XIX. Empirical Studies of Retirement Plans: Implications for Workers, Firms and Public Policy

Peer Effects and the Timing of Retirement: Evidence from Los Angeles School Teachers

KRISTINE M. BROWN
RON A. LASCHEVER
University of Illinois at Urbana-Champaign

Abstract

We examine the effect of peers on an individual's likelihood of retirement using data from the Los Angeles Unified School District. Using an administrative dataset of the full population of district teachers ages 55 and over, for the years 1997 to 2001 (*n =31,931)*, we construct school-level peer groups. We instrument for others' retirement using two large pension reforms that differentially impacted the financial incentives for retirement within and across schools. Controlling for individual and school characteristics, and including individual fixed effects, our instrumental-variable estimates of the effect of colleagues' retirement on a teacher's own likelihood of retirement are sizable and statistically significant.

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 one's 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 they lend support to the importance of

Authors' address: Department of Economics and School of Labor and Employment Relations, 504 E. Armory Ave., Champaign, IL 61820; kmb@illinois.edu, ronL@illinois.edu

co-workers and friends in retirement planning. Our paper builds on this literature by examining the direct effect of peers on the retirement decision. 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, Johnson, and Reuter (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 an individual's 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 to 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. 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 an instrumental variables framework to estimate the effect of peers on retirement behavior. We 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 someone's likelihood of retirement. The exogenous, unexpected shock to the pension financial incentives 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 one's likelihood of retirement by 1.8 percentage points. A host of robustness checks and two types of falsification tests provide 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 results highlight the importance of others' behavior in the retirement decision and are consistent with a social multiplier in retirement. For example, the rise in the Social Security normal retirement age could cause those who 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 preexisting groups. In the context of reforms of retirement financial incentives or any similar reform, a change in regulation for preexisting peer groups is more likely to occur than legislation seeking to rearrange the peer assignment of those about to retire.

Retirement Program for Los Angeles Teachers

The Los Angeles Unified School District (LAUSD) is the second largest school district in the United States, with over 820 schools and nearly 700,000 students. 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. Participation in the contributory program 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, and increasing in, years of service, age, and past salary.[3] The CalSTRS pension is relatively generous, and California teachers are not covered by Social Security, so for career teachers the pension is likely an important consideration for their retirement decisions.

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. The effect of the reform on individual financial incentives is a function of the individual's age and service at the time of the reform. This differential impact of the reforms is the foundation of our identification and estimation strategies.

In August of 1998, the California state legislature increased the generosity of the pension for California teachers retiring on or after January 1, 1999, with the passage of two bills, AB 1102 and AB 1150. The reforms increased the annual benefits for retirements at age 60 or older and/or at 30 years of service or more, but benefit levels for retirements at earlier ages or service levels were not affected. Current pension wealth, the present value of pension income for immediate retirement, increased by 20 percent for 63-year-olds and by at least 10 percent for those with 30 years of service. On the other hand, the financial return to working increased for those younger than age 63 and for those with less than 30 years of service. For example, the return to working an additional year doubled for 60-year-olds.

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. This reform provided a lump sum longevity bonus to teachers at three target service levels. For the average teacher who was eligible for the bonus at the time of the reform, pension wealth increased unexpectedly by 6 to 13 percent. For teachers who had not reached all service targets, the financial return to work increased.

Retirement Decision

Retirement Decision Without Peer Effects

In order to focus on the identification and estimation of the effect of peers, we estimate a parsimonious reduced-form model of retirement, as it allows us to leverage the pension reform described in the previous section. 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 individual's retirement decision is to treat it as a utility maximization problem over two goods—lifetime consumption and years in retirement subject to the budget constraint he or she faces. The pension features enter the lifetime budget constraint in two ways. The pension contributes to the level of lifetime earnings and also affects the return to working an additional year. Informally, an increase in the generosity of pension benefits at any age will create an income effect, providing an incentive for individuals to consume more leisure and thus to be more likely to retire, all else equal. On the other hand, an increase in the level of benefits for retirement at a future age will, by increasing the return to continued work, create a substitution effect, and thus an individual will be less likely to retire at the current age, all else equal.

We capture these essential features of the pension with two simple measures. First, current pension wealth, the present value of the stream of retirement income for retirement today, captures the level of benefits. For an individual who is age *t* in the current period, pension wealth is defined in our empirical model as $PW_t = \sum_{a=t}^{100} \pi_{a|t} (\frac{1}{1+r})^{a-t} B(t)$, where *B(t)* is the annual retirement benefit the person 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*.

Second, we use the peak value measure (Coile and Gruber 2007), the maximum potential increase in pension wealth that can be achieved with continued work. It captures the financial return to continued work and is defined as the difference between pension wealth for retirement at the current age and the pension wealth for retirement at the future age that maximizes pension wealth.[4] We use peak value rather than the one-year accrual of pension wealth as a measure of the financial return to working because it enables us to capture discontinuous jumps in pension wealth at age or service cutoffs.

