Job Separation and Retirement Leakage:

Evidence from Non-Rollover Distributions

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Abstract

I ask whether elevated retirement non-rollover around job loss is specific to involuntary displacement or also accompanies job separation more generally. I use two regimes of the Survey of Income and Program Participation to measure pension and retirement lump-sum distributions that respondents neither rolled over nor planned to roll over. Permanent layoff is associated with a 1.74-percentage-point increase in the annual non-rollover probability, relative to a 0.79-percent no-layoff mean. On an exact common sample, the permanent-layoff contrast is 1.55 points, whereas the involuntary–voluntary separation contrast is 0.40 points; their difference is 1.15 points ($p = .021$). I then restrict attention to prior workers who reported a 401(k) or thrift account before the outcome year. Relative to continuing workers, voluntary and involuntary separators have increases of 1.95 and 2.78 points. The incremental involuntary–voluntary difference is 0.83 points, with a 95-percent confidence interval from -0.47 to 2.13. The evidence documents a separation-associated elevation that recurs in both survey regimes, but it does not causally distinguish distribution access from financial distress because the survey neither links a distribution to the job that ended nor observes employer-plan rules.

JEL: D14, G51, J32, J63. *Keywords:* job separation, retirement leakage, rollover, pension distribution, household finance.

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1 Introduction

Leaving a job is both a labor-market event and a retirement-plan event. An involuntary separation reduces labor income and can make cash on hand more valuable. Any separation, however, may also require a worker to decide what to do with assets in an employer plan: leave them in place, roll them into another tax-advantaged account, or receive a distribution. A comparison between laid-off workers and workers with no layoff can therefore combine financial distress with the broader circumstances of leaving a job.

This paper measures that empirical overlap. I use the Survey of Income and Program Participation (SIPP) to observe pension and retirement lump-sum distributions, whether a distribution was rolled into another retirement account, and whether a future rollover was planned. The outcome is a *non-rollover distribution*: a reported lump sum that was neither rolled over nor reported as planned for rollover. It is an observed flow and reported disposition rather than a withdrawal inferred from a noisy change in account balances. The survey does not reveal the originating account, the employer plan, tax treatment, transaction date, or use of the proceeds.

The starting point is a large and reproducible benchmark. Across the 2014–2016 and 2018–2022 reference years, permanent layoff is associated with a 1.74-percentage-point increase in the annual probability of a non-rollover distribution, relative to a weighted no-layoff mean of 0.79 percent. The Taylor-design standard error is 0.40 points. The estimate is 2.78 points in the earlier SIPP regime and 1.10 points in the later one. The pooled sample contains 122,475 person-years and 1,034 non-rollover events, including 56 in permanent-layoff person-years.

The paper then makes two changes to the comparison. First, I place the permanent-layoff and separation-reason estimates on an exact common sample. The same 115,290 observations, outcome, weights, controls, fixed effects, strata, and sampling clusters produce a 1.55-point permanent-layoff contrast and a 0.40-point involuntary–voluntary contrast. Their difference is 1.15 points ($p = .021$), with covariance estimated from stacked survey scores. This is a comparison of coefficients, not a causal decomposition: job-specific layoff flags and annual separation-reason reports are not nested and can describe different jobs within the same person-year.

Second, I form a narrower population that was plausibly exposed to both sides of the retirement decision before the outcome year. It requires positive prior earnings and prior reported ownership of a 401(k) or thrift account. Continuing workers are required to have no job-ending reason and an affirmative job-continuity flag. In this risk set, the weighted non-rollover rate is 0.78 percent for continuers, 2.61 percent for voluntary separators, and 3.68 percent for involuntary separators. Adjusted differences relative to continuers are 1.95

and 2.78 points, respectively. The incremental involuntary–voluntary difference is 0.83 points, with a 95-percent confidence interval from -0.47 to 2.13. Both separator–continuer contrasts recur in the two SIPP regimes. The comparison between separator types is less precise and does not establish their equality.

These estimates change the interpretation of the benchmark without supplying a clean mechanism test. The elevated probability is not confined to workers coded as involuntarily separated; it also appears among voluntary separators who worked and reported an employer-plan account in the preceding year. That pattern is consistent with separation-centered institutions or selection contributing to observed leakage. It does not prove that a particular distribution came from the job that ended, that leaving the job created access, or that distress is unimportant. Voluntary and involuntary exits can differ in anticipation, tenure, balances, employers, plan rules, and subsequent employment. The estimands are adjusted associations.

