When They're Sixty-Four:

Peer Effects and the Timing of Retirement

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Abstract

This paper examines the effect of peers on an individual's likelihood of retirement using data from the Los Angeles Unified School District. We show that two large pension reforms differentially impacted the financial incentives for retirement within and across schools. Using an administrative dataset of the full population of district teachers ages 55 and over, covering the years 1997-2001 (*n=31,931)*, we construct school-level peer groups and calculate the impact of the reforms on pension financial incentives. We use a measure of the unexpected (reform-induced) change in the pension wealth of teachers in a school as an instrument for retirements at that school. After controlling for individual and school characteristics, and including individual fixed effects, our IV estimates of the effect of colleagues' retirement on a teacher's own likelihood of retirement are sizable and statistically significant. For example, we find that the retirement of an additional teacher in the previous year at the same school increases a teacher's own likelihood of retirement by an additional 1.8 percentage points.

Keywords: Retirement; natural experiment; pension reform; peer effects; California school teachers.

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1 Introduction

Choosing when to retire is one of the most important labor-market decisions an individual must make and mistakes can be costly. The importance of the decision and the public policy relevance of retirement timing have generated a large literature examining the retirement decision. The bulk of this research finds that financial incentives, as well as personal and family characteristics, are important considerations for the timing of retirement, but that these factors do not fully explain observed retirement behavior.[1] Motivated by this finding and a growing body of evidence on the importance of peers for other labor market decisions and economic outcomes,[2] we examine the effect of colleagues' retirement behavior on own likelihood of retirement and consider possible mechanisms through which the social effect may operate.

In the face of both recent and proposed pension and Social Security reforms, it is important to understand the role peers play in individuals' retirement decisions. Peer effects may lead to a "social multiplier" in retirement. That is, directly changing the retirement incentives of one individual may have a spillover effect on the retirement behavior of others, even those not influenced directly by an intended program. As a result, peer effects may alter the intended impacts of these reforms and could have potentially large effects on the financial well-being of the elderly and the derived costs of social insurance and safety nets.

While there is some evidence that peers are important for retirement-related decisions, there is little work examining the effect of peer behavior on retirement timing. Duflo and Saez (2002, 2003) find that enrollment in retirement plans is affected by the choices of colleagues and Hastings and Tejeda-Ashton (2008) find that peers and family members play a role in providing information for the choice of pension plans in Mexico. These studies do not address the decision of when to retire, but lend support to the importance of co-workers and friends in retirement planning. Our paper builds on this literature by examining the direct effect of peers on the retirement decision.

[1] For example, Krueger and Pischke (1992) and Burtless (1986), both looking at the labor supply response to Social Security benefit changes, find that Social Security can explain only a small part of the labor force participation trends of older individuals. Coile and Gruber (2007) find that the financial incentives created by Social Security significantly impact individual retirement decisions, but can not explain the high incidence of retirement at ages 62 and 65 with financial incentives alone.

[2] Researchers have examined peer effects in such settings as welfare take-up (Bertrand et al. 2000), drug use among college students (Duncan et al., 2003), social norms and unemployment duration (Stutzer and Lalive, 2004), recidivism (Bayer et al., 2009), etc.

Our findings provide some of the first evidence that peers may not only affect retirement decisions indirectly by influencing retirement savings, but also that they have a direct impact on whether or not an individual retires in a given year. Concurrent with our work, Chalmers et al. (2008) also examine the effect of co-workers on retirement outcomes. They too find that peers play an important role in individuals' retirement, though the results are not directly comparable with ours. Given their data and setting, they define peer groups at the employer level and use a different identification strategy. The detailed location information at our disposal allows us to focus on a more refined set of reference groups: all co-workers at the same physical location. Our identification strategy also varies in important ways, as we rely on an exogenous, yet salient, pension reform that permanently changed benefit levels and also shifted "key" retirement ages. This reform shares some key features with current and proposed changes to Social Security and private pensions, and thus is likely to be more informative about the role that peers may play in affecting the outcomes of these policy initiatives.

In this paper, we examine the relationship between peer retirement choices and own retirement decisions among Los Angeles public school teachers. Incorporating peer effects into any economic decision, including the retirement decision, introduces several challenges to identification and estimation. Importantly, there are potentially many factors, unrelated to peer effects, that could be mistakenly attributed to peer effects. For example, one might observe a correlation in the retirement of those who work together because of correlated tastes for leisure among colleagues or as a response to a demanding supervisor. Using a rich panel dataset derived from administrative records consisting of all Los Angeles school district teachers in the years 1997-2001, coupled with an unexpected pension reform, we are able to address the main challenges to peer effects estimation.

For this study, a crucial feature of the pension reform is that the change in the financial incentive to retire varies across the teaching population. The differential impact of the reform is essential for our identification strategy for two reasons. The first is that by affecting individuals differently it creates variation in the effect of the reform on each peer group due to differences in the composition of teacher characteristics. The second, perhaps more subtle reason, is that the unanticipated pension changes must affect teachers within a school in a differential manner. If all group members are affected in the same manner, even if the shock is completely exogenous, one cannot identify the effect of peers separately from the direct effect of the shock on an individual.

In addition to the differential impact of the reform on pension financial incentives across the teaching population, which is necessary for identification, there are several advantages to addressing the question of how colleagues affect one's own retirement decision with this particular data set. First, workplace colleagues might be a natural source of information about a work-related pension plan.[3] Given the school assignment information in the data, we are able to fully determine and observe each teacher's set of colleagues. We define each teacher's relevant reference group as the set of retirement-eligible teachers within the same school. We also use additional school-level controls for the entire teacher population. Using the school assignment information, we are able to match teachers to school characteristics that may be correlated with the work environment, such as student test scores. Also, California teachers are not covered by Social Security, and thus their employer-sponsored pension and reforms of this pension are likely to have a significant effect on teachers' employment-related retirement income. Finally, we are able to accurately identify the financial incentives that may influence individual retirement decisions, as the administrative data includes salary and other variables that are sufficient to calculate retirement benefits.

We use two main empirical frameworks to estimate the effect of peers on own retirement behavior. We first use a difference-in-difference framework to estimate the reduced-form effect of the reform as it operates through others in the group. We estimate the effect of an unexpected shock to the pension incentives of other teachers on own likelihood of retirement. The difference-in-difference framework allows us to control for variation in retirement rates across schools that may be caused by unobserved school characteristics or systematic differences in taste for retirement. We find a positive, sizable, and statistically significant effect of the degree to which the pension reform provided other teachers in one's school with a financial incentive to retire on own likelihood of retirement.

We then formally show that a differential instrumental variable coupled with an individual-level-fixed-effects specification can be used to identify the effect of others' retirement behavior on own likelihood of retirement. The exogenous, unexpected shock to the pension financial incentives

[3]Peers at the workplace have been shown to have an effect on productivity (e.g., Bandiera et al., 2009; Mas and Moretti, 2009), and some of the underlying social mechanisms in production may extend to the retirement decision.

of others is used as an instrument for the number of colleagues that retired in the previous year. We find that all else equal, an additional peer retirement in the previous year increases own likelihood of retirement by 1.8 percentage points. We include a host of robustness checks, and investigate alternative group specifications. We also perform two types of falsification tests providing further evidence that our findings are not an artifact of spurious correlation.

Finally, we further investigate two types of mechanisms through which the social effect may operate. We find no support for the hypothesis that school-specific retirement-age norms play an important role. We find evidence that the extent to which others do not wait to retire in order to maximize their pension benefits affects the degree to which an individual does not delay retirement in order to fully maximize the financial benefits of his or her own pension plan.

Our findings suggest that peers play an important role in influencing the retirement decision. Our results highlight the importance of others' behavior and are consistent with a "social multiplier" in retirement. For example, the rise in the Social Security normal retirement age could cause those that are not covered by Social Security to also delay retirement. Our results not only document and estimate the existence of peer effects, but also provide a direct dollar amount estimate of the spillover effect that providing one person with a financial incentive would have on his or her peers. For example, we find that an additional $100,000 of pension wealth given to all others in a school (in total, not to each teacher), self excluded, would increase one's own likelihood of retirement by 0.18 to 0.27 percentage points.

While much of the recent literature on peer estimation seeks to exploit exogenous or random assignment to groups, we are able to examine the effect of a reform on pre-existing groups. In the context of reforms of retirement financial incentives or any similar reform, a change in regulation for pre-existing peer groups is more likely to occur than legislation seeking to rearrange the peer assignment of those about to retire.

The next section introduces the retirement plan of Los Angeles teachers as well as the unanticipated reforms. Section 3 models the retirement decision and discusses the use of the reform to identify the peer effect. The data are described in Section 4. Section 5 describes the results, and in Section 6 we examine two types of possible mechanisms through which the social effect may operate. Section 7 concludes and is followed by the Appendix and tables.

2 Retirement Program for Los Angeles Teachers

The Los Angeles Unified School District (LAUSD) serves most of the city of Los Angeles, California and over two dozen adjoining cities and unincorporated county territory.[4] With over 820 schools and nearly 700,000 students,[5] it is second only to New York City in terms of size in the United States. Teachers in LAUSD are compensated in both salary and benefits.[6] The salary schedule is negotiated by the teachers' union and salary is a strict function of years of service in the district and level of education.[7] The benefits include both active employee and retiree health insurance (administered at the district level) and a retirement program. While many factors might enter an individual teacher's retirement decision, including health, family considerations, and working conditions, the pension is an important financial factor to include in any model of retirement decisions. So that we may discuss the retirement decisions of LAUSD teachers more specifically in the next section and because reforms of the pension are an important part of our identification strategy, we provide some details of the program below.

All full-time teachers in LAUSD are covered by a statewide defined benefit retirement plan that is administered through the California State Teachers' Retirement System (CalSTRS). The characteristics of the California teachers' defined benefit program closely resemble those of most employer-sponsored defined benefit retirement programs as well as those of Social Security. Participation is mandatory for teachers employed full-time in California public schools and upon retirement each CalSTRS member receives a lifetime annuity with an annual value based on years of service, age, and past salary.

The importance of employer-sponsored pensions in individual retirement portfolios will vary, but a few factors make CalSTRS likely to be a prominent component for Los Angeles teachers. First, CalSTRS members are not simultaneously covered by Social Security, so for career teachers this is their only source of employment-based retirement income. CalSTRS is also relatively generous; the average replacement rate for retired teachers is 59 percent of final annual salary,[8]

[4]Beverly Hills and a small portion of West Los Angeles are served by separate school districts.

[5]In 2007-08. (www.ed-data.k12.ca.us)

[6]The average salary paid was $63,000 in 2007-08. (www.ed-data.k12.ca.us)

[7]Education credits are earned both by earning a degree or though additional university classes and through special training courses offered strictly to teachers in the district.

[8]Authors' calculations using data in the statistical section of the CalSTRS Comprehensive Financial Report (2007).

compared to the replacement rate for the average Social Security annuitant which is only 40 percent of average annual lifetime earnings.[9]

The details of the defined benefit pension are as follows. While employed, members contribute 8 percent of their salaries[10] and are vested after five years of CalSTRS covered employment. The earliest age at which a member can claim his or her pension annuity, referred to as the "early retirement age," is age 55 for most members,[11] and age 60 is considered the "normal retirement age." Each retired CalSTRS member receives a lifetime annuity with an annual value calculated according to the following formula:

$$B(R, S) = k(R, S) \times S \times w_S^f \tag{2.1}$$

This "unmodified allowance" is a function of years of service S, retirement age R, final compensation w_S^f, and a benefit factor k.[12]

During the period under study there were two large, unexpected reforms of CalSTRS retirement benefits, both of which increased the generosity of the program but did so unevenly across the population. In August of 1998, the California State Legislature increased the generosity of the pension for all California teachers retiring on or after January 1, 1999 with the passage of two bills, AB 1102 and AB 1150. The 1998 reforms were unexpected and the details of the bills were not worked out until just days before they passed. Though unexpected, the reforms were well-communicated to teachers. The reforms were front page news in the Fall 1998 bulletin that circulated among active teachers, and the newsletter article not only provided technical details, but

[9] Social Security Administration (2006). The average replacement rate calculated for California teachers corresponds to an average retirement age that is just past 60, while the replacement rate reported for Social Security corresponds to retirement at age 65.

[10] Beginning in January 2000, 25 percent of mandatory member contributions are deferred to the new Defined Benefit Supplement (DBS) program. The DBS program is a cash balance program in which contributions are immediately vested and earn a guaranteed interest rate, 4.75 percent for the 2006-07 fiscal year, which is set annually by the Teachers' Retirement Board.

[11] Members that are at least age 50 and have a minimum of 30 years of service may now retire under the "30 and Out" alternative. The benefit factor is reduced, from 2.0 percent, by 0.12 percentage points for each year before age 60 that the individual retires. The first retirements under this alternative are observed in 2004.

[12] Final compensation is the average salary over the last three years of service for most teachers. The value of the benefit factor ranges from 1.4 percent to 2.4 percent, and is increasing in retirement age R and years of service S. Rather than receive the unmodified allowance, teachers can also purchase one of the program's joint survivor annuity options. Payments under these options are an actuarial conversion of the unmodified allowance.

also examples of how the reforms changed benefits.

The 1998 legislated reforms altered the pension benefits solely through changes to the benefit factor, k, while the structure of the program and the general allowance formula remained intact. To see how changes in the schedule of the benefit factor k as a function of age and service will alter retirement benefits, note that the fraction of a teacher's salary that is replaced by retirement benefits is $k \times S$. Therefore, if k increases at any age-service combination, annual benefits and pension wealth - the present value of the total retirement income a teacher will receive from the pension over her lifetime, will also increase. The value of the benefit factor as a function of years of service and retirement age for both the pre-reform and post-reform periods is given in Table 1. The benefit factor increased for those over age 60 as it grew to the new maximum of 2.4%, and for those with 30 years of service or more, there was a discontinuous jump of 0.2%. The interaction of age and service are also important for determining the exact impact of the reform.

