Regulatory reform and the cost of retail investing through life offices: 1988 ~ 2006

A report for the Financial Services Authority by 1776 Consulting

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Regulatory Reform and the Cost of Retail Investing Through Life Offices: 1988 — 2006

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I. Introduction and Summary

Between 1988 and 2006 the government and the financial market regulators launched two major sets of reforms designed to lower the cost of retail investing through Life Offices, to wit: Disclosure Reform (1992 to 1995) and the combination of Stakeholder Pensions, PIA Regulatory Update 64 (RU64), and the FSA’s Comparative Tables project (1999 to 2001). Each initiative aimed to reduce the cost of investing by making it easier for customers to find and identify low cost Life Office products, thereby promoting price competition between Life Offices. As it happens, the overall average cost of investing through retail-oriented Life Offices did in fact fall from an average of 3.85% of Funds Under Management (FUM) per year prior to Disclosure Reform to 1.86% of FUM per year after Stakeholder Pensions/RU64/Comparative Tables—a total fall of 199 basis points (bp).\(^1\) The FSA asked 1776 Consulting to investigate the role that these reform efforts played in bringing this decline about.\(^2\)

We find that the reforms together account for between 75% (146 bp) and 100% of this decline in the cost of investing, with the range being a function of what one assumes about the evolution of the cost of investing in the absence of any reforms.\(^3\) To err on the side of caution we focus upon the lower 146 basis point estimate of the impact of the reforms. Under this scenario, Disclosure Reform accounts for 142 bp of the decline, with Stakeholders/RU64/Comparative Tables accounting for remaining 4 bp of the decline.\(^4\)

At the end of 2006 retail-oriented Life Offices managed £580 billion. By lowering the cost of managing that investor wealth by 146 basis points, Disclosure Reform and Stakeholder Pensions/RU64/Comparative Tables create benefits of up to £8.5 billion / year.\(^5\)

The Logic of the Reforms

Intuitively, holding the product sold constant, a firm can attract business by either expending costly resources to seek out and attract customers (thereby adding to costs, and so, in equilibrium, prices) or by offering low prices so that customers who search the market find its offerings attractive. The trade-off a firm faces when deciding upon the balance to be struck between these two strategies depends upon the ability of customers to search among products on offer on the basis of price. If customers can’t search on the basis of price (if search costs are high), then a firm will attract little new business by offering low prices. In a high search cost environment, then, a firm will find the high cost/high price strategy of actively attracting customers a better bet than a low cost/low price strategy that relies upon customers finding the firm. If, on the other hand, search costs are low, then a firm charging a high price will lose its customers to (easily found) rivals offering lower prices. In a low search cost environment, then, firms will find a low cost/low price strategy superior to a high cost/high price strategy. This intuition is strongly supported by the existing theoretical and empirical research on this topic.
Prior to Disclosure Reform the search costs in the Life Office market were very high indeed. To illustrate, the "Key Features" document that an advisor was required to provide to a potential customer before purchase did not disclose the price of that product! Realizing that this (lack of) disclosure regime created strong incentives for Life Offices to pursue a high cost/high price strategy, the OFT launched a vigorous effort to convince the government and the regulators to increase price transparency so as to promote price competition. This effort eventually led to success in the form of Disclosure Reform, which lowered search costs by requiring Life Offices to disclose the prices they charged.

Even after Disclosure Reform buying a personal pension remained a complex task. To lower search costs still further, the Government launched Stakeholder Pensions. The Government designed Stakeholders to be low price investment products with good terms and conditions. A customer who found searching the market costly could therefore opt for a Stakeholder and be confident that he was getting a good—if not necessarily the best possible—deal. Since Stakeholders did offer a good deal to a wide range of investors, the FSA required in RU64 that any advisor recommending a personal pension either recommend a Stakeholder or explain why the product he was recommending was the superior choice. RU64 therefore put every personal pension into direct competition with good value Stakeholders. The FSA's Comparative Tables project collected information on price and other product features for a wide range of investment products and made this information available on the FSA website, further lowering the cost of searching the market for a good deal. Since Stakeholder Pensions, RU64, and Comparative Tables were all developed and took effect at roughly the same time, and since they were designed to work together to lower search costs, we consider them as a single package of reforms and analyze their joint impact.

**Assessing the Impact of the Reforms: Method**

To assess the impact of these price competition promoting reforms, we begin by assuming that Life Offices adopt the optimal strategy for the environment in which they operate. We further assume that a key aspect of that environment is the degree of price competition (which may be affected by regulation). The optimal strategy in turn implies a cost function that yields a Life Office's cost of doing business under that strategy as a function of that Office's characteristics (e.g. its size and the composition of its business). If the environment changes then the optimal Life Office strategy—and so the cost function—changes as well.

This logic suggests that one can detect and measure the impact (if any) of a change in the regulatory regime in the following manner. Suppose that a regulatory reform did promote price competition. In this case the reform will cause the optimal Life Office strategy to shift from a higher price/higher cost one pre-reform to a lower price/lower cost one post-reform. Hence, one would expect to observe that, holding Life Office characteristics constant, the post-reform cost function will imply a lower cost of investing than the pre-reform cost function. If, on the other
hand, the reform did not have any effect upon the degree of price competition in the market, then one would not expect that reform to affect the optimal Life Office strategy. If the strategy does not change, then the pre-reform and post-reform cost functions—and so the cost of investing holding Life Office characteristics constant—should be about the same. Thus, we can detect and measure the impact of a reform by comparing the cost of investing implied by the post-reform cost function with that implied by the pre-reform cost function holding Life Office characteristics constant.

To apply this approach, we first divide the 1988 to 2006 sample period into regulatory regimes on the basis of the milestone reform dates of 1992 (the effort leading to Disclosure Reform begins), 1995 (Disclosure Reform takes full effect), and 1999 (Stakeholders/RU64 begin to apply). These dates divide our 1988 to 2006 sample period into three regimes and a transition period:

- Pre-Disclosure Reform (PreDis), consisting of the years 1988 to 1991;
- A Pre-Disclosure to Post-Disclosure transition period consisting of the years 1992, 1993, and 1994;
- Post-Disclosure Reform (PostDis), consisting of the years 1995 to 1998; and
- Post-Stakeholder/RU64/Comparative Tables (PostStake), consisting of the years 1999 to 2006.

We further divide each regime into "In Sample" and "Out of Sample" years.

We estimate a cost function for each regime (using only that regime's In Sample years), and we then use two methods to detect if and when the Life Office cost function shifts:

- First, we compare the actual overall average cost of investing to that implied by each of the three cost functions we estimate;
  - Our idea here is that a regime cost function estimated with that regime's In Sample years will do a good job tracking that actual cost of investing until the true Life Office cost function shifts. The time at which a significant gap between the cost of investing implied by a regime cost function and the actual cost of investing appears will then date the time at which the cost of investing changed.
  - We measure the impact of a shift in the cost of investing by comparing what the cost of investing would have been if the cost function had not shifted to the actual cost of investing.
- Second, we examine the evolution of individual Life Office operating costs;
Our idea here is that by comparing each Life Office’s cost in each year to that predicted by each of the regime cost functions that we estimate, we can see which cost function is most accurately capturing costs. By seeing when the identify of the most accurate cost function changes, we can date the time at which the Life Office cost function shifts.

**Did the Cost of Investing Function Shift Down?**

We find that both the overall cost of investing analysis and the individual Life Office analysis yield a consistent picture for the evolution of the cost of investing through Life Offices over the sample period. Both methods find that:

- Life Offices operated on a high cost Pre-Disclosure Reform (PreDis) cost function until 1991;

- The Life Office cost function began to shift down between 1992 and 1994;

- Life Offices reached a new equilibrium in 1995, with the cost function remaining stable until 1998;

- The Life Office cost function shifted down again in 1999 and did not shift further during the remainder of the sample period (to 2006).

So, our analysis finds strong and consistent evidence of cost function shifts between the PreDis, PostDis, and PostStake regimes.

**Why Did the Cost of Investing Fall?**

To be sure, the mere fact that the cost of investing fell during a period of regulatory reform provides only weak support for the hypothesis that the reforms *caused* the decline, as it could be the case that the cost of investing was declining anyway (as the saying goes, “the sun rises whether or not the cock crows”). The case for reform causing the cost of investing to decline therefore lies more in the precise pattern of the cost function shifts rather than in the simple fact that the cost of investing was lower after the reforms than it was before. That is, if the reforms did bring about an increase in the degree of price competition that in turn caused the Life Office cost function to shift down, then one would expect to observe that: i) the cost of investing function shifted down between 1992 (with the OFT report that led to Disclosure Reform) and 1995 (at which point the industry would have adjusted to the new disclosure regime); ii) the cost function remained stable between 1995 and 1998 as the regulatory regime did not change; iii) the cost function shifted down again in 1999 when the combination of Stakeholder Pensions and RU64 took effect; iv) the cost function remained stable until 2006 as the regulatory regime did not change significantly between 1999 and 2006; and v) the cost function shift brought about by Disclosure Reform was more significant than that brought about by
Stakeholders/RU64/Comparative Tables as Disclosure Reform brought about a far more profound change in the degree of price transparency in the market. Since the cost function shifts we observe in the data precisely match this pattern, we conclude that there is a strong case for the hypothesis that the reforms caused the cost of investing to fall.

However, before concluding that reform did bring about the cost function shifts, we must also consider the possibility that the cost of investing fell due to ongoing cost-reducing innovations by Life Offices. We consider three alternative hypotheses on this theme: 1) the cost of investing fell as a result of the ongoing cost-reducing innovations by individual Life Offices acting independently; 2) the cost of investing fell because of the cumulative impact of general cost-reducing innovations in the UK Life Office industry; and 3) the cost of investing fell because of the cumulative impact of cost-reducing shocks to the global fund management industry. Consider each hypothesis in turn.⁶

If the decline in the cost of investing we observe occurred due to the cumulative impact of innovation by individual Life Offices, one would expect to see the overall Life Office cost function drift down over time as individual Life Offices from time to time hit upon cost-reducing innovations. However, the cost function shifts we observe are very discrete, occur over very short time periods, and affect all Life Offices at the same time. Hence, we do not think that the pattern of cost function shifts we observe can be explained by innovation by individual Life Offices.

We next consider the possibility that the cost declines were the result of (a series of) innovation shocks that hit the Life Office industry as a whole rather than individual firms. This hypothesis could be consistent with the cost function shifts affecting Life Offices in general at the same time. We test this hypothesis by estimating the plausible magnitude of the cumulative impact of such shocks over the PreDis to PostDis transition period based upon the distribution of the magnitude of possible shocks. We estimate the distribution of the magnitude of such shocks by examining the year to year variation in the cost of investing over our sample period (excluding the PreDis to PostDis transition years). We find that these non-reform shocks are far too small to explain the PreDis to PostDis decline in the cost of investing. It is however impossible to completely rule out the possibility that the smaller PostDis to PostStake decline was due to a random shock.

The final alternative hypothesis we examine is that the PreDis to PostDis decline was the result of the cumulative impact of cost-reducing innovations in the global retail fund management industry. To explore this possibility we need to compare the cost of retail fund management in the UK to the cost of retail fund management in a market outside of the UK regulatory net. We chose broad US equity mutual funds as our comparison on the basis that: i) any global innovation shock to the fund management industry would affect the cost of fund management in the US; ii) due to the close links between the fund management industries in the US and the UK, any general innovation shock to fund management that affected the US would also affect the UK; and iii) good data enables us to accurately measure the cost of investing through broad US equity mutual funds. We find that the cost of investing through US mutual funds is roughly constant over our
sample period. Consequently, we don’t think that a global shock to the economics of retail fund management could be the explanation for the decline in the cost of investing through UK Life Offices.

So, the empirical evidence demonstrates that the cost of investing (holding Life Office characteristics constant) did decline sharply from the PreDis to the PostDis regime, and that it most likely declined further from the PostDis to the PostStake regime. The regulatory reform hypotheses can explain these shifts, and the alternatives can not. Thus, we conclude that the 146 bp decline in the cost of investing that we observe was most likely due to the price competition promoting effects of regulatory reform.

**Comparing the Benefits and the Costs of the Reform Efforts**

Implementing these reforms entailed both one-off adjustment costs and on-going compliance costs for Life Offices and investment advisors. Fortunately, each of these initiatives was accompanied by a Cost-Benefit Analysis or Regulatory Impact Assessment. Thus, it is possible to quantify these costs and to compare them with the benefits that the greater degree of price competition brought about.

To do so, we add up the reported one-off and on-going costs for each of the above initiatives. One-off costs sum to about £350 Million (or say an annual cost of £20 Million @ 5%/year), and on-going costs sum to around £100 Million/year. In annual terms these costs amount to about £120 Million. Retail-orientated Life Offices manage about £580 Billion of investor wealth. The cost of implementing and complying with the regulations we analyze here therefore adds about 2 basis points to the overall average cost of investing through Life Offices (for example, increasing it from 1.83% of FUM to 1.85%). But of course, since these are costs that Life Offices already incur to comply with the regulations, these costs are already in Life Office costs. Thus, the fall in the cost of investing that we observe is in fact a net fall after any cost-increasing effect that regulation may have had. It follows that the net benefits of the price competition promoting regulatory reforms the government and the FSA (including its predecessor bodies) have implemented equal about £8.5 Billion/year.

**The Organization of the Paper**

In Section II we provide a brief review of the most relevant aspects of the economic literature on price competition in general and of previous work directly related to the cost of investing through UK Life Offices. We discuss the approach and the data we use to analyze the cost of investing through Life Offices in Section III, and we analyze the evolution of the cost of investing through Life Offices in Section IV. We assess the role that regulatory reform played in bringing about the decline in the cost of investing in Section V, and we conclude in Section VI.
II. Search Costs, Price Competition, and Costs

A. The General Economic Evidence

The reforms the regulators and the Treasury undertook during the 1988 to 2006 period aimed to increase price competition in the retail investing market by:

- Enabling Life Office customers to more clearly understand the price of a given product;
- Enabling customers to more easily compare the prices of alternative products; and
- Creating a good value benchmark product against which customers could compare other possibilities.

The general economic evidence overwhelmingly demonstrates both theoretically and empirically that reducing the cost of searching across products (“search” costs) tends to lead to lower prices.

Let us begin with the intuition for why lowering search costs leads to lower prices. When customers find it difficult to compare products, firms offer customers high prices knowing that they will not be able to find a better deal due to high search costs. Firms then compete for customers using non-price means (e.g., highly commission motivated sales forces). Firms are willing to pay to attract customers up to the point where the marginal cost of attracting an additional customer just equals the (high) amount that customer will pay, so competition to attract these high-paying customers leads to high costs for firms.