The paper contributes to a long literature on retirement distributions at job change. Using the Health and Retirement Study, Engelhardt (2003) related job-change reasons and employment prospects to the disposition of pension lump sums and interpreted the pattern as consistent with pension assets buffering shocks. Hurd and Panis (2006) studied cash-out incidence and determinants at job change or retirement and its concentration among people vulnerable to old-age poverty. Armour et al. (2017) updated the HRS evidence across cohorts and examined long-run outcomes, concluding that worse outcomes among cashers largely reflect adverse baseline characteristics and shocks rather than access to the cash-out option. Mahmoudi (2023) uses the 1996 and 2001 SIPP panels to study late-career unemployment, annual 401(k) balance depletion, the age-55 penalty waiver, and unemployment insurance. These papers make clear that job change, involuntariness, and retirement-account decisions have long been linked; the contribution here is not to discover that link.

Instead, I provide a recent two-regime measurement exercise that places two empirically non-nested contrasts on the same sample and specification. The analysis combines a direct reported disposition outcome, an exact common-sample bridge between permanent-layoff and separation-reason specifications, and a prior-worker/prior-plan risk set. This design shows precisely where the large layoff benchmark survives and where inference becomes limited. It complements administrative plan studies, which measure account-level behavior and plan architecture much more precisely but often lack household circumstances or directly observed separation reasons. In large recordkeeping samples, cash distributions at separation are common and vary sharply with account balance, default rules, and employer-contribution share (Hung et al., 2015; Wang et al., 2023; Goodman et al., 2023). SIPP offers a different tradeoff: weaker account linkage and fewer events, but self-reported job-ending reasons and predetermined household finances.

The labor literature documents persistent earnings, consumption, and wealth losses after displacement (Jacobson et al., 1993; Davis and von Wachter, 2011; Ganong and Noel, 2019). The household-finance literature shows that liquid wealth, credit, unemployment insurance, and retirement accounts mediate responses to income risk (Chetty, 2008; Basten et al., 2016; Argento et al., 2015; Landais and Spinnewijn, 2021; Andersen et al., 2025). The present evidence does not adjudicate between those mechanisms. It disciplines one empirical claim: a layoff–no-layoff gap should not by itself be labeled a distress effect when job separation also changes the institutional setting in which retirement balances are handled.

The remainder of the paper proceeds as follows. Section 2 describes the outcome, SIPP regimes, and samples. Section 3 defines the three comparisons and survey inference. Section 4 presents the benchmark, common-sample bridge, and prior-plan risk set. Section 5 states what can and cannot be learned from the design. Section 6 concludes. The Online Appendix reports harmonization, alternative coding and inference, overlap diagnostics, matching, event-study diagnostics, and complete machine-readable sample accounting.

2 Measurement and Data

2.1 A direct measure of non-rollover distributions

The SIPP asks whether a respondent received a lump-sum payment from a pension or retirement plan during the reference year (`ELMPTYP1YN`), whether that payment was rolled into another retirement account (`EROLLOVR1`), and, if not, whether a rollover was planned (`EROLLOVR2`). The primary outcome equals one when the respondent reports a lump sum, reports no completed rollover, and either reports no planned rollover or is routed past the follow-up by questionnaire structure. A raw-file audit confirms that qualifying observations do not depend on recoding a substantively unanswered future-rollover item as no. Requiring an explicit no produces nearly identical estimates.

I call this outcome a non-rollover distribution. The phrase *cash-out* is common in the literature, but the SIPP item is broader than an observed penalized 401(k) withdrawal. The originating arrangement may be a defined-contribution plan, an individual account, a pension, or another retirement vehicle covered by the question. The public-use files do not identify whether an additional tax applied, whether an exception was available, whether the proceeds were consumed, or whether they were later saved elsewhere.

Administrative studies answer a more account-specific question. For example, Hung et al. (2015) use Vanguard records on 536,636 eligible employees in 385 plans, including 212,692 who had separated, and Wang et al. (2023) analyze 162,360 employees in 28 plans. Their high cash-distribution shares are conditional on plan participants and separation-related account decisions. The SIPP rates below are unconditional person-year probabilities among a defined worker risk set, and the reported distribution need not come from the observed job. The levels are therefore not expected to match. The comparison is useful as an external check on the underlying behavior, not a validation of identical denominators.