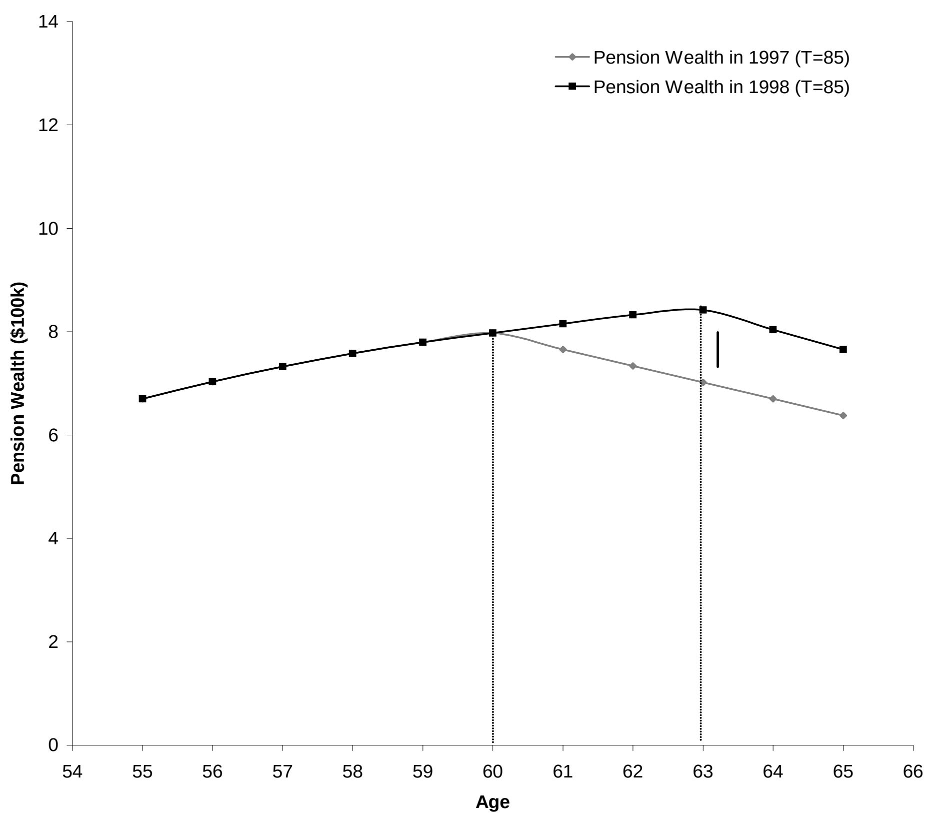

Figure 1a- Effect of the 1998 reform on the pension wealth-age profile, holding service fixed at 29 years.

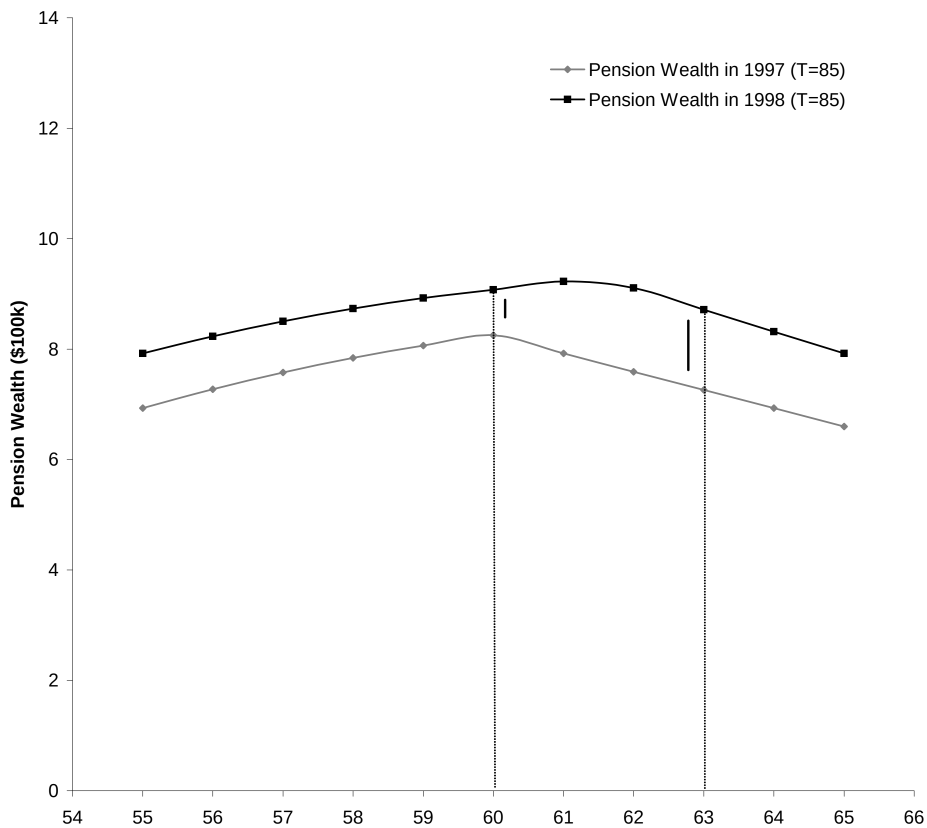

Figure 1b- Effect of the 1998 reform on the pension wealth-age profile, holding service fixed at 30 years.

In order to demonstrate how the reforms affected individuals differently along the age and service dimensions, we will refer to a stylized example shown in Figure 1. Both panels show pension wealth pre- and post-reform as a function of age, holding service constant.[13] In panel A, service at the time of the reform is set at 29 years, and in panel B, it is 30 years. First, in Panel A, consider two individuals with 29 years of service at the time of the reform. One is age 60 and the other is age 63. Notice that the reform does not affect the current value of pension wealth for the 60 year-old, but the unexpected change in pension wealth for the 63 year-old is $150,000. Also, the growth in pension wealth associated with continued work increases for the 60 year-old relative to the 63 year-old following the reform.

Comparing across panels allows us to hold age constant and look at the differential effect of the

[13] For the purpose of constructing the figures, salary is fixed at $55,000 per year, the individual is assumed to live until age 85 and future income is not discounted.

reform by service. Consider again the 60 year-old with 29 years of service and a 60 year-old with 30 years of service. In this case, just one year difference in service, holding age constant, results in a large difference in the unexpected change in pension wealth. The slightly more experienced 60 year old receives a large change in the value of his or her pension while the teacher with one year less experience does not.

Despite the seemingly small nature of the unanticipated pension reform, its potential impact on retirement benefits was quite large.[14] Post-reform, the financial return to working an additional year at age 60 nearly doubled. Pension wealth increased by 20 percent for retirements at age 63 and by at least 10 percent for retirements after 30 years of service.

A second reform, AB 1933, was passed by the state legislature on August 31, 2000 and went into effect for retirements on or after January 1, 2001. For this reform, like the 1998 reforms, last minute changes were made to this bill just as it was passed, so the exact nature of the pension reforms were a surprise to the teaching population. This reform did not work through the benefit factor k, but rather provided a lump sum "longevity bonus" to teachers at three target service levels. For the average teacher that was eligible for the bonus at the time of reform, his or her pension wealth increased unexpectedly by 6 to 13 percent.[15]

3 Retirement Decision

3.1 Retirement Decision without Peer Effects

The focus of this paper is the identification and estimation of the effect of peers on one's own likelihood of retirement. We estimate a parsimonious reduced-form model of retirement as it allows us to leverage the pension reform described in the previous section. Although structural

[14] Despite the anticipated increase in outlays, this legislation did not impose an increase in member contributions to the defined benefit program.

[15] This longevity bonus, $L(S)$, can be represented in the pension annuity equation $B(R,S) = k(R,S) \times S \times w_S^f + L(S)$. Specifically, the longevity bonus awarded teachers with thirty years of service at the time of retirement with an additional \$2400 a year in their retirement benefit, or $L = \$2400$. Teachers retiring with 31 years or 32 years or more of service would gain \$3600 or \$4800 a year, respectively. For all other teachers $L = 0$. This reform also affected individuals differently, but only along the service dimension. For example, for a teacher with 30 years of service, the reform created a positive wealth shock, but for a teacher with 29 years of service, his or her pension wealth did not change and in fact she or he had an incentive to work one extra year to get the bonus.

estimation has many advantages, as we are not concerned with estimating labor supply elasticity or other behavioral parameters, the computational burden and the need to specify a particular form for the individual utility function makes it less attractive for our purpose.[16] Below we present a very simple framework for thinking about how financial factors, especially the pension, will affect the individual retirement decision absent peer effects. We incorporate peer effects in the next section.

A convenient way to think about the retirement decision is to treat it as a utility maximization problem over two goods - lifetime consumption (C) and lifetime leisure (L), or years in retirement if all leisure is consumed in retirement. This simple model is enough to capture the essential features of the retirement decision for our reduced-form estimation strategy. The individual maximizes his or her utility over C and L, subject to the budget constraint he or she faces. The pension features enter the budget constraint. Assuming C is dollars of consumption, then the relevant budget constraint is simply

$$A_t = \sum_{a=t}^{T} \left(\tfrac{1}{1+r}\right)^{(a-t)} c_a \quad - \sum_{a=t}^{R-1} \left(\tfrac{1}{1+r}\right)^{(a-t)} w_a (1-\tau_c) \quad - \sum_{a=R}^{T} \left(\tfrac{1}{1+r}\right)^{(a-t)} B(R,S)$$

where T is the last period of life known with certainty, A_t is current wealth at age t, c_a is consumption in a single period, r is the real interest rate, $w_a(1-\tau_c)$ is annual salary net of the pension contribution and $B(R,S)$ is annual retirement income from the pension program for retirement at age R and with years of service S. The third term of the right-hand side of the budget constraint, $\sum_{a=R}^{T} (\frac{1}{1+r})^{a-t} B(R,S)$ is the present value at age t of the future stream of pension income given retirement at age R, or the pension wealth for retirement at age R. In this simple "lifetime budget constraint" model, it should be clear that the generosity of the pension, which affects the level of the budget constraint, and the growth of pension wealth, which affects the slope of the budget constraint, will enter the retirement decision and must be included in the empirical model.

We formulate a set of variables to capture these pension financial incentives. Current pension wealth, the present value of the stream of retirement income for retirement today, captures the pension contribution to the level of the forward-looking budget constraint. For an individual that is age t in the current period, it is defined in our empirical model as $\sum_{a=t}^{T} \pi_{a|t} (\frac{1}{1+r})^{(a-t)} B(t)$, where

[16] See Lumsdaine and Mitchell (1999) for a survey of the structural retirement literature.

$B(t)$ is the annual retirement benefit the individual would receive based on current age, service, and salary and $\pi_{a|t}$ is the probability of living until age a given survival until age t.[17]

Next consider the slope of the budget constraint, or the price of retirement. When working an additional year (or forgoing a unit of leisure), each teacher will increase his or her lifetime consumption by the sum of salary (net of pension contributions) and the change in pension wealth. In terms of the pension, an individual that chooses to work another year will be rewarded with a higher annual benefit amount in retirement $B(t) + B'(t)$, so that the individual gains this $B'(t)$ each year for the duration of his or her retirement period. However, there is also a pension cost associated with working an additional year - the loss of this year's benefits $B(t)$. So the magnitude and even the sign of the pension wealth gain for an additional year of work depends on the relative magnitudes of $B'(t)$ and $B(t)$ and the expected length of retirement.

If the annual return to working is a monotonic and weakly decreasing function of age and an individual's disutility of work is weakly increasing in age, then the level and slope of the budget constraint are sufficient financial measures to solve for the individual's optimal retirement age.[18] Whereas it may be reasonable to assume that the annual changes in salary are constant or diminishing with age, this is not an appropriate assumption for the pension portion of compensation. If, for example, annual retirement benefits increase discontinuously at 30 years of service, as they do with the longevity bonus described in Section 2, the one-year pension wealth growth is no longer sufficient to capture the financial incentives of the pension.[19]

Therefore, it is important to account for not only the gain in pension wealth associated with one additional year of work but for the full path of pension wealth accrual. This point is noted by Stock and Wise (1990) who present the idea that the return to an additional year of work is not only the direct current period financial reward, but also the value of the *option* to work in future periods (basing their estimation around the "option value" of retirement). Though their approach is less computationally burdensome than dynamic programming models of retirement, it still requires

[17] If we assume that a retirement-eligible individual will work continuously until retirement, then years of service at any possible retirement age is related to age by a constant. The pair R, S can then be replaced by age only.

[18] This also assumes that utility is separable in C and L.

[19] For example, an individual with 28 years of service may gain very little by working an additional year. However, if he waits two years to retire, he will receive a sizeable bonus at 30 years of service. This creates a disincentive for retirement at 28 years of service.

structural estimation.[20] Coile and Gruber (2007) translate the Stock and Wise (1990) structural model into a reduced-form model. We use their concept of "peak value" to capture the return to continued working, including the option value. [21]

The peak value is very similar to the Stock and Wise (1990) option value. However, the option value captures the gain in utility that can be expected from forgoing retirement at age t and instead retiring at the age in $[t+1, T]$ that would maximize utility, whereas the peak value captures the expected gain in pension wealth from forgoing retirement at age t and instead retiring in the period $[t+1, T]$ that would maximize pension wealth. Peak value at age t, PK_t, as defined by Coile and Gruber (2007), is the difference between pension wealth associated with retirement at the current age and the expected pension wealth for retirement at the future age that maximizes pension wealth:

$$PK_t = PW_{R^{max}} - PW_t \tag{3.1}$$

where pension wealth at any age t, PW_t, is defined as

$$PW_t = \sum_{a=t}^{T} \pi_{a|t} (\frac{1}{1+r})^{a-t} B(t) \tag{3.2}$$

and where R^{max} is the age in $[t+1, T]$ that maximizes equation 3.2. This measure essentially captures information about the forward-looking pension wealth growth in a single variable. No assumptions about the functional form of utility are made and we are able to include pension features and salary separately in our estimation. While peak value does not capture the structure of these benefits precisely, including terms to capture the exact features of the pension program would only be crucial if one were trying to estimate structural parameters such as retirement elasticity.[22]

[20] An individual will retire when

$$E_t(V(t)) - E_t(V(R^{Max})) > 0$$

where $V(s)$ is the discounted utility associated with retirement at age s, $E_t(.)$ represents an individual's expectations with the information available to her at time t, and R^{Max} is the retirement age $s \in [t+1, T]$ that maximizes discounted utility.

[21] The "peak value" estimation method has been used extensively in other work including a volume of international retirement studies edited by Gruber and Wise (2004) and in work by Asch et al. (2005) which looks at the retirement of Federal civil service workers. Samwick (1998) also developed a reduced-form variant of the option value model introduced by Stock and Wise (1990) to estimate the effect of Social Security and pensions on retirement behavior using the Survey of Consumer Finance and the Pension Providers Survey.

[22] For example, Blomquist and Newey (2002), develop a nonparametric approach of estimation in the presence of

In order to derive an econometric specification, we incorporate the peak value measure into a utility-based choice framework (McFadden, 1974). Let $U^*_{i,s,t}$ be the latent utility of individual i, in school s for retirement in year t. We specify the latent utility as an additively separable function of an individual's financial incentives, including the forward looking measures, personal characteristics, and year fixed effects:

$$U^*_{i,s,t} = \beta_0 + \beta_1 PK_{i,s,t} + \beta_2 PW_{i,s,t} + \beta_3 w_{i,s,t} + x'_{i,s,t}\beta_4 + \phi_t + \epsilon_{i,s,t}$$

$PK_{i,s,t}$ captures the option value of work due to the pension program, $PW_{i,s,t}$ is individual pension wealth at time t, and $w_{i,s,t}$ is the annual salary of individual i at time t. The annual salary and $PK_{i,s,t}$ reflect the price of retirement at time t. The vector $x_{i,s,t}$ contains personal characteristics that might affect retirement such as age and length of service, and ϕ_t captures any common period-specific effect, such as a macroeconomic shock. The unobservable shocks to the individual, $\epsilon_{i,s,t}$, capture changes such as health shocks. In the utility-based choice framework, an individual will choose:

$$\text{Retire if } U^*_{i,s,t} > c$$

The econometric specification of the retirement decision $y_{i,s,t}$ of individual i at time t can be written as:

$$\begin{aligned} \Pr(y_{i,s,t} &= 1|i,s,t) = \Pr(U^*_{i,s,t} > c_i) = \\ & f(\beta_0 + \beta_1 PK_{i,s,t} + \beta_2 PW_{i,s,t} + \beta_3 w_{i,s,t} + x'_{i,s,t}\beta_4 + \phi_t - c_i) \end{aligned} \tag{3.3}$$

If the threshold c is the same for all individuals, then one can normalize $c = 0$. Different functional forms for $f(.)$ would then translate to specifications such as logit or probit. In the case of the linear probability model ($f(k) = k$), we will allow the threshold c to vary across individuals, by using individual fixed effects. In the next section, we incorporate group characteristics and peer effects into the above framework.

nonlinear budget constraints and apply this to the hours of work decision. We adapted this estimation method for a nonparametric estimation of the lifetime budget constraint in the retirement model and found that the Peak Value approach captured the essential features of the pension program well enough given the focus of our paper.