The econometric specification of the retirement decision $y_{i,s,t}$ of individual *i* at time *t* can be written as follows[5]:

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

where $U^*_{i,s,t}$ is the latent utility of individual *i*, in school *s* for retirement in year *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. 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, $\varepsilon_{i,s,t}$, capture changes such as health shocks. 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.

Retirement Decision Incorporating Peer Effects

We consider three possible types of group-level effects and denote each individual's school by *s=1..S*.[6] The first type of measure is the behavior or outcomes of all others, $Y_{-i,s}$, where the notation *–i* denotes that one's 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. 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 an individual's 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.

We include the three types of social effects in the aforementioned latent-utility framework:

$$y_{i,s,t} = \beta_0 + x_{i,s,t}^{'}\beta + Z_{s,t}^{'}\gamma + \rho Y_{-i,s,t-1} + \phi_s + \phi_t + \varepsilon_{i,s,t}$$

The retirement decision $y = \{0,1\}$ is also a function of the individual covariates x_i and an error term $\varepsilon_{i,s,t}$, a scalar capturing the individual's unobservable taste for retirement. We focus on the linear specification. However, our estimates for the reduced-form specification using logit are qualitatively the same, and the marginal effects are similar.

The detailed panel data at our disposal allows us to examine how lagged (and therefore observable to teachers) retirements of others affect individuals. For institutional reasons, though our data contain the actual calendar date of every teacher's retirement, we group retirements by academic year.[7] We focus our investigation on lag-outcomes specifications. In addition to being more consistent with the institutional detail and typical academic year cycles, 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.[8]

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 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.

We 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), in the case of peer effects, a valid instrument—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. In Brown and Laschever (2009), we 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. The panel data structure allows us to address the concern regarding an unobservable individual-level (and school-level) fixed effect. 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.[9]

Data

In this study we used individual-level administrative data from the Los Angeles Unified School District (LAUSD)[10] that has been matched to school-academic-year-level characteristics available in the public CBEDS data from the California Department of Education.[11] The data includes an annual census of active teachers ages 45 and older for each of the academic years 1997 to 2003 and all retirement episodes during this time period. We focus on retirement-eligible teachers, those ages 55 or older, in academic years 1997 to 2001. Each person-year observation (*n = 31,931*) includes age, years of service, salary, gender, retirement date (if applicable) and school assignment (606 unique schools are observed). 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.

Table 1 shows the summary statistics of the data at the individual-level in panel A and at the school-level in panel B, where the school-level variables are the within-school-averages of individual level variables. The retirement-eligible LAUSD teachers are on average almost 60 years old and have 20 years of service within LAUSD. Over 70% are women, and the annual retirement rate for this group is 9 percent. As a group they have an average pension wealth of almost $500,000, with a peak value of over $90,000.[12] This implies that on average, these teachers could increase their pension wealth by up to 20 percent 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.

Key variables for our estimation are the retirement rate of peers and the unexpected change in pension wealth of peers, our instrument for peer retirements. The standard deviations in Panel B illustrate that there is substantial variation in these and other peer characteristics across schools. The average retirement rate across school-by-year observations is 8.8 percent, and the standard deviation is 11.3 percent. The mean total unanticipated change in peers' pension wealth for those whose pension wealth was affected by the reform is $350,000, with a standard deviation of $350,000.

Results

Baseline Estimates

It is of use to first consider a baseline case assuming there are no peer effects. We estimate a linear probability model.[13] Column 1 of Table 2 presents the results of our preferred baseline specification for retirement-eligible teachers in academic years 1999 and 2001 (the years that correspond to our IV estimation). It includes own pension financial measures, integer age dummies with age 55 as the excluded group, years of service, salary, gender, year fixed effects, and controls for school characteristics and characteristics of the teaching population in the school.

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 section. The coefficient estimates of both pension wealth and peak value are statistically significant at the 1 percent level and are of the predicted sign. A $100,000 increase in pension wealth is associated with a 6.5 percentage point increase in the probability of retirement. A $10,000 increase in peak value is associated with a 0.40 percentage point decrease in the likelihood of retirement.

The coefficient estimates on peak value and pension wealth are stable across alternative specifications, including one with school fixed effects. For the school-fixed-effects specification, 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.