Once customers can search, however, high price firms are at a disadvantage as a customer can more easily find a lower priced alternative. The potential for customers to shop around means that a firm can attract customers by lowering its prices, and competition between firms will lead them to pursue this option. Since a high cost firm cannot offer low prices (or compete with firms that do offer low prices in the low search cost case) and remain in business, any firm wishing to exploit the opportunities that consumer search creates must lower its costs (one way to do so is to spend less on non-price means to attract customers and to attract customers via low prices instead). Consequently, one would expect to find that a regulatory reform that did lower search costs will lead to lower cost firms providing lower prices to customers.

The validity of the intuitive case for the proposition that lowering search costs increases price competition is confirmed by rigorous theoretical models of the topic. The most relevant papers for our purposes here of the vast literature on this point are Stahl (1989) and Iossa (2007).

The theoretical argument that lowering search costs leads to greater price competition has been the subject of an enormous amount of empirical inquiry, and the results of these studies overwhelmingly confirm the link. This literature began with Benham (1972) who found that eliminating the prohibition on advertising for opticians (thereby lowering consumer search costs)
led to lower prices for spectacles. Some of the most relevant of the more recent studies of this matter are Brown and Goolsbee (2002) and Busse, Zettelmeyer, and Silva-Risso (2004). Brown and Goolsbee find that a decrease in search costs led to a significant price fall in the cost of term life insurance in the US, and Busse, Zettelmeyer, and Silva-Risso find that better informed consumers get much better prices when buying cars.

**B. The Reforms**

Prior to Disclosure Reform consumer search costs in this market were extremely high. Life Offices were required to reveal only very limited information about the price they charged for managing an investor's funds. Most importantly, the *Key Feature* document that an independent financial advisor (IFA) or a company salesman was required to provide before purchase to potential investors to inform them of the key features of the product he recommended did not actually reveal the price of that specific product, it instead disclosed an industry-wide standard price! As a Life Office operating under this regulatory regime would gain little by offering low prices that potential investors would not see, one would instead expect them to compete for business through costly marketing efforts (such as paying high commissions). In equilibrium, then, one would expect Life Offices to pursue a high product price/high operating cost strategy in the Pre-Disclosure Reform period.

The serious effort to reform this disclosure regime began in 1992 when the Office of Fair Trading (OFT) sent a report to the Treasury pointing out the perverse incentives the existing regime created for competition and making the case for designing a regime that promoted competition on the basis of price. In light of this report the Treasury requested that the regulators reform disclosure standards to promote price competition. Acting upon this request, the regulators devised a package of alterations to the existing regime that became known as Disclosure Reform. Crucially, Disclosure Reform required product specific price disclosures. This reform greatly enhanced the ability of potential investors to make purchase decisions on the basis of the price both directly by enabling them to see the prices of the products they were considering and indirectly by facilitating the ability of the financial press and other media outlets to compare products on the basis of price.8

After Disclosure Reform a Life Office could attract business by offering lower prices than its competitors, and one would expect Life Offices to act upon this incentive. Since a high cost Life Office cannot in equilibrium provide low price products, competing on the basis of price would lead Life Offices to adopt a low price/low operating cost strategy.

Even after Disclosure Reform purchasing a long-term saving product such as a personal pension remained a complex decision. Concerned that many people would find the process of making such a decision so daunting that they chose instead to avoid making pension investments all together, the government launched its Stakeholder Pensions initiative in December 1998 to provide a personal pension product with low charges and fair terms and conditions. Then, people
who would find the task of selecting a good value product from the range of offerings in the market too difficult could just opt for a Stakeholder and be confident that they were getting a good—if not necessarily the best possible—deal. The government developed the Stakeholder idea in 1999 and 2000, and Stakeholders became available for purchase in April 2001.9

Given that Stakeholders did offer a good deal for a wide range of investors, the FSA (acting through the PIA) issued RU64, which required any advisor recommending a personal pension to either recommend a Stakeholder or to explain why the product he did recommend was the superior choice. In effect, then, RU64 placed every personal pension product in competition with low-price Stakeholders, and so created an additional degree of price competition in the market. RU64 was issued in 1999 to prevent advisors from putting people into poor value pensions before Stakeholders became available.10

To further promote price competition in this market, the Treasury also announced in 1999 that the FSA would develop Comparative Tables for investment products including those sold by Life Offices. Under this initiative, the FSA collects information on key product terms (including price) and makes this information available on its website. Comparative Tables therefore further facilitate price comparisons across products.11 Comparative Tables went live in October 2001.

C. Previous Analyses of the UK Reforms

There has been little previous research on search costs and price competition in the UK Life Office market. The paper most closely related to our analysis here is Andrews (2009). This paper tests the hypothesis that Disclosure Reform led to more shopping around by potential customers and finds strong if indirect evidence in favor of it. Andrews (2009) therefore provides evidence that Disclosure Reform did lower search costs. James (2000) measured the cost of investing through Life Offices, but did not explore the impact of regulatory reform on that cost. O’Brien (2003) examines the limited information that regulatory disclosures provide on the price of specific investment products to examine the degree of competition in the Life Office market and finds that prices did decline in the period after Disclosure Reform (a finding consistent with our analysis below).

On a related topic, several papers investigate the more general question of UK Life Office efficiency (see, for example, Klumpes (2004) and Diacon, Starkey, and O’Brien (2002). Though these papers do not focus upon the cost of investing from the customer perspective, they do find that a Life Office’s product mix and scale play an important role in determining its overall level of efficiency. We therefore include these measures of these variables in our empirical analysis of Life Office costs.
D. The Impact of Regulatory Reform: A Hypothesis

The general theoretical and empirical evidence strongly supports the idea that lowering consumer search costs leads to more intense price competition and so to lower prices. Both Disclosure Reform and Stakeholder Pensions/RU64/Comparative Tables were designed to lower search costs, and Andrews (2009) finds that Disclosure Reform did lower search costs. Thus, there exists a strong presumption that these reforms caused the cost of investing through Life Offices to fall. We now turn to testing that presumption.
III. Approach, Data and Sample

A. Approach

A.1 Prices or Costs?

Ideally, we would investigate the impact of regulatory reform with detailed data on the investors in the market, the Life Office products available for purchase, and the decisions that investors actually made. Unfortunately, no such data exists. There is essentially no investor level data at all, and while the PIA did collect some price data on specific Life Office products (see their Disclosure Reports), the number of products for which price data was collected is very small and there is no information on sales volume or market share. Consequently, we can not construct an overall cost trend using the PIA data. Thus, it is impossible to study the effects of the reforms we focus upon with bottom-up data on investors and products.

However, as we argued above, a regulatory reform that increases price competition will lead Life Offices to shift from a higher price/higher cost strategy to a lower price/lower cost strategy. It follows that we can also investigate the impact of the reforms by looking at Life Office costs. Fortunately, Life Office regulators do require that Life Offices file detailed reports each year that contain the information we need to track the evolution of Life Office costs over our sample period.

We note that analyzing the impact of the reforms from the cost perspective has several disadvantages. First, one can not carry out the intricate tests of the impact of reform that one could with detailed consumer and product data. Second, while the cost data will enable us to track the overall impact of the reforms, it will not enable us to track the differential impact of the reforms on specific investor types or groups. Third, since we do not actually measure the price of investing, there exists the logical possibility that Life Offices could reduce costs without reducing prices. Needless to say, if Life Offices did behave in this manner then our inferences about the impact of reform would be wrong. And last, our focus on Life Office costs does not enable us to take the quality of the match between the investor and the specific product he purchases ("suitability") into account (with the quality of the match including the effect of the product’s non-price features). The third and fourth points merit further consideration, so consider each in turn.
We think it highly improbable that costs could fall without prices falling too as the Life Office market is highly competitive (even if the competition did not always focus on price). In a highly competitive market a cost fall generally leads to a corresponding price fall as firms compete for the now more profitable business. Furthermore, if Life Office costs did fall over this period without prices falling too then it is obvious that the cost fall is not the result of the reforms. Hence, one would not expect the timing of the reforms to predict the timing of any such non-reform related cost falls. We explore this point in more detail below. On balance, then, we are confident that we can track the impact of reforms with data on Life Office costs.

Turning to suitability, it is of course true that a product’s non-price features and the match between these features and an investor’s exact requirements has a significant effect upon the product’s value to an investor. Hence, if the average quality of the investor/product match declined over the sample period, then the quality-adjusted price of investing through Life Offices would not have fallen by the amount that the unadjusted data would suggest. Or, in other words, if Life Offices lowered cost by reducing the overall quality of their products they offered then (again) the inferences we draw about the impact of the reforms we analyze might be misleading. The lack of detailed investor and product data prevented us from rigorously analyzing this possibility. We instead consulted with industry experts both inside and outside the FSA, and we think that the consensus expert view is that the quality of the investor/product match has not declined. Thus, we don’t think that our inability to explicitly incorporate suitability into our analysis undermines our results.

A.2 Reporting Costs

In our empirical analysis below we focus upon investigating Life Office cost functions. Yet, results expressed in terms of absolute cost levels are not very meaningful on an intuitive level. Consequently, we generally report our results in terms of Explicit Expense Ratios (EERs) for individual Life Offices and Weighted Average EERs (WavEERs) for the market as a whole. A Life Office’s EER equals its costs divided by its Funds Under Management (FUM), and the WavEER for a year equals the weighted average of the EERs of the Life Offices operating in that year, with each Life Office’s weight equal to its share of total FUM. Our idea here is that investors hire Life Offices to manage the investments they make through them. The increase in investor wealth in a given year equals the total return on the Life Office’s portfolio in that year minus the Life Office’s costs for that year. Expressing this relationship in terms of rates of return, the return the Life Office’s representative or average investor obtains on the funds he entrusts to the Life Office in a given year \( Y \) \( (R_{\text{Representative,}Y}) \) is then:

\[
R_{\text{Representative,}Y} = \frac{\text{Total Portfolio Return}_Y}{FUM_Y} - \frac{\text{Cost}_Y}{FUM_Y}
\]

\[= R_{\text{Portfolio,}Y} - EER_Y \tag{1} \]

...
As equation 1 shows, a Life Office's $EER$ measures the gap between its net portfolio return and the return it provides to its investors, and $W\text{a}vEER$ measures this gap for the market as a whole. $EER$ and $W\text{a}vEER$ therefore provide a more intuitively meaningful measure of the cost of investing.\(^\text{12}\)

### B. Regulatory Regimes

To see if regulatory changes brought about a decline in the cost of investing, we divide our sample period into a series of regulatory regimes. To test whether or not the Life Office cost function shifts within what we consider to be a stable regime, we further divide each regime into "In Sample" and "Out of Sample" years. We estimate a regime's cost function with just its In Sample years. The extent to which a regime cost function can predict the cost of investing in that regime's Out of Sample years (and other non-regime years) in then an empirical matter.

We assume that a given regime persists until a major reform occurs. Consequently, we divide our sample period into the following regimes on the basis of the milestone reform dates we discussed above:

- **Pre-Disclosure Reform (PreDis), 1988 — 1991**
  - The Financial Services Act came into effect in 1988 and so forms the first regime. This regime ends in 1992 when the OFT's report to the Treasury begins the serious effort to promote price competition by reforming disclosure rules;
  - In Sample years: 1988 — 1990; Out of Sample years: 1991. Our goal here is to see when the Life Office cost function begins to shift down, so we estimate the PreDis cost function with data from the beginning of the regime.

- **Post-Disclosure Reform (PostDis), 1995 — 1998**
  - Disclosure Reform took full effect on 1 January 1995, so we date the beginning of the post-Disclosure regime to that date as Life Offices had time to prepare for operating under that regime. This regime ends in 1998, as Stakeholder Pension and Comparative Tables reforms began late 1998/1999, and RU64 was introduced in 1999;
  - In Sample years: 1997 — 1998; Out of Sample years: 1995 — 1996. Here we want to see if Disclosure Reform mattered, so we estimate the PostDis cost function with data from 1997 and 1998 (when Disclosure Reform would have whatever impact it was going to have);

- **Post-StakeholderRU64/Comparative Tables (PostStake), 1999 — 2006**
  - This regime begins with the Stakeholder Pension, RU64, and Comparative Tables reforms in 1999, and no other major reform occurs before the end of our sample
period in 2006;

○ In Sample years: 2003 — 2006; Out of Sample years: 1999 — 2002. Here we want to see if the Life Office cost function shifts down to a new PostStake cost function in 1999, so we estimate the PostStake cost function with data from 2003 to 2006 to see if the 1999 to 2002 period is best described by the PostDis (1997 — 1998) or the PostStake cost function.

We summarize these regimes in Table 1.

C. Data

C.1 Defining an Observation

We count as an observation a Life Office/Year combination, so a Life Office that operates in 10 sample years will appear as 10 observations in the analysis (one for each year).

C.2 Data Source

Each Life Office authorized by the FSA to operate in the UK must make annual filings with the FSA that disclose information relating to its financial soundness and business activities. These filings are known as FSA Returns.¹³ SynThyses Life (a division of Standard & Poors) collects these forms and puts them into electronic form, and we obtain the data we use in the analysis below from this source.

Life Offices offer highly intermediated investment products in the form of policies. The funds that investors provide to Life Offices to manage via these policies are premiums. A Life Office reports information on the number of new policies it sells during the year and the premiums it receives on these policies. A Life Office splits its activities and its costs between an Industrial Branch and an Ordinary Branch. Industrial Branch business consists of very small policies sold door to door. These policies are designed to help people save for events such as weddings or funerals. The Ordinary Branch deals with mainstream retail investing. While the Industrial Branch business was important historically, it now constitutes a trivial proportion of both total premiums raised and total FUM (James (2000)). Given the small size of this business and the fact that the Industrial Branch operates in an economically distinct market, we focus our analysis here upon the Ordinary Branch business.

Each Life Office’s FSA Return consists of a number of Forms, and the Forms are organized into (where necessary) Columns and Lines. When giving the source of a data item below, we shall refer to its FSA Return location by its Form, Column, and Line (for example, F46, C2, L12 refers to Form 46, Column 2, Line 12).
Table 1

Regulatory Regimes

We divide our sample period into regulatory regimes. We assume that a regime remains in place until a major reform occurs. The Pre-Disclosure Reform (PreDis) regime begins with the implementation of the Financial Services Act in 1988 and ends in 1991 as the effort to bring about Disclosure Reform begins in 1992. The Post-Disclosure Reform (PostDis) regime begins in 1995 with the full implementation of Disclosure Reform and ends in 1998. The Post-Stakeholder/RU64/Comparative Tables (PostStake) regime begins in 1999 when the Stakeholder Pension and Comparative Tables reforms begin and when RU64 comes into force. No additional major reforms occur between 1999 and the end of the sample period in 2006. To allow for the possibility that the Life Office cost function shifts within what we consider to be a stable regime, we further divide each regime into "In Sample" and "Out of Sample" years and we estimate the regime cost function with only its In Sample years.