2.2 Two SIPP regimes

The analysis combines the 2014 SIPP longitudinal panel with the 2018-and-later panels. The raw files cover reference years 2013–2016 in the earlier regime and 2017–2022 in the later regime. Specifications requiring a preceding annual observation begin in 2014 and 2018, respectively. Variable definitions are drawn from the Census SIPP user guide and panel dictionaries (U.S. Census Bureau, 2024). I re-extract the lump-sum, rollover, layoff, separation-reason, weight, stratum, and cluster variables from the monthly files and aggregate them to the person-year.

The public-use files labeled 2024 and 2025, covering reference years 2023 and 2024, are retained in the replication package but not pooled into the estimates. Census redesigned the retirement-income sequence beginning with the 2024 SIPP (U.S. Census Bureau, 2025b,a). The replacement fields record pension lump-sum receipt and amount, but not completed or planned rollover. Adding those years would therefore substitute a receipt outcome for the paper's non-rollover outcome rather than extend a comparable series.

The same substantive outcome and job-ending definitions are available in both regimes, though job-continuity variable names differ. The annual outcome and permanent-layoff indicators are constructed directly from the retained raw files and reconciled to the harmonized estimation data on the analytic keys. The Online Appendix reports the crosswalk and a sample-flow audit. It also documents a Census user note concerning missing 2014 Wave 4 person weights (U.S. Census Bureau, 2020); observations with nonpositive December weights are not used in weighted estimation.

2.3 Three analytic samples

The unit of observation is the person-year. All samples restrict age to 25–64 and require a positive December person weight.

The *benchmark sample* further requires an immediately preceding annual observation, common predetermined controls, and either a permanent layoff or no reported layoff. Temporary layoff person-years are excluded. The sample contains 62,555 observations in 2014–2016 and 59,920 in 2018–2022. Its 122,475 observations include 2,032 permanent-layoff person-years and 1,034 non-rollover events.

The *common bridge sample* is the exact intersection of the benchmark risk set and the mutually exclusive reason-coded universe. Voluntary work-related endings use `RSEND` codes 7–9. Involuntary economic endings use codes 1, 2, 3, and 6: plant or company closure, slack work or business conditions, position or shift abolished, and other involuntary reasons. Person-years with seasonal completion, discharge, retirement, personal reasons, or mixed categories are excluded. The intersection has 115,290 observations and 859 non-rollover events. It drops 7,185 benchmark observations that fall in the excluded other or mixed reason categories.

The *prior-plan risk set* addresses a more basic comparability concern: many nonseparators may not be workers with an employer plan at all. It requires observation in the preceding year, positive prior-year earnings, and prior reported ownership of a 401(k) or thrift account. A continuer has no observed ending reason in the outcome year and an affirmative harmonized continuity flag for the main job. Voluntary and involuntary categories use the codes above; other and mixed endings are excluded. The primary risk set contains 39,481 person-years: 35,612 continuers, 2,458 voluntary separators, and 1,411 involuntary separators.

Prior plan ownership narrows exposure but does not create job-level linkage. A distribution reported in the outcome year may come from a different current or former job, an individual account, or another pension arrangement. The SIPP also does not reveal the balance in the particular plan associated with the job ending. This limitation is maintained throughout the interpretation.

2.4 Weights and survey design

Annual estimates use the December value of `WPFINWGT`. Taylor-linearized standard errors use the public-use `GVARSTR` strata and `GHLFSAM` sampling clusters. Both identifiers are interacted with SIPP panel so that numeric codes reused in independent panels are not treated as the same sampling unit. The models use year or state-by-year fixed effects as described below.

The local public-use person files contain the final weight and Taylor design variables but no replicate-weight fields or separate replicate-weight file. Accordingly, the headline inference is Taylor linearization rather than Fay's balanced repeated replication. The replication package records this input audit explicitly. Household-within-panel clustered, unweighted, capped-weight, and alternative outcome-coding estimates are reported as sensitivities.

3 Empirical Strategy

3.1 The permanent-layoff benchmark

The benchmark linear probability model is

$$100Y_{ipt} = \alpha + \beta L_{ipt} + X'_{ip,t-1}\gamma + \lambda_t + \varepsilon_{ipt}, \tag{1}$$

where Y_{ipt} is a non-rollover distribution for person i in SIPP panel p and reference year t, L_{ipt} indicates a permanent layoff, and X contains age, age squared, education, homeownership, and log prior earnings. Calendar-year fixed effects are denoted by λ_t. The factor 100 puts coefficients in percentage points.

