at the same time displaying long right tails. This fact requires caution in the choice of dependent variable in (2). The main dependent variable throughout the paper is the inverse hyperbolic sine transformation (ihst) of the stock of investment.[27]

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[26]Note that our empirical specification is not affected by recent criticisms about DiD designs (de Chaisemartin and D'Haultfœuille 2018, Callaway and Sant'Anna 2021, Goodman-Bacon 2021). First, treatment is not 'fuzzy' as defined in de Chaisemartin and D'Haultfœuille (2018) because no client/fund unit is treated in the control group (defined as all observations related to advisors that joined the firm after 2010). Second, treatment affects all the (treated) advisors simultaneously and they stay treated for the remaining of our sample period. This rules out the concerns related to staggered treatment designs (Callaway and Sant'Anna 2021, Goodman-Bacon 2021). Finally, our parallel trend assumption does not rely on dynamic controls beyond those that define the variation we exploit in our panel (i.e. we don't need to condition on any control that varies at the same level as the treatment variable). Therefore, we can interpret our parallel trend assumption as unconditional, which rules out the concerns described in Callaway and Sant'Anna (2021) for two periods DiD designs.

[27]This transformation has the advantage that it can be used to estimate elasticities (like the log trans-

As alternative variables, we also use a positive investment dummy and the share of the investment value in the fund relative to the client's total investment. Standard errors are clustered at the advisor level.

It is important to emphasise that equation (2) includes all three pairwise sets of fixed effects. The inclusion of client/fund indicators implies that the estimate of λ does not capture either *within-advisor* or *across-advisor* selection effects. The inclusion of fund/time indicators accounts for any general shocks or trends that may have made certain funds more attractive in certain periods (e.g. any economy-wide move towards low-fee funds). Lastly, the client/time indicators control for the total amount of investment of certain clients in certain periods (e.g. the clients of pre-2010 advisors reducing their total investments over time, relative to the clients of post-2010 advisors).

The identification assumption is that no trend or contemporaneous shock caused existing clients to modify their investments across funds in a way that is correlated with the January 2018 within advisor/fund change in trailer fees. Note that the rigid character of the post-2018 compensation policy (i.e. equalising the share received both across advisors and across funds) reduces the scope for endogeneity and enhances the credibility of this assumption. This is, for instance, because it rules out increases in trailer fees narrowly targeted towards advisor/fund combinations in which the clients of an advisor were independently increasing their investments.[28]

Baseline Effects   We provide estimates from (2) in Panel A Table 2. The coefficient from Column 1 can be interpreted as an elasticity, and indicates that a 10% increase in the trailer fee leads to a 4.9% increase in investment. This is a very large elasticity.

We interpret the coefficient in Column 2 as follows: a 10% increase in the trailer fee is associated with a .44 percentage points increase in the likelihood that the client invests in the fund at all (this is 2.3% of the unconditional likelihood, which is 19 percentage points). The coefficient in Column 3 indicates that a 10% increase in the trailer fee leads to a .24

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formation), while also being defined for any real number, most importantly for zero or even negative values (Burbidge et al., 1988). Being defined for negative values implies that we can use it throughout all the dependent variables in Table 4, such as the net inflow. Table A2 shows that the estimates from Column 1 Table 2 are almost identical when instead using the log of investment plus one. Table A2 displays also estimates when: (a) using the dependent and independent variables in levels, and (b) a conditional quasi-maximum likelihood fixed-effect Poisson model, which has been shown to be a good alternative choice when dealing with data with many zeros (Santos Silva and Tenreyro, 2006).

[28]In a setting in which changes to the trailer fees are very idiosyncratic, we may worry about the possibility of reverse causality. Advisors with clients likely to invest in Asian funds in the future would have an interest in re-negotiating increases in the shares received from Asian funds. The rigid nature of the new compensation policy in 2018 rules out this type of reverse causality.

percentage points increase in the share of the total investment allocated to that fund (this is 4% of the average share, which is 6 percentage points). All the estimates are statistically significant.

An important literature in personnel economics shows that when firms pay workers to do something, workers typically respond by doing it (Lazear and Oyer, 2013). It is important to emphasise that the advisors in our firm differ from the relatively mechanical settings typically studied by that literature (e.g. the windshield installation in Lazear 2000) along important dimensions. Our setting includes an additional agent (i.e. the client) who needs to acquiesce in the advisor's recommendation, perhaps at their own detriment. Secondly, remember that the trailer fee through which advisors are compensated is payable every month that clients maintain their investment. This implies that encouraging investments that maximise trailer fees in the short-term may not be optimal for advisors, as it could lead to client disillusionment and withdrawal of capital in the long-term. Lastly, the existing clients studied in this section have a previous history with their advisors, prior to the 2018 change in incentives. Recommending investments aligned with their new post-2018 incentives might conflict with their pre-2018 advice and undermine advisors' credibility. These three features of our setting make finding the very large elasticity in Column 1 Panel A Table 4 quite remarkable.

Robustness to Controlling for Advisors' Own Investments   Foerster et al. (2017) and Linnainmaa et al. (2021) use a rich dataset on Canadian households to argue persuasively that the beliefs and preferences of financial advisors strongly influence their clients' investments. An important result supporting this claim is the finding that the personal investments of Canadian advisors strongly predict the investments of their clients (Foerster et al., 2017). These authors do not observe advisors' contracts and, as a result, their analyses are silent on the role of incentives. Nevertheless, their findings suggest an alternative, perhaps complementary, channel through which clients' portfolios may be distorted by their advisors.

Our baseline dataset does not include the 79 clients who are also advisors themselves. In this subsection, we use information on the personal investments of these advisors with the core objective of confirming that our baseline estimates from Panel A Table 2 are not somehow confounding the effect of advisors' beliefs, as proxied by their own investments. In particular, we replicate the baseline specification (2) but controlling for the own investment of the advisor.

The estimates are displayed in Panel B Table 2. We draw three main conclusions.

Firstly, we are able to replicate (within the context of our setting and empirical specification) the broad result of Foerster et al. (2017) that advisors' own investments predict the investments of their clients. Column 1 Panel B Table 2, for instance, displays a statistically significant elasticity of 3% between these two variables. Secondly, we find that the 'incentives' elasticities are virtually identical whether we control for advisors' own investment (i.e. Panel B) or not (i.e. Panel A). We interpret this robustness as evidence that the effects of incentives and advisors' own investments are broadly orthogonal to each other. Thirdly, we find that the incentive elasticities are an order of magnitude larger than the advisors' 'own investment' elasticities. At least in the context of our firm, it appears that the distortions caused by incentive misalignment are larger in magnitude than the distortions caused by advisors' beliefs (as captured empirically by their investments).

Dynamic Effects of the Change in Incentives Equation (2) treats all months on each side of the policy change equally. In this subsection, we instead allow the effect of the change in trailer fee to vary across the periods leading up to and following the beginning of 2018. We estimate these dynamic effects by interacting the change in trailer fee with a set of lead and lag indicators.

Our independent variable of interest is a continuous measure of the 2018 positive shock to incentives received by an advisor/fund combination:

$$SHOCK_{a(c)j} = Post18TrailerFee_j - Pre18TrailerFee_{a(c)j}$$

where the subscripts in the definition of $SHOCK_{a(c)j}$ emphasise that the post-2018 trailer fee varies across funds (due to differences in management fees) but not across advisors, while the pre-2018 trailer fee varies across both.[29] To decrease the noise in the estimates, we combine every three months into their corresponding quarter q, to create a dataset at the client/fund/quarter level and estimate:

$$Investment_{cjq} = \sum_{q=1...11}^{13...24} \pi_q \Big(SHOCK_{a(c)j} \times Quarter_q \Big) + \eta_{cq} + \kappa_{jq} + \mu_{cj} + \epsilon_{cjq} \qquad (3)$$

The dependent variable is the baseline ihst of investment, and the regression again controls for all three pairwise sets of fixed effects. Because the last quarter of 2017 is quarter twelve

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[29]The variable $SHOCK_{a(c)j}$ is in levels rather than logs, as it can take both positive and negative values. Figure 5 looks virtually identical when using the inverse hyperbolic sine transformation of $SHOCK_{a(c)j}$ (see Figure A3).

in the sample, the parameters $\hat{\pi}_1 \ldots \hat{\pi}_{11}$ capture the estimated leads to the compensation overhaul, while $\hat{\pi}_{14} \ldots \hat{\pi}_{24}$ capture the estimated delayed (or lagged) effects. We display the estimated dynamic effects in Figure 5.

We draw three conclusions from Figure 5. Firstly, there is no evidence of 'pre-trends', as the variable $SHOCK_{a(c)j}$ does not appear to correlate with the evolution of investment prior to 2018. Clients whose advisors would receive an increase in the trailer fee in specific funds did not start to invest in these funds prior to that increase, as it is only around the beginning of 2018 that a higher value of $SHOCK_{a(c)j}$ is associated with an increase in investment. Needless to say, this evidence does not unequivocally prove that the identification assumption in the baseline empirical strategy is satisfied. However, it does suggest that any potential confounding factor (in addition to taking place at the within advisor/fund level) should have been timed to precisely coincide with the end of 2017 and beginning of 2018.[30] We interpret Figure 5 as providing strong support for the identification assumption.

The second conclusion from Figure 5 is that investment starts to change immediately following the change in incentives. We interpret this evidence as reinforcing the conclusion that investment is highly responsive to incentives, not only in terms of the overall magnitudes but also in the swiftness of the response. A related finding here is that, while the trend reacts immediately, the level changes only gradually. This finding indicates that clients do not fully adjust their portfolios at the beginning of 2018 to suit the new incentives of their advisors. Instead, the slow adjustment of the stock is consistent with many clients adjusting in a gradual way, for instance when they have new money to invest or when their pre-scheduled meeting with their advisor takes place.[31]

Lastly, Figure 5 suggests that the move to a new long-run level of investment takes around eighteen months. After Q3 2019, investment has stabilised in its new steady state, as the response to the new incentives appears to be complete.[32]

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[30]The policy was implemented in January 2018 but communicated to advisors in the previous autumn. From Figure 5, it appears that the difference in estimates between Q3 2017 and Q4 2017 is positive, although not statistically significant. The difference between Q4 2017 and Q1 2018 is again positive and statistically significant. While the evidence can not determine whether it is the announcement or the implementation of the policy which affects investment, we nevertheless interpret it as broadly supportive of the identification strategy.

[31]The fact that the investment level changes gradually implies that the coefficients from Table 2, which treat all months equally, may underestimate the full extent of the impact as measured after a sufficient time lag. To evaluate the magnitude of this underestimation, we can re-estimate (2) but using a dataset which includes only the last active month of each client/fund, within each of the pre- and post-2018 periods. We find in Table A1 that the coefficients are indeed larger in this case, although the differences are relatively small.

[32]This finding prompts the question of whether the investments of the pre-2010 advisors' clients have fully converged to equalise the investments of post-2010 advisors' clients, given that after 2018 their incentives are

Residual Identification Concerns and Placebo Exercise In addition to encouraging more balanced incentives, MiFID II included additional provisions to regulate the relation between advisors and clients (see Section 2).

The firm's implementation of these additional provisions did not typically coincide with the January 2018 compensation overhaul. One example is the introduction of client surveys to evaluate the suitability of the financial products under consideration. In Appendix Figure A2, we display the timing of the suitability tests undertaken by the firm. It is apparent that these were introduced in a staggered way over several years, and there is no discontinuity around January 2018.

A second example is the additional qualifications that advisors had to undertake in order to continue working, such as those provided by the CISI (Chartered Institute for Securities and Investment) and EFPA (European Financial Planning Association). MiFID II allowed a four year grace period to obtain these qualifications. Many advisors in our firm already held them prior to January 2018, and others are proceeding to obtain them gradually. Panel A Table 1 shows that 29% of advisors held them in December 2020.

MiFID II also required a record of all communications between advisors and clients. According to the firm, all telephone interactions with clients to or from the firm's premises were already being recorded prior to January 2018. For other interactions the advisors were required to keep a written summary of the conversation.

The gradual introduction of these additional provisions supports a causal interpretation of the baseline estimates for the following reason. If these provisions were confounding the estimates from Table 2, the fact that they were introduced gradually implies that we should not expect a discontinuity in the effect of the change in trailer fee around 2018. Figure 5, however, displays a sudden change in the evolution of investment around the beginning of 2018, at the time that the incentives (and only the incentives) discontinuously changed.

We can create a placebo exercise to alleviate residual concerns that some other shock created by or correlated with MiFID II might be confounding our baseline estimates. We do this by taking advantage of the fact that the post-2010 advisors were affected by all other additional MiFID II provisions but not by a change in incentives. We restrict the baseline panel dataset to include only the post-2010 advisors and estimate the following regression

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identical. To examine this question, we repeat the analysis in Figure 3B using only the last active quarter of each client/fund. We find in Figure A1 that the investment of a client/fund, as observed in their last active quarter, is still positively correlated with the pre-2018 advisor trailer fee. This finding indicates that the investments of the clients of pre-2010 advisors have not fully converged to those of the clients of post-2010 advisors.

on their clients:

$$Investment_{cjt} = \delta(LogTrailerFee_j \times Post2018_t) + \eta_{ct} + \mu_{cj} + \epsilon_{cjt}, \tag{4}$$

where $LogTrailerFee_j$ is the log of the trailer fee in fund j (i.e. there is no across-time or across-advisor variation in this sample of post-2010 advisors) and $Post2018_t$ takes value one for all months after January 2018. The specification contains client/fund and client/time indicators, but can not include fund/time indicators as they are colinear with the main independent variable.

The rationale of this placebo test is as follows. The coefficient δ captures whether the trailer fee in a fund is more or less predictive of clients' investments after 2018, relative to before. Imagine a scenario in which the aforementioned additional MiFID II provisions implied that clients' investments ceased to be directed towards the funds that benefit their advisors most. If that was the case, then we would expect that the trailer fee displays a weaker correlation with investments in the post-2018 period, even for the group of advisors with no change in incentives. That is, we would expect $\hat{\delta}$ to be negative.[33]

We find in Table 3 that $\hat{\delta}$ is positive, and generally not statistically significant. This finding suggests that the January 2018 official implementation of MiFID II did not affect portfolio choices for clients whose advisors did not experience a change in incentives. We interpret this evidence as supporting the baseline identification strategy.

Understanding the Mechanism: Effects on Investment Flows The finding in Figure 5 that the stock of investment adapts gradually to the change in incentives prompts the question as to the mechanism by which this adjustment takes place.

Our dataset includes information on the date and size of the buy/sell transactions by each client on each fund. We use this information to split transactions into three types, depending on whether they represent: (a) 'incoming investment' by the client into the overall portfolio of internal funds, (b) 'outgoing investment', that is, money leaving the portfolio, or (c) 'investment reallocation', that is reallocation of existing investment across funds but within the client's internal fund portfolio.[34] One way to think about the incoming investment

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[33]Remember that the incentives of the post-2010 advisors are not balanced, as the trailer fee is proportional to the management fee and therefore varies across funds.