3.2 Retirement Decision Incorporating Peer Effects

To incorporate the possibility that an individual is affected by his or her peers, we introduce some additional basic notation as well as a very brief discussion of some of the issues inherent to the identification of group or social effects.[23] We denote each individual's school by $s = 1..S$, each with n_s members $i = 1, 2..n_s$. We consider three possible types of group-level effects. The first type of measure is the behavior or outcomes of all others, $\overrightarrow{y}_{-i,s}$, where the notation $-i$ denotes that own behavior is excluded. The second type of measure is the observable group characteristics summarized by the vector Z_s, such as school quality or the age composition of other teachers in the school. The third type of measure is the unobserved school-level effect, which is denoted by ϕ_s.

For each type of school-level measure, there are several possible channels through which that measure might influence an individual's latent utility from retirement $U^*_{i,s,t}$. Both the observed and unobserved school characteristics Z_s and ϕ_s are likely to influence individuals' utility from work. For example, it may be more enjoyable to teach high ability students, and this ability may be captured by high school test scores (observed). A friendly school principal or coworkers (unobserved) may also make work more pleasant, reducing the utility gain from retirement.

One reason individuals may be affected by others' retirement is that they derive utility from acting in accordance with others (social norms). In the case of retirement, that would be equivalent to retiring because everyone else around you is retiring. Alternatively, increased retirement behavior among others may increase own likelihood of retirement for other reasons. Deciding when to retire is complex, and thus individuals may mimic the behavior of others in order to reduce the computational burden of the decision. Or, the fact that others are retiring may prompt one to start seriously considering his or her own retirement. Brock and Durlauf (2001) discuss utility-based models in which own behavior depends on the utility and behavior of others $\overrightarrow{y}_{-i,s}$, and distinguish between two types of ways in which $\overrightarrow{y}_{-i,s}$ may enter one's own utility function: own utility increases in group-level outcomes, and own utility depends on the distance from the group-level outcome (social norm). In Section 6 we further explore two possible types of mechanisms: con-

[23] The interested reader is referred to Brock and Durlauf (2001) and Moffitt (2001) for a more thorough discussion of some of these issues.

forming to retirement-age norms, and the effect of others' financial maximization of the pension plan.

In this section, we are primarily concerned with correctly identifying the effect of others' behavior from alternative explanations such as workplace environment. We include three types of social effects in the aforementioned latent-utility framework, and rewrite equation 3.3 as:

$$\begin{aligned} \Pr(y_{i,s,t} &= 1|i,s,t) = \Pr(U^*_{i,s,t} > c_i) = \\ & f(\beta_0 + \beta_1 PK_{i,s,t} + \beta_2 PW_{i,s,t} + \beta_3 w_{i,s,t} + x'_{i,s,t}\beta_4 + Z'_s\gamma + \rho \cdot m(\overrightarrow{y}_{-i,s}) + \phi_s + \phi_t - c_i) \end{aligned} \tag{3.4}$$

Each individual is indexed by i, s, and as above, the retirement decision $y = \{0, 1\}$ is a function of the individual covariates x_i, and an error term ϵ_i, a scalar capturing the individual's unobservable taste for retirement. In addition, each individual's retirement might depend on the observable group characteristics summarized by the vector Z_s (for instance, school quality), as well as the retirement decisions of all other members in the group $\overrightarrow{y}_{-i,s}$. The unobserved school-level effect is denoted by ϕ_s. γ is often referred to in the literature as the contextual (or exogenous) effect, and ρ as the endogenous effect. It is possible, and in most instances quite likely, that Z_s depends on the characteristics of others. For instance, Z_s might just be the average of the group characteristic, $Z_s = \frac{1}{n_s}\sum_{i\in s} x_i$.

For the remainder of this section, we assume the linear probability model $f(k) = k$. Our empirical results focus on the linear specification. However, as discussed in Section 5.3, the estimates for the reduced-form specification using logit are qualitatively the same and the marginal effects are similar. Further, to reduce the notational burden, we rename all of the individual covariates that affect retirement, including the financial incentives measures, as the vector x'_i and the coefficient β. To demonstrate the issues surrounding identification, we first specify $m(\cdot, s) = E[y_j|j \in s]$ and examine a one-period linear-in-means specification:[24]

$$y_{i,s} = \beta_0 + x'_{i,s}\beta + Z'_s\gamma + \rho E[y_j|j \in s] + \phi_s + \epsilon_{i,s} \tag{3.5}$$

[24]Throughout this paper, in the empirical specifications, any group-level measure will be replaced with the corresponding self-excluded measure.

Two types of problems regarding identification were first formalized by Manski (1993) and are known as the "reflection problem." Consider equation 3.5. For purposes of exposition, assume $\phi_s = 0$, $Z'_s = E[x'_i \,|\, i \in s]$, and that $E[\epsilon_i \,|\, x_i, Z_s, i \in s] = 0$. Taking expectation of both sides of equation 3.5 and rearranging terms one obtains:

$$y_{i,s} = \frac{\beta_0}{1-\rho} + x'_{i,s}\beta + \frac{\beta\rho + \gamma}{1-\rho} E[x'_i \,|\, i \in s] + \epsilon_{i,s}$$

If group attributes depend on the characteristics of its members ($Z'_s = E[x'_i \,|\, i \in s]$), then ρ and γ, the endogenous and exogenous effects, cannot be separately identified. Note that this result was derived even though it was assumed that the error terms are independent of group and own characteristics. One could, however, identify the existence of a social effect, namely: $\frac{\beta\rho+\gamma}{1-\rho} \neq 0$. For example, the results in Section 5.2 can be interpreted as a reduced-form specification, and are consistent with the existence of a social effect.

The second type of "reflection" discussed in Manski (1993) is one in which the unobserved errors are correlated across group members and depend on the group attributes. Similar to the above case, one cannot separately identify whether the observed effect of the group outcomes is due to the "endogenous" effect or whether it is just a reflection of the group's unobservables.[25]

Finally, a concern that is common to many group settings is that often group formation is endogenous.[26] For example, teachers with a higher (unobserved) taste for work, may choose to work in schools with certain characteristics.

The detailed panel data at our disposal allows us to examine how lagged (and therefore observable to teachers) retirements of others affect individuals. For reasons further discussed in Section 4, though our data contain the actual calendar date of every teacher's retirement, we group retirements by academic year. We replace $E[y_{j,t}|j \in S]$ with the lagged, and therefore actual, realization of the

[25]To illustrate, suppose $\gamma = 0$ and that $E[\phi_s \,|\, X'_s, i \in s] = X'_s\psi$, then similarly to the previous case one cannot separately identify ρ, and ψ :
$y_{i,s} = \frac{\beta_0}{1-\rho} + x'_{i,s}\beta + \frac{\beta\rho+\psi}{1-\rho} E[x'_i \,|\, i \in s] + \epsilon_{i,s}$

[26]If the unobserved characteristics of members influence their choice of groups, and if that choice is based on the observed (or unobserved) characteristics of the group, then the usual selection problems arise. This selection problem is, of course, not unique to group settings, but is usually harder to address. In addition, while the selection problem is likely to result in the second type of reflection problem discussed above (correlated unobservables), even if group membership is exogenous, correlated unobservables might still exist if the researcher cannot control for all of the group characteristics or if there are measurement errors (see Moffitt, 2001).

group outcome: $\overrightarrow{y}_{-i,s,t-1}$ (others' retirement in the previous period, self excluded). The results we present in Section 5.3 focus on lag-outcomes specifications of the form:

$$y_{i,s,t} = \beta_0 + x'_{i,s,t}\beta + Z'_{s,t}\gamma + \rho\overrightarrow{y}_{-i,s,t-1} + \phi_s + \phi_t + \epsilon_{i,s,t}$$

Various institutional details (see Section 4) lead us to focus on the lag-specification in our empirical specifications.[27] In addition to being more consistent with the institutional detail discussed in Section 4, there are two main advantages of using $\overrightarrow{y}_{-i,s,t-1}$, the lag-outcomes of others: it no longer requires us to make the assumption that teachers (correctly) anticipate the retirement of others, and it reduces the simultaneity problem of the group decision.[28] To illustrate these two issues, consider equation 3.5. First, an empirical specification would have to invoke some form of rational expectations: $E[y_j|j \in s] = \frac{1}{n_s}\sum_{j\in s} y_j$. Second, the specification would then suffer from the problems associated with simultaneous equations specifications: $y_{i,s} = \beta_0 + x'_{i,s}\beta + Z'_s\gamma + \frac{1}{n_s}\sum_{j\in s} y_j + \phi_s + \epsilon_{i,s}$. However, though the use of lag outcomes can aid in separately identifying the endogenous and contextual effects, one still requires the identification strategies discussed below. For example, if there is an important observable or unobservable group-level variable (Z'_s or ϕ_s) that determines retirement, it is likely to affect previous-year retirements of others just as strongly as it affects (planned) current-year retirements of others. To emphasize that the source of the identification is not the use of lagged outcomes, we derive identification in Appendix I using the simultaneous retirement decision.

Our derivation can also be useful for other cases in which researchers do not have information on lagged-outcomes, or have reason to believe simultaneous decisions are more plausible. In this particular setting, given the institutional detail, and the availability of previous-period outcomes, we focus our empirical investigation on the actual lag retirements of others and not the expected contemporaneous retirement of others. However, we do examine alternative group-effect-timings in Section 5.4.

[27] The majority of teacher retirements occur in the summer months, when school is not in session. Teachers are also required to formally announce their retirement at least 30 days in advance (and perhaps earlier due to the school-year planning cycle). These two facts make the window in which teachers are able to respond to observed retirements of other teachers at their school by immediately retiring in the same academic year very small.

[28] See Angrist and Pischke (pp. 194-198, 2008) for a concise treatment of the drawbacks to using same-period outcomes of others.

Our identification strategy relies on three features of the data. First, we use the unanticipated and differential effect of the reform. Second, we make use of the panel data structure across years. Finally, we use the rich set of school characteristics as additional controls. Our empirical strategy first examines the effect of others' changes in financial incentives using a difference-in-difference approach. We then make use of the reform via an instrumental variables (IV) strategy to estimate the effect of others' behavior. However, as noted, for instance, by Brock and Durlauf (2001) and Krauth (2005), in the case of peer effects, a valid instrument i.e., one that is correlated with the endogenous group outcomes and uncorrelated with own unobservables, is not sufficient.

It is also necessary that the reform affected individuals within the group differently. Informally, consider a case in which a reform exposes all group members to the same exogenous incentive to retire. This new incentive will enter each individual's retirement decision and so should be included directly in the regression. Therefore, it can not also be used as an instrument for the group's retirement behavior, as it would be identical to the own-incentive measure. In this case, it will not be possible to distinguish an individual's response to his own change in incentives from his response to the group's reform-induced change in behavior, even though the instrument was not correlated with individual unobservables . We derive this formally in Appendix I, and show that a reform that affects individuals both within and across schools differently allows us to separately identify the endogenous and contextual effects.

The differential IV would address concerns regarding the endogeneity of the group-retirement outcomes, but it would not suffice if there is an important individual-level unobserved preference for retirement. An unobserved component that is correlated with other observed own characteristics would lead to a biased estimate even if peer effects played no role. Note that the case of an individual-level unobservable fixed component encompasses the less general case of an unobserved school-level fixed component.[29] The panel data structure allows us to address the concern regarding an unobservable individual-level (and school-level) fixed effect. In Appendix I, we show how both the differential IV and the panel data feature can be combined. We derive the case of a two-period model in which there is an unexpected differential reform in the second period and allow for an unobservable individual fixed effect. For both the difference-in-difference specifica-

[29] As discussed in Section 5, our results are robust to the inclusion of teachers who moved across schools, as they represent a very small portion of the sample.

tion and the IV estimates, the intuition behind our identification strategy is that the time dimension is used to difference out the individual (and school-level) fixed effect, and the differential reform addresses the social effect. We formally show identification is possible even if there is a suitable instrument in only one of the two periods.

4 Data

In this study we used individual-level administrative data for the complete population of teachers age 45+ that were employed by the Los Angeles Unified School District (LAUSD).[30] The data includes an annual census of active teachers for each of the academic years 1997-2003 and all retirement episodes during this time period. We focus on retirement-eligible teachers, those age 55 or older in academic years 1997-2001. Each person-year observation ($N = 31,931$) includes age, years of service, salary, gender, retirement date (if applicable) and school assignment. School assignment is the key variable with which we are able to identify each teacher's colleagues - all teachers age 55 or older working in the same school in the same academic year. A teacher-specific identifier also allows us to follow teachers over time. The administrative teacher-level data has been matched to school-academic-year-level characteristics using the publicly available CBEDS data from the California Department of Education.[31] These school characteristics include the average age, service, and education of all teachers (regardless of age) in the school as well as student characteristics such as test scores and grade level of the school.

We use an academic year as the time unit of analysis. The academic year is defined as starting in September and ending the following August (e.g., academic year 1997 covers the period September 1, 1997 - August 31, 1998). Although we know the exact date of all retirement occurrences, and could look at a finer unit, such as monthly or even daily retirement decisions, an annual measure is more natural for the LAUSD context. This is because retirements are not spread evenly across the year. The large majority of retirements occur during the summer months with over 60 percent occurring in June. This is not surprising as teachers may be reluctant to leave their classes mid-year. Finishing out the contract year is also important for maximizing retirement service credits. Pension

[30]This data was compiled with the assistance of the Office of Personnel Research and Assessment in LAUSD.
[31]http://www.cde.ca.gov/ds/

wealth and peak value are calculated for each teacher in each academic year using the salary, service, and age information available from the administrative data. Pension wealth is $PW_t = \sum_{a=t}^{100} \pi_{a|t}(\frac{1}{1+r})^{a-t} B_a(k_t, S_t, w_t)$, where $\pi_{a|t}$ is the probability of living to age a given that one is currently age t, and the real interest rate r is assumed to be 0.03.[32]

Summary statistics for LAUSD teachers are shown in panel A of Table 2. Each year nine percent of teachers age 55 and over retire. The retirement-eligible LAUSD teachers are on average just short of 60 years old and have 20 years of service within LAUSD, and over 70% are women. As a group they have an average pension wealth of almost $500,000, with a peak value of over $90,000. This means that these teachers could increase their pension wealth by up to 20 percent, on average, with continued work. For those individuals that had an unexpected gain in their pension wealth due to the reforms in academic years 1998 and 2000, the average gain in pension wealth was $80,000.