Equation (1) is a conditional association. The no-layoff category is not equivalent to continued employment: it can include other job endings. Permanent layoff can also be correlated with employers, plans, balances, tenure, and worker characteristics omitted from the public-use files. I therefore use the estimate as a benchmark and report it by regime.

3.2 An exact common-sample bridge

The first diagnostic changes only the treatment contrast. On the common bridge sample, I estimate a permanent-layoff indicator and, in a separate equation, the difference between involuntary and voluntary reason-coded separations. Both models use the same outcome, observations, December weights, controls, state-by-year fixed effects, strata, and sampling clusters. The common control vector adds sex and race to the predetermined variables in equation (1).

I estimate the difference between the two coefficients with stacked Taylor estimating equations. Stacking retains the covariance generated by using the same observations and survey clusters, rather than treating the estimates as independent. This exercise answers whether the two regression contrasts are numerically the same under identical sample and specification choices.

It does not decompose one effect into mechanisms. The job-specific permanent-layoff flag and the annual reason-coded categories are not nested. A person-year can contain more than one job, and the fields can refer to different jobs. Only 713 of 1,577 permanent-layoff rows in the bridge are classified as purely involuntary by the reason-code rule, and only 713 of 3,826 purely involuntary rows have the permanent-layoff flag. The complete crosswalk is reported in the Online Appendix.

3.3 A prior-worker, prior-plan risk set

The main risk-set model is

$$100Y_{ipt} = \alpha + \beta_V V_{ipt} + \beta_I I_{ipt} + X'_{ip,t-1}\gamma + \mu_{st} + \varepsilon_{ipt}, \tag{2}$$

where the omitted group is continuing workers, V indicates a voluntary work-related separation, and I indicates an involuntary economic separation. Controls are determined before the outcome year and include demographics, homeownership, standardized prior earnings, liquid assets, retirement balance, and baseline industry. State-by-year fixed effects are denoted by μ_{st}.

The coefficients β_V and β_I compare each separator group with prior workers who also reported a 401(k) or thrift account and continued in their main job. Their difference, $\beta_I - \beta_V$, is the incremental conditional association for involuntary rather than voluntary separation. The risk set improves comparability in prior labor-force attachment and reported plan exposure. It does not randomize the reason for separation or hold the employer and plan fixed.

I report receipt, explicit rollover, and non-rollover outcomes in the appendix, but make the unconditional non-rollover probability primary. Conditioning on receipt would select on an endogenous outcome-stage event that itself depends on plan rules, balances, and separation type. I also report coarsened exact matching and alternative exposure definitions. The overlap diagnostics are most favorable for separator–continuer comparisons and weaker for involuntary–voluntary matching, so the latter remains an explicitly imprecise contrast.

4 Results

4.1 Permanent layoff and non-rollover distributions

Table 1 reports the benchmark. Permanent layoff is associated with a 2.78-point increase in the early SIPP regime and a 1.10-point increase in the late regime. The pooled estimate is 1.74 points with a Taylor standard error of 0.40. Each estimate is large relative to the pooled weighted no-layoff mean of 0.79 percent.

The late-minus-early interaction is -1.68 points with a standard error of 0.89 ($p =$ .061). This difference counsels against interpreting the pooled coefficient as a time-invariant parameter. At the same time, the sign is positive in both regimes, and the earlier panel contributes an independent pre-pandemic replication and 30 treated events to the 26 in the later regime.

Table 1: Permanent layoff and retirement non-rollover across two SIPP eras

	2014–2016	2018–2022	Pooled	Late − early
Harmonized December-weight estimates				
Permanent layoff (pp)	2.78***	1.10***	1.74***	-1.68*
Taylor-design SE	(0.80)	(0.41)	(0.40)	(0.89)
Household-clustered SE	[0.71]	[0.42]	[0.37]	[0.82]
Control-group mean (pp)	0.75	0.82	0.79	–
Observations	62,555	59,920	122,475	122,475
Non-rollover events	521	513	1,034	1,034
Permanent-layoff non-rollover events	30	26	56	56

Notes. The dependent variable is 100 times an indicator for receiving a pension/retirement lump sum that was not rolled over and for which the respondent did not report a future rollover plan. The sample includes people ages 25–64 observed in the prior year and compares permanent layoffs with person-years containing no layoff; temporary layoffs are excluded. Specifications use the December person weight, calendar-year effects, and controls for age, age squared, education, homeownership, and log prior earnings. Parentheses report Taylor-linearized standard errors using GVARSTR strata and GHLFSAM sampling clusters interacted with SIPP panel; brackets report household-within-panel clustered standard errors. The final column is the permanent-layoff-by-late-era interaction. Weight, coding, and influence sensitivities appear in the Online Appendix. ***/**/* denote significance at 1/5/10 percent under Taylor-design inference.