[34]To be classified as incoming investment, a transaction must be a buy transaction and not follow any sell transaction in a different fund and in the previous two days. The purpose of this second restriction is to maximise the likelihood that the buy transaction represents 'new money', that is, investment into a fund that comes from outside the client's internal fund portfolio rather than from the sale of other funds. In the same spirit, we classify as outgoing investment any sell transaction that is not followed by a buy transaction

is as 'new money', whereas outgoing investment and investment reallocation are 'old money' that was already in the internal fund portfolio.

We aggregate these transactions at the client/fund/month level and use them as dependent variables in the baseline specification (2). Columns 1-3 Panel A Table 4 show that trailer fees affect client behaviour only when the client is bringing new money into their fund portfolio. On the other hand, clients selling funds to take the money out of the portfolio or to buy other funds are not significantly affected by their advisors' fees.[35] Lastly, we use as dependent variable in (2) the net inflow (i.e. the sum of all the transactions by a client on a fund during a month, regardless of the transaction type). In Column 4 Panel A Table 4 we find that an elasticity of around 12%. This 12% elasticity on the net inflow is on a monthly basis, and Figure 5 shows that it gradually accumulates to generate the 49% elasticity on the stock estimated in Column 1 Table 2.

The analysis in Panel A Table 4 controls for client/month indicators, and therefore for any increase or decrease of investment in the overall fund portfolio. In Panel B Table 4 we study whether a change in incentives affects not just the allocation of new money across funds, but also the arrival of new money into a client's overall fund portfolio. We aggregate the incoming investment, outgoing investment, investment reallocation and net inflow across all the funds and within a client/month. We then estimate the following DiD specification:

$$Investment_{ct} = \lambda(Pre2010Advisor_{a(c)} \times Post2018_t) + \beta_c + \psi_t + \epsilon_{ct}, \tag{5}$$

where λ captures whether the clients of advisors whose incentives changed in 2018 (i.e. the pre-2010 advisors) brought more money in, took more money out, etc from the internal fund portfolio.

Note that the variations that we exploit in (2) and (5) are orthogonal to each other. In (2) we control for client/month indicators and therefore study to what funds clients direct their investments *within a client/month*. The objective in (5) is to explain the client/month fixed effects, namely whether (aggregating across funds) clients bring money into their port-

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in a different fund and within two days in the future. We classify any other transaction as a reallocation transaction. Note that the definitions of incoming and outgoing investments do not take into account the amount transacted and are therefore quite restrictive. Imagine, for instance, that a client sells €1 in Fund A, buys €10,000 the next day in Fund B and does not undertake any additional transaction in that month. We classify both transactions as reallocation transactions, even though it is clear that the second one consists largely of money incoming into the internal fund portfolio.

[35]Clients selling one fund to buy another generate two separate non-zero transactions in the client/fund/time dataset, which could generate concerns about potential 'double counting' of a single action. The finding that the reallocation of existing investment is not affected by trailer fees suggests that this concern does not appear to be empirically salient.

folio in a particular month. In this respect, the purpose of (5) is to explain the controls in (2).

We display the estimates in Panel B Table 4. We find that the clients of post-2010 advisors brought more new money in (Column 1), did not take more old money out (Column 2) and as a result increased their total investment in the fund portfolio (Column 4). Therefore, the change in incentives prompted changes in investments along a dual mechanism. The clients of pre-2010 advisors brought more new money into the portfolio after 2018 and then, *controlling for how much new money they had brought in*, they disproportionally directed any incoming investment into the funds that had relatively higher fees after 2018.

Suggestive Evidence on the Mediating Roles of Trust and Knowledge   To conclude this section, we examine whether there are features of the client-advisor relationship which are mediating the effect of incentives on investments. We use the baseline specification (2) and interact the trailer fee with two types of characteristics: (a) the client and advisor characteristics displayed in Panels A and B Table 1, and (b) measures of social and geographic proximity between advisor and client.[36] The coefficients in Column 1 Table 5 are from separate regressions containing only the interaction with a single variable, while the coefficients in Column 2 are from a single regression containing all interactions together.

We find that the effects are substantially lower for clients who report understanding the general functioning of investment funds. A potential conclusion from this finding is that financial education could improve financial outcomes not only when clients make self-directed choices (Hastings et al., 2013) but also through reducing potential distortions when clients are advised.

Secondly, the baseline effects are larger for high tenure clients (i.e. clients who joined the firm and therefore their advisors before 2007.[37] An interpretation of pairs with a longer history is that they are associated with higher trust. Glaeser et al. (2000), for instance, show in an experimental setting that the length of the working relationship is a good predictor of the accumulated level of trust.

To study the mediating role of proximity, we interact the trailer fee with the geodesic distance between the addresses of advisor and client, and a dummy for whether they are

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[36]We expand on the motivation for the use of these variables in Section 6.

[37]Note that the coefficients in Table 5 are identified exclusively from existing clients, and therefore do not contradict the evidence in Section 6 that the effect is larger for new clients. In understanding the variation in the high tenure client interaction, remember that the log trailer fee is collinear with the client/fund fixed effects for the post-2010 advisors, while it varies within client/fund for the pre-2010 advisors. The variation in the estimation of the high tenure dummy interaction is therefore coming from comparing relatively recent clients of the pre-2010 advisors to relatively old clients of the pre-2010 advisors.

less than five years apart in age. The geographic coverage of our firm is disproportionately concentrated in the region of Catalonia. In this region, some individuals have Catalan first names (e.g. Jordi), and other individuals have Spanish first names (e.g. Jorge). Similarly, individuals sometimes have typically Catalan surnames (e.g. Vidal) and sometimes typically Spanish surnames (e.g. Garcia). We introduce an interaction with a dummy variable for whether advisors and clients either both have a Catalan first name or both have a Spanish first name. We do the same for typically Catalan and typically Spanish surnames.

Two of the four interactions with proxies for social and geographic proximity (i.e. the dummies for client and advisor having a first name in the same language, and living within 200 meters of each other) are statistically significant and large in magnitude. A natural interpretation of these variables is that they partially capture the strength of the relation between advisor and client, and therefore their mutual level of trust (Stolper and Walter, 2018). This interpretation would be consistent with the estimated interaction with the high tenure clients, as well as with the evidence in Section 6 below that proximate pairs are more likely to start an advice relationship.

We want to be careful in the interpretation of the Table 5 interactions. Even if the baseline effects from Table 2 are identified, the interactions here may not be, as they may instead be proxying for interactions with unobservables. In addition, the coefficients are sometimes imprecisely estimated. Despite using a dataset with more than three million observations, clustering at the advisor level implies that the standard errors are often quite large. Notwithstanding these caveats, we can at least interpret the evidence in Table 5 as being consistent with a mediating role for trust and knowledge in the effect of incentives on investments.

6   Selection and Treatment Effects on New Clients

In this section, we study how the 2018 change in incentive policy affected the types of clients joining the firm and the investments that advisors induced these new clients to undertake. Our analysis has three parts. First, we document that the formation of advisor/client relations in our firm appears to be strongly determined by their social proximity to each other. Secondly, we show that the 2018 change to incentives did not affect: (a) the observable characteristics of the clients joining the firm, (b) the role of proximity in predicting the formation of relations between advisor and client. We use these two findings to argue that within-advisor selection does not appear to be a major empirical factor in our setting. In the

last subsection, we estimate the treatment effects of trailer fees on the initial investments of new clients. Our main conclusion here is that the estimated effects are much larger than the equivalent effects on existing clients estimated in Section 5.

Understanding the Formation of Relations Between Advisors and Clients In this subsection, we present evidence that a prospective advisor and a prospective client are more likely to form a relation when they are socially closer to each other. We focus on the role of social distance to explain the formation of advice relations for two reasons. Firstly, our conversations with existing advisors suggest that they solicit new clients partly by tapping into their extended social networks and/or through referrals from their existing clients. Secondly, Gennaioli et al. (2015) and Stolper and Walter (2018) stress that trust is an important factor in financial advice, and that trust is higher between individuals who are more alike.[38]

Our analysis is conditional on the prospective advisor and the prospective client being both associated with our firm. We then ask empirically whether being closer to each other increases the likelihood that they are engaged in an advice relation with each other.[39] To examine this question, we create a dataset in which an observation is one of the potential combinations between the clients in our dataset ($c = 1 \ldots 6,133$) and the advisors in our dataset ($a = 1 \ldots 165$). We then drop all the clients living in the same address as an advisor and all the pairs which do not overlap in their time with the firm.[40] We then estimate regressions of the form:

$$Relation_{ac} = \zeta Distance_{ac} + \theta_a + \beta_c + v_{ac} \tag{6}$$

where $Relation_{ac} = 1$ if client c was actually advised by advisor a, θ_a are advisor fixed effects, β_c are client fixed effects, and $Distance_{ac}$ is a measure of the social distance between the two individuals.

Column 1 Table 6 displays the OLS estimates from (6), using a variety of proxies for social proximity. We first find that individuals that are less than five years apart in age

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[38]While we use the term 'social distance' in this subsection, we note that there is a variety of related terms in the economics and sociology literature. Stolper and Walter (2018) for instance prefer the term 'homophily', which is commonly used in the social networks' literature (Jackson, 2010).

[39]While informative, our analysis is subject to the caveat that we only observe individuals that are associated with the firm. Therefore, we do not capture the universe of counterfactual advisors of the clients in our sample, as some of these advisors may be working for other firms or banks.

[40]The first restriction eliminates clients who are likely to be close relatives of their advisors. The second restriction is meant to increase statistical power by maximising the chances that an advisor and a client empirically form a relation.

are .0026 more likely to form a relation. This is a large coefficient, as it represents around 31% of the unconditional likelihood in the sample (i.e. .0084). We also find that individuals matching along the linguistic dimensions of their names and surnames are more likely to form a relation. Specifically, the likelihoods are 20% and 18% higher (relative to the unconditional likelihood) for shared language first names and surnames, respectively.[41,42]

Lastly, we also include in the regression the log of the geodesic (or as-the-crow-flies) distance between the home addresses of advisor and client. We find that a 10% increase in the geodesic distance between addresses is associated with a 1.7% decrease in the likelihood that they form a relation. To gain intuition about the magnitude of this relation, we display in Figure 6 the estimates from a kernel regression between $Relation_{ac}$ and $Distance_{ac}$, restricted to distances below five kilometres. We find that the likelihood of two individuals forming a relation is twice as high when they live 200 metres away from each other, relative to living one kilometre away. This is a very large effect. We do not take a stand on whether geographic distance should be interpreted as a proxy for social distance or as significant factor in its own right. Regardless of the interpretation, Figure 6 is consistent with the broad message that advisors use very 'local' strategies to expand their portfolios of clients.

To conclude this subsection, we find empirical support for the notion that advisors are more successful at attracting clients who are more socially or geographically connected to them. This finding is perhaps unsurprising but it is nevertheless worth emphasising, as it might affect how much advisors can fine-tune their strategies of engagement with new clients. In particular, having to rely on existing social networks might constrain advisors' ability to significantly expand their portfolio of clients and/or attract new types of clients when their incentives change. We turn to this question next.

Within-Advisor Selection Effects   In this subsection, we examine whether within-advisor selection appears to be empirically relevant in our setting. We provide one direct test and one indirect test. In every test, we leverage the fact that the 2018 change in compensation affected pre-2010 advisors but not post-2010 advisors.

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[41]To account for the potentially confounding factor that these variables may be inversely correlated with the geographic distance between advisor and client, the regression controls for whether both agents live in Catalonia. As we explain below, notice that we also control for the geodesic distance between their home addresses.

[42]The selection of an advisor by a client is best modeled as a conditional logit problem, in which each advisor is one of the alternatives that clients choose among. Table A5 shows that the effects are quantitatively similar when estimating a conditional logit model. For instance, pairs with an age difference lower than five are 53% more likely to form a relation. Pairs with either both names in Catalan or both names in Spanish are 17% more likely to form a relation.

We test the relevance of within-advisor selection directly by studying whether the 2018 change in incentives caused advisors to engage with different types of clients. We take a dataset of all the clients active during our baseline period (i.e. 2015-2020), and examine whether client observable characteristics varied with the change in incentives (i.e. pre-2018 versus post-2018), separately for the pre-2010 and the post-2010 advisors. The regression is a standard DiD regression:

$$Charac_c = \theta_{a(c)} + \rho Post2018Client_c + \sigma(Pre2010Advisor_{a(c)} \times Post2018Client_c) + \xi_c \quad (7)$$

where $Charac_c$ is a client characteristic and $Pre2010Advisor_{a(c)} = 1$ if the advisor joined before 2010. The advisor fixed effects $\theta_{a(c)}$ control for any potential across-advisor selection, that is any propensity to match with clients of different types that differs across advisors but is time-invariant. The $Post2018Client_c$ dummy controls for any firm-wide time change in the characteristics of clients. σ therefore captures whether the characteristics of clients changed for the advisors who experienced a change in incentives, relative to the advisors whose incentives did not change in 2018.[43]

We display estimates from (7) in Panel B Table 7. We find that all the coefficients are statistically indistinguishable from zero. While this test does not unequivocally rule out the possibility of within-advisor selection, it finds no evidence of it in terms of client observable characteristics.[44]

In addition to testing directly whether client characteristics varied with the incentive change, we can also examine indirectly whether there is any evidence that advisors' search strategies were affected by the 2018 incentive change. The logic of this test is that a potential reaction of advisors looking for different types of clients could have been to rely less on familiar channels which leverage social and geographic proximity, and more on active strategies such as advertising and cold-calling. If so, the predictive power of proximity in the engagement of new clients should have decreased disproportionately for the pre-2010

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[43]Unfortunately, our dataset does not contain information such as wealth, number of children, or (perhaps self-reported) risk preferences. Ideally, we would use variables such as these to capture more comprehensively the underlying investing preferences of clients. Nevertheless, we believe that the characteristics that we use do partially capture client preferences. For instance, we show in Table A3 that these characteristics significantly predict the investing decisions of clients in the pre-2018 period.

[44]We can also use the 2015-2017 period and link cross-sectionally client characteristics to the cohort of their advisor (i.e. pre-2010 versus post-2010). We find in Panel A Table 7 that clients of pre-2010 advisors are 7% older on average. This is unsurprising, given that they are served by advisors joining the firm earlier. We also find that the clients of pre-2010 advisors are more likely to report that they understand how investment funds work. We do not find differences in terms of the other characteristics. Note that any cross-sectional differences across advisors are controlled for in any empirical model including advisor/fund fixed effects, such as equation (8) below.

advisors. We study whether there is any evidence of this adjustment by estimating a varia-
tion of (6) in which we allow the effect of $Distance_{ac}$ to vary across advisor pre/post-2010
status, client pre/post-2018 status, and their interaction. The coefficient associated with the
interaction captures whether advisors relied less on proximity to engage with new clients
when their incentives changed. In Column 2 Table 6, we find that the coefficients associated
with the interaction are all insignificant. Therefore, we conclude that there is no evidence
that proximity played a less important role for advisors when their incentives changed.[45]

To summarise this subsection, we find no evidence that the advisors whose incentives
changed in 2018 undertook large changes in their strategies of engagement with new clients,
and therefore with the types of clients that they attracted to the firm. The conclusion that
within-advisor selection does not appear to be empirically relevant in our setting is important
in light of the findings in the next subsection, where we show that the 2018 changes in trailer
fees affected the initial investments of new clients.