If co-workers are affecting individual retirement decisions, then it is not only own characteristics that matter, but the characteristics of one's group. For each teacher we define the relevant group for retirement to be all teachers age 55 or older that are working in the same school.[33] There are 606 unique schools observed in LAUSD over this time period and the average number of teachers age 55 or greater working in each school is 10.27. Looking at the distribution of school size across the district and time period covered reveals substantial variation. Of the 2,847 school by year observations in the data, over 62 percent of schools employ fewer than 10 teachers age 55 or greater. However, the tail of the distribution is long and 10 percent of schools employ more than 22 teachers age 55 or greater in a given year.

Panel B of Table 2 provides summary statistics for the schools. The table treats each school-year average as an observation, and reports the mean, median, and standard deviation of the average-

[32] A couple of additional notes for the pension wealth calculation: the pension 2 percent cost of living adjustment is included, and age, service and pension wealth are all calculated as of the last day of the academic year. All financial variables are adjusted to year 2000 dollars. The service used is years in LAUSD which will understate a teacher's true service if he or she has worked in another district in California.

A couple of notes for the peak value calculation: Peak value is defined as $PK_t = \underset{a\epsilon[t+1,T]}{Max}(PW_a) - PW_t$ is calculated by finding the pension wealth, as described above, for each possible future retirement date and taking the difference between the maximum and current pension wealth. For this calculation salary is assumed to grow at 2 percent annually.

[33] We chose 55 since in almost all cases that is the earliest retirement-eligible age. However, our results are robust to defining the group as those 45 and older.

within-school teacher characteristics. The standard deviation illustrates the amount of variation in "peer characteristics" across schools. The two most critical variables to our estimation are the school-level retirement rate and the unexpected change in pension wealth that will act as the instrument for school-level retirements. The average retirement rate across schools by year observations is 8.8 percent but the standard deviation is 11.3 percent. The mean total unanticipated change in peers' pension wealth (our primary instrument) for those whose pension wealth was affected by the reform is $350,000. There is also significant variation in this variable across schools. As indicated in Table 2, the standard deviation in the average total unexpected change in pension wealth across schools is $350,000.

5 Results

We present two approaches for estimating the social effect. First, we use a reduced-form difference-in-difference framework to examine the effect of a change in others' financial incentives on an individual's own likelihood of retirement. We then turn to estimating the effect of others' retirement outcomes. To overcome the inherent correlation of others' retirement with school unobservables, we exploit the reform using an instrumental variables strategy in Section 5.3. We conclude this section by examining alternative group specifications and two falsification tests in Section 5.4.

5.1 Baseline Estimates

It is of use to first consider a baseline case assuming there are no peer effects. The reduced form specification can be written as:

$$y_{i,s,t} = \beta_0 + \beta_1 PK_{i,s,t} + \beta_2 PW_{i,s,t} + \beta_3 w_{i,s,t} + x'_{i,s,t}\beta_4 + \phi_t + \epsilon_{i,s,t}$$

Table 3 reports the results of one's own likelihood of retiring as a function of various control variables. The functional form is a linear probability model.[34]

The results in Table 3 illustrate some of the factors that are related to the likelihood of retire-

[34] We examined a logit specification and found similar results.

ment. All specifications include the pension financial measures - own pension wealth and peak value, integer age dummies with age 55 as the excluded group, years of service, salary, gender, and year fixed effects. Column 1 is the base specification and includes only these individual level characteristics for the full sample of retirement-eligible teachers - those age 55 or older in academic years 1997-2001. The main variables of interest are pension wealth and peak value, as they are intended to capture the pension financial incentives and it is the unanticipated change in these incentives that is central to our estimation of peer effects in the following sections. The coefficient estimate of both pension wealth and peak value are statistically significant at the 1 percent level. As predicted, pension wealth has a positive effect on the probability of retiring. Specifically, a \$100,000 increase in pension wealth is associated with a 4 percentage point increase in the probability of retirement. Also as expected, peak value, which captures the growth in pension wealth for continued work, has a negative relationship with the probability of retirement. Specifically, a \$10,000 increase in peak value is associated with a 0.47 percentage point decrease in the likelihood of retirement.

The remaining columns include controls for school characteristics, the additional terms Z_s or ϕ_s (in the last two columns) from equation 3.4. The effect of the individual characteristics (included in the base specification) on retirement remain very stable across all specifications, within sample. Column 2 includes controls that may indicate school quality and capture features of the work environment - school size as measured by number of teachers, pupil to teacher ratio, school grade level, fraction of teachers with a Masters degree or higher, and the rank of the school computed via standardized math test scores. Some school characteristics do appear as important predictors of retirement. Working at a high school (not shown) is associated with a 2.8 percentage point increase in the likelihood of retirement, statistically significant at the 5 percent level and teachers in schools with more educated colleagues are less likely to retire. Column 3 includes characteristics of the teaching population in the school - the number of teachers eligible for retirement in the school, and the average age and service of the retirement-eligible teachers, self excluded. None are statistically significant at the 10 percent level. Column 4 repeats the specification in column 3 but the sample is limited to only those retirement-eligible teachers in academic years 1999 and 2001. This restricted sample will match that used in the instrumental variable regressions. The results of this specification for the full sample and restricted-years sample are qualitatively similar. Columns 5 and 6 include 5-digit zip code fixed effects for the full sample and two-year sample. Zip code fixed effects capture neighborhood differences, including differences in the socioeconomic background of the students. The addition of these fixed effects does not affect the point estimates very much.

The coefficients of the school-level variables may mask different types of social effects, as discussed in Section 3.2. For example, the coefficient on the average level of service might pick up the contextual effect of others' service, as teachers might have a preference for working with more experienced colleagues. However, it might instead, or in addition, pick up the endogenous effect, as others' years of service may just be a proxy for others' retirement behavior. Similarly, others' years of service may just be a proxy for some underlying school-level unobservable.

Before examining and attempting to estimate the effect of peers, it is useful to consider whether the school fixed effects have any explanatory power. For the school-fixed-effects specification of the last two columns in Table 3, an F-test of the joint significance of the school fixed effects rejects the null at the 5 percent level of significance. While this is not evidence of peer effects, since school fixed effects would encompass any constant feature of the school, if the school component did not play an important part in explaining retirement this would suggest a smaller likelihood of being able to estimate peer effects.

There are many factors that may be relevant for the retirement decision, but that are unobserved in our administrative data. Some unobserved personal or family characteristics may affect the utility of retiring at each age. For example, an individual in poor health may find it optimal to retire earlier than an otherwise identical teacher in good health.[35] Teachers may also have a preference for coordinating retirement decisions with their spouses. Other factors, such as the pension of a spouse, housing and other assets, will likely enter the retirement decision through the individual's budget constraint. If these unobserved factors are also correlated with any of the covariates in the model, then the estimates will be biased. The estimates from the baseline (no peers) model demonstrate the correlations between retirement and pension financial factors, age, service, and

[35] Recall that all teachers in LAUSD are able to continue their health insurance after retirement. During this time period the majority of teachers are eligible to have 100 percent of their premiums paid by the district at any retirement age.

school characteristics, though the estimates in Table 3 do not necessarily represent the causal effect of these variables on the retirement outcome. When we turn to estimation of the peer effects in the followings sections, we will again discuss these unobserved factors in the retirement decision and how our empirical strategies, difference-in-difference estimation and instrumental variable estimation with fixed effects, address this potential source of bias.

5.2 Difference-in-Difference Estimation

As described in Section 3.2, the CalSTRS pension reform, which caused an exogenous and differential shock to the financial incentives associated with retirement to one's peer group, can be exploited to identify and estimate the effect of peers on own retirement. Given the differential impact of the reform across peer groups, a natural way to think about leveraging the pension reforms is with a reduced-form difference-in-difference estimation method.

We examine whether the unexpected change in the financial incentives of other teachers in one's school affects own likelihood of retirement. An advantage of the difference-in-difference framework is that it controls for all unobserved individual and school characteristics that are fixed at the group level and may affect retirement behavior, such as an admired principal or different tastes for retirement leisure. Without this control, the effect of these factors on retirement may be misattributed to peer effects. Under the assumption that an unexpected change in the financial incentive of others does not *directly* affect own retirement behavior, a relationship between the two can be interpreted as evidence of a peer effect without the need to specify how this spillover occurs.

In the case of a single reform, the unconditional difference-in-difference estimation is given by the following equation.

$$y_{i,s,t} = \beta_0 + \beta_1 Treat_{i,s} \times Post_t + \beta_2 Treat_{i,s} + \phi_t + \epsilon_{i,s,t} \qquad (t = 0, 1)$$

$Treat_{i,s}$ indicates the group the individual belongs to and is fixed over time. In our case, the treatment reflects the degree to which peers were financially affected by the reform. The coefficient β_2 captures the difference in the likelihood of retirement for individuals that are surrounded by different peers (as measured by their likelihood of being affected by the reform) prior to the reform.

ϕ_t captures the year effect, or the change in retirement that would have occurred even without a reform. Finally, the coefficient on $Treat_{i,s} \times Post_t$, β_1, can be interpreted as the causal effect of a change to peers' financial incentives on own likelihood of retirement.

As mentioned in the previous section, there are factors, such as health and the retirement tastes of a spouse, that may affect retirement plans, but that are unobserved to us. If these unobserved factors are correlated with the retirement behavior of peers, then (even putting aside other issues discussed in Section 3.2), the estimate of the effect of peer retirement behavior on own retirement behavior from a simple OLS regression will be biased. The difference-in-difference framework allows us to address this issue. It is straightforward to see that as long as the composition of health, spousal retirement incentives or other unobserved characteristics are not changing within treatment groups over time, they will be "differenced out" in the estimation.[36,37]

Focusing on health, if the average health of the teaching population changes from the pre-reform to post-reform period, conditional on observables, this would only bias the estimates of the effect of the unanticipated change in peers' financial incentives on own retirement behavior if the changes in health are correlated with the impact of the pension reform on one's peers. For example, if teachers that work with many other teachers that experience an increase in pension wealth due to the reform also suddenly face a (relatively) negative health shock in the post-reform year, which causes them to retire earlier, then the difference-in-difference estimate will overstate the effect of peers on own retirement. The shock in health or other unobserved variables must be correlated with the treatment intensity in order to bias the estimate.[38] However, given that the reform only

[36] See Meyer (1995) for a discussion of difference-in-difference estimation. An important assumption for the difference-in-difference estimates to be valid is that the treatment and control groups would have experienced the same change in outcome absent the reform. Although, this can never be tested directly, it is common to show that the groups had "similar trends" before the reform. We do not have sufficient pre-reform data to test the similarity of the pre-reform trends in retirement. An additional concern for difference-in-difference estimation is that serial correlation can lead to understated standard errors (Bertrand et al., 2004). We consider this to be less of an issue for our results, as we are using very short panels (as few as two periods), rather than a long time series.

[37] Another issue involving spouses is that a spouse may also be a teacher working in the same school. If spouses coordinate retirement decisions, one may worry that some of the peer effects being captured are actually the result of household decision-making. The lag structure alleviates this concern, as it would have to be that spouses coordinate retirement such that one retires the year before the other. The reform, itself, however, does not address this concern. For example if one spouse has an unanticipated increase in pension wealth, then the household may decide that they will retire as a pair, overstating the effect of peers on the retirement decision. Based on authors' calculations using Census 2000 data, approximately 10% of teachers living in Los Angeles, CA are married to another teacher, though spouses do not necessarily work at the same school.

[38] While it would be interesting to examine the effect of health and spousal retirement status on retirement decisions

provided a financial shock to one's peers, we view this as an unlikely source of bias.

In order to implement the difference-in-difference estimation, it is necessary to group individuals based on their likelihood of having an exogenous increase in the number of teachers retiring at their school. The reform provides a means of doing this by changing the financial incentives faced by an individual's peers differentially across schools. An individual's "treatment" status will be indicated by the unanticipated change in the pension financial incentives of one's peers (all other retirement-eligible teachers in the same school, self-excluded), as calculated based on the characteristics of one's peers in the pre-reform period. In our case, the treatment group will be a continuous variable that can be considered the treatment intensity. We maintain the lag structure established in section 3.2, so that academic years 1999 and 2001 will be considered the post-reform years for each reform.

We define this unanticipated change in pension financial incentives as follows. For each individual i, for both the first and second reforms, we can compute an array of measures ∇FI_i defined as the unanticipated change in individual i's financial incentive:

$$\nabla FI_{i,t} \equiv FI_{i,t}(\text{using post-reform formula, year=}t) \text{ - } FI_{i,t}(\text{using pre-reform formula, year=}t)$$

The measure $\nabla FI_{i,t}$ holds all other characteristics of i such as age, salary, and service constant. It is the unanticipated change in the financial measure due solely to the reform-induced change in the way the pension benefits were computed. $FI_{i,t}(\text{using pre-reform formula, year=}t)$ would be what an individual would receive, if there were not a reform. For example, consider pension wealth for the first reform (1998), we define the unanticipated change to one's pension wealth as:

$\nabla PW_{i,1998}$ =*Pension wealth (individual i, year=1998, post reform formula) - Pension wealth (individual i,year=1998, pre-reform formula)*

Similarly, we compute this measure for the second reform, as well as other financial incentives, such as the peak value measure discussed in Section 2:

$\nabla PK_{i,1998}$ =*Peak Value (individual i, year=1998, post reform formula) - Peak Value (individual i,year=1998, pre-reform formula)*

The unanticipated change in pension wealth is expected to increase the likelihood of retirement

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and to compare their impacts to the effect of peers, we are not able to pursue this with our data set.

while a large unanticipated change in peak value would be expected to discourage retirement. Including both measures of the unanticipated change in financial incentives better captures how the reform changes an individual's likelihood of retirement. To achieve this with a single variable we construct an index that combines ∇PW and ∇PK , such that

$$\nabla pension\_FI\_index_{i,t} \equiv \alpha_{PK} \nabla PK_{i,t} + \alpha_{PW} \nabla PW_{i,t}$$

The values used for the weights α_{PK} and α_{PW} are the coefficient estimates for peak value and pension wealth from the baseline specification that included only individual and school characteristics (column 2) in Table 3. These coefficients indicate the sign and relative importance of each in the retirement decision. The weights are held constant across individuals and over time. Larger values of this index are associated with a larger unanticipated financial incentive to retire in reform years. The value of this index ranges from -0.06 to 0.09 for the reform years and is zero for all other years.

Recall that individuals' treatment intensity is determined not by their own change in pension financial incentives but by the change in pension financial incentives of their peers. We examine how the reform affected an individual via his or her peer group. As an analogous example, consider an experiment with the goal of estimating the health outcomes of children as a function of their exposure to second-hand smoking. As an intervention, nicotine patches are distributed to a randomly selected sample of parents. A child in the "treatment" group would be one whose parent received the nicotine patch. Similarly, the treatment intensity for each teacher is defined as the lag sum of the unanticipated change in the pension index of teachers in one's school in the pre-reform year, self-excluded:

$$Treatment_{i,t} \equiv \sum_{j \in s,\, i \neq j} \alpha_{PK} \nabla PK_{j,\tau} + \alpha_{PW} \nabla PW_{j,\tau}$$

where τ is 1997 for the first reform and 1999 for the second reform, and t is academic years 1998 and 1999 for the first reform and 2000 and 2001 for the second reform.