<table>
<thead>
<tr>
<th>Regime (Short Form)</th>
<th>Pre–Disclosure Reform (PreDis)</th>
<th>Post–Disclosure Reform (PostDis)</th>
<th>Post–Stakeholder (PostStake)</th>
</tr>
</thead>
</table>
C.3 Variables

In the analysis below we estimate a Life Office’s cost as a function of its FUM and its new business. We therefore need measures of cost, FUM, and new business. Consider each parameter in turn.

We label a Life Office’s cost in year $Y$ as $Cost_{j,Y}$. In consultation with actuaries at the FSA, we set a Life Office’s Cost equal to its reported Total Expenses ($F_{40}, L_{22}$). Total Expenses is the FSA Return reporting item that corresponds to operating costs, and it is essentially equal to the cost of acquiring new business (including commissions paid to IFAs) and managing the existing business.\(^\text{14}\) It does not include items which could be thought of as costs from the Office’s perspective but not from the investor’s perspective (e.g., policy claims).

We set $FUM_{j,Y}$ equal to the average value of Life Office $j$’s funds under management in year $Y$. FSA Returns contain data on the value of $FUM$ at the beginning ($F_{40}, L_{29}$) and end ($F_{40}, L_{59}$) each year. So, we set $FUM$ equal to the mean of these two values.

Turning to new business, we note that FSA Returns divide a Life Office’s new business into two business lines, Life and Pension (“Pen”). Each business line is further divided between two policy types, Single Premium (“SP”) and Regular Premium (“RP”). A Regular Premium policy is one for which an investor commits to investing a given amount each period, while a Single Premium policy is a one-off investment. To take both business volumes and business mix into account, we construct four new business variables: $RPLife$ ($F_{46}, L_{21}, C_{1}$), $SPLife$ ($F_{46}, L_{25}, C_{1}$), $RPPen$ ($F_{46}, L_{21}, C_{2}$), and $SPPen$ ($F_{46}, L_{21}, C_{2}$).\(^\text{15}\)

When comparing the size of Single Premium to Regular Premium business, industry observers generally treat a Single Premium policy as a Regular Premium policy with a 10 year span (about the industry average) and so divide the Single Premium business by 10. This practice began when Life Offices did primarily Regular Premium business, so it made sense to convert Single Premium policies into Regular Premium policies for purposes of comparison. However, this practice is misleading as it understates the expected present discounted value of the premiums a Life Office will receive as a result of the new business it obtains in a given year. It makes more sense to instead multiply Regular Premium business by 10. Taking the later approach, we multiply $RPPen$ and $RPLife$ by 10 and use these adjusted values in the analysis below.

We convert all of the monetary variables (which are reported in nominal terms) into constant 2008 GBP.
D. Sample

D.1 Selection Criteria

The government and the regulators aimed the reforms we analyze here at improving price competition in the retail market. To properly assess the impact of these reforms, then, we limit our sample to retail oriented Life Offices.\textsuperscript{16} And, among retail oriented Life Offices, we drop the smallest quartile of Offices (sorted by $FUM$). These small Offices manage an average of only 5\% of total $FUM$ in each sample year while accounting for 25\% of the observations, so including them would give the smaller Offices a disproportionate influence on the regression analysis. We also restrict our sample to Life Offices that were active throughout the year and to those whose FSA Returns satisfy minimal internal consistency and plausibility requirements.\textsuperscript{17}

We include Life Office $j$ in the sample in year $Y$ if it passes the following screens:

- **Retail Oriented**
  
  $\quad$— Average New Policy Size < £100,000;

  Note: Average New Policy Size equals $Total \ New \ Premiums/Number \ of \ New \ Policies$. We experimented cutoff values ranging from £25,000 to £1,000,000 for the Average New Policy Size defining a retail-oriented Life Office, and the precise cutoff value we chose did not alter the results below.

- **Not Too Small**
  
  $\quad$— $FUM$ > £133 Million;

- **Active Throughout the Year**
  
  $\quad$— $FUM$ > 0 at the beginning and end of the year;

  $\quad$— $Cost$ > Expenditure on Acquiring New Business > 0;

  $\quad$— $Total \ New \ Premiums$ > 0;

- **Plausible EER**
  
  $\quad$— 0.5\% < $EER$ < 7.5\%;

Note: In order to provide investors with a positive return then a Life Office’s $EER$ must be less than the return on its portfolio, and 7.5\% is a plausible (if not optimistic) long run rate of return. Hence, if data in a Life Office’s FSA Return implies an $EER$ of greater than 7.5\% (and some imply $EER$s in excess of 100\%), the data in the Return
may be in error. Similarly, an active retail-oriented Life Office would find it difficult to operate on an EER of less than 0.5%, so here too figures that imply an EER of less than 0.5% may be in error. Again, though, including Life Offices with EERs that fall outside of this range in the analysis below does not materially alter our results.

The Financial Services Act establishing a new set of regulatory arrangements for the Life Office market came into effect in 1988, so we begin our sample period in that year. We end our sample period in 2006 (the last year of data available when we began the study).

**D.2 Sample Overview**

To provide an overview of developments in the Life Office market over the sample period, we begin by looking at the overall trends in cost, number of offices, funds under management, and the composition of new business. We then look at these developments on the Life Office level.

Since the focus of our analysis is on the cost of investing, we first plot the Mean EER, Median EER, and WAvEER (Figure 1). The Mean EER is the simple average of the EERs of all Life Offices in the sample for that year. The Median EER is obtained by sorting the sample Life Office EERs for each year from highest to lowest and taking the middle value. WAvEER is the weighted average of the EERs of the Life Offices in the sample, with each Office’s weight equal to its share of total FUM in that year.

All three measures provide a consistent picture of the evolution of the cost of investing over the 1988 to 2006 period. Focusing upon the median EER and the WAvEER, which are much more robust measures than the mean,\textsuperscript{18} we see that the cost of investing shows a particularly steep decline during the PreDis to PostDis transition period, and then a more gradual decline in and between the PostDis and PostStake regimes.

The number of Life Offices in the sample drifted up between 1988 and 1994, and has then consistently fallen (Figure 2). By 2006 the number of Offices had fallen by about 50% from its peak 1994 value. This fall was due in part to M&A activity in the Life Office market.

Total funds under management by sample Life Offices more than doubled (in constant GBP) between 1988 and 1999/2000 before falling between 2000 and 2006 (Figure 3). The sharp fall in the number of Life Offices between 1994 and 2006 (Figure 2) combined with the significant increase in total funds under management implies that the average funds under management per office has increased significantly over the sample period. It will therefore be important to consider the implications of the increasing scale of Life Offices over time in the analysis below. *Total New Premiums* (Figure 3) have been relatively more constant over the sample period.
Figure 1
The Cost of Investing

Median EER  Mean EER  WAvEER

Note: A Life Office’s EER equals its Costs/FUM, where a Life Office’s FUM equals its funds under management. The WAvEER is the weighted average of the EERs of the individual Life Offices operating in that year, with each Life Office’s weight equal to its share of total FUM in that year. The Life Offices in the sample in a given year consists of retail oriented Life Offices (average policy size < £100,000) that pass the following screens: FUM > £133 Million; FUM > 0 at the beginning and end of the year; Cost > expenditure on acquiring new business > 0; Total New Premiums > 0; and 0.5% < EER < 7.5%. These screens left 1676 Life Office/Year observations in the sample (a given Life Office can appear in the sample for each year that it passes the above screens). The sample period consists of the years 1988 (the implementation of the Financial Services Act) to 2006 (the last year of available data when this project started). The PreDis (Pre-Disclosure Reform) regime begins in 1988 with the implementation of the Financial Services Act and ends in 1991, the last year before the effort to reform price disclosure began in earnest. The PostDis (Post-Disclosure Reform) regime begins in 1995 with the full implementation of Disclosure Reform and ends in 1998, the last year before Stakeholder Pensions and Regulatory Update 64 (RU64). The PostStake (Post-Stakeholder/RU64/Comparative Tables) regime begins in 1999 when RU64 required any advisor recommending a personal pension to compare that pension to a Stakeholder (though Stakeholders did not become available until 2001). The Life Office data is obtained from regulatory filings Life Office must make with the FSA now (FSA Returns) or with the DTI previously (DTI Returns), as collected by SynThyses Life.
Figure 2
Sample Life Offices By Year

Note: See Table 1 for the sample definition.
While *Total New Premiums* have been relatively constant over the sample period compared to *FUM* or the number of Life Offices, the composition of new business has changed considerably (Figure 4). The proportion of Regular Premium business has fallen from over 80% of *Total New Premiums* in 1988 to 40% in 2006. Business has also shifted away from Life towards Pension. Our discussions with industry experts revealed the belief that Single Premium business is less costly to acquire than Regular Premium business, so this business trend will have the potential to drive down the cost of investing independent of regulatory reform. We therefore control for the potential effects of changing business composition in the analysis below.

Turning now to developments at the level of the individual Life Office, we report summary statistics on *Cost, FUM* and business composition (*SPLife, RPLife, SPPen, and RPPen*) for each regulatory regime (Table 2). As the overall trends in *FUM* (Figure 3) and the number of Life Offices (Figure 2) implied, the average *FUM* per Office increased by almost 400% from the Pre-Disclosure regime to the Post-Stakeholder/RU64/Comparative Tables regime. This trend in *FUM* suggests that scale effects might be important, so we incorporate scale effects in the regression analysis below.

The decline in the number of Life Offices (Figure 2) combined with the relatively constant level of *Total New Premiums* (Figure 3) has led to a significant increase in new business per Office over the sample period. As Figure 4 above suggested, new business has shifted from Life towards Pension, and from Regular Premium towards Single Premium over time.

The increasing levels of *FUM* and new business per Office has led to an increase in the average Cost per Office, but this increase has been less than proportional. Consequently, the typical Life Office *EER* has fallen over this period (Figure 1). We now turn to estimating the extent to which the cost of investing fell by more than one would have expected given these changes in Life Office scale and business composition.
Figure 3
Total Funds Under Management and Total New Premiums

<table>
<thead>
<tr>
<th>Year</th>
<th>£Billions</th>
</tr>
</thead>
<tbody>
<tr>
<td>1990</td>
<td>100</td>
</tr>
<tr>
<td>1995</td>
<td>200</td>
</tr>
<tr>
<td>2000</td>
<td>300</td>
</tr>
<tr>
<td>2005</td>
<td>400</td>
</tr>
</tbody>
</table>

Note: Total FUM (Funds Under Management) for a given year equals the sum of FUM for the sample Life Offices in that year. FSA Returns divide a Life Office’s new business into two business lines (Life and Pension) and two types (Single Premium and Regular Premium). A Regular Premium policy is one for which an investor commits to investing a given amount each period, while a Single Premium policy is a one-off investment. To put the value of new Regular Premium business and new Single Premium business on comparable terms, we multiply new Regular Premiums by 10 to arrive at a rough expect present discounted value of the premiums due to the Life Office from the new Regular Premium policies. Total New Premiums equals the sum of new Single Premiums and new Regular Premiums (adjusted as above) for sample Life Offices. All values are reported in constant 2008 GBP. See Table 1 for the sample definition.
Note: *FSA Returns* divide a Life Office’s new business into two business lines: Life and Pension ("Pen"). Each business line is further divided into two policy types: Single Premium ("SP") and Regular Premium ("RP"). We measure premiums due to each line/type combination: *RPLife, RPPen, SPLife, SPPen*. A Regular Premium policy is one for which an investor commits to investing a given amount each period, while a Single Premium policy is a one-off investment. To put the value of new Regular Premium business and new Single Premium business on comparable terms, we multiply new Regular Premiums by 10 to arrive at a rough expect present discounted value of the premiums due to the Life Office from its new Regular Premium policies. *Total New Premiums* equals the sum of new Single Premiums and new Regular Premiums (adjusted as above) for sample Life Offices. Each line/type’s proportion of new business equals that line/type’s new premiums divided by *Total New Premiums*. See Table 1 for the sample definition.
Table 2

Sample Summary Statistics

In this table we report summary statistics for the sample observations sorted by regulatory regime (see Table 1 for regime definitions and Figure 1 for the sample definition). *Cost* is a Life Office’s operating cost (including commissions paid to acquire new business), *FUM* is Funds Under Management, *SPLife (RPLife)* is the value of new Single (Regular) Premium Life premiums, *SPPen (RPPen)* is the value of new Single (Regular) Premium Pension premiums. To put new Regular Premium business onto comparable terms with new Single Premium business we multiply Regular Premium business by 10 so as to arrive at a rough expected present discounted value of the premium flow entailed by the Regular Premium policies sold in that year. All nominal values are reported in millions of constant 2008 GBP.

<table>
<thead>
<tr>
<th>Variable</th>
<th>PreDis Mean (St. Dev.)</th>
<th>PostDis Mean (St. Dev.)</th>
<th>PostStake Mean (St. Dev.)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cost</td>
<td>101.846 (160.205)</td>
<td>111.25 (154.867)</td>
<td>169.013 (276.379)</td>
</tr>
<tr>
<td>FUM</td>
<td>2646.4 (4140.17)</td>
<td>5230.29 (8282.69)</td>
<td>9063.99 (14204.6)</td>
</tr>
<tr>
<td>SPLife</td>
<td>65.705 (128.218)</td>
<td>162.157 (343.34)</td>
<td>318.669 (737.216)</td>
</tr>
<tr>
<td>RPLife</td>
<td>287.513 (471.56)</td>
<td>186.537 (250.51)</td>
<td>184.947 (303.165)</td>
</tr>
<tr>
<td>SPPen</td>
<td>98.077 (200.833)</td>
<td>191.913 (425.711)</td>
<td>363.476 (718.463)</td>
</tr>
<tr>
<td>RPPen</td>
<td>307.066 (582.643)</td>
<td>291.218 (673.802)</td>
<td>378.57 (891.162)</td>
</tr>
</tbody>
</table>
IV. The Evolution of the Cost of Investing

A. Regime Cost Functions

A.1 The Cost Function Estimates

We model a Life Office’s Cost as a function of its FUM and the value and composition of its new business. We think that the principal determinant of a Life Office’s Cost will be its FUM. As we discussed above, the net return a Life Office can provide to its investors as a whole equals its portfolio return minus its EER, which is Cost/FUM. As an Office increases its Cost (holding FUM constant) it can on the one hand devote more resources to attracting new business, but on the other hand the implied lower net return to investors will reduce new business. An Office will therefore find an optimal trade off between the business enhancing and business reducing effects of increasing EER to arrive at its optimal level of Cost (with the business reducing effects of increasing EER increasing in strength as the degree of price competition in the market increases). It follows that a Life Office’s optimal level of Cost will be roughly proportional of its FUM—with that proportion declining as the degree of price competition increases.