Treatment Effects on New Clients In this subsection, we estimate the treatment effects
of incentives on the initial investments of new clients. To do this, we restrict the baseline
sample to include only clients joining the firm between 2015 and 2020. For these new clients,
our focus is on the investments in their first quarter with the firm.[46]

The estimating equation is

$$Investment_{cj} = \phi LogTrailerFee_{a(c)jq(c)} + \beta_c + \kappa_{jq(c)} + \iota_{a(c)j} + \omega_{cj}, \tag{8}$$

where $LogTrailerFee_{a(c)jq(c)}$ is the trailer fee that the advisor a of client c received from
fund j in the first quarter q after the client joined the firm. As dependent variables, we
use the stock investment variables from Columns 1-3 Table 2, although the exclusive focus
on clients' first quarter implies that these variables can also be regarded as measures of net
inflows.

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[45]Lastly, we can provide an additional indirect test by examining whether the characteristics of the overall
portfolio of clients are correlated with the advisor type, differentially before and after 2018. The dataset here
is a panel of advisors and months. In Panel A Table A4, we find cross-sectional differences across advisor
types. Specifically, using information for the 2015-2017 period we find that pre-2010 advisors have more
clients and a higher number of client exits per month. Only the number of client exits changes differentially
after 2018. The most important conclusion from Table A4 is that the coefficient for the number of client
entries is statistically significant throughout. This indicates that advisors whose incentives changed in 2018
did not engage on average with more new clients. It therefore provides additional evidence that advisors did
not vary their engagement strategies with new clients following the change in incentives.

[46]Using only the first month subjects the analysis to potentially substantial measurement error, because
some clients may start investing on the last day of the month and other clients on the first day of the month.
In Table A5 we show however that the estimates are qualitatively similar when using only the first month.

Note that the empirical model includes every set of fixed effects that can feasibly be included given the structure of the dataset. The client fixed effects control for the total amount of initial investment. The fund/quarter fixed effects control for any firm-wide shock that might have changed the attractiveness of certain funds at particular moments in time (e.g. a general move towards low-fee funds). Most importantly, the inclusion of advisor/fund fixed effects controls for across-advisor selection. It implies that the variation arises from comparing the new clients' initial choices during periods in which the same advisor/fund combinations are associated with different trailer fees.

Panel A Table 8 shows that the new clients of an advisor make initial choices strongly geared towards the funds in which that advisor receives a higher compensation *at that point in time*. Consistently with our conclusion in the previous subsection that there was no evidence of within-advisor selection, we interpret these coefficients as largely reflecting treatment effects. However, in Panel B Table 8 we confirm this interpretation by controlling for selection on observables explicitly in our empirical model. Specifically, we include interactions between client characteristics and fund fixed effects in (8), to account for the fact that certain types of clients may have an inherent tendency to invest in certain funds. In Panel B Table 8 we find that the coefficients are virtually identical after controlling for selection on observables. This finding reinforces our belief that equation (8) is mostly capturing treatment effects.

An important finding from Table 8 is that the estimated effects are substantially larger in magnitude than the equivalent effects for existing clients in Table 2. For instance, the estimated elasticity from Column 1 Table 8 is 150%, three times larger than the corresponding elasticity in Column 1 Table 2. The effects on the positive investment likelihood and the share of total investment are approximately twice as large. These larger effects are consistent with the finding in Panel A Table 4 that advisors affect investments mostly when the client is expanding its internal fund portfolio. This is, by definition, the case of new clients, so it is reassuring that the effects are larger for them.

The larger effects for new clients suggest that changes in incentives potentially motivated by new regulations will vary in their aggregate effects depending on whether advisor/client relations are very stable over time. Specifically, settings in which relations are stable and clients rarely bring new money into their portfolios (after the initial allocation) will be associated with weak effects of policy. In settings where clients often add new money to their original investments and there is a high turnover of clients across advisors, the effects are likely to be much stronger.

7 Measuring Client Utility Loss

In previous sections, we have found that advisors' incentives affect their clients' choices of individual funds. If the returns of different funds were perfectly correlated with each other, clients would experience no decrease in welfare. More generally, the covariance structure across products will affect the utility loss suffered. In this section, we propose a framework that quantifies this loss while explicitly taking into account this covariance structure. An important objective of this framework is to incorporate the notion that utility losses may result from clients investing in funds that are not necessarily worse on average (relative to other available funds), but happen to not match their risk preferences.

Our starting point is a simple portfolio-choice model in which investors have mean-variance preferences over portfolio returns, and in which their expectations about these returns are influenced by the incentives of their advisors. Specifically, we assume that advisors influence their clients' portfolio choice by strategically communicating the expected returns of the various internal funds. Based on this information clients then optimise over their mean-variance preferences. We estimate the parameters of the model and use these estimates to compute average client utility loss, both prior to and following the 2018 change in compensation policy.

Clients' Preferences   We assume that clients' preferences, as defined over their portfolio returns, can be written as:

$$E_c[U] = w_c^{'}\left(E_c[R] - R_f\right) + R_f - \frac{\gamma}{2}w_c^{'}\Sigma w_c, \tag{9}$$

where R is a vector of asset returns, $E_c[R]$ is a vector of client c's subjective expected asset returns, R_f is the return of the risk-free asset, Σ is the variance-covariance matrix of asset returns, and w_c is the vector of portfolio weights. The client's optimal portfolio is then given by:

$$w_c^* = (\gamma\Sigma)^{-1}(E_c[R] - R_f). \tag{10}$$

We model expected utility such that clients are heterogeneous in their beliefs (i.e. $E_c[R]$) but homogeneous in their preferences, as reflected in the common risk aversion coefficient γ.[47] Note that we focus on the allocation of investments *across funds* (i.e. we assume that

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[47]This is without loss of generality because it is well-known that, in a mean-variance framework, heterogeneity in preferences and heterogeneity in beliefs are equivalent from a modelling perspective. In our setting, we can reinterpret the numerator in equation (10) as the subjective risk premium per unit of individual risk aversion, and this allows us to set γ to a constant for all clients.

advisors do not affect the overall level of investment, consistently with the main DiDiD empirical strategy in (2)).

Advisors' Preferences   Advisors are risk neutral on the income they generate from clients. They wish to maximise their income, but also partially internalise client welfare.[48] Therefore, advisor a's preferences are $U_a = \phi \mathrm{E}_c[U] + (1 - \phi)w_c' TF_{a(c)}$, where $\phi \in (0,1)$ captures the concern for client welfare. For ease of interpretation, we interpret the vector of trailer fees $TF_{a(c)}$ as being demeaned at the advisor level to reflect the fact that advisors' incentives are determined by the *relative* trailer fees they receive from the funds. U_a can be rewritten as:

$$U_a = \phi(w_c' (\mathrm{E}_c[R] - R_f) + R_f - \frac{\gamma}{2}w_c'\Sigma w_c) + (1 - \phi)w_c' TF_{a(c)}, \tag{11}$$

The advisor's desired investment by their client can be written as:

$$w_a^* = (\gamma\Sigma)^{-1} \left(\mathrm{E}_c[R] - R_f + \alpha TF_{a(c)}\right) = w_c^* + \frac{\alpha}{\gamma}\Sigma^{-1}TF_{a(c)}. \tag{12}$$

where $\alpha = \frac{1-\phi}{\phi}$ is the *bias* in the preferences of the advisor, relative to those of the client.

Information Transmission   (12) shows that the (demeaned) trailer fees of advisors linearly affect their *preferred* clients' investments. Given this linear bias in the preferred decision, we can motivate a linear relation between the incentives of advisors and the *actual* clients' investments with a simple strategic information transmission game à la Kartik et al. (2007).

Assume that the advisor sends a set of messages about the expected return of each fund and, on the basis of these messages, the (potentially naïve) client forms (potentially distorted) beliefs $\hat{\mathrm{E}}_c[R]$. Kartik et al. (2007) show that, in equilibrium, the advisor can induce client beliefs equal to $\hat{\mathrm{E}}_c[R] = \mathrm{E}_c[R] + \alpha TF_{a(c)}$, where α (i.e. the 'bias') can be reinterpreted as the sensitivity of the client beliefs to the advisor's incentives.[49] In turn,

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[48]Partially internalising client welfare can be interpreted as the reduced form of a model in which the advisor can acquire a good reputation and takes into account the stream of trailer fees over an infinite horizon (Mailath and Samuelson, 2001).

[49]An intuitive feature of this expression is that advisor-induced distortions in expected beliefs about a fund are only relative to other funds, and wash out in aggregate (remember that $TF_{a(c)}$ is a vector of mean zero).

these beliefs can induce the client to choose their actual investment $\hat{w}_c^*$ as follows:

$$\hat{w}_c^* = arg\text{Max}_{\hat{w}_c}\hat{E}_c[U] = arg\text{Max}_{\hat{w}_c}\hat{w}_c'(\hat{E}_c[R] - R_f) + R_f - \frac{\gamma}{2}\hat{w}_c'\Sigma\hat{w}_c$$
$$\Rightarrow \hat{w}_c^* = (\gamma\Sigma)^{-1}(\hat{E}_c[R] - R_f) \tag{13}$$

where $\hat{E}_c[U]$ are the client's expected utility given distorted beliefs. Note that (13) makes the actual investment chosen by the client (i.e. $\hat{w}_c^*$) equal to the preferred investment by the advisor (i.e. w_a^*).

Estimating the Parameters of the Model   We use the framework above to conceptualise and quantify client utility loss. To do this, we need to estimate the unknown parameters of the model, starting with the (common) risk aversion coefficient γ. We can infer clients' risk aversion from the properties of their portfolios under the distorted beliefs. Multiplying both sides of (13) by $\gamma\hat{w}_c^{*'}\Sigma$ we have:

$$\gamma\sigma_c^{*2} = \hat{E}_c[R]^* - R_f, \tag{14}$$

where $\hat{E}_c[R]^*$ and σ_c^{*2} are the (distorted) expected return and variance of the client's optimal portfolio. In reality, optimal portfolio weights are subject to a set of unobservable, individual-specific constraints and frictions. To mitigate the effect of such idiosyncratic noise, we aggregate all individual clients into a representative client (on an investment-value-weighted basis), and rewrite equation (14) for this representative investor as:

$$\gamma = (\hat{E}[R_m]^* - R_f)\sigma_m^{*-2}, \tag{15}$$

where $\hat{E}[R_m]^*$ and σ_m^{*2} are the (distorted) expected return and variance of the aggregate 'market' portfolio.[50] We further assume that, in the absence of advisor influence, clients hold a common belief about the expected market return (which, for example, can be inferred from historical returns). We then estimate γ, together with the bias parameter α, using a recursive fixed-point method which we describe in more detail below. In Table 9, we use (15) to compute the value of γ given the observed moments of the aggregate portfolio and a set of reasonable assumptions on the risk-free rate R_f.

The second parameter we need to estimate is the effect of advisor incentives on client

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[50]Note that advisor influence washes out if we take an equal-weighted average across all internal funds. This relation, however, does not hold exactly when we take an investment-value-weighted average across internal funds, as the investment value in each fund is partially determined by advisor influence.

beliefs, α. We exploit the fact that the compensation policy in January 2018 changed advisors' trailer fees and, through them, the distortion in the beliefs of their clients. We rewrite (13) for both the pre and post January 2018 periods.

$$
\begin{aligned}
\hat{w}_c^{*pre} &= (\gamma\Sigma)^{-1}(\hat{E}_c^{pre}[R] - R_f) \\
\hat{w}_c^{*post} &= (\gamma\Sigma)^{-1}(\hat{E}_c^{post}[R] - R_f).
\end{aligned}
$$

Taking the difference and assuming that $E_c[R]$ does not vary over time, we have:

$$
\Delta\hat{w}_c^* = \hat{w}_c^{*post} - \hat{w}_c^{*pre} = (\gamma\Sigma)^{-1}\alpha\Delta TF_{a(c)}. \tag{16}
$$

where $\Delta TF_{a(c)}$ is a vector of changes in the (demeaned) trailer fees. Given γ and Σ, we can then estimate α by running a simple regression of changes in portfolio weights on changes in relative trailer fees.

Lastly, quantifying clients' utility loss requires calculating their subjective expected returns in the absence of the advisor-induced distortion in beliefs. Note that these can be easily computed by rewriting equation (13) as:

$$
E_c[R] = \gamma\Sigma\hat{w}_c^* - \alpha TF_{a(c)} + R_f.
$$

After calculating $E_c[R]$ for every client we can compute the optimal portfolio weights w_c^*, in the absence of distorted beliefs, as:

$$
w_c^* = (\gamma\Sigma)^{-1}(E_c[R] - R_f).
$$

Quantifying Utility Loss We use equation (9) to compute the difference between the realised client utility (which depends on $\hat{w}_c^*$) and the utility that clients would have obtained in the absence of distortions (which depends on w_c^*).

$$
Loss_c = (w_c^* - \hat{w}_c^*)'(E_c[R] - R_f) - \frac{\gamma}{2}(w_c^{*'}\Sigma w_c^* - \hat{w}_c^{*'}\Sigma\hat{w}_c^*).
$$

Note that $\hat{w}_c^*$ is observed, and w_c^* and $Loss_c$ can be computed, separately for the periods before and after January 2018. We can then calculate $\Delta U_c = Loss_c^{pre} - Loss_c^{post}$ to quantify the improvement in client utility resulting from the 2018 change in compensation policy.

Data and Results   The previous subsection has outlined a methodological framework to infer client utility and its change around January 2018. In this subsection we apply this framework to our data and discuss the resulting empirical findings.

We have seen in Sections 5 and 6 that the January 2018 change in incentives affected new clients much more than existing clients. Because of this, we estimate α separately for new clients and existing clients. We define pre-2018 new clients as those joining the firm in the twelve months before July 2017. We define post-2018 new clients as those joining the firm in the twelve months after July 2018. For each set of clients, we use their average holdings in the six months after joining to calculate their portfolio weights.

We define existing clients are those joining the firm at any point before July 2017 and remaining active in the post-2018 period. Remember that the main finding from Figure 5 is that existing clients adjust their portfolios gradually in response to changes in advisor incentives. Because of this, we eliminate from our estimation the period during which clients' investments were converging to their new steady state, and treat the average holdings in the twelve months after July 2018 as the portfolios for the post-2018 period. For symmetry, we also use the average holdings in the twelve months before July 2017 as the portfolios for the pre-2018 period.