The two reforms are stacked in the estimation such that retirement observations in all academic years 1998-2001 are used. Accordingly, 1998 and 2000 are treated as pre-reform years and 1999 and 2001 as post-reform years

$$y_{i,s,t} = \beta_0 + \beta_1 Treat_{i,s} \times post_t + \beta_2 Treat_{i,s} + \beta_3 w_{i,s,t} + x'_{i,s,t}\beta_4 + \phi_t + \epsilon_{i,s,t} \qquad (t = \text{1998-2001})$$

where post takes the value of one in 1999 and 2001 and is zero otherwise. Year fixed effects, ϕ_t, are included instead of post directly, and $x'_{i,s,t}$ represents all of the individual's own characteristics including own financial incentives, such as pension wealth and peak value.

The results are in Table 4. Column 1 shows the unconditional difference-in-difference estimates. The interaction term, $Treat \times post$, is of primary interest. The coefficient estimate shows a positive and statistically significant (at the 10 percent level) relationship between the financial shock to other teachers in one's school and own likelihood of retirement. The coefficient estimate of the $Treat$ variable is an order of magnitude smaller than that of the interaction term and is not statistically significant. This indicates that the likelihood of retirement was not correlated with treatment intensity (or treatment group) before the reform took place. Column 2 adds own $\nabla pension\_FI\_index$ in the previous year and its interaction with post. The point estimate of the coefficient on $Treat \times post$ does not change appreciably and is still statistically significant at the 10 percent level. Own $\nabla pension\_FI\_index$ in the previous year is positive and statistically significant at the 1 percent level. This would indicate that those whose own pension financial incentives were affected by the reform in a way to encourage retirement were in fact more likely to retire. The zero interaction term indicates that this effect was not changing over the period we consider. Column 3 adds individual level controls, including pension wealth and peak value. Again the point estimate on $Treat \times post$ increases only slightly, but the standard error also decreases, and the estimate is significant at the 5 percent level. Again own $\nabla pension\_FI\_index$ in the previous year is statistically significant but the sign reverses once individual levels of pension wealth and peak value are included. This may indicate that those who were provided a financial incentive to retire by the reform were less responsive to the changes in financial incentives, once controlling for the existing financial incentives. Column 4 includes school level controls. The coefficient on the treatment interaction term decreases but it is still positive and statistically significant at the 10 percent level. The coefficients on individual- and school-level controls do not change.

The difference-in-difference estimation is an appealing reduced-form method for estimating peer effects in the presence of a reform. We find that the unexpected change in the financial incentives of other retirement-eligible teachers in a school affects own likelihood of retirement, after controlling for own financial incentives. This result is consistent with the existence of peer effects. However, the difference-in-difference framework does not allow us to estimate the effect of other teachers' retirement outcomes on the individual retirement decision. We turn to an IV estimation in the next section to estimate the effect of an additional retirement of another teacher in one's school on one's own retirement decision.

5.3 Estimating the Effect of Peers on Retirement Using the Unexpected Change in Financial Incentives

Our findings in the previous section are consistent with the existence of a social effect. However, the reform consisted solely of changing financial incentives, and it is not likely that a change in others' financial situation is the actual social-effect mechanism. In this section, we examine the effect of others' retirement behavior on own likelihood of retirement. Rather than focusing on the direct effect of the reform, we use the reform to overcome some of the identification issues discussed in Section 3.2 via an instrumental variable (IV) strategy. We use the two reforms (academic years 1998 and 2000) to construct measures of the unanticipated change in financial incentives faced by each individual. Recall from the discussion in Section 2 that the reform was not only unanticipated, but its financial impact can be accurately and fully computed by using the administrative data made available to us. As motivated in Section 3.2, crucial for identification, the reforms had a differential effect on teachers. In addition, the large variation across schools allows us to estimate the effect.

Before turning to our IV estimates, similar to the previous section, we begin by directly using the unanticipated financial incentives as predictors of change in retirement behavior. It is of use to consider the reduced-form estimates that correspond directly to the IV estimates. Another advantage of considering these reduced-form estimates is that, unlike the results in the previous section that used the composite index, these results have a straightforward economic interpretation. We first consider for each individual i, for both the first and second reforms, the unanticipated change in individual i's pension wealth:

$\nabla PW_{i,t}$ =*Pension wealth (individual i, year=t, post reform formula) - Pension wealth (individual i,year=t, pre-reform formula)*

The advantage of this measure is that it is denominated in actual dollars. For the different specifications we use, it is important to note that we always control for an individual's own pension wealth and peak value, which would include the above measures. Our results are also robust to the inclusion of own unexpected change in financial incentives. Hence, our measures of interest are the unanticipated shocks for all others, self excluded.

The results of the reduced-form specification are presented in Table 5. The variable in the first row, the lag total unanticipated change in pension wealth of peers (self excluded), is one of the components used in the previous section to compute the reform intensity index, and will be used as the IV in the specification discussed below. Columns 1-6 of Table 5 correspond to the IV estimate in columns 2-7 in Table 6 and will be discussed in more detail below.

In all cases, the lagged total unanticipated change in pension wealth of all other 55+ year-olds in one's school, self excluded, has a positive and statistically significant effect on own likelihood of retirement. The point estimate ranges from 0.18 to 0.27 percentage points. Using the effect of one's own pension wealth, the point estimate suggests that other colleagues (self excluded) receiving an extra $100,000 in pension wealth (in total, not each) have the same effect on one's own retirement as receiving an additional $3,200 to $4,300 (depending on the specification chosen) to one's own pension wealth.

The results of Table 5 suggest that a change to others' financial incentives has an effect on one's own likelihood of retirement after controlling for a host of personal, group-level, and school characteristics. Though the estimates obtained from Table 5 are useful in understanding the spillover effect of a change in others' financial incentives, it is likely that others' finances do not directly affect one's own retirement, but rather people may be affected by others' behavior.

Table 6 investigates the effect of others' retirement behavior on one's own retirement using the unanticipated change in financial incentives of others as an IV. The specification we estimate is:

$$y_{i,s,t} = \beta_0+\beta_1 PK_{i,s,t}+\beta_2 PW_{i,s,t}+\beta_3 w_{i,s,t}+x'_{i,s,t}\beta_4+Z'_s\gamma+\rho Y_{-i,t-1,s}+\phi_t+\mu_i+\epsilon_{i,s,t} \qquad (t = 1999, 2001)$$

The variable of interest is $Y_{-i,t-1,s}$, the lagged retirement of others. In Table 6, we use the number of retirements in the previous year. We obtain similar results when we examine the lagged average retirement of others in the same school, self excluded (see Section 5.4).

The first column of Table 6 includes the naive OLS estimate. The number of retirements of others is found to have a positive and statistically significant effect on the likelihood of own retirement. Using the point estimate of column 1, one additional retirement of a colleague would increase one's own likelihood of retirement by 0.63 percentage points. Since peer retirement measures are lagged one year and include a host of individual, year, and school level controls we view these results as meaningful. However, to address any lingering concern regarding these group measures, we turn to the IV estimates in columns 2 through 7. The performance of the instrument used is reported in Panel B of Table 6. Column 2 is the same as 1, but now the peer-retirement measure is instrumented by the unanticipated change in pension wealth of others. The results for the peer measure are statistically significant at the 5 percent level. As might be expected, the standard errors are larger. Using the point estimate of column 2, each retirement of one's colleagues increases the likelihood of own retirement by an additional 1.85 percentage points. The second stage goodness of fit and the F-statistic of the instrument illustrate the strength of our instrument. Column 3 includes as an additional control the same IV variable used for the group measure for own (the lag unanticipated change in own pension wealth). The results remain very similar.

In addition to having a differential effect, a valid instrument should be correlated with the variable of interest. Since each individual is affected by the unanticipated change in their financial incentives, it is likely their outcomes as a group would be affected by a change in the group's incentives. This is supported by the effect the instrument has in the first stage.

The most crucial question in any IV setting is whether the instrument is uncorrelated with the unobservable component. We argue that in this case two features make this more likely to hold. First, the reform was unanticipated. Second, we are able to observe all the factors that affect our IV. In particular, the IV is solely a function of the age, service, and salary composition of teachers in a given school, all of which are measures we fully observe. In all specifications, we control for the average age, size, and years of service of those eligible to retire (age 55+).[39] Column 4 includes

[39]Our results are robust to using the average service and age of those over 45, the average service of those in the entire school, as well as a host of school size measures such as number of full-time-equivalent teachers, and the number of those over 45. All of these measures were used both lagged and contemporaneously. The results are available upon request from the authors.

additional school-level service composition measures (15-19, 20-24, 25-29, and 30+ years) as well as average salary of others. The size of the group measure remains statistically significant at the 5 percent level and is of the same magnitude. Column 5 illustrates the results are robust to the inclusion of the number of eligible retirees (in addition to number of full-time staff) as a control. In column 6, we expand the sample to include academic year 2000 (between the two years used in columns 1-5). The peer effect now has a larger standard error (significant at the 10 percent level) and the instrument is weaker. This is likely due to the fact that for the added year, the instrument takes the value of zero as there was no reform during that year.

The IV estimates address potential concerns regarding the peer-retirement measure, but the results may still be biased if the individual unobservable component is important in determining retirement. For example, those with a strong preference for work would have more years of service or perhaps higher salaries. Similarly, there may be an underlying unobservable component that is correlated with some of the school controls we have used.

The rich data set at our disposal allows us to link teachers over time, and thus purge any individual fixed effect. We implement the strategy discussed in Section 3.2 and Appendix I. The last column of Table 6 includes an individual-level fixed effect, corresponding to μ_i in the above equation. Note that in the case of conditioning on those who haven't moved, the individual fixed effect would also control for any fixed school unobservable.[40] Of course, this specification only includes those variables that change over time (both for self and at the school level). The effect of others' previous year retirement remains positive and statistically significant at the 1 percent level. The point estimate is now more than double, but still of reasonable magnitude. As in the case of column 6, the larger standard error could be due to the inclusion of two non-reform years.

All of the results in Tables 5 and 6 included a single IV, the unexpected change in others' pension wealth. However, as explained in Section 3, the non-linear nature of the pension plan and the reforms cannot be captured with a single measure. We therefore are able to examine all of our specifications using an additional instrument, the unanticipated change in others' peak value. We obtained almost identical results both for the point estimates and the significance levels when

[40] The results in the previous sections and for this specification are the same whether or not we exclude teachers who moved between schools, as only a small proportion of teachers move between schools this late in their career. In our sample, on average, less than 160 teachers over the age of 55 move between schools every year.

we used two instruments: the unanticipated change in pension wealth (as before), as well as the unanticipated change in peak-value. One example is provided in column 2 of Table 7.[41] In all specifications (using both 2SLS and GMM), both instruments are statistically significant and of the expected signs in the first stage. However, the peak value based instrument is weaker in each of the specifications and has less of a direct economic interpretation. Having two IVs at our disposal allowed us to also examine "over-identification tests." In all six specifications corresponding to Table 6, using both GMM and 2SLS we fail to reject the null, and the smallest p-value among all specifications is 0.51. This provides further support for our use of the reform as a valid instrument.

Taken together, our findings of peer effects remain after using three types of controls at the same time. We control for any individual-level fixed effect, we examine the lag, rather than the contemporaneous, peer retirement measure, and we instrument that lag measure using the unanticipated change in financial incentives experienced by others.

5.4 Alternative group specifications and robustness checks

We examine alternative group specifications as well as summarize the results of two falsification tests. The results are reported in Table 7. In column 1 we examine the average retirement rate of others (lagged, self excluded) rather than the number of other retirees. As an instrument (first stage results reported in Panel B), we use the average unanticipated change in pension wealth of others. The result for the average-measure are statistically significant at the 10 percent level and are both qualitatively and quantitatively the same as the case of number of retirements. In column 2, we once again examine the effect of the number of other retirees using two IVs as discussed in the previous section. We next turn to an examination of alternative-timing in the decision of others in columns 3-6. In all cases, we examine the effect of the number of other retirees. In column 3, we examine the number of current, same-period, retirees, and find no statistically significant effect. We instrument the measure using the (current) unanticipated change in pension wealth of others, and the excluded instrument F-statistic is 9.04. For this specification we assumed that teachers correctly know and anticipate the number of teachers that plan to retire at the same time as they are making the decision. We argue that this provides further support in favor of the lag-specification.

[41] The full set of results are available upon request from the authors.

Column 4 includes a 2-year lag of others' retirements, column 5 includes both 1-year and 2-year lags of others' retirements, and column 6 includes 1-year, 2-year, and 3-year lags. Because the reform does not occur every single year, only the specification in column 4 can be instrumented. The results suggest that the strongest and the only statistically significant effect is for the 1-year measure.

In column 7 of Table 7 we present the results of a falsification test in which both the lag-retirement of others and the future-retirement of others is considered (in the year following the current year). By exploiting the specific timing of the two reforms, we are able to instrument both the lag and future measures.[42] We find that the lag-retirement of others remains large, and statistically significant, whereas the future-retirement of others has a small point estimate, and is not statistically significant.[43] This finding is consistent with retirements of others having an actual causal effect on individual retirements, rather than merely being a proxy for the work-conditions in a given school. Last, in column 8, we examine an identical specification to the ones in the previous section, but as a falsification test, the dependent variable and group measures are salary rather retirement. While own characteristics, such as years of service, and age, are found to affect one's own salary, the salary of others (self excluded) has no statistically significant effect (and the point estimate is -0.0001). Since teachers have very little control over their own salary (beyond years of service or education), this finding is consistent with the retirement of others having an actual effect, rather than picking up a spurious correlation in the attributes of teachers in schools.

6 Mechanisms

The results presented in the previous section suggest that peers have an effect on one's retirement decision. In this section we further examine some possible mechanisms through which this effect may operate. We focus on two possible types of mechanisms, the retirement age of others, and the extent to which others maximize the financial value of their pension benefits. We find that

[42] The reforms were two years apart, and so we examine the retirement behavior of those in the year after the first reform and before the second reform.

[43] For the future-retirement measure, the first stage F-test for the excluded instrument is statistically significant at the 10 percent level but quite small, suggesting the IV is weak. In part, this is because the second reform is less powerful from an IV standpoint, and the need to include both lag- and lead- measures reduces the sample by half.

individuals are not affected by the retirement age of others, but that the financial utilization of the retirement plan by others does have an impact on own financial utilization.

6.1 Age Norms

One way in which individual retirements may be affected by group retirement behavior is if individuals have a preference to retire at the same age as others in their peer group. The occurrence of spikes in the retirement hazard rate at particular ages is well documented (e.g., ages 62 and 65 for Social Security claimants). While these spikes often coincide with the features of the retirement program under study, the program features do not completely explain the bunching of retirees at particular ages.[44]

If a retirement age norm exists, an individual can increase his or her utility by retiring at this age, all else equal. We incorporate this into our framework by allowing individual utility to also depend on the age-distance between the individual's current age and the norm retirement age. The true norm retirement age, if any, is unobserved so the modal retirement age at a school in the preceding year (or over the two preceding years) is used as the norm retirement age in the estimation. The "age gap" squared, which is equal to the number of years between own age and the modal retirement age squared, is used to capture the effect of the age norm on retirement. Note that our examination of norms focuses on school-specific age-norms. There may be a system-wide (or economy-wide) age norm. Any such norm would be picked up by our age dummy controls.