The appendix shows that the pooled coefficient ranges from 1.47 to 2.00 points when each reference year is omitted in turn. It is 1.65 after dropping the highest-weight treated event and 1.47 after dropping the three highest. Mean-month weights, capped weights, unweighted estimation, strict future-rollover coding, and household clustering yield the same qualitative benchmark. These checks address numerical fragility, not omitted-variable bias.

4.2 Holding sample and specification fixed

Table 2 compares treatment contrasts on the exact common intersection. The pooled permanent-layoff coefficient is 1.55 points (standard error 0.43), while the involuntary–voluntary reason contrast is 0.40 points (standard error 0.37). The difference is 1.15 points with a standard error of 0.50 and a joint p-value of .021.

The early regime produces the larger contrast: 2.58 points for permanent layoff and 0.25 points for involuntary rather than voluntary separation. In the late regime the corresponding estimates are 0.95 and 0.56 points, and their difference is not statistically resolved. The bridge therefore shows that sample composition, outcome coding, weights, covariates, fixed effects, and survey design do not explain the pooled numerical gap between the two coefficients.

What remains is an estimand difference. The permanent-layoff comparison places a relatively specific job event against a broad no-layoff category. The reason-coded comparison

Table 2: A common-sample comparison of the two treatment contrasts

Era	Permanent layoff − no layoff	Involuntary − voluntary	Difference	p-value	N
2014–2016	2.58***	0.25	2.33	0.009	58,697
	(0.91)	(0.50)	(0.89)		
2018–2022	0.95**	0.56	0.38	0.538	56,593
	(0.41)	(0.53)	(0.62)		
Pooled	1.55***	0.40	1.15	0.021	115,290
	(0.43)	(0.37)	(0.50)		

Notes. The intersection holds observations, years, outcome, December weights, controls, state-by-year fixed effects, and Taylor survey design fixed across treatment definitions. It includes 859 non-rollover events. Standard errors are in parentheses. The Difference column is the first coefficient minus the second; its standard error and p-value use cross-equation covariance from stacked Taylor estimating equations. The treatment indicators are not nested and can refer to different jobs within an annual record, so the difference is a comparison of coefficients, not a causal decomposition. $^{***}p < 0.01$, $^{**}p < 0.05$.

conditions on two types of economic job endings. Because the indicators are not nested and the distribution is not linked to either job, the 1.15-point difference cannot be labeled the share caused by access or the share not caused by distress. Its value is narrower: it demonstrates that the empirical conclusion depends materially on which labor-market counterfactual is used.

4.3 Prior workers with prior plan exposure

Table 3 turns to the more comparable prior-plan risk set. Non-rollover distributions occur in 0.78 percent of continuing-worker person-years, 2.61 percent of voluntary-separator person-years, and 3.68 percent of involuntary-separator person-years. The unweighted event counts are 291, 67, and 49.

After predetermined controls and state-by-year fixed effects, voluntary separation is associated with a 1.95-point increase relative to continuation; involuntary separation is associated with a 2.78-point increase. Both are statistically precise. The pooled incremental involuntary–voluntary estimate is 0.83 points, but its standard error is 0.66 and its 95-percent confidence interval is $[-0.47, 2.13]$. The corresponding 80-percent-power minimum detectable difference is approximately 1.85 points. These data support a shared elevation for observed separators, but they do not rule out a substantively meaningful additional association for involuntary separation.