To compute the risk-aversion coefficient γ, we assume that the representative investor uses historical returns to form their opinions about the market expected return and variance in the absence of advisor influence. Note that $\hat{\text{E}}[R_m]^*$ in equation (15) is the expected market return with distortion, which depends on the value of α. We therefore take a recursive approach to estimating γ and α. Specifically, we start with an α of zero to estimate the value of γ using equation (15). We then plug in the estimate of γ to equation (16) to derive a new estimate for α. We keep iterating this two-step procedure until arriving at a fixed-point solution for both γ and α.

Finally, while the optimal portfolio choice in (13) is an interior solution, in practice we observe that most clients invest in a small number of internal funds.[51] To approximate the empirical framework to the assumptions of the model, we aggregate clients with the same advisor to a client group (by summing up their investment in each internal fund), and estimate the expected utility loss at the advisor level. We further aggregate investments in all external products into one investment, so in total we have 15 fund products (14 internal plus 1 external).

Table 9 displays the results, with risk-free rates ranging from 0 to 5%. In Panel A,

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[51]This can be consistent with investors facing a fixed cost to invest in an additional mutual fund.

we consider all clients in the sample. We report utility losses separately for new clients and existing clients. We draw several conclusions from Panel A Table 9. Firstly, our estimates for the risk aversion coefficient (i.e. between 6 and 7) are largely in line with estimates from prior work.[52] Secondly, we find substantial utility losses (close to 9% for new clients and close to 6% for existing clients) prior to the enactment of MiFID II. These are large magnitudes when compared with the average annual management fee, which is 1.5%. Lastly and perhaps most importantly, the January 2018 compensation policy decreased losses by around 50% for new clients and 20% for existing clients. Specifically, assuming the same set of risk-free rates, we find post-2018 utility losses of around 5% for both new and existing clients.

In Panel B, we repeat our quantification exercise for the subset of clients that report understanding 'all or most of the terms and functioning associated with investment funds' in the suitability surveys provided by the firm. Consistently with our reduced form estimates from Table 5, these 'financially sophisticated' clients are much less affected by their advisors' incentives. Specifically, we find utility losses of around 3.4% (new clients) and 1.3% (existing clients).

Overall, our analysis identifies substantial utility losses associated with the misallocation of investments caused by the misalignment of incentives.

8   Concluding Remarks

We have provided evidence on the effects of advisors' incentives on the investments of the clients. The effects are economically sizeable and lead to substantial client utility losses. However, our study identifies grounds for optimism for firms and regulators aiming to decrease these losses. For instance, financial knowledge makes clients less willing to adjust their investments to suit their advisors' interests. More importantly, we have seen that a more balanced incentive structure (such as the one introduced by our firm) can have immediate (if limited) effects on existing clients and very sizeable effects on new clients. We hope that future research will continue to uncover avenues for a decrease in incentive misalignment.

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[52]See, for example, Friend and Blume (1975), Kydland and Prescott (1982), and more recently Calvet, Campbell, Gomes, and Sodini (2021).

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FIGURES

FIGURE 1: ILLUSTRATION OF VARIATION IN SHARES AND CORRESPONDING VARIATION IN TRAILER FEES
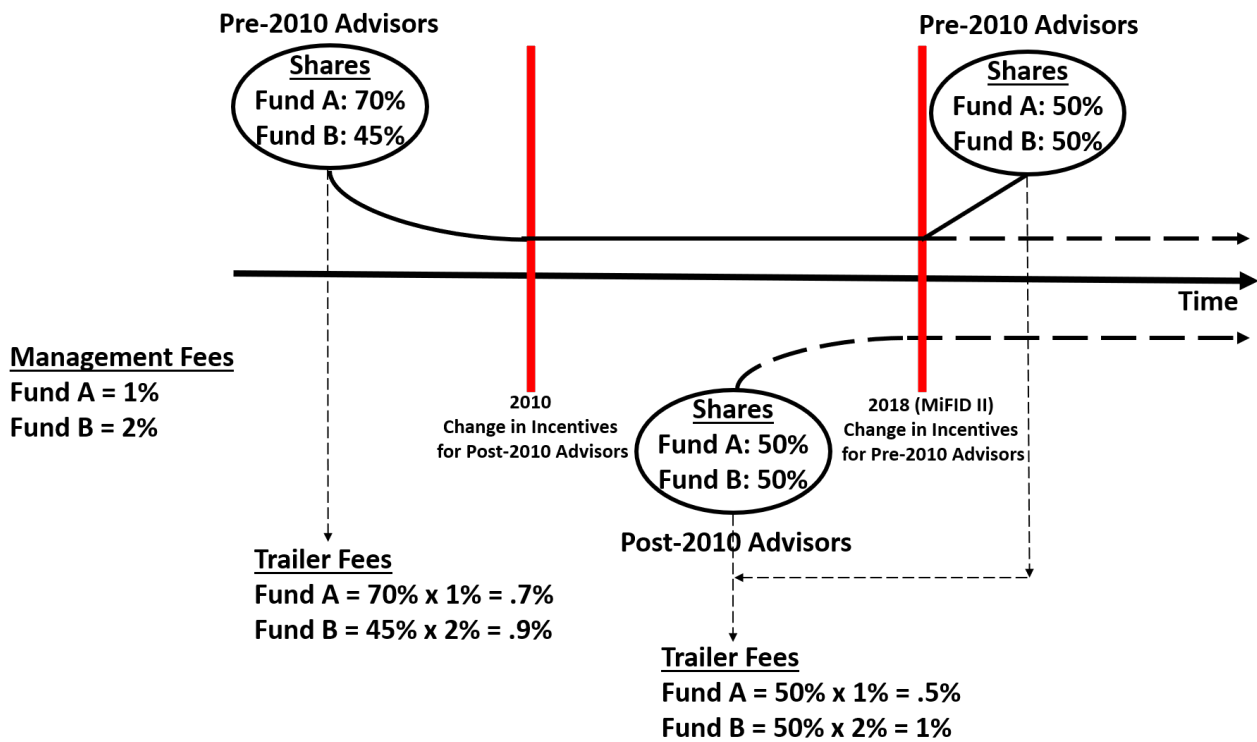
This Figure illustrates the sources of variation in the contractual arrangements of advisors. Advisors joining before 2010 were offered a contract with different shares across funds. As an example, the figure displays shares of 70% and 45% for Funds A and B, respectively. Assuming respective management fees of 1% and 2%, this translates into trailer fees of .7% and .9%. Advisors joining after 2010 were offered a contract with the same share across funds, such as the displayed 50%. This translates into trailer fees of .5% and 1% for Funds A and B. In 2018, the pre-2010 advisors were given the same contract as the post-2010 advisors.

FIGURE 2: VARIATION IN INCENTIVES

Across-Fund Distribution of Trailer Fees for Typical Advisors
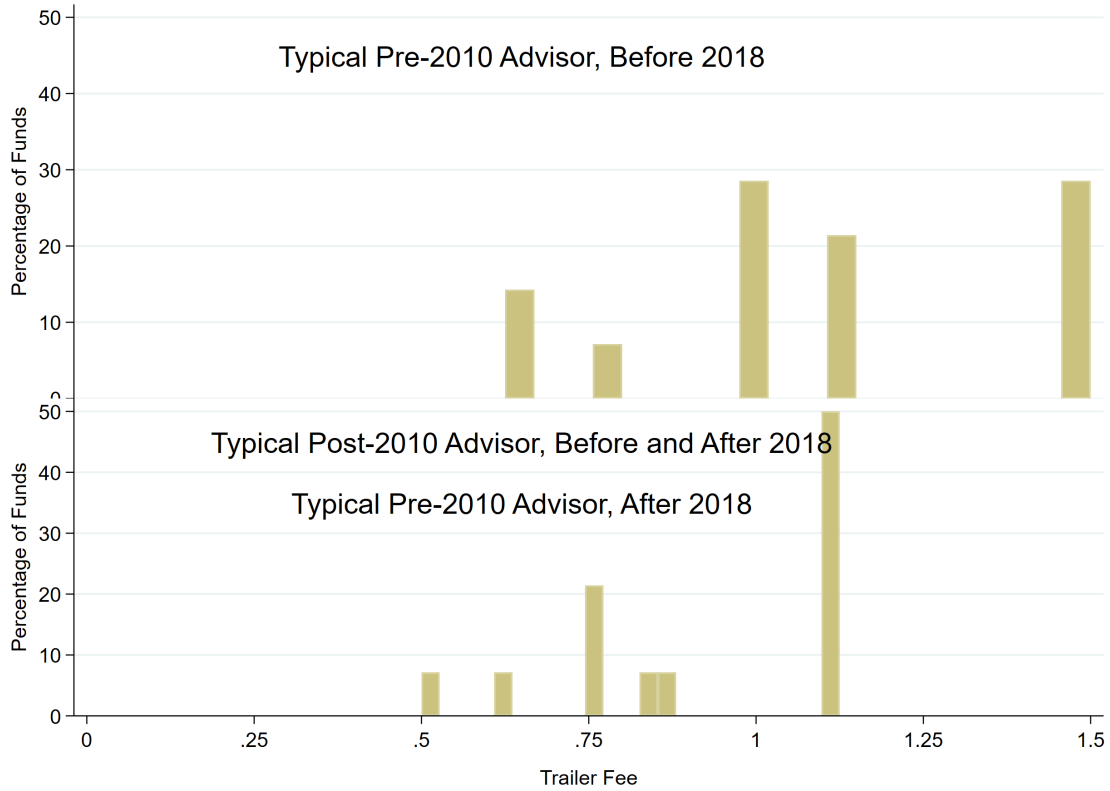
This Figure displays the distribution of trailer fees paid from different funds to different advisor types. The top panel displays the distribution for a typical advisor joining the firm before 2010. The bottom panel displays the distribution for a typical advisor joining the firm after 2010. In 2018 the trailer fees of the pre-2010 advisors were changed to match the trailer fees of the post-2010 advisors, so the bottom panel also reflects the post-2018 trailer fees of the pre-2010 advisors.

FIGURE 3: DESCRIPTIVE EVIDENCE
Client Investments and Pre-2018 Advisor Trailer Fees
Only Clients Active in 2015-2017
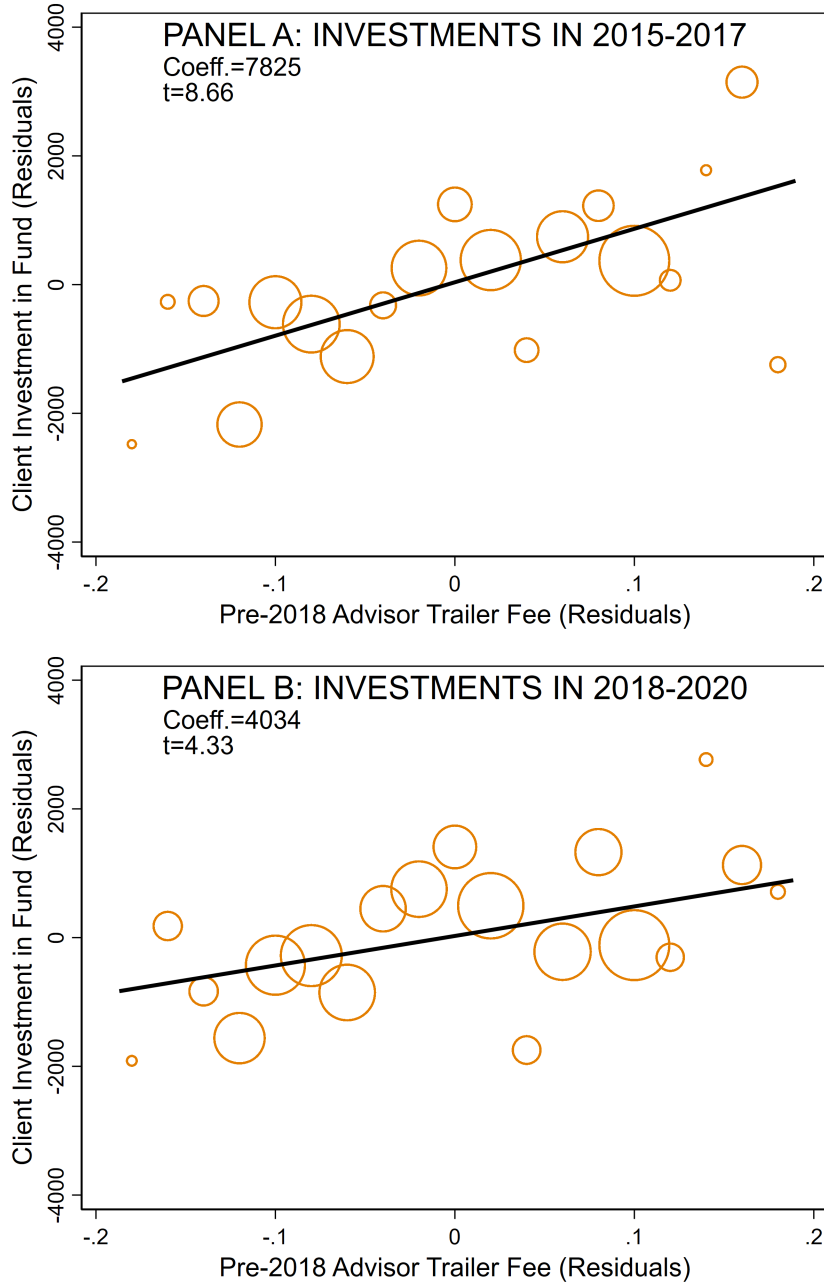
This Figure displays correlations between clients' investments and the pre-2018 trailer fees of their advisors. Both panels include only the clients active in the 2015-2017 period. In Panel A the dependent variable is the investments of these clients in the 2015-2017 period. In Panel B the dependent variable is the investments of these clients in the 2018-2020 period. In both cases the investments are averaged over all the months in which the clients were active in the period of the respective panel. The independent variable is the trailer fee received by the client's advisor in that fund prior to 2018. Both variables are net of client fixed effects and fund fixed effects. The plotted line is the OLS regression line. To display the data, we average the investment across all observations within trailer fee cells of .02 size. The area of the circles is proportional to the number of observation within each cell.