To refine that “roughly” we incorporate scale effects into our cost function as it might be the case that size conveys a competitive advantage. For example, a larger Life Office will have more existing customers and so might be able to get more “word of mouth” recommendations than a smaller Office. Hence, its optimal EER might be lower than a smaller Office’s. To capture this possibility we include two scale effect variables in our regressions: FUM75 and FUM90. FUM75 (FUM90) equals a Life Office’s FUM if that FUM is greater than £6 (£13) Billion, and equals 0 otherwise. We chose the £6 Billion threshold for FUM75 as it equals (to the nearest Billion) the 75th percentile of the value of FUM for sample observations, and the £13 Billion threshold for FUM90 as it equals (to the nearest Billion) the 90th percentile value of FUM for sample observations. The specification is very flexible as it allows for the possibility of no economies of scale (FUM75 = 0 and FUM90 = 0), increasing economies of scale (FUM75 < 0 and/or FUM90 < 0), or U-shaped economies of scale (FUM75 < 0 and FUM90 > 0).

A Life Office’s Cost may also be influenced by the value and composition of its new business, so we include SPLife, RPLife, SPPen, and RPPen into the specification.

The regression equation we estimate is:

\[
\text{Cost}_{j,Y} = \text{Constant} + \beta_1 \text{FUM}_{j,Y} + \beta_2 \text{FUM75}_{j,Y} + \beta_3 \text{FUM90}_{j,Y} + \beta_4 \text{SPLife}_{j,Y} + \beta_5 \text{RPLife} + \beta_6 \text{SPPen} + \beta_7 \text{RPPen}
\]
We estimate this equation separately for each regime using only each regime’s In Sample years (Table 3).

Inspecting the cost functions in Table 3, we note first that the $R^2$ for each regression is extremely high (between 0.89 and 0.95). That is, the regressions can explain almost all of the cross-sectional variation in Cost for the sample Life Offices. It follows that these cost functions should enable us to infer whether or not the cost of investing has changed over the sample period. Second, the coefficient estimates are in line with what we would expect given our discussion above of the determinants of Life Office costs. Comparing the path of WAvEER (Figure 1) with the coefficients on FUM, one can see that Life Office costs more or less equal a proportion of FUM, with that proportion falling over the sample period. This basic relationship is modified by scale effects, as we find that the expected Cost for larger life offices is a smaller proportion of their FUM than is the case for smaller Life Offices (though we do find some evidence for diseconomies of scale in the PostStake period). Furthermore, we generally find that Regular Premium business adds more to Cost than Single Premium business (though these effects are not large).

These cost function regressions establish that the cost of investing did change between regulatory regimes. We now turn to establishing the time at which the Life Office cost function changed more precisely and to measuring the magnitude of the differences in the cost of investing that these cost function changes brought about. But, before doing so, we first check the robustness of our regression specification.

**A.2 Robustness Test: Measuring Economies of Scale**

In our specification above we capture scale effects with our two scale variables $FUM_{75}$ and $FUM_{90}$, but of course capturing economy of scale effects in this manner is a bit arbitrary. To limit the possibility that the decline in the cost of investing that we observe is due to a mis-measured economies of scale effect, we experimented with four alternative methods. In the first we kept two size threshold variables but varied the cutoff levels. In the second we included only one size threshold variable and tried various cutoff levels. In the third we incorporated scale effects in a more continuous manner by including $FUM^2$ terms rather than threshold variables. And, in the fourth, we just dropped small Offices from the sample to see if the results we obtain below hold for just the larger Offices (we dropped the smallest 25%, the smallest 50%, and the smallest 75%). For each specification we repeated all of the analysis below to see if the precise manner in which we incorporated scale effects had any material effects upon our results, and we found that it did not make much difference. So, we are confident that the decline in the coefficient on FUM from regime to regime is not the result of not capturing economy of scale effects.
Table 3

Life Office Cost Functions

In this Table we report our OLS estimates of the Life Office cost function for the PreDis, PostDis, and PostStake regimes. The dependent variable is Cost. See Table 1 for variable definitions and summary statistics. FUM75 (FUM90) equals FUM if FUM > £6 (£13) Billion. We estimate the PreDis (PostDis) (PostStake) regression with observations from 1988 to 1990 (1997 to 1998) (2003 to 2006). We use the White correction for heteroskedasticity when estimating the standard errors. A * (**) (***)) denotes statistical significance at the 1% (5%) (10%) level.

<table>
<thead>
<tr>
<th>Variable</th>
<th>PreDis</th>
<th>PostDis</th>
<th>PostStake</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>-4.645***</td>
<td>-12.59**</td>
<td>-11.25</td>
</tr>
<tr>
<td>(2.375)</td>
<td>(4.898)</td>
<td>(7.912)</td>
<td></td>
</tr>
<tr>
<td>FUM</td>
<td>0.043*</td>
<td>0.026*</td>
<td>0.017*</td>
</tr>
<tr>
<td>(0.005)</td>
<td>(0.004)</td>
<td>(0.003)</td>
<td></td>
</tr>
<tr>
<td>FUM75</td>
<td>-0.006*</td>
<td>-0.005***</td>
<td>-0.004</td>
</tr>
<tr>
<td>(0.002)</td>
<td>(0.003)</td>
<td>(0.003)</td>
<td></td>
</tr>
<tr>
<td>FUM90</td>
<td>—</td>
<td>-0.005*</td>
<td>0.003***</td>
</tr>
<tr>
<td>(—)</td>
<td>(0.002)</td>
<td>(0.002)</td>
<td></td>
</tr>
<tr>
<td>SPLife</td>
<td>-0.085***</td>
<td>0.051</td>
<td>0.007</td>
</tr>
<tr>
<td>(0.045)</td>
<td>(0.035)</td>
<td>(0.008)</td>
<td></td>
</tr>
<tr>
<td>RPLife</td>
<td>0.048**</td>
<td>0.115**</td>
<td>0.135*</td>
</tr>
<tr>
<td>(0.019)</td>
<td>(0.047)</td>
<td>(0.046)</td>
<td></td>
</tr>
<tr>
<td>SPPen</td>
<td>-0.034</td>
<td>-0.023</td>
<td>0.005</td>
</tr>
<tr>
<td>(0.043)</td>
<td>(0.015)</td>
<td>(0.031)</td>
<td></td>
</tr>
<tr>
<td>RPPen</td>
<td>-0.015</td>
<td>-0.032</td>
<td>0.01</td>
</tr>
<tr>
<td>(0.01)</td>
<td>(0.022)</td>
<td>(0.027)</td>
<td></td>
</tr>
<tr>
<td>R²</td>
<td>0.95</td>
<td>0.89</td>
<td>0.91</td>
</tr>
</tbody>
</table>
A.3 Robustness Test: Pooled Cross-Section or Panel Specification?

An important assumption we make in our specification above is that we can treat each Life Office/year observation as independent. We therefore estimate the parameters for each regime by pooling together all of our observations into a single pooled cross-section regression. One could instead assume that a Life Office's Cost is a function of both the observable variables that we include in the cost functions above and a Life Office specific factor which remains constant for each Life Office (but which may vary across Life Offices). To capture these Life Office specific effects, one would estimate the cost function using a fixed effect panel specification (essentially, one adds a dummy variable for each Life Office to the regression). We therefore experimented with a panel specification to see if it was the appropriate choice.

Before turning to the analysis, it may seem obvious that the most logical way to analyze the data is to use a fixed effects panel since it is impossible to rule out the possibility of Life Office specific effects in advance. However, this approach potentially suffers from two major drawbacks. First, even if it is in fact true that the observable variables alone largely determine Cost, if the differences in Cost between Life Offices are large but the year to year variation in Cost and the observable variables for a given Life Office is small, a panel estimate may falsely attribute the between Office variation in Cost to Life Office specific effects (in other words, the individual Life Office dummy variables will seem to explain most of the variation in Cost). Second, if what one counts as a single Life Office for purposes of the panel in fact undergoes a significant change over the estimation period (e.g., due to a merger or a strategy change), the impact of the change on Cost might lead to a (false) statistically significant Life Office specific effect while simultaneously distorting the estimate of the impact of the observable variables. So, it is possible that one could find statistically significant fixed effects (with a distortionary impact upon observed variable parameter estimates) when in fact fixed effects are not there. Consequently, using a panel estimate could lead to highly misleading results.

Ideally, we would decide which specification was superior on the basis of its ability to predict the cost of investing "out of sample". That is, if the panel really does capture important effects that the pooled cross-section leaves out, then the panel should do a better job of predicting Cost outside of the estimation period. Unfortunately, the short 4 year span of the PreDis regime is not long enough to test the pooled cross-section against the panel in this manner. So, instead, we evaluate the panel for the PreDis regime as follows. First, we restrict the sample to large Life Offices that were present in each year of the PreDis regime (it is more likely that we will find a stable fixed effect for a more stable large Office). Second, we split the PreDis years into a 1988/1989 group and a 1990/1991 group and estimate the cost function above adding in fixed effects for each group separately. If Life Office specific fixed effects are in fact present in the data, then the "fixed effect" for Life Office j in the 1988/1989 period should equal the "fixed effect" for that Life Office in the 1990/1991 period (that's what "fixed" means). To see if this relationship holds, we regress the 1988/89 fixed effects on the 1990/91 fixed effects. We find that the "fixed
effect” of a given Life Office in the 1988/1989 period contains essentially no information about the "fixed effect" for that Life Office in the 1990/1991 period, so we conclude that including "fixed effects" in the PreDis cost function is inappropriate (see the Appendix for the details of this analysis).

We have a longer run of data for PostStake regime (1999 to 2006), so here it is possible to directly compare the out of sample predictive power of the panel and pooled cross-section specifications. To do so we again restrict the sample to large Life Offices that operated throughout this period. We estimate the two cost functions using data from 2003 to 2006, and we then use each cost function to calculate a predicted WAvEER for the years 1999, 2000, 2001, and 2002. We find that the pooled cross-section outperforms the panel, so we conclude that the pooled cross-section is the superior specification (see the Appendix for details of this analysis).

In light of this analysis, then, we base our analysis of the cost of investing upon the pooled cross-section cost functions estimated above.

**B. When Does the Cost Function Shift?: Market Level Data**

**B.1 Dating the Cost Function Shift**

Our first method to identify when a cost function shifts is to compare the actual cost of investing to that predicted by the regime cost functions we estimated above. Our idea here is that each of the estimated cost functions will do a good job of tracking the actual cost of investing for their In Sample years. So, if the true cost of investing function does not change between a regime’s In Sample years and a given year Y, then that regime’s cost function will also do a good job of tracking the actual cost of investing in year Y. If, on the other hand, the actual cost of investing differs considerably from that implied by a regime cost function, then we can conclude that the true cost function has shifted away from that regime cost function.

To carry out this analysis, note that the cost of investing predicted by a given cost function for a given year depends upon both the cost function and the characteristics of the Life Offices in the sample in that year. To compare the actual and predicted cost of investing, then, we compute the cost of investing implied by the three cost functions we estimated above as follows. For each Life Office observation we calculate the Cost implied by each of the three cost functions given that Office’s characteristics, yielding Cost\(_{j,Y,\text{PreDis}}\), Cost\(_{j,Y,\text{PostDis}}\), and Cost\(_{j,Y,\text{PostStake}}\) where Cost\(_{j,Y,R}\) is the implied Cost of Life Office \(j\) in year \(Y\) given regime \(R\)’s cost function. Then for each sample year we calculate WAvEER\(_{Y,\text{PreDis}}\), WAVEER\(_{Y,\text{PostDis}}\), and WAvEER\(_{Y,\text{PostStake}}\), with
\[ W_{AvEER_{Y,\text{Regime}}} = \frac{\sum j \ Cost_{j,Y,\text{Regime}}}{\sum j \ FUM_{j,Y}} \]  

We begin this transition analysis by plotting \( W_{AvEER_{Y,\text{Actual}}} \), \( W_{AvEER_{Y,\text{PreDis}}} \), \( W_{AVEER_{Y,\text{PostDis}}} \), and \( W_{AvEER_{Y,\text{PostStake}}} \) (Figure 5). Inspecting this plot reveals that \( W_{AvEER_{Actual}} \) initially tracks \( W_{AvEER_{\text{PreDis}}} \) and then switches to tracking \( W_{AvEER_{\text{PostDis}}} \) and then switches one more time to tracking \( W_{AvEER_{\text{PostStake}}} \). Let us examine each of these transitions in turn.

We start with the PreDis to PostDis transition (Figure 6). We estimate the PreDis cost function with data from 1988, 1989, and 1990. Examining how well this cost function predicts the cost of investing for the Out of Sample years, we see that the cost of investing it implies matches the actual cost of investing almost exactly in 1991. It follows that the Life Office cost function in 1991 is the same as that in 1988 to 1990. From 1992 onwards, however, the gap between the cost of investing implied by the PreDis cost function and the actual cost of investing increases rapidly. We conclude that the Life Office cost function begins to shift in 1992.

In 1992, 1993, and 1994 the actual cost of investing lies between that implied by the PreDis cost function and that implied by the PostDis cost function. The PostDis cost function is estimated with data from 1997 and 1998. Working backwards, we see that this cost function does an excellent job of matching the actual cost of investing in 1996 and a good job of matching the actual cost of investing in 1995. From 1994 and before, though, the gap between the cost of investing implied by the PostDis cost function and the actual cost of investing increases rapidly as well. Hence, we conclude that Life Offices operated under the same cost function between 1995 and 1998, and under a different cost function for the years 1994 and before.