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TABLE 1
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 school's mean, standard deviation, and median		
Variable	Mean	Std dev	Median	School's mean–mean	School's std dev–mean	School's median–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
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	

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 2
OLS and Two-Stage-Least-Squares Estimates

All LAUSD teachers ages 55–75; 2SLS; dependent variable: retirement; academic years 1999 and 2001

	1	2	3	4	5	6
Number of retirees among those 55+ in previous year (self excluded)			0.0063***	**0.0185****	**0.0183****	**0.0436*****
			(0.0019)	**(0.0075)**	**(0.0086)**	**(0.0133)**
Lag total unanticipated change in pension wealth of peers, self excluded ($100K)		0.0027***				
		(0.0010)				
Peak value ($10K)	-0.0040***	-0.0061***	-0.0043***	-0.0043***	-0.0061***	0.0022
	(0.0007)	(0.0008)	(0.0007)	(0.0007)	(0.0008)	(0.0019)
Pension wealth ($100K)	0.0655***	0.0709***	0.0628***	0.0628***	0.0708***	0.0842***
	(0.0056)	(0.0057)	(0.0058)	(0.0058)	(0.0056)	(0.0107)
Unexpected change in own pension wealth ($100K) last year		-0.1169***			-0.1160***	
		(0.0140)			(0.0139)	
Salary ($10K)	0.0645***	0.0745***	0.0724***	0.0725***	0.0739***	0.5394***
	(0.0056)	(0.0062)	(0.0063)	(0.0063)	(0.0062)	(0.0269)
Years of service in LAUSD	-0.0189***	-0.0238***	-0.0188***	-0.0189***	-0.0237***	
	(0.0013)	(0.0015)	(0.0014)	(0.0014)	(0.0015)	
Years of service in LAUSD squared	0.0001*	0.0002***	0.0001**	0.0001**	0.0002***	0.0011***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0002)
Female	-0.0240***	-0.0169**	-0.0218***	-0.0214***	-0.0161**	
	(0.0067)	(0.0069)	(0.0069)	(0.0069)	(0.0070)	
Number of full-time equivalent teachers at school	-0.0002	-0.0003**	-0.0002**	-0.0004**	-0.0004**	-0.0019***
	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0007)
Average age of teachers 55+ (self excluded)	-0.0021	-0.0028	-0.0022	-0.0023	-0.0023	0.0099**
	(0.0017)	(0.0018)	(0.0017)	(0.0018)	(0.0018)	(0.0039)
Average service of teachers 55+(self excluded)	-0.0004	-0.0013**	-0.0006	-0.0005	0.0008	0.0007
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0014)	(0.0012)
Individual fixed-effects						Yes
Lagged years of service categories of other teachers and others average salary					Yes	
Constant	0.0007	0.0411	-0.0483	-0.0305	0.0706	
R-squared of second stage	0.157	0.165	0.159	0.156	0.164	0.276
Sample size	13,555	12,602	12,602	12,602	12,602	22,381

Panel B: First stage of the above specifications (the variable instrumented above is in bold)

Instrument [t-stat in square-brackets]	4	5	6
Lag total unanticipated change in pension wealth of peers, self excluded ($100k)	0.1408***	0.1280***	0.0402***
	(0.0055)	(0.0057)	(0.0041)
	[25.43]	[22.27]	[9.74]

	4	5	6
F-statistic for excluded instrument	18.81	14.57	94.82
P-value for the above test	0.0000	0.0001	0.0000
R-squared of first stage	0.43	0.46	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 master's 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 1998-2001 for those who did not move between schools.

The coefficient estimates on peak value and pension wealth are stable across alternative specifications, including one with school fixed effects. For the school-fixed-effects specification, 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.

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

In this section, we examine the effect of others' retirement behavior on an individual's likelihood of retirement. We use the unexpected pension reform to overcome some of the aforementioned identification issues via an IV strategy. We use the two reforms (academic years 1998 and 2000) to construct measures of the unanticipated change in pension financial incentives faced by one's peers, which we use to instrument for the retirement of others. 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, we begin by directly using the unanticipated change in pension financial incentives as a predictor of change in retirement behavior. For each individual i, for both the first and second reforms, we define the unanticipated change in individual *i*'s pension wealth as $\nabla PW_{i,t}$ = *Pension wealth (individual* i*, year = t, postreform formula) – Pension wealth (individual* i*, year = t, prereform formula).* We define the unanticipated change in the pension financial incentives of one's peers as the (lagged) sum of the unexpected change in pension wealth of all other 55+-year-olds in one's school, self excluded.

The results of our preferred reduced-form specification, in which we control for an individual's own pension wealth and peak value and own unexpected change in pension wealth, are presented in column 2 of Table 2. 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 one's likelihood of retirement, with a point estimate of 0.27 percentage points. Using the effect of one's own pension wealth for calibration, 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 $4,300 to one's own pension wealth. Though the estimates obtained from column 2 of Table 2 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.