FIGURE 4: DESCRIPTIVE EVIDENCE
Client Investments and Pre-2018 Advisor Trailer Fees
New Clients of Advisors Active in 2015-2017
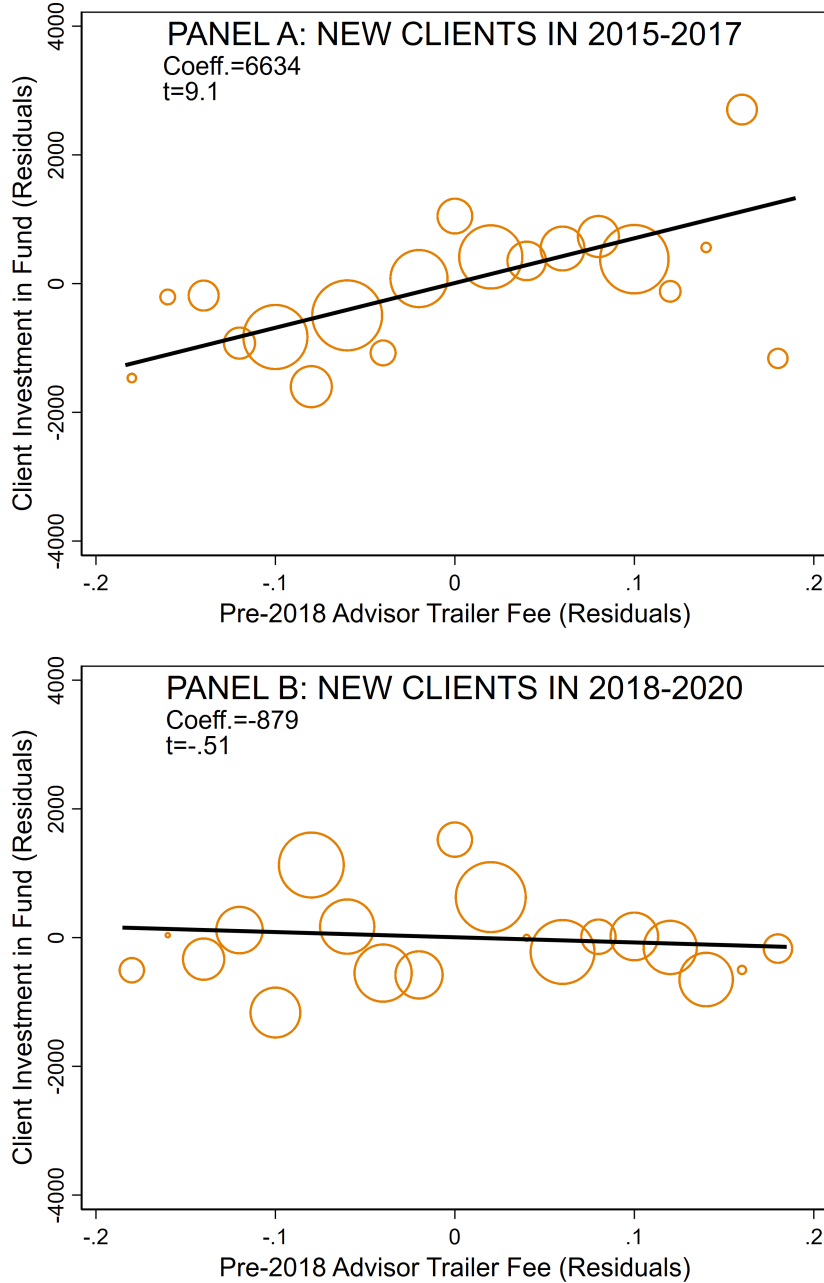
This Figure displays correlations between the investments of new clients in their first month with the firm and the pre-2018 trailer fees of their advisors. Both panels include only advisors who were active in the 2015-2017 period. Panel A includes clients joining the firm at some point in the 2015-2017 period. Panel B includes clients joining the firm at some point in the 2018-2020 period. The dependent variable is the investment of a client in a fund. The independent variable is the trailer fee received by the client's advisor in that fund prior to 2018. Both variables are net of client fixed effects and fund fixed effects. The plotted line is the OLS regression line. To display the data, we average the investment across all observations within trailer fee cells of .02 size. The area of the circles is proportional to the number of observation within each cell.

FIGURE 5: TRIPLE DIFFERENCES EVIDENCE
Dynamic Relation Between Client Investments and Advisors' 2018 Shock to Trailer Fees
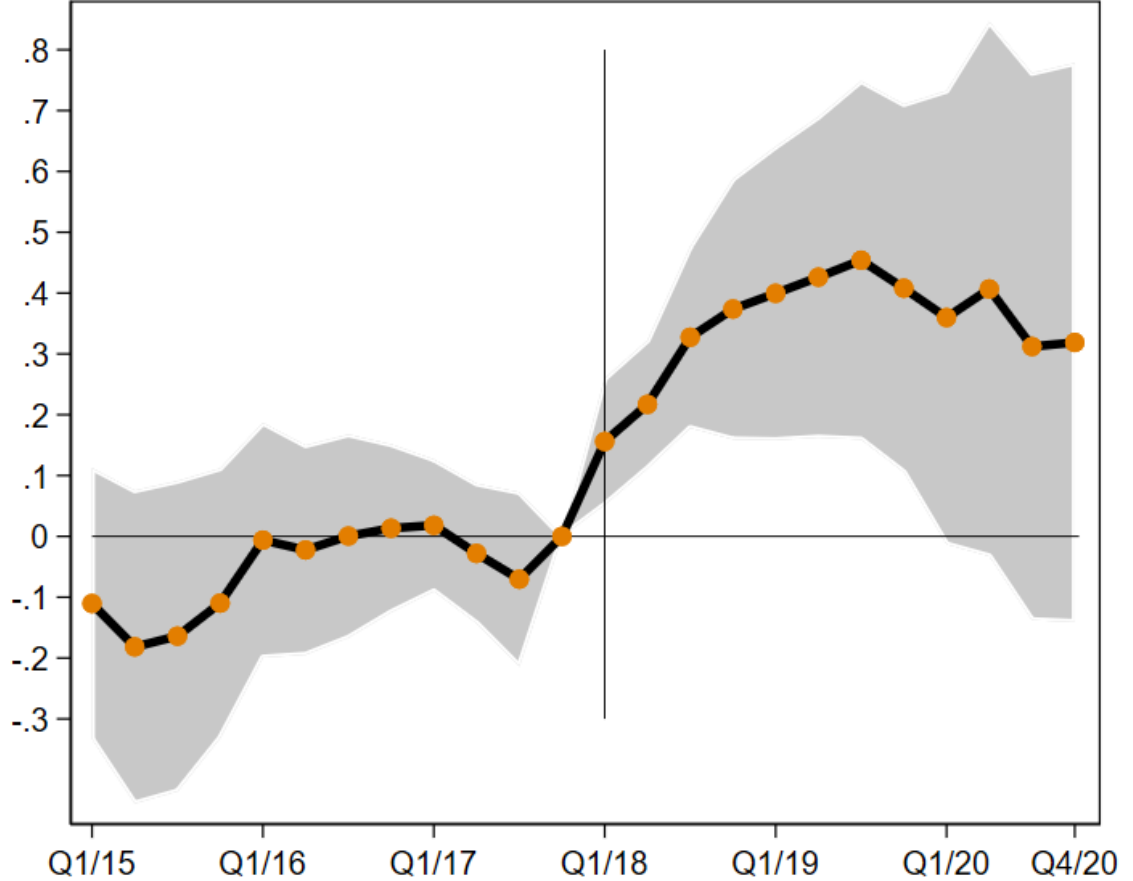
This figure displays the 24 coefficients π_t from estimating:

$$Investment_{cfq} = \sum_{q=1\ldots11}^{13\ldots24} \pi_q\Big(SHOCK_{a(c)f} \times Quarter_q\Big) + \eta_{cq} + \kappa_{fq} + \mu_{cf} + \epsilon_{cfq}$$

where $SHOCK_{a(c)f} = Post18TrailerFee_f - Pre18TrailerFee_{a(c)f}$.

The unit of observation is a client/fund/quarter combination. The number of observations is 1,239,966. The number of clients is 6,133. The number of advisors is 165. The number of quarters is 24 (from Q1 2015 to Q4 2020). The variable for Q4 2017 is the omitted variable in the regression. The post-2018 trailer fee is computed as the fund's management fee (which is fixed both over time and across clients) multiplied by the share of the management fee that the advisor received after January 2018 (which is fixed across all advisors and funds). The pre-2018 trailer fee is computed as the fund's management fee multiplied by the share of the management fee that the advisor received prior to January 2018 (which varies both across advisors and across funds). Investment is the inverse hyperbolic sine transformation of the client's average investment in the quarter. The regression controls for client/quarter, quarter/fund and client/fund indicators. Standard errors are clustered at the advisor level. 90% confidence intervals are displayed in the shaded grey area.

FIGURE 6:
Probability of a Relation between Advisor and Client as a Function of the Geodesic Distance between their Home Addresses
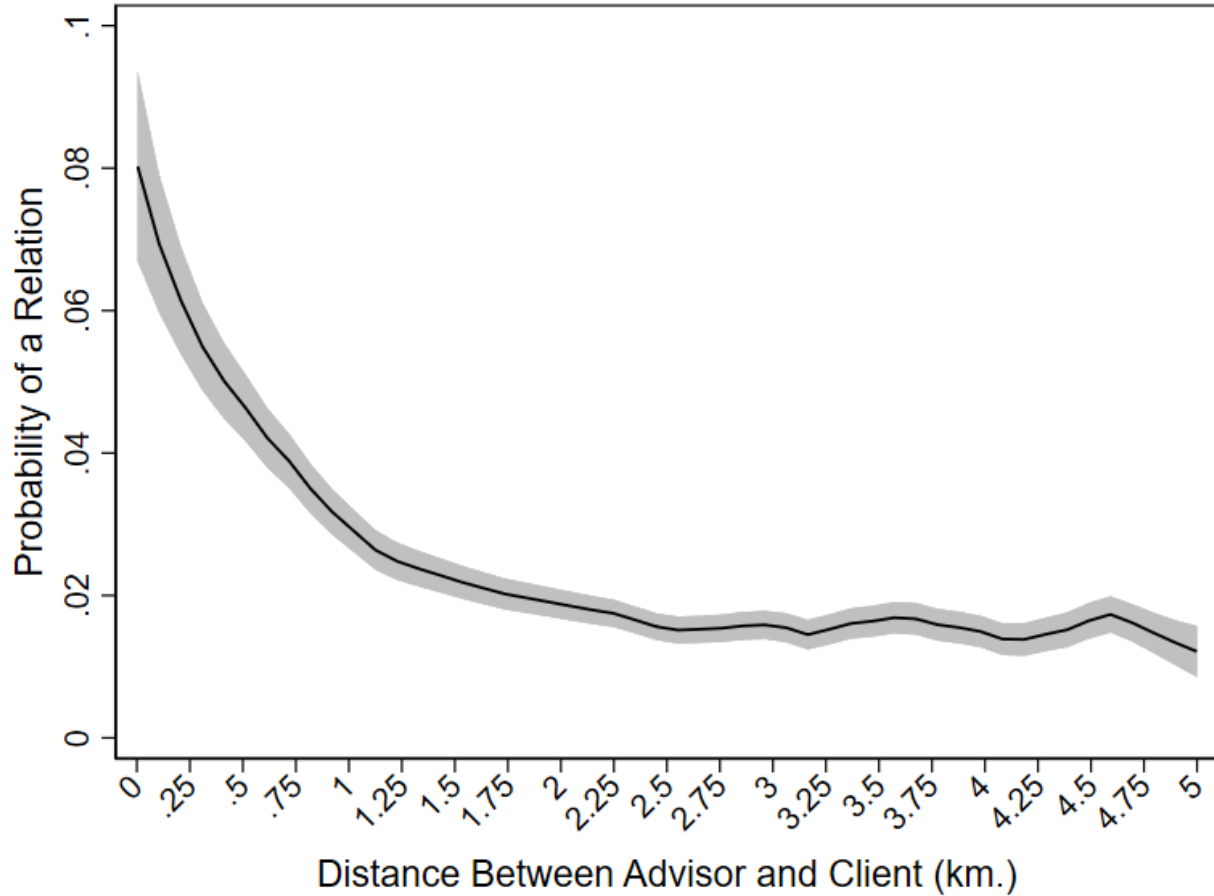
This figure displays estimates from a kernel regression between the likelihood that an advisor and a client in our dataset are in a relation, and the geodesic distance between their home addresses. The estimating equation is:

$$Match_{ac} = \alpha + \beta Distance_{ac} + \epsilon_{ac}$$

where $Match_{ac} = 1$ when a advises c, and $Distance_{ac}$ is the geodesic (or as-the-crow-flies) distance between their home addresses. An observation in the dataset is a combination between each of the advisors in the sample and each of the clients in the sample. The figure has been truncated to display only pairs with a distance below 5km. The regression also excludes pairs in which advisor and client live in the same address. The regression uses an Epanechnikov kernel with optimal bandwidth. 90% confidence intervals are displayed in the shaded grey area.

TABLES

TABLE 1 - SUMMARY STATISTICS

Panel A: Advisors (N=165)	Mean	SD	p10	p25	p50	p75	p90	Proportion Non-Missing
Year of Contract	2007	8	1993	2000	2010	2013	2015	1
Post-2010 Dummy	.54	.5	0	0	1	1	1	1
Certified Dummy	.29	.46	0	0	0	1	1	1
Number of Clients	41	57	3	7	17	54	116	1

Panel B: Clients (N= 6,133)	Mean	SD	p10	p25	p50	p75	p90	Proportion Non-Missing
Male	.66	.47	0	0	1	1	1	1
Year of Birth	1963	17	1942	1950	1963	1974	1986	.9
Financial Education Dummy	.17	.37	0	0	0	0	1	.48
Financial Profession Dummy	.05	.21	0	0	0	0	0	.48
Financial Knowledge Dummy	.32	.47	0	0	0	1	1	.45
High Income Dummy	.1	.31	0	0	0	0	1	.66
Year of Joining Firm	2007	30	1996	2000	2010	2014	2017	1

Panel C: Client/Fund/Month (N= 3,637,970)	Mean	SD	p10	p25	p50	p75	p90	Proportion Non-Missing
Positive Investment Dummy	.19	.39	0	0	0	0	1	1
Investment	3,978	24,719	0	0	0	0	7,472	1
Share of Total Investment	.06	.19	0	0	0	0	.21	1
Total Client Investment	65,548	200,952	2,086	6,918	21,504	59,575	144,311	1
Net Investment Inflow	5	4,883	0	0	0	0	0	1
Trailer Fee	.97	.26	.63	.75	1	1.13	1.13	1

This Table displays summary statistics for the advisors (Panel A), clients (Panel B), and observations in the baseline client/fund/month dataset (Panel C). The last column displays the proportion of observations for which the variable is non-missing. Year of Contract is the year in which the advisor joined the firm. Post-2010 Dummy takes value one if the advisor joined the firm in 2010 or later. Certified Dummy takes value one if the advisor has acquired before December 2020 at least one of the approved financial advisor qualifications provided by the CISI (Chartered Institute for Securities and Investment) and EFPA (European Financial Planning Association). Number of clients is the total number of clients of the advisor over the 2015-2020 period. The Financial Education Dummy is constructed on the basis of the questionnaire that clients have to fill as part of MiFID II. There are four possible answers: (a) 'No university education', (b) 'University education that is not related to maths or economics', (c) 'University education related to maths or economics', (d) 'Education that is specific to financial markets and investment funds'. The variable takes value one if the client answered (c) or (d). The Financial Profession Dummy captures whether the client 'works or has worked in a profession related to the financial markets', a question that clients have to answer as part of MiFID II. There are four possible answers: (a) 'I have never worked in a profession related to the financial markets', (b) 'I have a job that, occassionally, is related to the financial markets', (c) 'I have had a job that is related to the financial markets', (d) 'I have a job that is related to the financial markets'. The variable takes value one if the client answered (c) or (d). The Financial Knowledge Dummy is constructed on the basis of the questionnaire that clients have to fill as part of MiFID II. One of the questions investigates whether the client is familiar with the 'nature, characteristics, and risks associated with investment funds'. The question specifically asks about the 'degree of knowledge regarding the risks of the solicited products'. There are four possible answers: (a) 'I do not understand any of the terms', (b) 'I understand some of the terms and their descriptions', (c) 'I understand all the terms and their general functioning', (d) 'I understand all the terms and their functioning in detail'. The variable takes value one if the client answered (c) or (d). The High Income Dummy is constructed on the basis of the questionnaire that clients have to fill as part of MiFID II. Clients are asked to report which bracket their income falls into: (a) '0-20,000 Euros', (b) '20,000-60,000 Euros', (c) '60,000-100,000 Euros', (d) 'More than 100,000 Euros'. The variable takes value one if the client answered (c) or (d). The Positive Investment Dummy takes value one if the client invested in the fund during that month. Investment is the amount invested in the fund in that month. Share of Total Investment is the share of the total client's portfolio that is invested in the fund in that month. Total Client Investment is the total investment by the client in that month, including in the funds that are not in the baseline dataset. Net Inflow is the net value of all trades undertaken by the client on the fund in that month. Trailer Fee is computed as the fund's management fee (which is fixed both over time and across clients) multiplied by the share of the management fee that the advisor receives (which varies within advisor/fund in January 2018).