Figure 1 shows that separator–continuer elevations recur in both periods. The voluntary–continuer coefficient is 2.52 points in 2014–2016 and 1.53 points in 2018–2022. The involuntary–continuer coefficient is 2.89 and 2.68 points. The involuntary–voluntary contrast is 0.37 points in the earlier regime and 1.15 points in the later regime; neither is precisely estimated, and

Table 3: Non-rollover distributions among prior workers with a prior 401(k)/thrift account

	Continuer	Voluntary separation	Involuntary separation
Panel A. Pooled weighted rates and unweighted counts			
Non-rollover rate (%)	0.78	2.61	3.68
Non-rollover events	291	67	49
Person-years	35,612	2,458	1,411
	Voluntary − continuer	Involuntary − continuer	Involuntary − voluntary
Panel B. Adjusted differences in percentage points			
2014–2016	2.52***	2.89***	0.37
	(0.70)	(0.92)	(1.12)
2018–2022	1.53***	2.68***	1.15
	(0.42)	(0.75)	(0.81)
Pooled	1.95***	2.78***	0.83
	(0.38)	(0.58)	(0.66)
95% CI, pooled	[1.20, 2.71]	[1.63, 3.92]	[-0.47, 2.13]

Notes. The sample contains people ages 25–64 observed in the immediately preceding reference year, with positive prior earnings and prior ownership of a 401(k)/thrift account. Continuers report no job-ending reason and an affirmative job-continuity flag. Voluntary endings use RSEND 7–9; involuntary economic endings use RSEND 1, 2, 3, and 6. Other and mixed endings are excluded. Regressions use December person weights, predetermined demographics and finances, prior industry, and state-by-year fixed effects. Parentheses report Taylor-linearized standard errors using panel-interacted SIPP strata and PSUs. SIPP does not link a distribution to the job that ended. $^{***}p < 0.01$.

their era difference is unresolved.

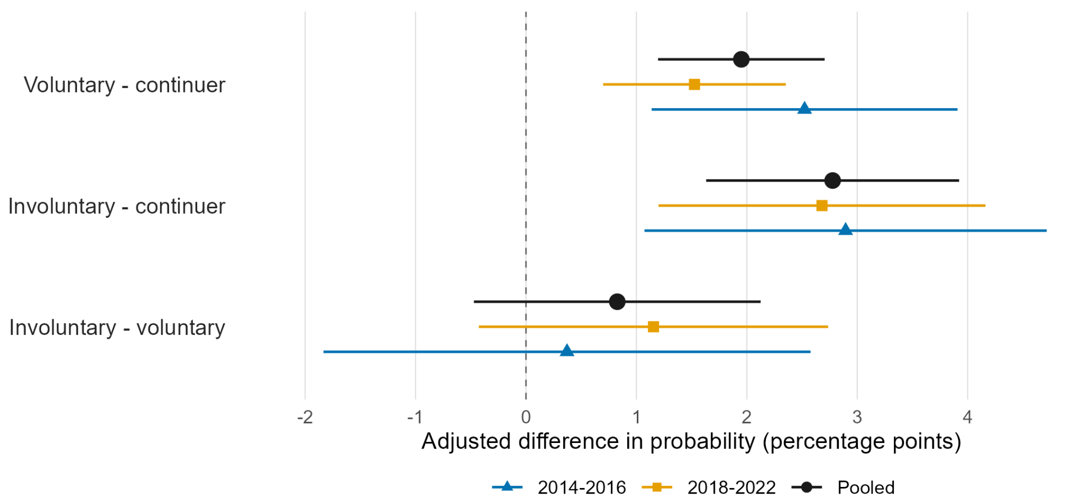

Figure 1: Non-rollover differences in the prior-worker, prior-plan risk set. Points are adjusted differences from equation (2), in percentage points; horizontal bars are 95-percent confidence intervals from Taylor-linearized standard errors. The sample requires positive prior earnings and prior reported ownership of a 401(k) or thrift account. All regressions use December person weights, predetermined controls, prior industry, and state-by-year fixed effects.

The appendix tests whether the pattern is an artifact of the exact exposure rule. Removing the job-continuity restriction yields an involuntary–voluntary coefficient of 0.75 points; broadening prior ownership to any reported retirement arrangement yields 0.90; requiring a positive prior retirement balance yields 0.84; and unweighted estimation yields 0.55. Coarsened exact matching produces 1.00 points with a 95-percent confidence interval from -0.46 to 2.47. Matching substantially improves observed balance, but common support is thinner for involuntary versus voluntary separators than for either separator group versus continuers. These sensitivities support the separator–continuer finding and reinforce caution about the incremental reason contrast.