Turning now to the PostDis to PostStake transition (Figure 7), consider first the situation at the end of the PostDis regime (1997 and 1998). Here the PostDis cost function tracks the actual cost of investing almost perfectly (\( W_{AvEER_{Actual}} \approx W_{AvEER_{\text{PostDis}}} \)), while the actual cost of investing exceeds what implied by the PostStake cost function (\( W_{AvEER_{Actual}} > W_{AvEER_{\text{PostStake}}} \)). In 1999 and 2000 this situation reverses itself: the PostStake cost function predicts the actual cost of investing almost perfectly (\( W_{AvEER_{Actual}} \approx W_{AvEER_{\text{PostStake}}} \)) while the cost of investing predicted by the PostDis cost function exceeds the actual cost of investing (\( W_{AvEER_{\text{PostDis}} > W_{AvEER_{Actual}}} \)). Since the PostDis cost function is estimated with data from 2003 to 2006, this Figure implies that: 1) the Life Office cost function in 1999 and 2000 was the same as that in 2003 to 2006; and 2) the Life Office cost function in 1995 to 1998 was different from that in 1999 to 2006.
Figure 5
The Cost of Investing Under Different Cost Functions

Note: In this figure we plot the actual cost of investing ($W_{avEER_{Actual}}$) and the cost of investing implied by the three regime cost functions in Table 3, $W_{avEER_{PreDis}}$, $W_{avEER_{PostDis}}$, and $W_{avEER_{PostStake}}$. To calculate the cost of investing implied by regime R's cost function we first calculate the $Cost_{j,Y,R}$ implied by R's cost function for each Life Office $j$ in each year $Y$. It follows that

$$W_{avEER_{Y,R}} = \frac{\sum Cost_{j,Y,R}}{\sum FUM_{j,Y}}$$
Note: In this Figure we plot the actual cost of investing \((W\text{AvEER}\text{Actual})\) and the cost of investing implied by the PreDis \((W\text{AvEER}\text{PreDis})\) and the PostDis \((W\text{AvEER}\text{PostDis})\) cost functions. See Table 3 for the cost function estimates and Figure 5 for how to construct a cost of investing given a cost function estimate. The PreDis regime spans the years from 1988 to 1991 and the PreDis cost function is estimated with data from 1988 to 1990 (indicated by a blue "In Sample"). The PostDis regime spans the years 1995 to 1998, and the PostDis cost function is estimated with data from 1997 and 1998 (indicated by an orange "In Sample").
Figure 7
Stakeholder Pensions/RU64/Comparative Tables

\[
\begin{array}{c}
W_{\text{AvEER}}^{\text{Actual}} \quad W_{\text{AvEER}}^{\text{PreDis}} \quad W_{\text{AvEER}}^{\text{PostDis}} \\
\end{array}
\]

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<tr>
<td>EERs</td>
<td></td>
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Note: In this Figure we plot the actual cost of investing \((W_{\text{AvEER}}^{\text{Actual}})\) and the cost of investing implied by the PostDis \((W_{\text{AvEER}}^{\text{PostDis}})\) and the PostStake \((W_{\text{AvEER}}^{\text{PostStake}})\) cost functions. See Table 3 for the cost function estimates and Figure 5 for how to construct a cost of investing given a cost function estimate. The PostDis regime spans the years from 1995 to 1998 and the PostDis cost function is estimated with data from 1997 and 1998 (indicated by a orange "In Sample"). The PostStake regime spans the years 1999 to 2006, and the PostStake cost function is estimated with data from 2003 and 2006 (indicated by a yellow "In Sample").
B.2 The Statistical Significance of the Cost Function Shifts

Our results thus far suggest that the cost of investing shifted down from the PreDis to the PostDis regime and from the PostDis to the PostStake regime. However, we know that the cost functions we use to make these inferences are estimated with sample data, and that sample data contains noise. Thus, it is possible that these findings are due to a particular set of random shocks influencing our cost functions rather than to the Life Office cost function actually shifting down. To judge the extent to which our results may be due to the influence of random shocks in the data, we estimate the plausible range of:

- \( \Delta_{\text{PreDis/Y9598}} \): the average difference between the cost of investing implied by the PreDis cost function and actual cost of investing for the years 1995 to 1998 (the PostDis regime);

- \( \Delta_{\text{PreDis/Y9906}} \): the average difference between the cost of investing implied by the PreDis cost function and the actual cost of investing for the years 1999 to 2006 (the PostStake regime);

- \( \Delta_{\text{PostDis/Y9906}} \): the average difference between the cost of investing implied by the PostDis cost function and the actual cost of investing for the years 1999 to 2006; and

- \( \Delta_{\text{PostStake/Y9598}} \): the average difference the cost of investing implied by the PostStake cost function and the actual cost of investing for the years 1995 to 1998.

We illustrate these parameters in Figure 8.

While the plausible range of a regression coefficient can simply be taken from its estimated standard error, there is no such simple solution to determing the plausible range of the cost of investing in a given year as it is a function of both all of the parameters in the cost function at once and of the characteristics of the Life Offices in that year. In these situations, the standard solution is to estimate this plausible range with a "bootstrap" (Efron and Tibshirani (1994)).

The intuition behind the bootstrap is this: imagine that one could rewind the clock to 1988 and generate a new set of Life Office data (with a new set of random shocks). One would then estimate the cost functions as above for this new set of data and calculate the implied cost of investing for each one. After repeating this experiment numerous times, one would have the range of the cost of investing implied by each cost function given the distribution of shocks that hit Life Office Costs. With this data one can then compute the plausible range of the gap between the cost of investing implied by a given cost function and the actual cost of investing.
Figure 8
Measuring Statistical Significance

Note: In this Figure (not drawn to scale) we show the parameters we use to investigate the statistical significance of the difference between the cost of investing implied by the PreDis, PostDis, and PostStake cost functions and the actual cost of investing.
Of course, we can not rewind the clock. But, we can do the next best thing, which is to create new samples by making random draws with recall from our original sample ("with recall" means that each draw is independent, so in a given new sample one might draw Life Office 16 three times and Life Office 29 not at all). The idea here is that each Life Office’s Cost in our original sample equals its expected Cost plus a random shock. Our sample therefore provides a good estimate of the distribution of the random shocks that hit Life Office costs. By drawing a new sample from our original sample we therefore get a sample with a different set of random shocks, and by repeating this process numerous times we can estimate the plausible range of the cost of investing implied by a regime’s cost function.

So, to calculate the plausible range of the average difference between the cost of investing implied by the regime cost functions and the actual cost of investing, we draw 10,000 random samples (with each sample a "trial") with recall from our original sample. For each trial we: 1) estimate PreDis, PostDis, and PostStake cost functions as above using the data from that trial; 2) compute the WAvEERs implied by each of these trial cost functions for each year using the observations in the original sample; and 3) calculate Δ’s we defined above. This process produces 10,000 values for each Δ. We then sort these values for each Δ, and we take this sorted list of Δ values to be the distribution of Δ. We then measure the plausible range of each Δ by the bounds of the middle 50% of the distribution (the interquartile range) and the middle 95% (the 95% confidence interval). We report these plausible ranges in Table 4.

The results we report in Table 4 demonstrate that the cost of investing did fall between the PreDis regime and the PostDis and PostStake regimes. To focus on the PreDis to PostStake comparison (we find similar results for the PreDis to PostDis comparison), the point estimate of \( \Delta_{\text{PreDis,Y9906}} \) is 146 bp. In other words, we estimate that, between 1999 and 2006, the actual cost of investing was 146 bp lower than it would have been if Life Offices had continued to operate on the PreDis cost function instead of shifting to the PostStake cost function. The 95% confidence interval on this 146 bp estimate is 92 bp to 183 bp. So, while the true magnitude of the impact of the shift in the cost of investing between the PreDis and PostStake regimes could have been a bit larger or a bit smaller than our point estimate of 146 bp, it is almost certainly the case that the fall in the cost of investing that we observe was due to a shift in the Life Office cost function rather than to a large random shock.
Table 4

The Statistical Significance of Cost Function Shifts

In this table we report the plausible range of the average difference between the cost of investing implied by the regime cost functions estimated in Table 3 and the actual cost of investing for the PostDis (1995 to 1998) and the PostStake (1999 to 2006) periods. In particular, we estimate this range for: the PreDis cost function for the PostDis period ($\Delta_{\text{PreDis,Y9598}}$); the PreDis cost function for the PostStake period ($\Delta_{\text{PreDis,Y9906}}$), the PostStake cost function for the PreDis period ($\Delta_{\text{PostStake,Y9598}}$); and the PostDis cost function for the PostStake period ($\Delta_{\text{PostDis,Y9906}}$). We estimate these plausible ranges using a bootstrap as follows: 1) we draw (with recall) 10,000 random samples from our original sample (with each draw a "trial"); 2) we estimate a PreDis, PostDis, and PostStake cost function for each trial as in Table 3; 3) we compute WAVEER implied by each trial cost function for each year using the data from the original sample; and 4) we calculate the $\Delta$'s above. The process yields 10,000 values for each $\Delta$. We sort the values for each $\Delta$ in order, and we take this ordered list to be the $\Delta$ distribution. The interquartile range of the distribution is then equal to the bounds on the middle 50% of this list, and the 95% confidence interval is equal to the bounds on the middle 95% of this list. The point estimate of each is calculated using the cost functions in Table 3.

<table>
<thead>
<tr>
<th>Point Estimate</th>
<th>Interquartile Range</th>
<th>95% Confidence Interval</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\Delta_{\text{PreDis,Y9598}}$</td>
<td>133 bp</td>
<td>116 bp — 140 bp</td>
</tr>
<tr>
<td>$\Delta_{\text{PreDis,Y9906}}$</td>
<td>146 bp</td>
<td>130 bp — 158 bp</td>
</tr>
<tr>
<td>$\Delta_{\text{PostStake,Y9598}}$</td>
<td>-20 bp</td>
<td>-14 bp — 29 bp</td>
</tr>
<tr>
<td>$\Delta_{\text{PostDis,Y9906}}$</td>
<td>4 bp</td>
<td>-9 bp — 15 bp</td>
</tr>
</tbody>
</table>
This evidence provides more mixed support for a cost-function shift between the PostDis and PostStake regimes. We investigate this shift from both directions. Looking first at $\Delta_{\text{PostStake}, Y_{9598}}$, we find that the point estimate of the difference between the cost of investing implied by the PostStake cost function and the actual cost of investing between 1995 and 1998 is -20bp (that is, the actual cost of investing is higher), with a 95% confidence interval of -43 bp to 1 bp. So, the cost of investing in the PostDis regime was higher than it would have been if Life Offices had been operating on the PostStake cost function. On the other hand, while the point estimate of $\Delta_{\text{PostDis}, Y_{9906}}$ is positive at 4 bp, this point estimate is not statistically significant as its 95% confidence interval extends well into negative numbers. This evidence does not allow us to be confident that the lower cost of investing in the PostStake regime was due to a shift in the Life Office cost function. This lack of statistical significance could be due to either the fact that the Life Office cost function did not shift down or to the fact that this test is not powerful enough to identify real if relatively small shifts in the cost of investing. We therefore turn now to investigating the timing of Life Office cost function shifts using individual Life Office level data to see if we can illuminate this matter further.

Before doing so, we note that we carried out our bootstrap estimates of the confidence intervals using two other specifications as a robustness check. In the bootstrap above we drew a trial sample by drawing a new random sample with recall from the original sample. A trial sample constructed in this way may not have the same size distribution or time distribution of the original sample, and so may produce misleading cost functions. To make sure that this was not the case, we also ran bootstraps in which we drew a trial sample by: 1) drawing a random sample with recall from each sample year, and then combining these 19 samples into a trial before proceeding as above; and 2) sorting the sample firms into size quartiles and then drawing with recall from each quartile. These two approaches produced the same results as above.

C. When Does the Cost Function Shift?: Office Level Data

C.1 The Test: Intuition

Intuitively, it seems clear that if regime $Z$'s cost function implies higher costs than the true cost function in a given year, then the actual Cost of most (> 50%) Life Offices operating in that year will be less than that implied by the regime $Z$ cost function. If, however, a regime $Z$'s cost function is the true cost function in a given year, then the probability that a Life Office's actual cost exceeds that implied by the regime $Z$ cost function will equal 50% as its actual Cost will differ from that implied by the cost function by only a random shock which is as likely to be positive as negative. Let us refer to the probability that a Life Office's Cost $j, Y, Z > Cost_{j, Y, \text{Actual}}$ as $\pi_{Y, Z}$.

Now suppose that the true Life Office cost functions shifts down from a higher cost regime $W$ cost function to a lower cost regime $Z$ cost function at some point in time, and we want to identify the
time of the shift. What pattern should we expect to observe in \( \pi_{Y,Z} \)? Since it will initially be the case that the true cost function will imply higher Costs than the regime Z cost function, \( \pi_{Y,Z} \) will initially fall below 50%. At the point that the Life Office function shifts down to make the regime Z cost function the true cost function, \( \pi_{Y,Z} = 50\% \). So, it follows that one can detect when the true Life Office cost function shifts from a higher cost regime W cost function to a lower cost regime Z cost function by seeing when \( \pi_{Y,Z} \) increases to 50%.

We first formally derive this test to be sure that our intuition is correct, and then we apply this test to detect when the Life Office cost function shifted.

**C.2 The Test: Derivation**

We can think of a Life Office j’s Cost in a given year Y as being equal to:

\[
\text{Cost}_{j,Y,\text{Actual}} = \text{Cost}_{j,Y,\text{Expected}} + \gamma_{j,Y}
\]  

(4)

where \( \text{Cost}_{j,Y,\text{Expected}} \) is a Life Office’s expected Cost given the true Life Office cost function in year Y, and \( \gamma_{j,Y} \) is a random shock. We can also think of the relationship between Office j’s expected Cost and that implied the regime Z cost function (\( \text{Cost}_{j,Y,Z} \)) in the following manner:

\[
\text{Cost}_{j,Y,Z} = \text{Cost}_{j,Y,\text{Expected}} + \delta_{j,Y,Z}
\]  

(5)

where \( \delta_{j,Y} \) measures the gap between the Cost implied by regime Z’s cost function and that implied by the true cost function for that year (we note that our test here requires only that \( \delta_{j,Y,Z} \) have the same sign for all Life Offices in a given year, not that it has the same value). It follows that:

\[
\text{Cost}_{j,Y,Z} - \text{Cost}_{j,Y,\text{Actual}} = \text{Cost}_{j,Y,\text{Expected}} + \delta_{j,Y,Z} - \text{Cost}_{j,Y,\text{Expected}} - \gamma_{j,Y}
\]

\[= \delta_{j,Y,Z} - \gamma_{j,Y}
\]  

(6)

Let us now consider the \( \pi_{Y,Z} \). It follows from equation 6 that

\[
\pi_{Y,Z} = \text{Prob}[\text{Cost}_{j,Y,Z} - \text{Cost}_{j,Y,\text{Actual}} > 0] = \text{Prob}[\delta_{j,Y,Z} - \gamma_{j,Y} > 0]
\]  

(7)

We know that \( \gamma_{j,Y} \) is a random shock and so is as likely to be positive as negative, and we know that:

- \( \delta_{j,Y,Z} > 0 \) if the Cost implied by regime Z’s cost function exceeds that implied by the true cost
function;

- $\delta_{j,Y,Z} < 0$ if the Cost implied by regime Z's cost function is less than that implied by the true cost function; and

- $\delta_{j,Y,Z} = 0$ if the regime Z cost function is the true cost function.