TABLE 2 - EFFECT OF TRAILER FEES ON INVESTMENT
EXISTING CLIENTS

2015-2020; N= 3,635,660; Clients=6,134; Advisors=165.

Dependent Variable:	(1) ihst Investment	(2) Positive Investment	(3) Share of Total
Panel A: Without Controlling for Advisor's Own Investment			
Log Trailer Fee	.494**	.044**	.024***
	(.218)	(.021)	(.009)
Client/Fund Fixed Effects	Yes	Yes	Yes
Client/Month Fixed Effects	Yes	Yes	Yes
Fund/Month Fixed Effects	Yes	Yes	Yes
Panel B: Controlling for Advisor's Own Investment			
Log Trailer Fee	.492**	.044**	.024***
	(.221)	(.022)	(.009)
Log Investment (Advisor)	.028***	.003***	.001**
	(.008)	(.001)	(0)
Client/Fund Fixed Effects	Yes	Yes	Yes
Client/Month Fixed Effects	Yes	Yes	Yes
Fund/Month Fixed Effects	Yes	Yes	Yes

This Table displays estimates of regressions of clients' fund investments on the trailer fees that the clients' advisors receive when the clients invest in these funds. The estimating equation in Panel A is:

$$Investment_{cjt} = \lambda LogTrailerFee_{a(c)jt} + \eta_{ct} + \kappa_{jt} + \mu_{cj} + \epsilon_{cjt},$$

The trailer fee is computed as the fund's management fee (which is fixed both over time and across advisors/clients) multiplied by the share of the management fee that the advisor receives (which varies, within advisor/fund, in January 2018). In Panel B, the estimating equation controls for the log of the advisor's (or their close family member) investment. The unit of observation is a client/fund/month combination. In (1) the dependent variable is the inverse hyperbolic sine transformation of the client investment in the fund in that month. In (2) the dependent variable is an indicator for whether the client invests a positive amount in the fund in that month. In (3) the dependent variable is the share of the total client's portfolio invested in the fund in that month. Standard errors are clustered at the advisor level.

TABLE 3 - PLACEBO EXERCISE
DIFFERENTIAL EFFECT OF TIME-INVARIANT
TRAILER FEE ON INVESTMENT AFTER 2018
ONLY POST-2010 ADVISORS

2015-2020; N= 1,382,206; Clients=6,134; Advisors=165.

Dependent Variable:	(1) ihst Investment	(2) Positive Investment	(3) Share of Total
Log Trailer Fee X Post-2018 Dummy	.154 (.15)	.013 (.014)	.014** (.007)
Client/Fund Fixed Effects	Yes	Yes	Yes
Client/Month Fixed Effects	Yes	Yes	Yes
Fund/Month Fixed Effects	No	No	No

This Table displays estimates of regressions of clients' fund investments on the trailer fees that the clients' advisors receive when the clients invest in these funds. The unit of observation is a client/fund/month combination. The dataset includes only advisors joining the firm after 2010, for whom the trailer fee is time-invariant. The estimating equation is:

$$Investment_{cjt} = \delta(LogTrailerFee_j \times Post2018_t) + \eta_{ct} + \mu_{cj} + \epsilon_{cjt},$$

The trailer fee is computed as the fund's management fee (which is fixed both over time and across advisors/clients) multiplied by the share of the management fee that the advisor receives (which, for advisors joining after 2010, is also time-invariant as well as fund-invariant). In (1) the dependent variable is the inverse hyperbolic sine transformation of the client investment in the fund in that month. In (2) the dependent variable is an indicator for whether the client invests a positive amount in the fund in that month. In (3) the dependent variable is the share of the total client's portfolio invested in the fund in that month. Standard errors are clustered at the advisor level.

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TABLE 4 – UNDERSTANDING THE MECHANISM
EFFECTS ON INVESTMENT FLOWS

Panel A: DiDiD at the Client/Fund/Month Level

2015-2020; N= 3,635,660; Clients=6,134; Advisors=165.

Dependent Variable:	(1) ihst Incoming	(2) ihst Outgoing	(3) ihst Reallocation	(4) ihst Net Inflow
Log Trailer Fee	.104***	.025	-.01	.119***
	(.031)	(.017)	(.009)	(.035)
Client/Fund Fixed Effects	Yes	Yes	Yes	Yes
Client/Month Fixed Effects	Yes	Yes	Yes	Yes
Fund/Month Fixed Effects	Yes	Yes	Yes	Yes

Panel B: DiD at the Client/Month Level

2015-2020; N= 259,733; Clients=6,134; Advisors=165.

Dependent Variable:	(1) ihst Incoming	(2) ihst Outgoing	(3) ihst Reallocation	(4) ihst Net Inflow
Pre-2010 Advisor X Post-2018 Month	.113***	.021	.008	.159***
	(.046)	(.033)	(.009)	(.062)
Client Fixed Effects	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes

Panel A of this Table displays estimates of regressions of clients' fund investments on the trailer fees that the clients' advisors receive when the clients invest in these funds. The unit of observation is a client/fund/month combination. The estimating equation is:

$$Investment_{cjt} = \lambda LogTrailerFee_{a(c)jt} + \eta_{ct} + \kappa_{jt} + \mu_{cj} + \epsilon_{cjt},$$

The trailer fee is computed as the fund's management fee (which is fixed both over time and across advisors/clients) multiplied by the share of the management fee that the advisor receives (which varies, within advisor/fund, in January 2018). Panel B of this Table displays estimates of regressions of clients' investment in the overall fund portfolio on the interaction between a Pre-2010 Advisor dummy and a Post-2018 Month dummy. The unit of observation is a client/month combination. The estimating equation is:

$$Investment_{ct} = \lambda(Pre2010Advisor_{a(c)} \times Post2018_t) + \beta_c + \psi_t + \epsilon_{ct},$$

In (1), (2) and (3) of Panel A the dependent variables are the inverse hyperbolic sine transformations of the net value of all trades undertaken by the client on the fund in that month conditional on these trades occuring on days in which there were only incoming flows, outgoing flows or both types of trades, respectively. In (4) of Panel B the dependent variable is the inverse hyperbolic sine transformation of the net value of all trades undertaken by the client on the fund in that month. In Panel B the dependent variables are the equivalents for Panel A, but aggregated across all the funds in a client/month combination. Standard errors are clustered at the advisor level.

TABLE 5 - HETEROGENEITY OF EFFECT
OF TRAILER FEES ON INVESTMENT (EXISTING CLIENTS)

2015-2020; N= 3,635,660; Clients= 6,134; Advisors= 165.

Dependent Variable: IHST Investment	(1)	(2)
Interaction of Log Trailer Fee with:		
Client Male	-.012	.002
	(.111)	(.107)
Client Above 65 years	.126	.075
	(.117)	(.116)
Client Financial Education	-.197	.075
	(.167)	(.246)
Client Financial Profession	-.233	-.143
	(.226)	(.3)
Client Financial Knowledge	-.558***	-.489***
	(.184)	(.175)
Client High Income	.307	.405
	(.243)	(.247)
Client High Tenure	.719***	.669***
	(.223)	(.188)
Advisor Qualified	-.19	-.554
	(.32)	(.394)
Client-Advisor Age Difference Below 5	-.167	-.12
	(.174)	(.14)
Client-Advisor Same Language (First Name)	.437**	.657***
	(.202)	(.241)
Client-Advisor Same Language (Surname)	.154	.26
	(.199)	(.258)
Client-Advisor Distance Between Addresses Below 200m.	1.007	1.022*
	(.637)	(.617)
Client/Fund Fixed Effects	Yes	Yes
Client/Month Fixed Effects	Yes	Yes
Fund/Month Fixed Effects	Yes	Yes

This Table displays estimates of the heterogenous effects of trailer fees on clients' investments. The unit of observation is a client/fund/month combination. The second column displays the twelve coefficients from a single estimating equation, which is:

$$Investment_{cjt} = \lambda_0 LogTrailerFee_{a(c)jt} + \sum_{k=1}^{12} \lambda_k (LogTrailerFee_{a(c)jt} \times Dk_{c,a(c)}) + \eta_{ct} + \kappa_{jt} + \mu_{cj} + \epsilon_{cjt},$$

where $Dk_{c,a(c)}$ is a dummy for each of the twelve characteristics displayed in each of the rows. The regression further includes interactions between the trailer fee and dummy variables for whether these characteristics are missing in the dataset. The first column displays the twelve coefficients from twelve different regressions, one for the interaction with each of the twelve characterisitics. Each regression further includes an interaction between the trailer fee and a dummy variable for whether the corresponding characteristic is missing in the dataset. The trailer fee is computed as the fund's management fee (which is fixed both over time and across advisors/clients) multiplied by the share of the management fee that the advisor receives (which varies, within advisor/fund, in January 2018). The characteristics are defined in Table 1 and Table 5. Standard errors are clustered at the advisor level.

TABLE 6 - PREDICTORS OF RELATION BETWEEN
A CLIENT AND AN ADVISOR

2015-2020; N= 710,734; Clients= 6,088; Advisors= 158.

Dependent Variable: Relationship Dummy (Mean=.0084)	(1)	(2)
Age Difference Below Five	.0026***	.003***
	(.0011)	(.0012)
Age Difference Below Five X Pre-2010 Advisor X Post-2018 Client		.0004
		(.0021)
Same Gender	-.0006	-.0005
	(.0006)	(.0006)
Same Gender X Pre-2010 Advisor X Post-2018 Client		-.0018
		(.0021)
Same Language (First Name)	.0017***	.0016**
	(.0006)	(.0008)
Same Language (First Name) X Pre-2010 Advisor X Post-2018 Client		-.0014
		(.0019)
Same Language (Surname)	.0015***	.0006
	(.0005)	(.0007)
Same Language (Surname) X Pre-2010 Advisor X Post-2018 Client		.0006
		(.0038)
Log Distance Between Addresses	-.0169***	-.014***
	(.0028)	(.0024)
Log Distance Between Addresses X Pre-2010 Advisor X Post-2018 Client		-.0003
		(.0002)

This table displays estimates of regressions of the likelihood of a relation between a prospective advisor and a prospective client and measures of their social distance. An observation in this sample is a client/advisor combination. We restrict the sample to advisors and clients overlapping in their time with the firm for at least one month, and who do not live at the same address. The estimating equation is:

$$Relation_{ac} = \zeta Distance_{ac} + \theta_a + \beta_c + v_{ac}$$

where $Relation_{ac} = 1$ if client c was actually advised by advisor a, θ_a are advisor fixed effects, β_c are client fixed effects, and $Distance_{ac}$ is a measure of the social distance between the two individuals. Each column displays the estimates from a separate regression. Age difference below five is a dummy taking value one if the advisor and the client are less than five years apart in their age. Same gender is a dummy taking value one if the advisor and the client are of the same gender. Same language (first name/surname) are dummies taking value one if the advisor and the client's names are either both in Spanish or both in Catalan. Log distance between addresses is the log of the geodesic distance between the home addresses of the advisor and the client. The regression also includes a dummy taking value one if both advisor and client are based in Catalonia. In Column (2), we interact the distance variables with: (a) a dummy taking value one if the advisor joined the firm before 2010, (b) a dummy taking value one if the client joined the firm after 2018, and (c) the interaction between the dummies in (a) and (b). For conciseness, only the third interaction is displayed in the table. Standard errors are clustered at the advisor level.