Receipt and rollover accounting is also consistent with a separation-associated pattern. In the pooled prior-plan sample, adjusted receipt differences relative to continuers are 2.87 points for voluntary separators and 3.61 points for involuntary separators. Explicit rollover increases by 0.92 and 0.83 points. The remaining non-rollover differences are therefore not constructed from a hidden decline in rollover for one group; rather, both separator groups report more distributions, some rolled over and some not. The survey cannot say whether these distributions came from the job that ended.

5 Interpretation

5.1 What the evidence establishes

Three facts survive the revised design. First, permanent layoff is robustly associated with non-rollover distributions in two SIPP regimes. Second, changing the comparison from permanent layoff versus no layoff to involuntary versus voluntary separation materially reduces the coefficient on an otherwise identical common sample. Third, among prior workers who reported a 401(k) or thrift account, both voluntary and involuntary separation are associated with more non-rollover distributions than continued employment.

Together, these facts are consistent with a separation-associated pattern. Job endings may activate administrative defaults and balance-dependent options and change communications or worker attention; they may also select workers and firms with different plan characteristics (Hung et al., 2015; Wang et al., 2023; Goodman et al., 2023). These channels are economically important even if they are not separately identified here. The results also connect the contemporary SIPP evidence to earlier HRS evidence linking job-separation circumstances and economic or health shocks to pension disposition (Engelhardt, 2003; Hurd and Panis, 2006; Armour et al., 2017).

5.2 What the evidence does not establish

The paper does not show that plan access rather than financial distress causes leakage. The public-use SIPP lacks a job-to-distribution link, employer identifiers, plan rules, account-specific balances, and transaction timing. A voluntary separator may anticipate a new job; an involuntary separator may face a longer income loss. The two groups can differ in unobserved tenure, earnings risk, firm quality, account size, and defaults. Even the prior-plan risk set does not hold those characteristics fixed.

The estimates also do not show that voluntary and involuntary separators behave identically. The pooled risk-set confidence interval permits an incremental involuntary association above two percentage points, and overlap is weaker for the direct separator-type comparison. A statistically unresolved coefficient is not evidence of a zero mechanism. The strongest supported statement is that elevated non-rollover is common to both observed separator groups and that the SIPP is not precise enough to estimate tightly a smaller incremental involuntary-separation association.

Finally, a non-rollover distribution is not necessarily consumed or penalty-bearing. The survey does not identify an early-distribution tax, an exception, a later recontribution, debt repayment, or saving outside retirement accounts. The analysis therefore does not attach a welfare loss to every event and does not estimate a penalty elasticity. Studies with administrative transactions and tax treatment are better suited to those questions (Coyne et al., 2022; Goda et al., 2022; Andersen et al., 2025).

5.3 Implications for research and policy

The evidence motivates attention to the retirement-plan choice architecture surrounding all job endings, not only involuntary ones. Automatic portability, rollover defaults, small-balance rules, and timely information could affect the common separator elevation. Yet the present design cannot rank those policies or determine whether broader access to retirement wealth raises or lowers welfare.

A decisive next design would link plan or tax records to employer-side separations. It would observe the account attached to the ended job, its balance, plan defaults, distribution and rollover dates, tax treatment, and subsequent employment. Firm closures or mass layoffs could then supply variation in involuntariness while holding the institutional object of the decision in view. That design would separate opportunity, selection, and financial distress more sharply than repeated robustness checks in a household survey can.

6 Conclusion

Permanent layoff is associated with a sizable increase in reported pension and retirement distributions that are not rolled over and are not planned for rollover. The positive association appears in both SIPP regimes and survives alternative weights, coding, influence checks, and inference conventions.

The interpretation changes when the labor-market comparison changes. On the same observations and specification, the permanent-layoff contrast is 1.55 percentage points and the involuntary–voluntary separation contrast is 0.40 points. Among prior workers with prior reported 401(k) or thrift ownership, voluntary and involuntary separators have increases of 1.95 and 2.78 points relative to continuing workers. The direct difference between separator types is imprecise.

The paper therefore does not conclude that access, rather than distress, explains retirement leakage. It establishes a more defensible result: non-rollover distributions are elevated around job separation broadly, while a layoff comparison against workers without a layoff does not distinguish that general separator association from any additional association with involuntary separation. Distinguishing those associations requires linked job, plan, and transaction data. Until such data are available, the choice of labor-market counterfactual is part of the economic result, not a secondary robustness decision.

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