Hence, it follows that:

$$\delta_{j,Y,Z} > 0 \Rightarrow \delta_{j,Y,Z} - \gamma_{j,Y} > \gamma_{j,Y} \Rightarrow \pi_{Y,Z} < 50\%$$

$$\delta_{j,Y} < 0 \Rightarrow \delta_{j,Y} - \gamma_{j,Y} < \gamma_{j,Y} \Rightarrow \pi_{Y,Z} > 50\%$$

If $\delta_{j,Y} = 0 \Rightarrow \delta_{j,Y} - \gamma_{j,Y} = \gamma_{j,Y} \Rightarrow \pi_{Y,Z} = 50\%$

As this formal analysis confirms our intuition, we now proceed to applying the test.

**C.3 Applying the Test**

We begin by splitting the sample in half by size (with size measured by \textit{FUM}) to explore the evolution of large Office and small Office Costs separately. We focus on the large firm sample as it contains over 90% of total sample \textit{FUM}, and so it is obviously these firms that drive the \textit{WAVETR} results above.

Beginning with the large Office sample, recall that we calculated the implied Cost for each Office for each year for the PreDis, PostDis, and PostStake cost functions. It is therefore a simple matter to compare these cost estimates with each Life Office's actual Cost in each year to compute $\pi_{Y,\text{PreDis}}, \pi_{Y,\text{PostDis}},$ and $\pi_{Y,\text{PostStake}}$. We plot these parameters in Figure 9.

The evidence in Figure 9 clearly demonstrates that the Life Office cost function shifted twice over the sample period. The first shift was from the PreDis cost function in 1991 to the PostDis cost function in 1995, and the second shift was from the PostDis cost function in 1998 to the PostStake cost function in 1999. Let us begin by considering the PreDis to the PostDis cost function shift.

We know that large Life Offices were operating on the PreDis cost function until 1991 as $\pi_{1991,\text{PreDis}} \approx 0.5$ and as $\pi_{1991,\text{PostDis}} < 50\%$ and $\pi_{1991,\text{PostStake}} < 50\%$ (we examine the statistical significance of these relationships in a moment). Our finding that $\pi_{1991,\text{PreDis}} \approx 0.5$ shows that a Life Office's actual Cost differed from that implied by the PreDis cost function by only a random shock, implying that there was no systematic difference between the PreDis cost function and the true Life Office cost function at that time. The fact that both $\pi_{1991,\text{PostDis}} < 50\%$ and $\pi_{1991,\text{PostStake}} < 50\%$ shows that a Life Office's \textit{Cost} had a very high probability of exceeding
that implied by the PostDis and PostStake cost functions, clearly demonstrating that these Life Offices in 1991 were not operating on either of these cost functions.

Life Offices began to shift away from the PreDis cost function in 1992, with the speed of the shift increasing markedly in 1993 and 1994 as demonstrated by the rapid increase in \( \pi_{Y, \text{PreDis}} \) for those years. By 1995, a Life's actual Cost had an 88% chance of being less than that implied by the PreDis cost function, so it is clear that the true Life Office function had shifted down by that year.

Looking at the evolution of \( \pi_{Y, \text{PostDis}} \), we can see that the shift from the PreDis to the PostDis cost function became complete in 1995, with much of the shift occurring between 1993 \( (\pi_{1993, \text{PostDis}} = 33\%) \) and 1995 \( (\pi_{1995, \text{PostDis}} = 52\%) \). Life Offices continued to operate on the PostDis cost function between 1995 and 1998, as demonstrated by the fact that \( \pi_{Y, \text{PostDis}} \approx 50\%, \pi_{Y, \text{PreDis}} > 50\% \), and \( \pi_{Y, \text{PostStake}} < 50\% \) for these years.

Between 1998 and 1999 \( \pi_{Y, \text{PostDis}} \) increases from 55% to 63%, indicating that the Life Office cost function is again shifting down. At the same time, \( \pi_{Y, \text{PostStake}} \) increases from 41% to 49%, and it remains at approximately 50% between 1999 and 2002, and also (with a bit more volatility) between 2003 and 2006. This evidence therefore suggest that the PostStake cost function became the Life Office cost function in 1999, and that it remained the Life Office cost function until the end of the sample period in 2006.

One possible objection to this analysis is that the mechanics estimating regime Z's cost function by OLS (as we do) more or less constrains \( \pi_{Y, Z} \) to equal 50% during the years of that regime. Intuitively, this critique goes: estimating a regression with OLS tends to put the regression line through the middle of the data, so it is not therefore much of a surprise that about 50% of the observations have a Cost higher than that implied by the regression equation.
Figure 9
The Evolution of Life Office Cost: Large Offices

Note: In this Figure we plot the probability that a large Life Office's actual cost is less than that implied by the PreDis, PostDis, and PostStake cost functions (Table 3), with a large Life Office observation being one with $FUM$ above the sample median of £1.795 Billion. We denote these probabilities by $\pi_{Y,\text{PreDis}}$, $\pi_{Y,\text{PostDis}}$, and $\pi_{Y,\text{PostStake}}$. We divide each regulatory regime into an "In Sample" and an "Out of Sample" period, and we use only observations from that regime's "In Sample" years to estimate its cost function.
Mindful of this possible problem, we note that we estimate a regime's cost function with only a subset of the years in that regime (the "In Sample" years) and we base our cost function shift analysis upon the other years (the "Out of Sample" years). To judge the extent to which this process protects us from the above critique, we indicate the In Sample and Out of Sample years for the PostDis and PostStake regime in Figure 9. As one can see, 1999 is fully 4 years away from the PostStake In Sample period of 2003 to 2006. Even if one supposes that \( \pi_{Y, \text{PostStake}} \) must converge to 50% in 2003 (which is not the case), this fact will in no way force \( \pi_{1999, \text{PostStake}} \) to equal 50% as \( \pi_{Y, \text{PostStake}} \) can move a great deal over a 4 year period (it was 21% four years earlier, for example). The PostDis regime did not last as long as the PostStake regime, so the first Out of Sample year of 1995 is not as far from that regime's In Sample years of 1997 and 1998. Still, even if one supposes that \( \pi_{Y, \text{PostDis}} \) must equal 50% in 1997, this fact does not force \( \pi_{1995, \text{PostDis}} \) to equal 50% two years beforehand (it was 33% in 1993 before hitting 50% in 1995, for example). So, we don't think that the mechanics of OLS estimation drive the cost function transition results.

The \( \pi_{Y,Z} \) estimates we have analyzed above are point estimates. To assess statistical significance of the comparisons we draw between specific \( \pi_{Y,Z} \) values and 50%, we take as our null hypothesis that \( \pi_{Y,Z} = 50\% \) and then see if the \( \pi_{Y,Z} \) we observe is too high or too low to be consistent with that hypothesis. To do to do so we create an indicator variable \( g_{j,Y,Z} \), with \( g_{j,Y,Z} = 1 \) if \( \text{Cost}_{j,Y,Z} > \text{Cost}_{j,Y,\text{Actual}} \) and \( g_{j,Y,Z} = 0 \) otherwise, and we assume that the realization of \( g_{j,Y,Z} \) is independent for each Life Office in each year. For each year for each cost function we sum the relevant \( g_{j,Y,Z} \) to yield \( G_{Y,Z} \). Under the null, then, the number of Life Offices for which the Cost predicted by a given cost function exceeds the actual Cost is binomially distributed (as is the number of heads one gets when flipping a coin). One can then assess the probability of observing a given value of \( G_{Y,Z} \) under the null. If \( G_{Y,Z} \) is too small to be consistent with the null (\( \text{Prob}[G \leq G_{Y,Z} \text{ given the null}] < 0.5\% \)), then we put \( \pi_{Y,Z} \) into the "< 50%" bin. If, on the other hand, \( G_{Y,Z} \) is too high to be consistent with the null (\( \text{Prob}[G \geq G_{Y,Z} \text{ given the null}] < 0.5\% \)), then we put \( \pi_{Y,Z} \) into the "> 50%" bin. If neither of these two cases holds, then we put \( \pi_{Y,Z} \) into the "= 50%" bin.

To illustrate this test, suppose that one flipped a coin 100 times and one wanted to know if the probability of getting a heads was greater than 50%. One would take as the null that the probability of getting a heads equals 50%. If one flips a fair coin 100 times the binomial distribution tells us that one should expect to observe between 37 and 62 heads (there is less than a 0.5% chance of observing 36 or fewer heads under the null, and there is also less than a 0.5% chance of observing 63 or more heads under the null). So, if one does observe 63 or more heads one would conclude that the probability of getting heads is greater than 50%. 
To apply this test to the PreDis/PostDis and PostDis/PostStake transitions, we organize the sample years into the following groups on the basis of the regime and In Sample/Out of Sample dates discussed above: 1989/90, 1991, 1992/1994, 1995/1996, 1997/1998, and 1999/2000. We sort the sample years into groups in this way for this test as the larger the number of observations in any one group the greater the chance of detecting that $\pi_{Y,Z} \neq 50\%$. We find that the analysis here confirms the cost function transition results above. That is, we find that:

- $\pi_{Y,\text{PreDis}}$ is not significantly different from 50% on or before 1991, and that it is significantly greater than 50% after 1991;

- $\pi_{Y,\text{PostDis}}$ is significantly less than 50% before 1995, not significantly different from 50% between 1995 and 1998, and significantly greater than 50% after 1998; and

- $\pi_{Y,\text{PostDis}}$ is significantly less than 50% before 1999 and not significantly different from 50% in 1999/2000.

We now turn briefly to consider the case of the smaller (below median size) Life Offices. We plot $\pi_{Y,\text{PreDis}}, \pi_{Y,\text{PostDis}},$ and $\pi_{Y,\text{PostStake}}$ as above for these Life Offices in Figure 11. We can see that small and large Offices operated on the same cost function in the PreDis regime ($\pi_{Y,\text{PreDis}} \approx 50\%$). However, the smaller Life Offices never managed to make the transition to the lower PostDis and PostStake cost functions, as witnessed by the fact that both $\pi_{Y,\text{PostDis}}$ and $\pi_{Y,\text{PostStake}}$ never reach 50%. To see what impact operating under relatively higher cost had, we also plot the proportion of total sample FUM managed by the small Offices for each year. We find that this proportion declined significantly over our sample period. This decline is what one would expect to observe given the cost of investing analysis here, and this finding lends additional support to this analysis.
Figure 10
The Evolution of Life Office Cost: Statistical Significance

\[ \pi_{Y,PreDis} \pi_{Y,PostDis} \pi_{Y,PostStake} \]

Probability

Note: In this figure we assign a \( \pi_{Y,Z} \) estimate into one of three bins. We put it into the "< 50%" ("> 50%") bin if it is statistically significantly less than (greater than) 50% at the 1% level, and we put it into the "= 50%" bin if it is neither statistically significantly less than or greater than 50%. We assess statistical significance by creating an indicator variable \( g_{j,Y,Z} \), with \( g_{j,Y,Z} = 1 \) if \( \text{Cost}_{j,Y,Z} > \text{Cost}_{j,Y,Actual} \) and 0 otherwise. We take as our null hypothesis that \( \text{Prob}[\text{Cost}_{j,Y,Z} > \text{Cost}_{j,Y,Actual}] = 50\% \), and we assume that the realization of \( g_{j,Y,Z} \) is independent for each Life Office for each year. For each year we sum up the relevant \( g_{j,Y,Z} \) to obtain \( G_{Y,Z} \). Under the null the \( G \) is distributed binomially with parameters \{number of sample Life Offices in Y, 0.5\}. We put \( \pi_{Y,Z} \) into the "< 50%" ("> 50%") bin if \( \text{Prob}[G \leq (\geq) G_{Y,Z}] < 0.5\% \), and into the "= 50%" bin otherwise. To apply the test we sort the sample years into groups on the basis of the regime and "In Sample"/"Out of Sample" dates discussed above, as a larger group increases our ability to detect if \( \pi_{Y,Z} \neq 50\% \).
Figure 11
The Evolution of Life Office Cost: Small Offices

\[ \pi_{Y,\text{PreDis}} \quad \pi_{Y,\text{PostDis}} \quad \pi_{Y,\text{PostStake}} \quad \text{SmallFUM} \]

Probability

Note: In this Figure we plot the probability that a small Life Office’s actual cost is less than that implied by the PreDis, PostDis, and PostStake cost functions (Table 2), with a small Life Office observation being one with \textit{FUM} below the sample median of £1.795 Billion. We denote these probabilities by \( \pi_{Y,\text{PreDis}}, \pi_{Y,\text{PostDis}}, \) and \( \pi_{Y,\text{PostStake}} \). We also plot SmallFUM, which is the proportion of total sample FUM managed by small Offices in the sample each year.
D. A Fourth Regime?

In addition to the reforms we consider above, the FSA implemented Depolarization and the Menu in June 2005. Depolarization ended Polarization, under which someone selling Life Office investment products had to be either an IFA (entailing the requirement that he consider the entire market when advising a customer on which products were suitable) or be tied to a single Life Office. Now there is an intermediate step between tied agents and IFAs consisting of people who offer products from a limited number of Life Offices. The Menu further improved commission disclosure. Since these reforms took effect only at the very end of our sample period, we lack the data required to investigate their impact in any depth. Instead, then, we offer only suggestive evidence on their effects based upon the analysis above.

If Depolarization/The Menu did lower the cost of investing, then the actual cost of investing should be lower in 2006 than the PostStake cost function predicts. Looking at the overall cost of investing evidence, we do not find that $W_{AVfEER_{2006,Actual}} < W_{AVfEER_{2006,PostStake}}$ (Figure 7). Looking at the individual Life Office data, we also do not find much evidence to support the idea that Life Office cost function shifted down in 2006 (Figure 9). Thus, our data does not indicate that Depolarization/The Menu had much impact upon the cost of investing. However, we note that given the limited data we have, the impact of these two reforms would have had to have been large in order for us to detect it. So, we can certainly not rule out the possibility that these two reforms are having a small but beneficial effect upon the cost of investing (and we do not investigate suitability at all).