The regression results are presented in Table 8. Columns 1 and 2 show the effect of the age gap on the probability of retirement. The first column shows the OLS regression results. The coefficient on age gap squared (divided by 10) is both statistically insignificant and small in magnitude. The second column adds an interaction of age gap squared with a dummy that is equal to one if the teacher is older than the modal retirement age in the previous year at his or her school. The interaction term allows the age gap to have a different effect depending on whether the individual is younger or older than the school's norm retirement age. Neither the coefficient on the age

[44] Social norms have been widely suggested as a potential contributing cause of retirement age spikes. For example, see Asch et al. (2005) and Mastrobuoni (2009). However, some structural work has shown that the "unexplained" portion of the retirement age spikes, after controlling for financial incentives, heterogeneous time preferences, etc., is likely to be small (e.g., Gustman and Steinmeier, 2005).

gap squared nor the coefficient on the interaction term are statistically significant, however the magnitude of the point estimate on the age gap interaction term is an order of magnitude larger than the main effect.

To control for the endogeneity of the retirement decision of others, columns 3 through 6 include the number of peers that retired in the previous year. As in section 5.3, the number of retirements is instrumented. Columns 3 and 4 are the same, but in column 4 the modal retirement age is calculated using the previous two years rather than only the single year. Columns 5 and 6 repeat columns 3 and 4 but allow the effect of the age gap to vary depending on whether the individual is younger than or older than the school's modal retirement age. In all cases the estimates of the coefficient on the age gap squared is small and statistically insignificant. The point estimate of the effect of the number of retirees in the previous year on one's own likelihood of retirement is similar to the instrumental variable estimates in Table 6.

We interpret the results in this section as evidence that a norm retirement age (as we have defined it here) does not play a large role in individual retirement decisions and is not driving the differences in retirement behavior across schools.

6.2 The Effect of Others' Pension Maximization

To explore one possible mechanism through which networks may operate, we focus on the effect of others' financial response to the retirement plan, by taking advantage of the detailed pension plan information of each individual. We maintain the assumption that people act optimally, and maximize their utility. However, since the financial aspect of a retirement plan is not the only factor entering into an individual's decision, it is possible to retire before the optimal-pension-plan retirement date and thus "leave money on the table." Our finding is that while a potential large gain from waiting reduces the likelihood of retirement (as expected), being surrounded by others who retired before fully maximizing the financial value of their plan, mitigates, and largely reduces the effect of own potential gain from waiting. In other words, teachers that are surrounded by others who "leave money on the table" when retiring, all else equal, are more likely to do the same when considering their own retirement.

For any point in time, we can compute whether retirement would maximize pension wealth. We use the peak value measure to compute how much money (in pension wealth) an individual would forgo if she retired today versus the date in the future that maximizes pension wealth. Similarly, we examine how much additional pension wealth would be accrued by waiting one additional year before retirement. Then, for each retirement that occurs we can evaluate to what extent an individual could have further increased their financial gain by delaying retirement. Of course, it is possible that someone would rationally retire before maximizing their pension wealth for a variety of reasons such as health, preference for leisure, other sources of wealth, etc. In fact, the utility-based choice framework assumes all retirements are optimal given a particular individual's circumstances. Using the measures of financial-under-utilization, we can examine whether a higher rate of under-utilization among past retirees within a school moderates the importance an individual places on fully maximizing their own pension wealth.

The results are presented in Table 9. The key variables of interest here are the measures of the extent to which one's peers who retired in the previous year maximized their pension wealth interacted with whether or not an individual is still "far" from fully maximizing the plan's potential financial benefits. Because the measure of others' pension maximization is a function of others' retirement, we instrument this measure using the same instrumental variable strategy discussed in Section 5.3.

The "not-fully-maximizing" measure used in columns 1 and 2 is whether or not the peak-value measure is over $50,000 (i.e., whether the gap in the present value of pension wealth between retirement today and the present value of pension wealth at the optimal date is more than $50,000). The key variable of interest is therefore the number of retirees in the previous years whose peak-value was over $50,000 interacted with whether an individual considering retirement has a peak value greater than $50,000. As before, this measure is instrumented using the unanticipated change in pension wealth of others. In both columns, the results are quite similar, and statistically significant at the 10 percent level or better, both for the effect of own peak-value being over $50,000, and the effect of others' large peak value at retirement interacted with own measure. As to be expected, an individual's own peak value has a negative effect on the probability of retirement, but a higher number of peers who retired before maximizing their benefits reduces this effect. For example, in column 2, having a peak value of over $50,000 reduces the likelihood of retirement by

6.2 percentage points. However, the number of peer retirees who had retired when their peak value was over $50,000 interacted with whether own peak value is over $50,000 increases the likelihood of retirement by 8.4 percentage points.

Columns 3 and 4 use one-year accrual in pension wealth as an alternative measure of the gains to postponing retirement. The results are qualitatively the same and statistically significant at the 10 percent level. Once again, a higher potential accrual reduces the likelihood of retirement but that effect is reduced if others retired when their potential accruals were high. For these columns we define high accrual as those in the top quartile of accrual levels among all retirees (over $32,400). We obtained similar results when we used a two-year forward looking accrual measure rather than one-year.

There are two, not necessarily mutually exclusive, types of possible explanations consistent with our findings. The first is that a failure to fully maximize pension benefits is a proxy for financial sophistication regarding the program. Hence, less savvy peers might lead an individual to retire at a date that does not fully maximize his or her own benefits. The second is that the preferences of others, in this case preferences that downweight the importance of (pension) financial gain versus leisure, might cause an individual to be less apprehensive about forgoing some financial benefit.

7 Conclusion

A unique data set and features of reforms of the California State Teachers' Retirement System allowed us to identify and estimate the effect of one's peers on one's own retirement decision. We demonstrated how one can identify the existence of peer effects using this natural experiment that had a differential effect on the members of each group. We found a statistically and economically significant peer effect. Our results suggest that for each additional peer retirement that is observed, a teacher's own likelihood of retirement increases by an additional 1.6-2.0 percentage points. The reforms, which consisted solely of a change in pension financial incentives allow us to directly calculate, in economic terms, the effect of unexpectedly changing others' pension wealth on one's own likelihood of retirement.

Our identification strategy could be used in other settings where a program has a differential effect on group members. Our strategy highlights the importance of having differential treatment effects within a group if one wishes to study the role of social effects. Further, in contrast to many studies that exploit exogenous assignment to groups, we are able to examine the impact of an unanticipated reform on existing networks of peers. In turn, such a setting may be more relevant for cases in which changing the nature of networks and associations among peers may be harder to accomplish.

Peer effects will play a role in shaping how individuals understand both recent and future Social Security reforms and changes to other retirement savings programs, such as 401(k)'s, and the speed and composition of the behavioral response. It is important to take into account the peer effect component of the retirement decision which generates a "spillover effect"- the behavior of one individual affects the behavior of his or her peer, and so on. Change in benefits or program rules will have both a direct effect on those targeted by the reform, and also on those affected by the retirement decision of others. To properly predict the effect of a reform one must both accurately estimate the direct effect and the spillover effect.

However, various types of social effects mechanisms imply very different policy and behavioral responses to change in policy. If the retirement of individuals is a response to the retirement of others, and people are influenced by or mimic others' retirement behavior, then any change to a specific segment of potential retirees will have far reaching effects even on those not initially targeted by the policy. If the retirement of individuals depends in part on being surrounded by better-informed peers or the dissemination of information is highly dependent on peers, then the effectiveness of any public information initiative is likely being underestimated. We examined some possible mechanisms through which the peer effects we found may operate. We found no evidence of school-specific retirement-age norms, but found some evidence that others' pension financial maximization behavior has an effect on own maximization of pension wealth. Given the importance of identifying the mechanism for public policy design, in future research we hope to further examine the mechanisms underlying the social effect at work in the retirement decision.

Appendix I- Identification and Estimation of Peer Effects using The Differential Reform and Individual Fixed Effects

We first show that the reform's differential effect is necessary for the identification of peer effects. We show that an unexpected shock that would affect all group members the same, would not allow for identification. Throughout this section, we assume a simultaneous retirement decision to both illustrate that a lag-specification is not the source of identification, and since this derivation may be useful for other settings in which lag-outcomes data is not available.[45] In our empirical investigation, we do make use of our panel data and lag-outcomes and are therefore not required to assume that individuals perceive and (correctly) predict others' future retirement as part of our framework.

We begin with the case of one period and illustrate the crucial role a differential reform plays.[46] To reduce the notational burden, and without loss of generality, we first consider a two-person model, L and H, who both belong to school s.

$$\begin{aligned} y_{L,s} &= \beta_0 + x'_L\beta + \gamma Z_s + \rho \cdot y_{H,s} + \epsilon_{L,s} \\ y_{H,s} &= \beta_0 + x'_H\beta + \gamma Z_s + \rho \cdot y_{L,s} + \epsilon_{H,s} \end{aligned} \tag{7.1}$$

Next, define P_L and P_H the effect of the reform on person L and H, and with out loss of generality assume $P_H > P_L$.

Suppressing the group affiliation s, and assuming the contextual effect is summarized by all other covariates (self-excluded) we can write the system of equations:

$$\begin{aligned} y_L &= \beta_0 + x'_L\beta + x'_H\gamma + \rho \cdot y_H + P_L + \epsilon_L \\ y_H &= \beta_0 + x'_H\beta + x'_L\gamma + \rho \cdot y_L + P_H + \epsilon_H \end{aligned} \tag{7.2}$$

Defining $D = P_H - P_L$, looking at the difference of the above, and rearranging terms we obtain:

$$y_H - y_L = (x'_H - x'_L)\frac{(\beta - \gamma)}{1+\rho} + \frac{D}{1+\rho} + \frac{\epsilon_H - \epsilon_L}{1+\rho}$$

Recall that D is the difference of the impact of the reform. If D (or a proxy of it) were known, then by estimating the above difference equation, we could identify and estimate ρ, the endogenous social effect.[47] Once ρ is identified one could then identify the contextual effect γ. Because the exact financial change in plan can be computed for each individual, if the effect of financial incentives on retirement were known, we could then compute D. Note that absent an endogenous

[45] In the case of a lag-specification (i.e., peers' previous period outcomes enter own decision), the derivation would be somewhat more involved but quite similar. Note that the lag-specification would allow to separately identify the contextual and endogenous effects even absent a reform, but that it would not suffice for addressing an unobservable school level effect- that would still require a differential reform.

[46] We are not the first to observe the role an exclusion restriction could play in identification. For example, see Moffitt (2001) for a discussion in the context of social interactions.

[47] This assumes, $E[\epsilon_H - \epsilon_L | X, D] = 0$, and so the reform must be uncorrelated with the unobservable error term. Note that the first type of reflection problem discussed in Section 3.2 exists even if $E[\epsilon_i \,|\, x_i, Z_s, i \in s] = 0$.

social effect ($\rho = 0$), the difference would just be D, the usual estimator. If $D = 0$, that is, the reform had the same effect on all participants, then one could not separate out the social effect.

In the case of two periods, it is straightforward to show that the above model would be identified even if an individual or school fixed effect were present. The period-difference would purge the individual fixed effect, and the within-school difference would be similar to the above derivation.

We now turn to the more general case, let P_i denote the unexpected financial incentive measure faced by agent i in school s. There are two time periods: $t = 0$ is the pre-reform year, and $t = 1$ is the post-reform year. and therefore $P_{i,t=0} = 0$ for all i. We allow for an individual fixed effect, μ_i, (which encompasses the case of a school fixed effect):

$$y_{i,t,s} = \beta_0 + x'_{i,t,s}\beta + \overline{X}'_{-i,t,s}\gamma + \rho\overline{Y}_{-i,t,s} + \mu_i + \epsilon_{i,t,s} \quad \text{for } t = 0 \qquad (7.3a)$$

$$y_{i,t,s} = \beta_0 + x'_{i,t,s}\beta + \overline{X}'_{-i,t,s}\gamma + \rho\overline{Y}_{-i,t,s} + P_i + \mu_i + \epsilon_{i,t,s} \quad \text{for } t = 1 \qquad (7.3b)$$

where we already substituted: $\overline{X}'_{-i,t,s} = \frac{1}{n_s-1}\sum_{j\in s, j\neq i} x_{j,t}$ and $\overline{Y}'_{-i,t,s} = \frac{1}{n_s-1}\sum_{j\in s, j\neq i} y_{j,t}$

Denoting Δ as the time-difference operator ($\Delta k = k_{t=1} - k_{t=0}$), and subtracting equation (7.3a) from (7.3b), the first difference with respect to year can be written as:

$$\Delta y_{i,s} = \Delta x'_{i,s}\beta + \Delta\overline{X}'_{-i,s}\gamma + \rho\Delta\overline{Y}_{-i,s} + P_i + \Delta\epsilon_{i,s}$$

Assuming that $E[\Delta\epsilon_{i,s}|P_i; s] = 0$, taking expectations of both sides of the above equation and rearranging terms yields the reduced-form:

$$\Delta y_{i,g} = \Delta x'_{i,s}\theta_1 + \Delta\overline{X}'_{-i,s}\theta_2 + \theta_3 P_{i,s} + \theta_4\overline{P}_{-i,s} + \Delta\epsilon_{i,s}$$

where $\overline{P}_{-i,s} = \frac{1}{n_s-1}\sum_{j\in s, j\neq i} P_j$. As before, note that if $P_i = p_s$ for all $i \in s$, one could no longer identify the above equation. Though the expressions for each coefficient θ depend on the variables $\beta, \gamma, \rho, \delta$ and the group size n_s, solving the system of equations, for the above specification, one can derive:

$$\rho = \frac{\frac{\theta_4}{\theta_3}(n_s - 1)}{\frac{\theta_4}{\theta_3}(n_s - 2) + n_s - 1}$$

and hence, the endogenous social effect ρ is identified. As before, given that ρ is identified we can then identify the contextual effect γ. Note that we did not require the existence of a differential reform (P) in both periods.

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Table 1: Pre- and Post-Reform Benefit Factor by Age and Service

Age	Pre-reform (Academic year 1997) All	Post-reform (Academic years 1998 and later) Years of Service <30	30+
55	1.40	1.40	1.60
56	1.52	1.52	1.72
57	1.64	1.64	1.84
58	1.76	1.76	1.96
59	1.88	1.88	2.08
60	2.00	2.00	2.20
61	2.00	2.13	2.33
62	2.00	2.27	2.40
63	2.00	2.40	2.40
64	2.00	2.40	2.40
65 and beyond	2.00	2.40	2.40

Notes:

This table reflects the change in the benefit factor due to the first CalSTRS reform (effective January 1, 1999). The benefit factor multiplied by years of service determines the fraction of salary that will be replaced by retirement benefits. For example, an individual that retires at age 60 with 30 years of service would receive an annual retirement benefit of (2.0*30=) 60% of salary before the reform and (2.2*30=) 66% of salary following the first reform.