TABLE 7 - WITHIN ADVISOR SELECTION
RELATION BETWEEN CLIENT CHARACTERISTICS AND ADVISOR TYPE BEFORE AND AFTER 2018

Dependent Variable:	(1) Male Dummy	(2) Log Age	(3) Financial Education Dummy	(4) Financial Profession Dummy	(5) Financial Knowledge Dummy	(6) High Income Dummy
Panel A = Cross-Section						
Pre-2010 Advisor	.017	.066**	.058	.003	.116**	-.045
	(.02)	(.029)	(.057)	(.023)	(.05)	(.039)
Mean Dependent Variable	.66	3.95	.32	.1	.71	.15
Observations = Clients (2015-2017)	4,912	4,614	2,019	2,019	1,835	3,041
Panel B = Diffences-in-Differences						
Pre-2010 Advisor X Post-2018 Client	-.007	.008	.045	-.005	-.1	-.003
	(.04)	(.031)	(.041)	(.022)	(.064)	(.028)
Advisor Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Post-2018 Client Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dependent Variable	.66	3.94	.33	.1	.64	.16
Observations = Clients (2015-2020)	5,888	5,544	2,821	2,821	2,821	3,881

This Table investigates whether the pre-determined client characteristics are correlated with the contract type of the advisor. The estimating equation in Panel A is:

$$Charac_c = \alpha + \beta Pre2010Advisor_{a(c)} + \epsilon_c$$

The estimating equation in Panel B is:

$$Charac_c = \theta_{a(c)} + \rho Post2018Client_c + \sigma(Pre2010Advisor_{a(c)} \times Post2018Client_c) + \xi_c$$

An observation is a client. The number of observations differ across columns depending on whether the dependent variable is non-missing for that observation. Pre-2010 advisors had (prior to 2018) contracts with shares of the management fee that differed across funds, while post-2010 advisors had always had contracts that specified a constant share of the management fee. In Column (3), the dependent variable is a dummy capturing the client's (financial) education, as reported in the questionnaire that clients have to fill as part of MiFID II. There are four possible answers: (a) 'No university education', (b) 'University education that is not related to maths or economics', (c) 'University education related to maths or economics', (d) 'Education that is specific to financial markets and investment funds'. The variable takes value one if the client answered (c) or (d). In Column (4), the dependent variable is a dummy for whether the client 'works or has worked in a profession related to the financial markets', a question that clients have to answer as part of MiFID II. There are four possible answers: (a) 'I have never worked in a profession related to the financial markets', (b) 'I have a job that, occasionally, is related to the financial markets', (c) 'I have had a job that is related to the financial markets', (d) 'I have a job that is related to the financial markets'. The variable takes value one if the client answered (c) or (d). In Column (5), the dependent variable is a financial knowledge dummy. As part of MiFID II, clients have to fill a questionnaire outlining their financial knowledge. One of the questions investigates whether the client is familiar with the 'nature, characteristics, and risks associated with investment funds'. The question specifically asks about the 'degree of knowledge regarding the risks of the solicited products'. There are four possible answers: (a) 'I do not understand any of the terms', (b) 'I understand some of the terms and their descriptions', (c) 'I understand all the terms and their descriptions', (d) 'I understand all the terms and their general functioning', (d) 'I understand all the terms and their functioning in detail'. The variable takes value one if the client answered (c) or (d). In Column (6), the dependent variable is a dummy for the client's income. Clients are asked to report which bracket their income falls into: (a) '0-20,000 Euros', (b) '20,000-60,000 Euros', (c) '60,000-100,000 Euros', (d) 'More than 100,000 Euros'. The variable takes value one if the client answered (c) or (d). Standard errors are clustered at the advisor level.

TABLE 8 - EFFECT OF TRAILER FEES ON
THE INITIAL INVESTMENT OF NEW CLIENTS

2015-2020; N= 36,358; Clients=2,726; Advisors=143.

Dependent Variable:	(1) ihst Investment	(2) Positive Investment	(3) Share of Total
Panel A: Without Client Characteristics X Fund Fixed Effects			
Log Trailer Fee	1.5***	.105***	.057***
	(.532)	(.041)	(.024)
Advisor/Fund Fixed Effects	Yes	Yes	Yes
Client Fixed Effects	Yes	Yes	Yes
Fund/Month Fixed Effects	Yes	Yes	Yes
Client Characteristics X Fund F.E.	No	No	No
Panel B: With Client Characteristics X Fund Fixed Effects			
Log Trailer Fee	1.477***	.103***	.058***
	(.512)	(.04)	(.023)
Advisor/Fund Fixed Effects	Yes	Yes	Yes
Client Fixed Effects	Yes	Yes	Yes
Fund/Month Fixed Effects	Yes	Yes	Yes
Client Characteristics X Fund F.E.	Yes	Yes	Yes

This table displays estimates of regressions of clients' fund investments (in the first quarter in which the clients join the firm) on the trailer fees that the clients' advisors receive when the clients invest in these funds. The estimating equation in Panel A is:

$$Investment_{cj} = \phi TrailerFee_{a(c)jt(c)} + \beta_c + \kappa_{jt(c)} + \iota_{a(c)j} + \omega_{cj},$$

The unit of observation is a client/fund combination. The sample is restricted to include only clients joining the firm after January 2015. The sample further includes only the first quarter of these clients. The trailer fee is computed as the fund's management fee (which is fixed both over time and across advisors) multiplied by the share of the management fee that the advisor receives (which varies, within advisor/fund, in January 2018). In Column (1) the dependent variable is the inverse hyperbolic sine transformation for the client investment in the fund in that quarter. In Column (2) the dependent variable is an indicator for whether the client invests a positive amount in the fund in that quarter. In Column (3) the dependent variable is the share of the total client's portfolio invested by the client in the fund in that quarter. The equation in Panel A includes advisor/fund, client and fund/quarter indicators. The equation in Panel B further includes interactions between the fund indicators and the following client characteristics: gender, age, a financial education dummy, a financial profession dummy, a financial knowledge dummy, and a high income dummy. The regression further includes interactions with indicators capturing whether the client characteristics above are missing. Standard errors are clustered at the advisor level.

TABLE 9 - QUANTIFYING CLIENT UTILITY LOSS

	Panel A: All Financial Advisors						Panel B: High Financial Knowledge Advisors					
Assumed r_f	0%	.5%	1%	1.5%	3%	5%	0%	.5%	1%	1.5%	3%	5%
Computed												
γ	6.72	6.69	6.65	6.62	6.53	6.40	6.78	6.75	6.72	6.69	6.59	6.46
α	11.43	11.38	11.32	11.27	11.11	10.89	1.01	1.00	1.00	0.99	0.98	0.96
	Existing Investors						*Existing Investors*					
$\overline{Loss}^{Pre2018}$	5.99%	5.96%	5.92%	5.89%	5.80%	5.68%	1.30%	1.29%	1.29%	1.28%	1.26%	1.23%
$\overline{Loss}^{Post2018}$	4.84%	4.82%	4.80%	4.78%	4.71%	4.62%	0.62%	0.62%	0.62%	0.61%	0.60%	0.59%
$\Delta\overline{Loss}$	1.14%	1.13%	1.12%	1.12%	1.09%	1.06%	0.68%	0.67%	0.67%	0.66%	0.65%	0.64%
	New Investors						*New Investors*					
$\overline{Loss}^{Pre2018}$	8.84%	8.81%	8.77%	8.74%	8.64%	8.50%	3.40%	3.40%	3.39%	3.38%	3.35%	3.31%
$\overline{Loss}^{Post2018}$	4.09%	4.07%	4.05%	4.02%	3.95%	3.86%	2.48%	2.48%	2.48%	2.47%	2.46%	2.44%
$\Delta\overline{Loss}$	4.75%	4.74%	4.73%	4.71%	4.68%	4.64%	0.92%	0.91%	0.91%	0.90%	0.89%	0.87%

This Table shows the estimated client utility loss before and after the change in compensation policy implemented in January 2018 due to the enactment of MiFID II. Since clients adjust their portfolios gradually in response to their advisors' changes in incentives, we use the average holdings during the second half of 2018 as the portfolios for the post-period. We use the six months prior to November 2017 as our pre-period because there is a slight anticipation leading up to the policy implementation in January 2018. Since we need to estimate γ and α jointly, we take a recursive approach: we start with an α of zero to estimate the value of γ; we then use the estimated γ to derive a new estimate for α. We keep iterating this two-step procedure until arriving at a fixed-point solution for both γ and α. Given these parameters, we can then quantify clients' utility loss both before and after January 2018. We aggregate clients with the same advisor to a client group (by summing up their investment in each internal fund), and estimate the expected utility loss at the advisor level. Panel A includes all clients and all advisors in the sample, Panel B includes only the subset of financial advisors whose compensation contracts were significantly impacted by the introduction of MiFID II, while Panel C includes only the subset of clients who are classified as financially sophisticated.

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APPENDIX FIGURES AND TABLES

FIGURE A1: DESCRIPTIVE EVIDENCE
Client Investments and Pre-2018 Advisor Trailer Fees
All Clients Active in 2015-2017
### Only Last Quarter in the Post-2018 Period
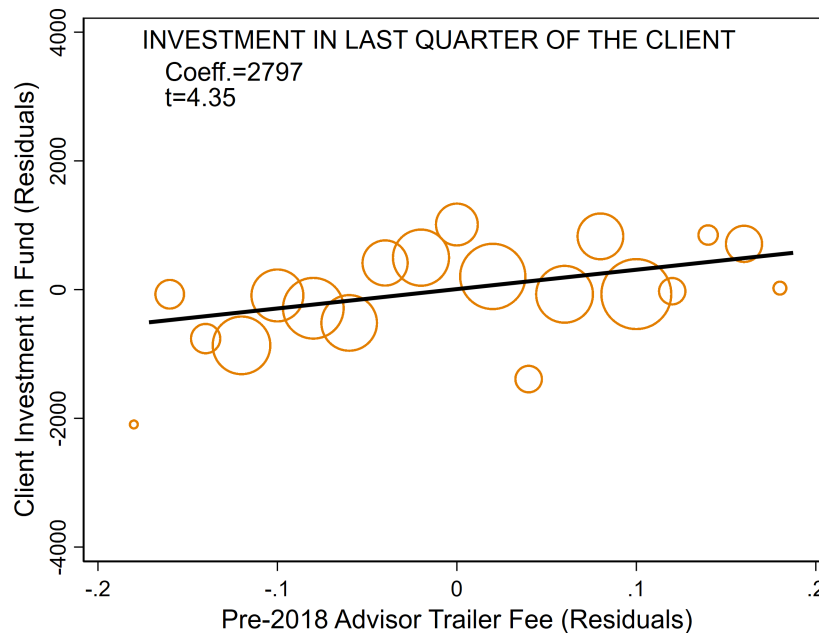
This Figure displays correlations between clients' investments and the pre-2018 trailer fees of their advisors. The sample includes clients active in the 2015-2017 period, but measured only in the last quarter in which they are active in the 2018-2020 period. The independent variable is the trailer fee received by the client's advisor in that fund prior to 2018. Both variables are net of client fixed effects and fund fixed effects. The plotted line is the OLS regression line. To display the data, we average the investment across all observations within trailer fee cells of .02 size. The area of the circles is proportional to the number of observation within each cell.

FIGURE A2: TIMING OF SUITABILITY SURVEYS COMPLETED BY THE FIRM CLIENTS
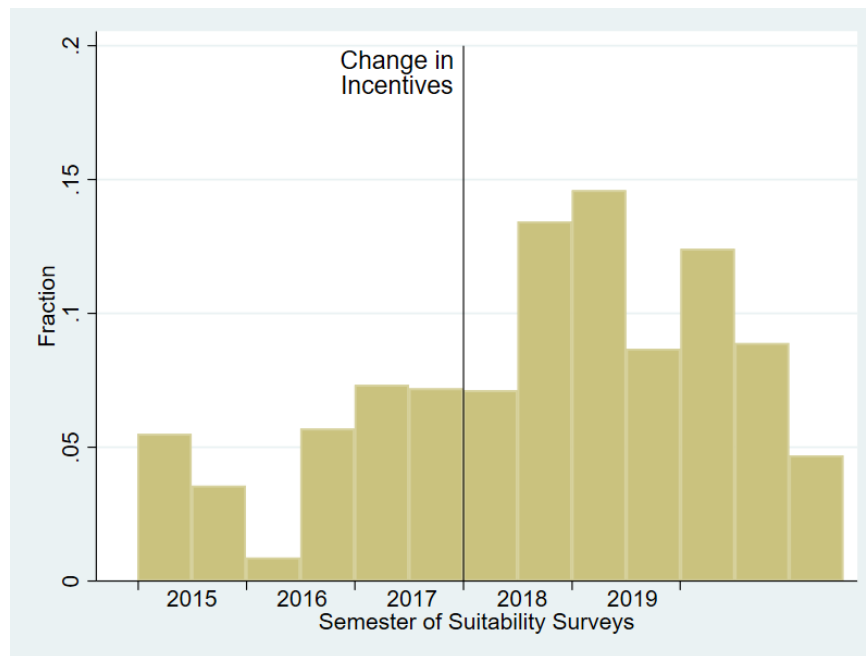
This histogram displays the timing of the suitability surveys introduced by the firm and prompted by the approval of MiFID II in April 2014. The vertical black line denotes the January 2018 change to incentives in the firm.

FIGURE A3: TRIPLE DIFFERENCES EVIDENCE
Dynamic Relation Between Client Investments
### and Advisors' 2018 Shock to Trailer Fees
USING IHST OF SHOCK
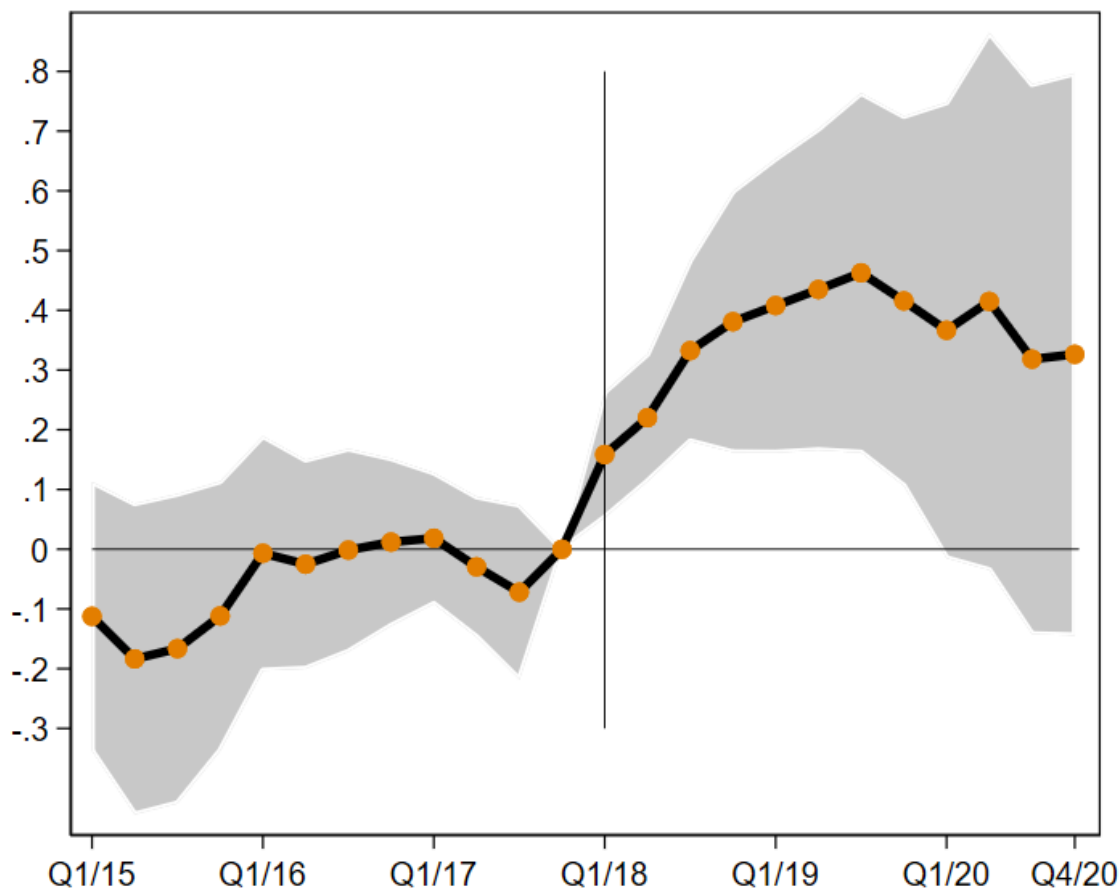
This figure displays the 24 coefficients π_t from estimating:

$$Investment_{cjq} = \sum_{q=1...11}^{13...24} \pi_q \Big(ihst(SHOCK_{a(c)j}) \times Quarter_q\Big) + \eta_{cq} + \kappa_{jq} + \mu_{cj} + \epsilon_{cjq}$$

where $SHOCK_{a(c)j} = Post18TrailerFee_j - Pre18TrailerFee_{a(c)j}$ and ihst is the inverse hyperbolic sine transformation. The unit of observation is a client/fund/quarter combination. The number of observations is 1,239,966. The number of clients is 6,133. The number of advisors is 165. The number of quarters is 24 (from Q1 2015 to Q4 2020). The variable for Q4 2017 is the omitted variable in the regression. The post-2018 trailer fee is computed as the fund's management fee (which is fixed both over time and across clients) multiplied by the share of the management fee that the advisor received after January 2018 (which is fixed across all advisors and funds). The pre-2018 trailer fee is computed as the fund's management fee multiplied by the share of the management fee that the advisor received prior to January 2018 (which varies both across advisors and across funds). Investment is the inverse hyperbolic sine transformation of the client's average investment in the quarter. The regression controls for client/quarter, quarter/fund and client/fund indicators. Standard errors are clustered at the advisor level. 90% confidence intervals are displayed in the shaded grey area.