E. The Evolution of the Cost of Investing

We find that both the overall cost of investing analysis and the individual Life Office analysis suggest that Life Offices:

- operated on the PreDis cost function until 1991;
- began to switch to the PostDis cost function in 1992;
- switched to the PostDis cost function in 1995; and
- switched once again from the PostDis to the PostStake cost function in 1999.
V. What Caused the Cost of Investing to Fall?

A. The Hypotheses

Having established that the cost of investing did fall by 146 bp over our sample period as the result of two downward shifts in the Life Office cost function, we now turn to exploring the cause of those two cost shifts.

Our main hypothesis is of course that the cost function shifted in response to regulatory reforms that promoted price competition between Life Offices by lowering search costs. The alternative hypothesis is that the cost of investing was falling anyway due to ongoing innovation by Life Offices. If the cost of investing was falling over our sample period anyway, then if one divides the sample at some arbitrary date one will tend to find that the cost of investing was lower after this arbitrary date than it was before. We consider three variants of the ongoing innovation hypothesis. In the first we assess the possibility that the cost of investing fell due to the cumulative impact of independent innovation by individual Life Offices. In the second we assess the possibility that the decline was the result of the cumulative impact of innovation shocks that affected the Life Office sector generally. And, in the third, we consider the possibility that the decline was a result of the cumulative impact of innovation shocks that affected the global fund management industry. Consider each hypothesis in turn.

B. Regulatory Reform

The case for regulatory reform bringing about the decline in the cost of investing that we observe is one of timing. Since Life Offices adopt the optimal strategy (and the entailed cost function) for their environment, their strategy (and hence the cost function) changes when the environment changes. Regulatory reforms that do increase the degree of price competition by lowering search costs would definitely count as a change in the environment. One would therefore expect a reform that did lower search costs to cause a shift in the Life Office cost function.

So, if Disclosure Reform did lower search costs, one would expect the reform to begin to influence the Life Office cost function in 1992 when the OFT’s report to the Treasury began the serious reform effort. One would further expect the new equilibrium to be more or less in place in 1995 when Disclosure Reform took full effect. Both the overall cost of investing analysis and the individual Life Office analysis above finds that the evolution of the Life Office cost function followed precisely this path.

Turning now to Stakeholders/RU64/Comparative Tables, the reform hypothesis predicts that the cost function shifts in 1999 with the combination of Stakeholder Pensions and RU64 (specifically designed to influence behavior before Stakeholders became available in 2001). And, again, both
the evolution of the Life Office WAVEER and in particular the individual Life Office analysis offer strong support to the contention that the Life Office cost function did shift at this time.

So, the regulatory reform hypothesis does an excellent job of explaining the path of the cost of investing through Life Offices over our sample period. Let us now consider the alternatives.

C. Innovation by Individual Life Offices

If individual Life Offices engage in the process of developing and implementing cost reducing innovations, then one would expect to observe that an individual Life Office's Cost will fall (holding its characteristics constant) from time to time when it is successful. If Life Offices generally engage in this pursuit of cost reducing innovations, one would expect that the cumulative impact of the individual Life Office cost-reducing innovations will cause the overall cost of investing to drift down over time. Could this process account for the cost of investing declines that we observe?

If innovation by individual Life Offices was the cause of the fall in the cost of investing over our sample period, we would expect the cost to decline gradually over time as the cumulative impact of the individual innovations builds up. Instead, though, we observe that the Life Office cost function shifts in two distinct steps, and that it is stable at other times. The discrete nature of the cost function shift is most apparent when looking at the individual Life Office data, when the Life Office cost function shifted rapidly from the PreDis cost function in 1991 to the PostDis cost function in 1995. Since this rapid and profound shift affected the entire industry, the only way to attribute it to the cumulative impact of individual Life Office innovation is to suppose that Life Offices just happened to be uniquely successful in their innovation quest at exactly the time that the reform hypothesis predicts that the Life Office cost function will shift. We do not find that explanation very plausible.

D. Innovations in the UK Life Office Industry

The main drawback to the individual Life Office innovation hypothesis (that a large number of Life Offices must independently innovate at the same time) can be overcome by supposing that the innovation instead occurs at the level of the industry as a whole. For example, the fall in the price of IT may open up the possibility of a business process innovation that all Life Offices can exploit at the same time. These industry-wide innovation shocks will drive down the cost of investing from one year to the next.

If the year to year declines in the cost of investing are the result of industry-wide innovation shocks, then the magnitude of these year to year cost of investing changes provide us with an idea of the distribution of these innovation shocks. Based upon this logic, we estimate the plausible magnitude of the cumulative impact of these industry-wide innovation shocks as follows. First, we calculate the year to year change in the cost of investing for each consecutive pair of years in
the sample (i.e., 1988/1989, 1989/1990, etc.) and put these changes into a list. Second, we drop the cost of investing changes for the possible reform years of 1991/1992, 1992/1993, 1993/1994, 1994/1995, and 1998/1999 from this list because those are the changes that we are seeking to explain. Then, we calculate the distribution of the cumulative impact of four years of industry innovation shocks by using a bootstrap as above (i.e., taking four random draws with recall from that list and summing them up).

We find that the point estimate of the cumulative impact of four years of typical Life Office innovation is -19 bp, and the 99% confidence interval for this impact is -67 bp to 31 bp (i.e., the value of this parameter is not statistically significantly different from 0). It follows that it is highly unlikely that the decline in the cost of investing we observe between 1991 and 1995 could be due to the cumulative impact of typical industry innovation.

E. Innovations in the Global Fund Management Industry

The magnitude of the decline in the cost of investing between 1991 and 1995 is far too large to plausibly be the result of the cumulative impact of typical innovation shocks hitting the Life Office industry. There remains the possibility, though, that the decline in the cost of investing we observe over this period was due to a one-off innovation shock to the global retail fund management industry.

To explore this possibility, we examine the evolution of the cost of investing in the US mutual fund industry from 1991 to 2006 (prior to 1991 the dataset is very incomplete). We focus on the US for several reasons. First, US mutual funds are obviously as capable of exploiting technical and business process advances as UK Life Offices, and the US regulatory environment for fund management has been relatively stable compared to the enormous changes that have occurred in the UK. Second, the fund management industries in the US and the UK are deeply connected. And, third, the CRSP Survivorship Bias Free dataset enables us to carry out a cost of investing calculation for the US. So, if a global innovation shock to fund management did occur over the 1991 to 2006 period that could have influenced the cost of investing in the UK, one would also expect this shock to affect the cost of investing in the US.

Our US sample consists of diversified equity mutual funds as defined in Barber, Odean, and Zheng (2005). The standard measure of the cost of investing through mutual funds (the expense ratio) excludes aspects of acquisition costs that we include in our measure of the cost of investing for the UK. So, to make the cost of investing numbers comparable, we add in acquisition costs for the US as follows. An upfront payment to an investment advisor is known as a Load Fee in the US. Load Fees are expressed as a percentage of the investment made. For each fund for each year we calculate total payments to advisors by multiplying total new investment in that fund in that year by that fund’s maximum Load Fee. This calculation will put an upper bound on acquisition costs, as advisors can offer discounts on Load Fees, and as a fund’s Load Fees
generally decline with the amount invested. We express this total payment to advisors as a percentage of $FUM$, and then add it to the fund’s expense ratio to arrive at its $EER$.

Using these $EER$ figures, we calculate the Weighted Average $EER$ for US mutual funds ($WAveEER_{Y,US}$) and we plot it in Figure 12. We find that $WAveEER_{Y,US}$ declines from 1.29% in 1991 to 1.19% in 1996, and that it increases from 1.06% in 1998 to 1.11% in 2002. Plainly, global factors were not bringing about material declines in the cost of investing during the UK reform periods.

F. Regulatory Reform and the Cost of Investing

We find that the Regulatory Reform hypothesis makes precise predictions about when the cost of investing will fall, and these predictions are confirmed in the data. The alternative hypothesis—that the decline in the cost of investing is the result of the cumulative impact of normal innovative activity and so would have happened with or without the reforms—can not explain the timing of the cost of investing shifts. Furthermore, the magnitude of the cost of investing shift around Disclosure Reform is too large to be consistent with the normal innovation hypothesis. Hence, we conclude that the regulatory reform offers the best explanation for the decline in the cost of investing over the 1988 to 2006 period.

We note here that it is likely that our method understates the impact of the reforms as we treat all "within cost function" reductions in the cost of investing as being due to factors other than the reforms, which is almost certainly not the case. To elaborate, each of the Life Office cost functions we estimate has within it some scope for a Life Office to reduce its Cost. An Office could become larger to exploit economies of scale, or it could switch from more costly to less costly business lines. And, indeed, we find that Life Offices did exploit this avenue to reduce their Cost. To illustrate, we find that the cost of investing would have fallen from 3.83% of $FUM$/year to 3.32% of $FUM$/year between the PreDis and PostStake regimes even if Life Offices had remained on the PreDis cost function for the entire sample period. Previous research on the likely impact of Disclosure Reform (Nera (1994), Addison and Pike (1995)) suggested that Life Offices would respond to the reform by exploiting economies of scale. Hence, it seems probable that a considerable proportion of the "within" cost function reductions that occurred over the sample period too are due to the reforms.
Figure 12
The Cost of Investing Through US Mutual Funds

Note: In this Figure we plot the Weighed Average EER (WAveER) of US broad equity mutual funds as defined by Barber, Odean, and Zheng (2005) using the CRSP Survivorship Bias Free Mutual Fund dataset. We set a mutual fund’s EER equal to its expense ratio plus (New Investment × Maximum Load Fee)/FUM. The WAveER for a given year is equal to average of the individual fund EERs, with each fund’s weight equal to its share of FUM in that year. The sample begins in 1991 due to very incomplete coverage for previous years.
VI. Conclusion

One common view of the task of financial market regulation is that it exists to correct market failures. Yet, market failures inevitably permeate all real world markets, and these market failures inevitably bring about a market response (for good or for ill). So, setting one's self the task of correcting market failures (without analyzing the market response) does not provide much of a guide about what to do. The success that regulators have had in lowering the cost of retail investing through UK Life Offices suggests that it may be more productive for regulators to instead set themselves the task of making important financial markets work well.

In order to make a market work well regulators must understand how that market works and how well the market works (judging performance from the perspective of actors in the real economy who directly or indirectly use the financial services the market provides). Equipped with this understanding, regulators can know if the tools at their disposal will enable them to enhance or replace the market solution to a market failure in a manner that makes the market work better (using cost-benefit analysis to properly assess the net benefits of regulatory options).

Disclosure Reform provides an excellent illustration of the benefits that can follow from this approach. Examining the market, it is clear that search costs were high and that the market response to this situation accentuated rather than ameliorated the fundamental problem. That is, in an environment with little price transparency, Life Offices had a strong incentive to compete for business by engaging in costly efforts to attract customers rather than by lowering price. Life Offices therefore adopted a (very) high cost/high price strategy, with the cost of investing through retail oriented Life Offices in the Pre-Disclosure Reform period equal to 3.85% of FUM/Year. With costs at this level the cost of managing investor wealth consumed a considerable proportion of the total return that Life Office portfolios generated. In sum, the Life Office retail investing market was not working well.

By increasing price transparency in the market, Disclosure Reform not only brought about a direct increase in the degree of price competition, it shifted the strategic choice away from high costs/high prices towards low costs/low prices. Disclosure Reform therefore brought about a substantial improvement in how well this market worked (we find that Disclosure Reform ultimately brought about a 140 bp decline in the cost of investing).

The combination of Stakeholder Pensions, RU64, and Comparative Tables provide a further illustration of the virtues of the "make markets work well" approach. The Government launched Stakeholder Pensions to improve the personal pension market by lowering search costs for people who might otherwise find the task of selecting a personal pension too daunting. By requiring that every advisor recommending a personal pension either recommend a Stakeholder or explain why the product he is recommending is the superior choice, the FSA's RU64 essentially put every personal pension into direct competition with a good value alternative, thereby promoting price competition. The FSA's Comparative Tables project further increased transparency by offering a
reliable source of information on retail investment products. We find that this combination of reforms brought about a 4 bp decline in the cost of investing (4 bp/Year on £580 billion = £234 million/year).

The enormous success of Disclosure Reform and the combination of Stakeholders, RU64, and Comparative Tables does not, however, mean that the retail investing market now works perfectly. The cost of retail investing through Life Offices is now at about 185 bp/year, yet the cost of investing through a low cost index tracking fund is about 25 bp/year (James (2000)). It is of course true that Life Offices offer a different proposition than index tracking funds, so one would expect some cost differences. Nonetheless, the significant gap between the cost of investing through Life Offices and the low cost alternative suggests that there is considerable scope for further improvement in this market.

We were able to analyze the impact of major regulatory reforms on how well the market for retail investing through Life Offices was working because we had a good (if far from perfect) performance measure from the perspective of retail investors and data that enabled us to track this measure over an extended period of time. For most financial markets, though, it is difficult to both devise good performance measures from the perspective of the real economy and to find the data required to track such a measure over an extended period of time (obviously these two problems are mutually reinforcing). Since it is difficult to know how to make a market work better if one does not know how it works at all, regulators are now not able to deploy the tools at their disposal in the most effective manner. Our analysis here suggests that developing a deeper understanding of how the economy’s key financial markets work will significantly enhance the ability of regulators to evaluate the effectiveness of current regulations and to develop ideas for how to make the financial markets work better from the perspective of the real economy.
Appendix: Is the Pooled Cross-Section Specification Superior to the Fixed Effects Panel Specification?

When estimating the cost function for each regulatory regime we treat each Life Office/Year observation as independent—that is, we assume that each Life Office’s Cost is determined by the observable variables we include in the cost function regressions (e.g., FUM). This being the case, the best way to estimate the values of the parameters of the cost function is to pool all of the observations from each regime’s In Sample years into a single cross-section regression (a pooled cross-section specification)—and this is what we do. Alternatively, one could suppose that a given Life Office’s Cost is a function of both the observable variables and a specific (“fixed”) effect for that Life Office. To illustrate, a given Life Office’s Cost could be £100 million less than that implied by the observable variables alone due to some specific factor affecting that particular Life Office. One can incorporate these fixed effects into the cost function estimation by including a dummy variable for each Life Office. Such a specification is known as a fixed effects panel. In this Appendix we evaluate the pooled cross-section and the fixed effects panel specifications to see which is the better choice for the Life Office data we analyze.