The remaining columns of Table 2 investigate 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

$$y_{i,s,t} = \beta_0 + \beta_1 PK_{i,s,t} + \beta_2 PW_{i,s,t} + \beta_3 w_{i,s,t} + x_{i,s,t}^{'}\beta_4 + Z_s^{'}\gamma + \rho Y_{-i,t-1,s} + \phi_t + \mu_i + \varepsilon_{i,s,t}$$

$$(t = 1999, 2001)$$

The variable of interest is $Y_{-i,t-1,s}$, the lagged retirement of others. In Table 2, 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.

The third column of Table 2 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 3, 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 4 through 6. The performance of the instrument used is reported in Panel B of Table 2.

Column 4 is the same as column 3, 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 4, each retirement of colleagues increases the likelihood of one's own retirement by an additional 1.85

percentage points. Column 5 includes as an additional control the lag unanticipated change in one's pension wealth and additional measures of the service and salary of other teachers in one's school. The results remain very similar.

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 and control for in all specifications. In all specifications, we control for the average age, size, and years of service of those eligible to retire (age 55+).[14] Column 5 includes additional school-level service composition measures (15–19, 20–24, 25–29, and 30+ years) as well as average salary of others.

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. The last column of Table 2 includes an individual-level fixed effect. Note that in the case of conditioning on those who haven't moved between schools, the individual fixed effect would also control for any fixed school unobservable.[15] 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. The larger standard error could be due to the inclusion of two nonreform years.

In Brown and Laschever (2009), we examine alternative group specifications and perform a host of robustness checks, and two types of falsification tests provide further evidence that our findings are not an artifact of spurious correlation.

Mechanisms

The results presented in the previous section suggest that peers have an effect on retirement decisions. In this section we further examine some possible mechanisms through which this effect may operate. We examine 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 individuals are not affected by the retirement age of others. That is, proximity to the modal retirement age in one's school does not affect own probability of retirement.[16]

We find evidence that the financial utilization of the retirement plan by others does have an impact on one's financial utilization. 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 in 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 who 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.

We compute two measures of pension financial underutilization. The first is the peak value, which measures how much money (in pension wealth) an individual would forgo if she retired today versus the date in the future that maximizes pension wealth. The second is the pension wealth accrual for waiting one additional year to retire. These measures capture the extent to which an individual could have further increased his or her pension wealth by delaying retirement. We define high financial underutilization as retirement with peak value over $50,000 or one-year pension wealth accrual in the top quartile (over $32,400). Using these measures of financial underutilization, we can examine whether a higher rate of underutilization among past retirees within a school moderates the importance an individual places on fully maximizing personal pension wealth.

The results are presented in Table 3. 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 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 IV strategy discussed in the previous section.

TABLE 3
The Effects of Others' Pension Maximization

All LAUSD teachers ages 55–75 in academic years 1999 and 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).

Columns 1 and 2 use the peak value measure of financial underutilization. In both columns, the results are quite similar, of the expected sign, 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. 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.

There are two types of possible explanation, not necessarily mutually exclusive, 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.

Conclusion

A unique data set and features of reforms allowed us to identify and estimate the effect of one's peers on one's own retirement decision. 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 to 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 an indirect effect on those affected by the retirement decisions of others. To properly predict the effect of a reform, one must accurately estimate both 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. 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.

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Notes

[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.

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

[3] The annual benefit is calculated by multiplying years of service, final compensation, and a benefit factor that ranges from 1.4 percent to 2.4 percent, and is increasing in retirement age and years of service.

[4] $PK_t = PW_{R^{\max}} - PW_t$, where PW_t is the pension wealth for retirement at the current age and $PW_{R^{Max}}$ is the present-value of pension wealth for retirement at the later age, $R^{\max} \in [t + 1, 100]$, that maximizes the present value of pension wealth.

[5] In order to derive an econometric specification, we incorporate the pension wealth and peak value into a utility-based choice framework (McFadden 1974).

[6] The interested reader is referred to Brock and Durlauf (2001) and Moffitt (2001) for a more thorough discussion of some of the issues surrounding the identification of peer effects.

[7] 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). This makes the window in which teachers are able to immediately respond to others' observed retirements in the same year very small.

[8] See Angrist and Pischke (2008:194–8) for a concise treatment of the drawbacks to using same-period outcomes of others.

[9] We formally show that identification is possible even if there is a suitable IV in only one of the two periods and incorporating individual fixed effects in Brown and Laschever (2009).

[10] Data was compiled with the assistance of the Office of Personnel Research and Assessment in LAUSD.

[11] http://www.cde.ca.gov/ds.

[12] All financial variables are in year 2000 dollars, and we assume a real annual interest rate of 3 percent and that salary grows at 2 percent annually.

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

[14] Our results are robust to using the average service and age of those over 45 and 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.

[15] The results 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.

[16] The norm retirement age results can be found in Brown and Laschever (2009).

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