Table 2: Summary Statistics for Academic Years 1997-2001; All LAUSD teachers, Ages 55-75

	Panel A: Teacher-level Characteristics (Teacher x Year); N=31,931			**Panel B: School-Level Characteristics** (School x Year); N=2,847		
				Reports the mean, std. dev., and median of the **school**		
variable	*Mean*	*Std. Dev.*	*Median*	*Mean of School-Mean*	*Std. Dev. of School-Mean*	*Median of School-Mean*
Retired during this academic year	0.089	0.285		0.088	0.113	0.059
Pension Wealth ($100k)	4.920	3.196	4.426	4.963	1.456	4.880
Peak Value ($10k)	9.400	8.302	9.302	2.410	2.741	2.678
Unexpected change in pension wealth ($100k) conditional on being affected by a reform[a]	0.803	0.349	0.839	0.822	0.201	0.835
Unexpected change in peak value ($10k) conditional on being affected by a reform[b]	3.231	3.815	3.022	2.410	2.678	2.741
Salary ($10k)	5.770	0.798	6.015	5.761	0.371	5.788
Age	59.850	3.906	58.950	59.815	1.620	59.698
Service	20.720	11.314	20.077	20.769	5.017	20.965
Female	0.722	0.448		0.803	0.203	0.857
Average peers' pension wealth ($100k)	4.916	1.274	4.867	4.956	1.452	4.874
Average peers' peak value ($10k)	9.348	3.305	9.301	9.461	3.763	9.421
Total of peers' unexpected change in pension wealth conditional on reform year ($100k)[c]	5.825	4.861	4.382	3.506	3.500	2.536
Average of peers' unexpected change in pension wealth conditional on reform year ($100k)[c]	0.342	0.188	0.318	0.356	0.215	0.335
Total of peers' unexpected change in peak value conditional on reform year ($10k)[c]	36.782	52.922	13.765	22.546	35.424	7.964
Average of peers' unexpected change in peak value conditional on reform year ($10k)[c]	2.197	2.397	1.021	2.295	2.502	1.490
Number of teachers age 55+ at school	17.726	12.911	14.000	10.265	9.159	7.000
Number of full-time equivalent teachers at school	79.556	44.325	71.000	55.443	34.600	46.820
Average age of all teachers at school	44.256	3.420	44.400	43.402	3.957	43.300
Average teaching experience of all teachers at school	13.087	2.718	13.000	12.613	3.106	12.374
Pupil to teacher ratio	21.972	3.381	21.649	20.554	3.364	19.889
Math test ranking at school level	5.544	2.612	5.500	5.498	2.657	5.333
Fraction of all teachers with at least an MA at school	0.290	0.091	0.291	0.271	0.101	0.259
Elementary school	0.507	0.500		0.721	0.449	
Middle school	0.191	0.393		0.124	0.330	
High school	0.252	0.434		0.086	0.280	
K-12 school	0.006	0.079		0.004	0.062	
Special Ed. School	0.002	0.046		0.003	0.053	

Notes:

[a] Observations are included conditional on a having non-zero value of the unexpected change in pension wealth at the individual level (5,464 individual-year observations).

[b] Observations are included conditional on having a non-zero value of the unexpected change in peak value at the individual level (8,792 individual-year observations).

[c] These include only observations in the years 1998 and 2000 (12,855 individual-year observations).

Table 3: Determinants of Retirement Excluding Peers' Retirement Behavior

All LAUSD Teachers in Academic Years 1997-2001, Ages 55-75; Linear Probability model; Dependent variable - retire

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Peak Value ($10k)	-0.0047***	-0.0047***	-0.0047***	-0.0040***	-0.0048***	-0.0041***	-0.0048***	-0.0039***
	(0.0005)	(0.0005)	(0.0005)	(0.0007)	(0.0005)	(0.0007)	(0.0005)	(0.0008)
Pension Wealth ($100k)	0.0398***	0.0398***	0.0398***	0.0655***	0.0396***	0.0662***	0.0409***	0.0669***
	(0.0036)	(0.0036)	(0.0036)	(0.0056)	(0.0036)	(0.0057)	(0.0037)	(0.0059)
Salary ($10k)	0.0453***	0.0457***	0.0457***	0.0645***	0.0463***	0.0642***	0.0478***	0.0639***
	(0.0036)	(0.0036)	(0.0037)	(0.0056)	(0.0037)	(0.0056)	(0.0039)	(0.0059)
Years of service in LAUSD	-0.0124***	-0.0123***	-0.0123***	-0.0189***	-0.0122***	-0.0190***	-0.0126***	-0.0191***
	(0.0008)	(0.0008)	(0.0008)	(0.0013)	(0.0008)	(0.0013)	(0.0009)	(0.0013)
Years of service in LAUSD (Squared)	0.0001***	0.0001***	0.0001***	0.0001*	0.0001***	0.0001*	0.0001***	0.0001*
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Is 56 year-old	-0.0346***	-0.0347***	-0.0347***	-0.0315***	-0.0349***	-0.0321***	-0.0350***	-0.0330***
	(0.0043)	(0.0043)	(0.0043)	(0.0061)	(0.0043)	(0.0061)	(0.0043)	(0.0064)
Is 57 year-old	-0.0577***	-0.0581***	-0.0580***	-0.0586***	-0.0585***	-0.0580***	-0.0594***	-0.0586***
	(0.0047)	(0.0047)	(0.0047)	(0.0070)	(0.0047)	(0.0071)	(0.0049)	(0.0074)
Is 58 year-old	-0.0690***	-0.0695***	-0.0694***	-0.0768***	-0.0705***	-0.0780***	-0.0720***	-0.0782***
	(0.0058)	(0.0058)	(0.0058)	(0.0083)	(0.0058)	(0.0084)	(0.0059)	(0.0088)
Is 59 year-old	-0.0698***	-0.0703***	-0.0702***	-0.0673***	-0.0721***	-0.0674***	-0.0730***	-0.0690***
	(0.0068)	(0.0068)	(0.0068)	(0.0102)	(0.0068)	(0.0104)	(0.0070)	(0.0106)
Is 60 year-old	-0.0119	-0.0124	-0.0123	-0.0435***	-0.0144	-0.0446***	-0.0153	-0.0433***
	(0.0093)	(0.0093)	(0.0093)	(0.0132)	(0.0093)	(0.0133)	(0.0094)	(0.0137)
Is 61 year-old	-0.0242**	-0.0245**	-0.0244**	-0.0194	-0.0261**	-0.0210	-0.0262**	-0.0182
	(0.0105)	(0.0105)	(0.0105)	(0.0155)	(0.0105)	(0.0157)	(0.0107)	(0.0162)
Is 62 year-old	-0.0061	-0.0065	-0.0065	-0.0168	-0.0078	-0.0171	-0.0055	-0.0094
	(0.0109)	(0.0109)	(0.0110)	(0.0163)	(0.0109)	(0.0164)	(0.0112)	(0.0171)
Is 63 year-old	-0.0191	-0.0192	-0.0191	-0.0020	-0.0204	-0.0023	-0.0180	0.0044
	(0.0124)	(0.0125)	(0.0125)	(0.0194)	(0.0125)	(0.0196)	(0.0128)	(0.0205)
Is 64 year-old	-0.0160	-0.0158	-0.0155	-0.0086	-0.0171	-0.0086	-0.0149	-0.0008
	(0.0132)	(0.0133)	(0.0133)	(0.0208)	(0.0132)	(0.0207)	(0.0135)	(0.0214)
Is 65 year-old	0.0098	0.0100	0.0104	0.0118	0.0090	0.0113	0.0116	0.0185
	(0.0176)	(0.0176)	(0.0177)	(0.0257)	(0.0177)	(0.0261)	(0.0180)	(0.0268)
Age over 65	0.0445***	0.0451***	0.0455***	0.0555***	0.0445***	0.0560***	0.0480***	0.0647***
	(0.0123)	(0.0123)	(0.0124)	(0.0186)	(0.0125)	(0.0191)	(0.0130)	(0.0198)
Female	-0.0103**	-0.0077*	-0.0076*	-0.0240***	-0.0066	-0.0242***	-0.0067	-0.0256***
	(0.0043)	(0.0043)	(0.0043)	(0.0067)	(0.0043)	(0.0067)	(0.0045)	(0.0069)
Number of full-time equivalent teachers		-0.0002*	-0.0001	-0.0002	-0.0003**	-0.0003*		
		(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0002)		
Number of teachers age 55+			-0.0005	0.0004	-0.0001	0.0009*		
			(0.0004)	(0.0004)	(0.0003)	(0.0005)		
Average age of teachers 55+			-0.0006	-0.0021	0.0009	-0.0013		
			(0.0012)	(0.0017)	(0.0012)	(0.0018)		
Average service of teachers 55+			-0.0001	-0.0004	-0.0003	-0.0012**		
			(0.0004)	(0.0006)	(0.0004)	(0.0006)		
Fraction of teachers that are female		0.0155	0.0186	0.0448	0.0388	0.0763*		
		(0.0259)	(0.0259)	(0.0382)	(0.0296)	(0.0417)		
Fraction of teachers with Masters or higher		-0.0797***	-0.0719***	-0.0919**	-0.0600*	-0.0638		
		(0.0275)	(0.0276)	(0.0397)	(0.0325)	(0.0467)		
Pupil to teacher ratio		-0.0002	-0.0001	0.0000	0.0003	0.0016		
		(0.0008)	(0.0008)	(0.0015)	(0.0008)	(0.0016)		
Avg. rank on standardized math test		-0.0005	-0.0005	-0.0025**	-0.0008	-0.0037**		
		(0.0008)	(0.0008)	(0.0012)	(0.0011)	(0.0016)		
Constant	-0.0857***	-0.0625**	-0.0312	0.0007	-0.1355*	-0.0694	-0.1035***	-0.1461***
	(0.0140)	(0.0311)	(0.0767)	(0.1082)	(0.0747)	(0.1138)	(0.0148)	(0.0212)
Zip code fixed effects					Yes	Yes		
School fixed effects							Yes	Yes
Only academic years 1999 and 2001				Yes		Yes		Yes
R-squared	0.113	0.114	0.114	0.157	0.119	0.166	0.136	0.197
Sample size	31,931	31,931	31,931	13,555	31,931	13,555	31,931	13,555

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors, in parentheses, are clustered at the school level, allowing any correlation across individuals and years within school.

All specifications include controls for year fixed effects, and school grade level categories (except columns (7) and (8)). Age 55 is the excluded age group.

Table 4: Difference-in-Difference

All LAUSD Teachers in Academic Years 1998-2001, Ages 55-75; Linear Probability model; Dependent variable - retire

	(1)	(2)	(3)	(4)
Post x Treat	0.0404*	0.0423*	0.0439**	0.0375*
	(0.0237)	(0.0231)	(0.0218)	(0.0223)
Treat	0.0062	-0.0058	-0.0210	-0.0220
	(0.0177)	(0.0168)	(0.0170)	(0.0189)
Academic year 1999	-0.0006	0.0085	-0.0005	-0.0002
	(0.0055)	(0.0060)	(0.0058)	(0.0059)
Academic year 2000	0.0004	-0.0262***	-0.0280***	-0.0264***
	(0.0067)	(0.0068)	(0.0071)	(0.0074)
Academic year 2001	0.0053	-0.0146**	-0.0176***	-0.0158**
	(0.0064)	(0.0064)	(0.0067)	(0.0071)
Own unanticipated change in index of pension		1.9888***	-0.3440**	-0.3572**
financial incentives in the previous year		(0.1080)	(0.1362)	(0.1386)
Post x Own unanticipated change in index of		-0.0863	0.0209	0.0694
pension financial incentives in the previous year		(0.1618)	(0.1535)	(0.1550)
Peak Value ($10k)			-0.0053***	-0.0053***
			(0.0006)	(0.0006)
Pension Wealth ($100k)			0.0411***	0.0418***
			(0.0042)	(0.0043)
Salary ($10k)			0.0467***	0.0469***
			(0.0042)	(0.0043)
Years of service in LAUSD			-0.0128***	-0.0126***
			(0.0010)	(0.0010)
Years of service in LAUSD squared			0.0001***	0.0001***
			(0.0000)	(0.0000)
Additional Individual level controls			Yes	Yes
School level controls				Yes
R-squared	0.001	0.035	0.118	0.120
Sample size	24,447	24,447	24,447	23,766

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors, in parentheses, are clustered at the school level, allowing any correlation across individuals and years within school.

All specifications include observations from academic years 1998-2001. The two reforms are stacked so that there are two post periods, academic years 1999 and 2001. We enter year effects into the regression individually rather than including a single "Post" dummy. Including the single Post dummy does not alter the results. The individual's treatment assignment (Lag total unanticipated change in index of pension financial incentives of peers) is fixed, based on the group characteristics in one's school in the year before each pension reform (1997, 1999). Individual-level controls include peak value, pension wealth, age dummies, month of birth dummies, years of service, salary, and dummy for female. The school-level controls include average age and service of retirement-eligible teachers, school size (full-time equivalents), school grade level, fraction of teachers in the school that have a masters degree, fraction of teachers in the school that are female, and rank of the school based on student standardized test scores. Some observations are missing one or more school-level characteristics, reducing the number of observations in specification (4).

Table 5: The Effect of Peers' Unanticipated Change in Financial Incentives on Own Retirement

All LAUSD Teachers Ages 55-75; Dependent variable- Retirement; Linear probability model

	(1)	(2)	(3)	(4)	(5)	(6)
Lag total unanticipated change in pension wealth of peers, self excluded ($100k)	0.0026*** (0.0010)	0.0027*** (0.0010)	0.0023** (0.0010)	0.0027** (0.0011)	0.0017** (0.0007)	0.0018*** (0.0005)
Peak Value ($10k)	-0.0042*** (0.0007)	-0.0061*** (0.0008)	-0.0061*** (0.0008)	-0.0042*** (0.0007)	-0.0042*** (0.0006)	0.0028 (0.0018)
Pension Wealth ($100k)	0.0628*** (0.0058)	0.0709*** (0.0057)	0.0708*** (0.0056)	0.0628*** (0.0058)	0.0466*** (0.0046)	0.0859*** (0.0104)
Unexpected change in own pension wealth last year ($100k)		-0.1169*** (0.0140)	-0.1166*** (0.0139)			
Salary ($10k)	0.0723*** (0.0062)	0.0745*** (0.0062)	0.0740*** (0.0062)	0.0723*** (0.0062)	0.0509*** (0.0047)	0.5399*** (0.0267)
Years of service in LAUSD	-0.0187*** (0.0014)	-0.0238*** (0.0015)	-0.0237*** (0.0015)	-0.0187*** (0.0014)	-0.0143*** (0.0011)	
Years of service in LAUSD squared	0.0001** (0.0000)	0.0002*** (0.0000)	0.0002*** (0.0000)	0.0001** (0.0000)	0.0001*** (0.0000)	0.0011*** (0.0002)
Female	-0.0223*** (0.0069)	-0.0169** (0.0069)	-0.0168** (0.0069)	-0.0223*** (0.0069)	-0.0109* (0.0056)	
Number of full-time equivalent teachers at school	-0.0003** (0.0001)	-0.0003** (0.0001)	-0.0003** (0.0001)	-0.0003 (0.0002)	-0.0003*** (0.0001)	-0.0012** (0.0006)
Number of teachers age 55+ (self excluded)				0.0000 (0.0004)		
Average age of teachers 55+ (self excluded)	-0.0029* (0.0018)	-0.0028 (0.0018)	-0.0027 (0.0018)	-0.0029* (0.0018)	-0.0025 (0.0016)	0.0002 (0.0025)
Average service of teachers 55+ (self excluded)	-0.0013** (0.0006)	-0.0013** (0.0006)	-0.0016 (0.0010)	-0.0013** (0.0006)	-0.0006 (0.0006)	-0.0014 (0.0010)
Additional individual, year, and school controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual Fixed-Effects						Yes
Lagged years of service categories of other teachers and others average salary			Yes			
Constant	0.0101	0.0411	0.0464	0.0097	0.0819	-24.3512
R-squared	0.159	0.165	0.166	0.159	0.134	0.312
Sample Size	12,602	12,602	12,602	12,602	18,895	24,708

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors, in parentheses, are clustered at the school level, allowing any correlation across individuals and years within school.