Now, it may seem obvious that the panel is the best specification to use because it includes more parameters. So, if fixed effects are present the panel specification will pick them up, and if fixed effects are not present then the individual Life Office dummy variables will all equal 0 and the regression will be the same as the pooled cross-section. However, this may not always be the case. Suppose that the observable variables do in fact determine Cost and that Cost (and the values of the observable variables) varies a great deal between Life Offices but does not vary much for a given Life Office for the estimation period (recall that each of our estimation periods consists on only a few years). Then, the cross-sectional variation that is really due to the variation in the observable variables will tend to get picked up by the individual Life Office dummy variables. In this case the Life Office cost function will (falsely) imply that Cost will not vary by much as the values of the observable variables change.

Since the panel will not always be the right choice, one must somehow decide which specification to use. Ideally, one would evaluate the two specifications on the basis of how well they forecast Cost out of sample. That is, one would divide the years in a given regulatory regime into an In Sample and an Out of Sample group. One would then estimate the two cost functions with data from the In Sample group and then see how well they predict Cost for the Out of Sample period. If the fixed effects of the panel are really there, then the panel specification will do a better job of predicting Cost for the Out of Sample period than the pooled cross-section. If, on the other hand, the fixed effects are just artifacts of the estimation, then the pooled cross-section will do a better job of predicting Out of Sample.

Of course, in order to assess the Out of Sample performance of a specification, one requires a reasonably long Out of Sample period in which to run the comparison. In the case of the
PostStake regime (1999 to 2006) we can divide the regime into an In Sample period consisting of the years 2003 to 2006 and then perform the pooled cross-section/fixed effects panel comparison over the years 1999 to 2002. The four year Out of Sample period here is long enough to shed some light on the specification choice. In the case of the PreDis regime, however, the entire regime lasted only from 1988 to 1991, so we can not both estimate the specifications and perform a very meaningful Out of Sample comparison of them. Instead, we test for the presence of fixed effects by splitting the sample in half and estimating the fixed effects specification for each partition of the data. If "fixed effects" are really present, then the estimated fixed effect for a given Life Office $j$ in 1988/1989 will equal the estimated fixed effect for that Life Office in 1990/1991 (that's what the "fixed" in fixed effects means). Consider the PreDis and the PostStake periods in turn.

**A. The PreDis Period**

We first estimate the PreDis cost function with fixed effects for the years 1988/1989 and 1990/1991 (see Table A1). So as to be able to compare the fixed effect for Offices in the 1988/89 group with those in the 1990/1991 group, and to explore what the panel implies for the evolution of WAVEER over the PreDis to PostDis regimes for interest, we limit the sample to Life Offices that operated in each of the years from 1988 to 1998. In order to detect fixed effects, we also limit the sample to larger and presumably more stable Life Offices by eliminating the Life Offices with FUM of less than the sample median of £824 million in 1988 (the included Life Offices still manage an average of over 90% of total FUM and so are a representative sample).

Each of the panel estimates in Table A1 yield a fixed effect for each Life Office. Denote the fixed effect for Life Office $j$ in period $y$ by $FE_{j,y}$. To see if we can accurately think of fixed effects being present in the data, we then estimate:

$$FE_{j,1988/1989} = \alpha + \beta FE_{j,1990/1991}$$

If there are Life Office fixed effects, then we expect to find that $\alpha = 0$, that $\beta = 1$, and that the adjusted regression $R^2$ is high. Instead, we find that: $\alpha = 69$ (s.e. 21), $\beta = -0.25$ (s.e. 0.12), and $R^2$ (adj) = 0. It follows that a Life Office's "fixed" effect from the 1988/1989 period contains essentially no information about that Office's "fixed" effect for the 1990/1991 period. Hence, we conclude that using a fixed effects panel specification to estimate the PreDis cost function is inappropriate.
Table A1

PreDis Life Office Cost Functions: Panel Specification

In this Table we estimate the PreDis cost function using a fixed effects panel. The dependent variable is Cost. See Table 1 for variable definitions and summary statistics. FUM75 equals FUM if FUM > £6 Billion. The panels consist of 34 Life Offices. The 1988/1989 and 1990/1991 regressions consists of 68 observations, and the 1988—1991 regression consists of 136 observations. We use the White correction for heteroskedasticity when estimating the standard errors. A (*) (**) (***) denotes statistical significance at the 1% (5%) (10%) level.

<table>
<thead>
<tr>
<th></th>
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<tbody>
<tr>
<td></td>
<td>Mean (White SE)</td>
<td>Mean (White SE)</td>
<td>Mean (White SE)</td>
</tr>
<tr>
<td>FUM</td>
<td>0.034 (2.375)</td>
<td>0.020* (0.005)</td>
<td>0.015* (0.003)</td>
</tr>
<tr>
<td>FUM75</td>
<td>0.008 (0.019)</td>
<td>—0.016** (0.007)</td>
<td>—0.000 (0.001)</td>
</tr>
<tr>
<td>SPLife</td>
<td>0.055 (0.104)</td>
<td>0.087* (0.009)</td>
<td>0.065* (0.017)</td>
</tr>
<tr>
<td>RPLife</td>
<td>0.052*** (0.028)</td>
<td>0.094* (0.028)</td>
<td>0.062* (0.013)</td>
</tr>
<tr>
<td>SPPen</td>
<td>—0.056 (0.049)</td>
<td>0.152* (0.033)</td>
<td>0.054** (0.022)</td>
</tr>
<tr>
<td>RPPen</td>
<td>—0.008 (0.017)</td>
<td>—0.022 (0.023)</td>
<td>0.005 (0.006)</td>
</tr>
<tr>
<td>Fixed Effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Jointly Significant?</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>R²</td>
<td>0.99</td>
<td>0.99</td>
<td>0.99</td>
</tr>
</tbody>
</table>
Suppose, however, that one wished to estimate the impact of Disclosure Reform with a fixed effects panel specification anyway. As in the main body of the text above, we carry out this calculation by computing the $WAvEER$ implied by the panel specification (this time estimated with all 4 of the PreDis years) and compare it to the actual $WAvEER$. We find that: i) the actual $WAvEER$ falls relative to the panel $WAvEER$ in 1992, 1993, and 1994; and ii) the panel $WAvEER$ is an average of about 60 bp higher than the actual $WAvEER$ for the 1995 to 1998 period. It follows that, even when using the panel, there is evidence that Disclosure Reform brought about a decline in the cost of investing (the actual $WAvEER$ is lower in the PostDis period than that implied by the PreDis cost function). However, the magnitude of that decline is much smaller when with the panel (60 bp) then with the pooled cross-section (133 bp).

**B. The PostStake Period**

We first estimate the pooled cross-section and the fixed effects panel specifications of the PostStake cost function (see Table A2). We estimate the regressions with data from 2003 to 2006, and for the reasons given above we restrict the sample to large Life Offices that operated throughout the 1999 to 2006 period. Next, we compute the $WAvEER$ implied by each specification for the years 1999 to 2002 and compare them to the actual $WAvEER$ (see Figure A1). These years too are in the PostStake regime and so Life Offices in these years should be operating under the same cost function as in the 2003 to 2006 period. As one can see, the pooled cross section clearly outperforms the fixed effects panel in this out of sample test as the gap between the pooled cross-section’s predicted and actual $WAvEER$ is smaller on average and smaller in three of the four years than the fixed effect panel’s.

**C. The Choice of Specification**

We find that in the main body of the text that the parsimonious pooled cross-section does an excellent job of explaining the cross-sectional variation in Cost over our sample period. Introducing fixed effects into this pooled cross-section has the potential to seriously distort and bias the cost function coefficient estimates. Thus, we think that it would be sensible to prefer the panel specification only if it is demonstrably internally consistent and superior to the pooled cross-section. We find that the panel is neither internally consistent nor superior to the pooled cross-section. Thus, we conclude that the pooled cross-section specification is superior to the fixed effects panel specification for the Life Office data we analyze here.
Table A2

PostStake Life Office Cost Functions

In this Table we estimate the PostStake cost function using a pooled cross-section and a fixed effects panel. The dependent variable is Cost. See Table 1 for variable definitions and summary statistics. FUM75 equals FUM if FUM > £6 Billion. The panels consist of 24 Life Offices and the regressions consist of 96 observations. We use the White correction for heteroskedasticity when estimating the standard errors. A * (**) (***) denotes statistical significance at the 1% (5%) (10%) level.

<table>
<thead>
<tr>
<th>Variable</th>
<th>Pooled CS Mean (White SE)</th>
<th>Panel Mean (White SE)</th>
</tr>
</thead>
<tbody>
<tr>
<td>FUM</td>
<td>0.016* (0.002)</td>
<td>0.008** (0.004)</td>
</tr>
<tr>
<td>FUM75</td>
<td>0.003 (0.003)</td>
<td>−0.001 (0.001)</td>
</tr>
<tr>
<td>SPLife</td>
<td>−0.011 (0.007)</td>
<td>0.060 (0.038)</td>
</tr>
<tr>
<td>RPLife</td>
<td>0.079** (0.034)</td>
<td>0.126 (0.079)</td>
</tr>
<tr>
<td>SPPen</td>
<td>−0.041 (0.072)</td>
<td>−0.004 (0.041)</td>
</tr>
<tr>
<td>RPPen</td>
<td>0.051 (0.059)</td>
<td>0.089 (0.053)</td>
</tr>
<tr>
<td>Fixed Effects</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Jointly Significant?</td>
<td>—</td>
<td>Yes</td>
</tr>
<tr>
<td>R²</td>
<td>0.93</td>
<td>0.99</td>
</tr>
</tbody>
</table>
Figure A1
Comparing the Pooled CS and the Panel

Note: In this Figure we plot the gap between the WAvEER implied by the pooled cross-section specification of the PostStake cost function and the actual WAvEER ("Pooled CS") and the gap between the fixed effects panel ("Panel") specification of that cost function and the actual WAvEER for the years 1999 to 2002. The cost functions are estimated with data from 2003 to 2006 (see Table A2).
End Notes

1. We measure the cost of investing through a Life Office by its Explicit Expense Ratio (EER), which equals its Total Expenses divided by its FUM, and we measure the overall average cost of investing for a given year by the FUM weighted average EERs of the Life Offices operating in that year. The 3.85% figure is the mean of the average cost of investing for the years 1988 to 1991, and the 1.86% figure is the mean of the average cost of investing for the years 1999 to 2006.

2. The FSA launched a more minor package of reforms in 2005 that eliminated the polarization regime (Depolarization) and enhanced commission disclosure (The Menu). These reforms took effect at the end of the sample period under investigation here, and so we do not have enough data to rigorously analyze their impact upon the cost of investing. The evidence we do have suggests that they did not have much effect, and this finding is consistent with other FSA research on this point (see, for example, "Depolarization Disclosure", FSA Consumer Research Paper 64 or CRA (2007)). We discuss this evidence in the body of the report.

3. The 199 basis point impact assumes that the evolution of business composition from more expensive to less expensive lines (e.g., from regular premium business to single premium business) and the evolution of industry structure towards larger Life Offices capable of exploiting economies of scale was a response to the increased price competition that the reforms brought about. The 146 basis point estimate assumes that evolution of business composition and industry structure were independent of the reforms.

4. The 50% (95%) confidence interval on our 146 bp estimate for the impact of the reforms is 130 bp to 158 bp (92 bp to 183 bp).

5. This estimate assumes that, with no regulatory reform, all current business would have been transacted at the pre-reform average expense ratio. However, if some of the current business were a result of the lower prices the reforms brought about, then the benefits of the reforms would be lower than this estimate.

6. It is important to note that the cost of investing also fell over this period due to the increased exploitation of economies of scale and the changing business mix of Life Offices. Our cost functions enable us to control for these scale and business mix effects.

7. Life Office Disclosure Reform: NERA (1994); Stakeholder Pensions: Jarvis (2001); Comparative Tables: FSA Response Paper 28; Depolarization: NERA (2002); The Menu: FSA CP04/03. The cost of implementing RU64 was too small to merit a formal CBA.


10. FSA press release FSA/PN/073/2005 provides a description of RU64.

11. The FSA discussed its thinking for the Comparative Tables project in Consultation Paper 28
"Comparative Information for financial services" (released in October 1999).

12. There is another layer of implicit costs between the (unreported) gross return on a Life Office's portfolio and its net return, namely, the costs involved in managing the portfolio (e.g., trading costs). We do not analyze these portfolio management costs here (in part due to lack of data), but previous research puts them at about 100 bp per year (James (2000)).

13. Before the FSA took responsibility for managing required Life Office disclosures, the DTI had responsibility for this matter. During this time these regulatory filings were known as, unsurprisingly, DTI Returns. However, neither the reporting requirements nor the reporting forms changed with the transition from the DTI to the FSA, so it is possible to combine data from both forms.

14. FSA Returns also include items that directly report the cost of acquiring new business (F43, L11, C1 + F43, L11, C2 + F43, L13, C1 + F43, L13, C2) and managing the existing business (F43, L12, C1 + F43, L12, C2 + F43, L14, C1 + F43, L14, C2). As a check on our measure of cost compared it to the sum of these items here and found that they were virtually identical.

15. The Life and Pension premiums we measure here are those from an Office's UK Life and Pension business, and include only Direct Insurance business. That is, we exclude international business and reinsurance business. International business is a small part of the business of the UK authorized Life Offices (though the larger corporations of which UK Life Offices are part may do a great deal of business internationally), and Reinsurance business is a small part of the business of the retail-investor oriented Life Offices that we include in our sample.

16. Some institutions that manage large group pension plans organize themselves as Life Offices for legal reasons. For example, in 2006 the wholesale Life Office Barclays Pension Management had 78 new customers while taking in £11 Billion in premiums. The retail Life Office Scottish Widows, on the other hand, had 361,202 new Life and Pension customers and took in about £5 Billion of new UK Direct Life and Pension premiums. Plainly, the wholesale and retail Life Offices operate in completely distinct markets, and it makes little sense to include the wholesale offices in our analysis here.

17. As a robustness check, we did repeat the analysis below without applying any of these screens on the Life Offices we include in the sample. This larger sample did not produce materially different results.

18. The mean is a fragile statistic in that its value can be heavily influenced by outliers, as is the case here where small Life Offices with high EERs push the value of the mean up. Outliers have much less effect on the median as it is simply the middle EER in the sample, so if what should be a high EER is reported instead as an extremely high EER, this reporting error will not affect the median at all (though it could have a big effect upon the mean). Small Life Offices, which are more prone to report extreme EERs, have little impact upon the WavEER as their high EERs are weighted by their size when determining its value.
References


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O’Brien, C. (2003), “Is the UK life insurance industry competitive? Evidence from companies’ charges”, presentation at the Faculty and Institute of Actuaries Life Convention, Birmingham