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FIGURE A4: PERCENTAGE OF CAPITAL INVESTED IN EXTERNAL FUNDS
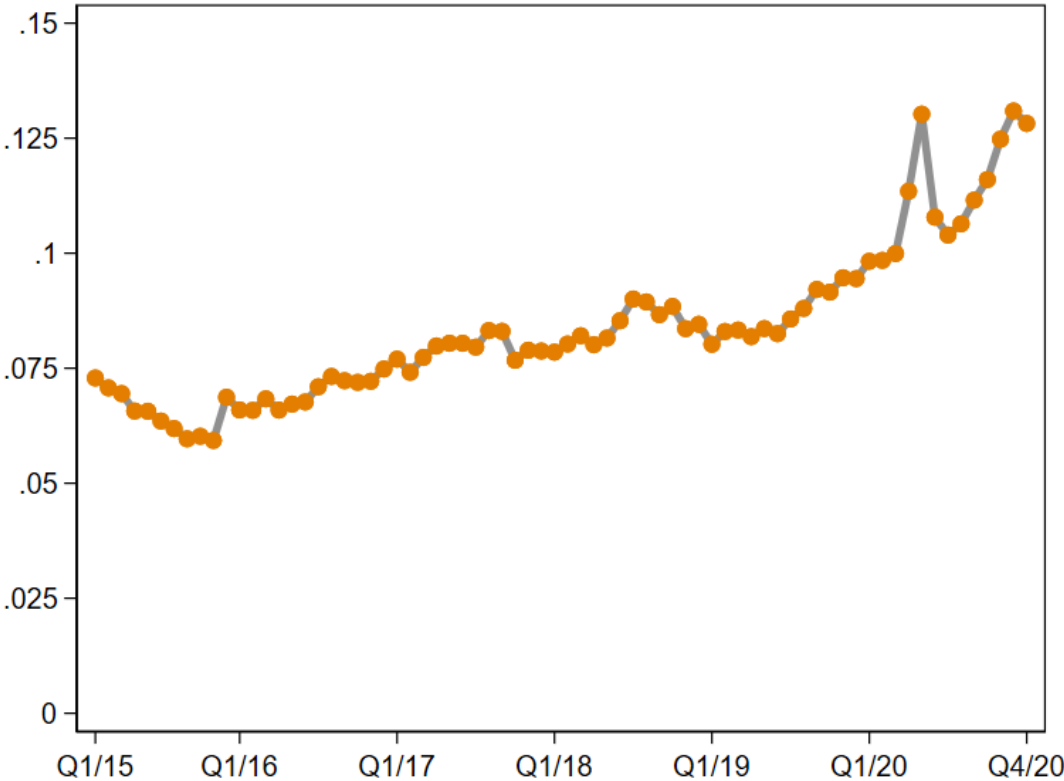
This figure displays the percentage of the capital invested in external funds, for the average firm client.

2015-2020; N= 96,628; Clients= 6,133; Advisors= 163.

Dependent Variable:	(1) ihst Investment	(2) Positive Investment	(3) Share of Total
Log Trailer Fee	.504 (.308)	.041 (.031)	.021** (.009)
Client/Fund Fixed Effects	Yes	Yes	Yes
Client/Month Fixed Effects	Yes	Yes	Yes
Fund/Month Fixed Effects	Yes	Yes	Yes

This Table displays estimates of regressions of clients' fund investments on the trailer fees that the clients' advisors receive when the clients invest in these funds. The unit of observation is a client/fund/month combination. The dataset includes only two observations per client/fund. The first one is in the last active month of the client within the pre-2018 period. The second one is in the last active month of the client within the post-2018 period. The estimating equation is:

$$Investment_{cjt} = \lambda LogTrailerFee_{a(c)jt} + \eta_{ct} + \kappa_{jt} + \mu_{cj} + \epsilon_{cjt},$$

The trailer fee is computed as the fund's management fee (which is fixed both over time and across advisors/clients) multiplied by the share of the management fee that the advisor receives (which varies, within advisor/fund, in January 2018). In (1) the dependent variable is the inverse hyperbolic sine transformation of the client investment in the fund in that month. In (2) the dependent variable is an indicator for whether the client invests a positive amount in the fund in that month. In (3) the dependent variable is the share of the total client's portfolio invested in the fund in that month. Standard errors are clustered at the advisor level.

TABLE A2 - ROBUSTNESS TO DIFFERENT FUNCTIONAL FORMS
EFFECT OF TRAILER FEES ON INVESTMENT (EXISTING CLIENTS)

2015-2020; N= 3,635,702; Clients= 6,133; Advisors= 165.

Dependent Variable:	(1) ihst Investment	(2) Log Investment+1	(3) Poisson Model Investment	(4) Investment	(5) Investment
Log Trailer Fee	.493**	.462**	1.826***	2911.42**	
	(.218)	(.203)	(.312)	(1326.712)	
Trailer Fee					2773.788***
					(1114.398)
Client/Fund Fixed Effects	Yes	Yes	Yes	Yes	Yes
Client/Month Fixed Effects	Yes	Yes	Yes	Yes	Yes
Fund/Month Fixed Effects	Yes	Yes	Yes	Yes	Yes

This Table displays estimates of regressions of clients' fund investments on the trailer fees that the clients' advisors receive when the clients invest in these funds. The estimating equation is:

$$Investment_{cjt} = \lambda LogTrailerFee_{a(c)jt} + \eta_{ct} + \kappa_{jt} + \mu_{cj} + \epsilon_{cjt},$$

The trailer fee is computed as the fund's management fee (which is fixed over time and across advisors/clients) multiplied by the share of the management fee that the advisor receives (which varies, within advisor/fund, in January 2018). The unit of observation is a client/fund/month combination. In (1) the dependent variable is the inverse hyperbolic sine transformation of the client investment in the fund in that month. In (2) the dependent variable is the log of investment plus one. In (3) we estimate a conditional quasi-maximum likelihood fixed-effect Poisson model. We display the incidence rate ratio (i.e. the exponential of the coefficient). In (4)-(5) the dependent variable is the investment level. In (5) the independent variable is the trailer fee. Standard errors are clustered at the advisor level.

Characteristic:	Dependent Variable: ihst Investment					
	(1) Male Dummy	(2) Log Age	(3) Financial Education Dummy	(4) Financial Profession Dummy	(5) Financial Knowledge Dummy	(6) High Income Dummy

Panel A: Separate Regressions For Each Characteristic

F-Statistic	7.4	6.6	4.3	1.2	3.3	3.5
p-value	0	0	0	.282	0	0
Fund Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Client Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	91,686	87,102	37,493	37,493	34,442	56,069
Number of Clients	6,549	6,247	2,692	2,692	2,471	4,013

Panel B: All Characteristics in a Joint Regression

F-Statistic	6.2	3.5	1.9	1	3.1	6.8
p-value	0	0	.003	.488	0	0
Fund Fixed Effects				Yes		
Client Fixed Effects				Yes		
Observations				91,686		
Number of Clients				6,549		

This Table investigates whether the pre-determined client characteristics statistically predict clients' allocations across the firm funds. The estimating equations in Panel A are:

$$Investment_{cj} = \sum_{k=1}^{K} \chi_k (Charac_c \times Fundk_j) + \beta_c + \gamma_j + \omega_{cj},$$

where η_c are client fixed effects and κ_j are fund fixed effects. $Charac_c$ is the client characteristic in the corresponding column. $Fundk_j$ is a dummy variable taking value one when the observation refers to fund k. The reported F-statistics are based on tests that that the coefficients $\hat{\lambda}_k$ are jointly equal to zero. The reported p-values are based on tests of the same hypothesis. In Panel B, the estimating equation is identical with the exception that all the interactions with the client characteristics are included in the same regression. An observation is a client/fund combination. The sample includes only the first month in which a client appears in the sample. The sample is further restricted to clients joining the firm prior to 2018. In Panel A, the number of observations differs across columns depending on whether the dependent variable is non-missing for that observation. In Panel B, the regression includes interactions with whether the clients characteristics are missing. In Column (3), the dependent variable is a dummy capturing the client's (financial) education, as reported in the questionnaire that clients have to fill as part of MiFID II. There are four possible answers: (a) *'No university education'*, (b) *'University education that is not related to maths or economics'*, (c) *'University education related to maths or economics'*, (d) *'Education that is specific to financial markets and investment funds'*. The variable takes value one if the client answered (c) or (d). In Column (4), the dependent variable is a dummy for whether the client *'works or has worked in a profession related to the financial markets'*, a question that clients have to answer as part of MiFID II. There are four possible answers: (a) *'I have never worked in a profession related to the financial markets'*, (b) *'I have a job that, occassionally, is related to the financial markets'*, (c) *'I have had a job that is related to the financial markets'*, (d) *'I have a job that is related to the financial markets'*. The variable takes value one if the client answered (c) or (d). In Column (5), the dependent variable is a financial knowledge dummy. As part of MiFID II, clients have to fill a questionnaire outlining their financial knowledge. One of the questions investigates whether the client is familiar with the *'nature, characteristics, and risks associated with investment funds'*. The question specifically asks about the *'degree of knowledge regarding the risks of the solicited products'*. There are four possible answers: (a) *'I do not understand any of the terms'*, (b) *'I understand some of the terms and their descriptions'*, (c) *'I understand all the terms and their general functioning'*, (d) *'I understand all the terms and their functioning in detail'*. The variable takes value one if the client answered (c) or (d). In Column (6), the dependent variable is a dummy for the client's income. Clients are asked to report which bracket their income falls into: (a) *'0-20,000 Euros'*, (b) *'20,000-60,000 Euros'*, (c) *'60,000-100,000 Euros'*, (d) *'More than 100,000 Euros'*. The variable takes value one if the client answered (c) or (d). Standard errors are clustered at the advisor level.

**TABLE A4 - DIFFERENCES BETWEEN
PRE-2010 AND POST-2010 ADVISORS
IN TERMS OF THEIR PORTFOLIO OF CLIENTS**

	(1) Log Number Clients	(2) Log Client Exits	(3) Log Client Entries
Panel A			
Pre-2010 Advisor	.548***	.062***	-.006
	(.195)	(.026)	(.036)
Month Fixed Effects	Yes	Yes	Yes
Advisor Fixed Effects	No	No	No
Observations (2015-2017)	4,816	4,816	4,816
Panel B			
Pre-2010 Advisor X Post-2018	-.058	.069***	.01
	(.074)	(.027)	(.029)
Month Fixed Effects	Yes	Yes	Yes
Advisor Fixed Effects	Yes	Yes	Yes
Observations (2015-2020)	8,241	8,241	8,241

This Table investigates whether the characteristics of the overall portfolio of clients are correlated with the contract type of the advisor, differentially before and after 2018. The estimating equation in Panel A is:

$$Charac_a = \lambda_t + \tau Pre2010Advisor_a + \epsilon_{at}$$

The estimating equation in Panel B is:

$$Charac_a = \theta_a + \psi_t + \tau(Pre2010Advisor_a \times Post2018_t) + \epsilon_{at}$$

An observation is an advisor/month. In Column (1), the dependent variable is the advisor's (log of) total number of clients. In Column (2), the dependent variable is the advisor's (log of) number of clients who leave the firm. In Column (3), the dependent variable is the advisor's (log of) number of clients who join the firm. Both panels control for fixed effects for the month. Standard errors are clustered at the advisor level.

TABLE 5 - PREDICTORS OF RELATION BETWEEN
A PROSPECTIVE CLIENT AND A PROSPECTIVE ADVISOR

2015-2020; N= 664,080; Clients= 6,087; Advisors= 158.

Dependent Variable: Relationship Dummy (Mean=.0084)	(1)
Age Difference Below Five	1.53***
	(.15)
Same Gender	.82
	(.13)
Same Language (First Name)	1.17***
	(.07)
Same Language (Surname)	1.12
	(.07)
Log Distance Between Addresses	.36***
	(.05)

This table displays estimates of regressions of the likelihood of a relation between a prospective advisor and a prospective client and measures of their social distance. An observation in this sample is a client/advisor combination. We restrict the sample to advisors and clients overlapping in their time with the firm for at least one month, and who do not live at the same address. The estimating equation is:

$$Relation_{ac} = \zeta Distance_{ac} + \theta_a + \beta_c + v_{ac}$$

where $Relation_{ac} = 1$ if client c was actually advised by advisor a, θ_a are advisor fixed effects, β_c are client fixed effects, and $Distance_{ac}$ is a measure of the social distance between the two individuals. Each column displays the estimates from a separate regression. Age difference below five is a dummy taking value one if the advisor and the client are less than five years apart in their age. Same gender is a dummy taking value one if the advisor and the client are of the same gender. Same language (first name/surname) are dummies taking value one if the advisor and the client's names are either both in Spanish or both in Catalan. Log distance between addresses is the log of the geodesic distance between the home addresses of the advisor and the client. The regression also includes a dummy taking value one if both advisor and client are based in Catalonia. In Column (2), we interact the distance variables with: (a) a dummy taking value one if the advisor joined the firm before 2010, (b) a dummy taking value one if the client joined the firm after 2018, and (c) the interaction between the dummies in (a) and (b). For conciseness, only the third interaction is displayed in the table. Standard errors are clustered at the advisor level.