Columns (1)-(6) correspond to the IV specifications in Table 6 columns (2)-(7). All specifications include controls for age dummies, year fixed effects, pupil to teacher ratio, school grade level categories, fraction of teachers with a masters degree or higher, fraction of teachers that are female, and average rank on students' standardized math test scores. Sample for columns (1)-(4) is academic years 1999 and 2001 (with the lagged variables computed using academic years 1998 and 2000). Sample for column (5) is academic years 1999-2001. Sample for Column (6) is academic years 1998-2001 for those who did not move between schools.

Table 6: Two-Stage-Least-Squares Estimates of Peer Effects

All LAUSD Teachers Ages 55-75; 2SLS; Dependent variable- Retirement

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Number of retirees among those 55+ in previous year (self excluded)	0.0063***	**0.0185****	**0.0193****	**0.0183****	**0.0165****	**0.0213***	**0.0436*****
	(0.0019)	**(0.0075)**	**(0.0077)**	**(0.0086)**	**(0.0081)**	**(0.0116)**	**(0.0133)**
Peak Value ($10k)	-0.0043***	-0.0043***	-0.0062***	-0.0061***	-0.0043***	-0.0042***	0.0022
	(0.0007)	(0.0007)	(0.0008)	(0.0008)	(0.0007)	(0.0006)	(0.0019)
Pension Wealth ($100k)	0.0628***	0.0628***	0.0709***	0.0708***	0.0628***	0.0465***	0.0842***
	(0.0058)	(0.0058)	(0.0057)	(0.0056)	(0.0058)	(0.0047)	(0.0107)
Unexpected change in own pension wealth ($100k) last year			-0.1163***	-0.1160***			
			(0.0140)	(0.0139)			
Salary ($10k)	0.0724***	0.0725***	0.0747***	0.0739***	0.0725***	0.0505***	0.5394***
	(0.0063)	(0.0063)	(0.0062)	(0.0062)	(0.0063)	(0.0047)	(0.0269)
Years of service in LAUSD	-0.0188***	-0.0189***	-0.0239***	-0.0237***	-0.0188***	-0.0144***	
	(0.0014)	(0.0014)	(0.0015)	(0.0015)	(0.0014)	(0.0011)	
Years of service in LAUSD squared	0.0001**	0.0001**	0.0002***	0.0002***	0.0001**	0.0001***	0.0011***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0002)
Female	-0.0218***	-0.0214***	-0.0160**	-0.0161**	-0.0215***	-0.0105*	
	(0.0069)	(0.0069)	(0.0070)	(0.0070)	(0.0069)	(0.0056)	
Number of full-time equivalent teachers at school	-0.0002**	-0.0004**	-0.0004**	-0.0004**	-0.0004**	-0.0005***	-0.0019***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0007)
Number of teachers age 55+ (self excluded)					0.0002		
					(0.0003)		
Average age of teachers 55+ (self excluded)	-0.0022	-0.0023	-0.0022	-0.0023	-0.0023	-0.0019	0.0099**
	(0.0017)	(0.0018)	(0.0018)	(0.0018)	(0.0017)	(0.0015)	(0.0039)
Average service of teachers 55+ (self excluded)	-0.0006	-0.0005	-0.0005	0.0008	-0.0005	-0.0003	0.0007
	(0.0006)	(0.0006)	(0.0006)	(0.0014)	(0.0006)	(0.0005)	(0.0012)
Additional individual, year, and school controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Fixed-Effects							Yes
Lagged years of service categories of other teachers and others average salary				Yes			
Constant	-0.0483	-0.0305	-0.0014	0.0706	-0.0236	0.0333	
R-squared of second stage	0.159	0.156	0.162	0.164	0.157	0.129	0.276
Sample Size	12,602	12,602	12,602	12,602	12,602	18,895	22,381

Panel B- First Stage of the above specifications (the variable instrumented above is in bold)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Instrument [t-stat in square-brackets]							
Lag total unanticipated change in pension wealth of peers, self excluded ($100k)		0.1408***	0.1409***	0.1280***	0.1606***	0.0776***	0.0402***
		(0.0055)	(0.0055)	(0.0057)	(0.0065)	(0.0041)	(0.0041)
		[25.43]	[25.43]	[22.27]	[24.42]	[18.76]	[9.74]
F-statistic for excluded instrument		18.81	18.83	14.57	18.39	10.05	94.82
P-value for the above test		0.0000	0.0000	0.0001	0.0000	0.0016	0.0000
R-squared of first stage		0.43	0.43	0.46	0.44	0.40	0.08

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors, in parentheses, are clustered at the school level, allowing any correlation across individuals and years within school.

All specifications include controls for age dummies, year fixed effects, pupil to teacher ratio, school grade level categories, fraction of teachers with a masters degree or higher, fraction of teachers that are female, and average rank on students' standardized math test scores. Sample for columns (1)-(5) is academic years 1999 and 2001 (with the lagged variables computed using academic years 1998 and 2000). Sample for column (6) is academic years 1999-2001. Sample for Column (7) is academic years 1998-2001 for those who did not move between schools.

Table 7: Alternative Group Specification and Falsification Tests

All LAUSD Teachers Ages 55-75; Dependent variable- Retirement (cols. 1-7), Salary (col. 8); Variable for which IV is used in bold; OLS and 2SLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rate of retirement among those 55+ in previous year	**0.1814*** **(0.0965)**							
Number of retirees among those 55+ in previous year		**0.0183**** **(0.0072)**			0.0054*** (0.0017)	0.0060** (0.0023)	**0.0108*** **(0.0060)**	
Number of retirees (self excluded) among those 55+ in **current** year			**-0.0224** **(0.0170)**					
Number of retirees (self excluded) among those 55+ two years prior				**0.0001** **(0.0068)**	-0.0007 (0.0020)	-0.0016 (0.0025)		
Number of retirees (self excluded) among those 55+ three years prior						0.0025 (0.0025)		
Number of retirees (self excluded) in the **following** year (lead)							**0.0043** **(0.0128)**	
Lag total salary of peers, self excluded ($10k)								-0.0001 (0.0002)
Constant	-0.1348	-0.0308	-0.1999	0.0595	0.0196	0.2423	-0.1791	3.35
R-squared of second stage	0.158	0.156	0.079	0.084	0.134	0.124	0.244	0.688
Sample Size	12,602	12,602	12,855	12,300	17,532	11,092	6,004	12,602

Panel B- First Stage of the above specifications (the variable instrumented above is in bold)

Instrument [t-stat in square-brackets]	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total unanticipated change in pension wealth of peers, self excluded ($100k) corresponding to the previous/current/future year	0.2066*** (0.0125) [16.49]	0.1514*** (0.0057) [26.24]	0.1025*** (0.0057) [17.99]	0.1346*** (0.0053) [25.37]			IV for Previous Year: 0.2615*** (0.0076) [34.12] IV for Following Year: 0.0562*** (0.0130) [4.31]	
Total unanticipated change in peak value of peers, self excluded ($10k) in previous year		-0.0023*** (0.0004) [5.27]						
F-statistic for excluded instrument(s)	78.03	10.65	9.04	19.97			24.58 for prev.; 2.79 for future yr.	
P-value for the above test	0.0000	0.0000	0.0000	0.0000			0.000 for prev.; 0.062 for future yr.	
R-squared of first stage	0.09	0.44	0.43	0.43			.53 for previous year; .41 for future year	

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors, in parentheses, are clustered at the school level, allowing any correlation across individuals and years within school.

All specifications include controls for own gender, years of service, peak value, pension wealth, salary, age dummies, year fixed effects, average years of service among other teachers, average age of other teachers, number of full time equivalent teachers, pupil to teacher ratio, school grade level categories, fraction of teachers with a masters degree or higher, fraction of teachers that are female, and average rank on students' standardized math test scores. Sample for columns 1, 2 and 8 is academic years 1999 and 2001. Sample for column 3 is academic year 1998 and 2000. Sample for column 4 is academic years 2000 and 2002. Sample for column 5 and 6 is academic years 1999-2001. Sample for column 7 is academic year 1999.

Table 8- The Effect of Age Norms on Retirement

All LAUSD Teachers in Academic Years 1999, 2001, Ages 55-75;
OLS (cols. (1)-(2)) and 2SLS (cols. (3)-(6)); Dependent variable - retire

	(1)	(2)	(3)	(4)	(5)	(6)
Age gap squared	0.0005	-0.0001	0.0008		0.0006	
	(0.0005)	(0.0004)	(0.0006)		(0.0006)	
Age gap squared x Older than modal retirement age		0.0026			0.0012	
		(0.0019)			(0.0021)	
Age gap squared (2yr mode)				0.0009		0.0007
				(0.0007)		(0.0007)
Age gap squared x Older than modal retirement age (2 yr mode)						0.0008
						(0.0017)
Number of retirees among those 55+ in previous year			**0.0230****	**0.0168****	**0.0229***	**0.0169****
			(0.0116)	**(0.0082)**	**(0.0117)**	**(0.0082)**
Peak Value ($10k)	-0.0044***	-0.0043***	-0.0044***	-0.0044***	-0.0044***	-0.0043***
	(0.0009)	(0.0009)	(0.0009)	(0.0008)	(0.0009)	(0.0008)
Pension Wealth ($100k)	0.0632***	0.0647***	0.0628***	0.0640***	0.0635***	0.0645***
	(0.0071)	(0.0071)	(0.0071)	(0.0065)	(0.0072)	(0.0065)
Salary ($10k)	0.0688***	0.0677***	0.0688***	0.0679***	0.0683***	0.0675***
	(0.0078)	(0.0077)	(0.0077)	(0.0070)	(0.0078)	(0.0069)
Years of service in LAUSD	-0.0186***	-0.0188***	-0.0187***	-0.0182***	-0.0188***	-0.0183***
	(0.0017)	(0.0017)	(0.0017)	(0.0015)	(0.0017)	(0.0015)
Years of service in LAUSD squared	0.0001*	0.0001*	0.0001*	0.0001	0.0001*	0.0001
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
R-squared	0.159	0.160	0.154	0.155	0.154	0.155
Sample size	8,525	8,525	8,525	10,317	8,525	10,317

Panel B- First Stage of the above specifications (instrumented variable in bold)

Instrument [t-stat in square-brackets]						
Lag total unanticipated change in pension wealth of peers, self excluded ($100k)			0.1112***	0.1359***	0.1107***	0.1370***
			(0.0064)	(0.0061)	(0.0064)	(0.0060)
			[17.22]	[22.45]	[17.34]	[22.73]
R-squared of first stage			0.31	0.38	0.32	0.38

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%
Standard errors, in parentheses, are clustered at the school level, allowing any correlation across individuals and years within school.
All specifications include observations from academic years 1999 and 2001. The Age gap squared is the number of years between own age and the modal retirement age at one's school in the previous year (columns (1),(2), (3) and (5)) or in the previous two years (columns (4) and (6)), squared. In specifications (2), (5), and (6) Age gap squared is allowed to have an additional effect on retirement if the individual is older than the school's modal retirement age. Columns (3) – (6) control for the number of retirees in one's school in the previous year, which is instrumented as in Table 6. Additional controls included in all specifications are age dummies, dummy for female, average age and service of other retirement-eligible teachers in the school, school size (full-time equivalents), school grade level, fraction of teachers in the school that have a masters degree, fraction of teachers in the school that are female, rank of the school based on student standardized test scores, and year fixed effects.

Table 9: The Effects of Others' Pension Maximization

All LAUSD Teachers Ages 55-75 in AY 1999, 2001; 2SLS; Dependent variable- Retirement	(1)	(2)	(3)	(4)
Number of retirees last year with Peak Value over $50K X Own Peak Value over $50K	**0.0777*** **(0.0463)**	**0.0842*** **(0.0500)**		
Number of retirees last year with 1-year accrual in top quartile X own accrual in top quartile			**0.1919*** **(0.0998)**	**0.2067*** **(0.1091)**
Own Peak Value over $50K		-0.0621** (0.0292)		
Own 1-year accrual in top quartile				-0.1015** (0.0451)
Peak Value ($10k)	-0.0049*** (0.0009)		-0.0087*** (0.0026)	-0.0034*** (0.0010)
Pension Wealth ($100k)	0.0650*** (0.0059)	0.0722*** (0.0053)	0.0515*** (0.0086)	0.0649*** (0.0062)
Years of service in LAUSD	-0.0185*** (0.0014)	-0.0209*** (0.0013)	-0.0195*** (0.0015)	-0.0186*** (0.0014)
Years of service in LAUSD squared	0.0001 (0.0000)	0.0001** (0.0000)	0.0001*** (0.0000)	0.0001* (0.0000)
Salary ($10k)	0.0720*** (0.0063)	0.0610*** (0.0056)	0.0810*** (0.0083)	0.0718*** (0.0068)
Female	-0.0253*** (0.0071)	-0.0394*** (0.0063)	-0.0266*** (0.0073)	-0.0259*** (0.0072)
Additional individual, year, and school controls	Yes	Yes	Yes	Yes
Constant	-0.1543	-0.13	-0.0199	-0.0664
R-squared of second stage	0.135	0.13	0.102	0.102
Sample Size	12,602	12,602	12,602	12,602

Panel B- First Stage of the above specifications (instrumented variable in bold)

Instrument [t-stat in square-brackets]				
Lag total unanticipated change in pension wealth of peers, self excluded ($100k)	0.0365*** (0.0034) [10.58]	0.0353*** (0.0033) [10.41]	0.0148*** (0.0020) [7.40]	0.0139*** (0.0018) [7.46]
R-squared of first stage	0.21	0.24	0.16	0.24

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors, in parentheses, are clustered at the school level, allowing any correlation across individuals and years within school.

All specifications include controls for age dummies, year fixed effects, average years of service among other teachers, average age of other teachers, number of full time equivalent teachers, pupil to teacher ratio, school grade level categories, fraction of teachers with a masters degree or higher, fraction of teachers that are female, and average rank on students' standardized math test scores. Sample for columns (1)-(4) is academic years 1999 and 2001 (with the lagged variables computed using academic years 1998 and 2000).