Impatience or illiquidity? The case of early 401(k) withdrawals*

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September 2025

Abstract

A third of 401(k) participants cash out their balances when they leave their jobs. While the prevalence of pre-retirement withdrawals is well documented, empirical evidence on underlying mechanisms is lacking. Using a novel paycheck-level income panel covering millions of participants, I show that workers with greater sub-annual income volatility are significantly more likely to cash out. The relationship between earnings volatility and cashout probability is conditional on annual income, most pronounced among low- and middle-income workers, and driven by mean-zero as well as asymmetric negative shocks. The empirical results are consistent with a model in which workers use – and present-biased workers overuse – retirement assets as insurance against short-run income shocks. A linked survey confirms that many participants with volatile incomes lack liquid savings, despite contributing regularly to illiquid retirement accounts.

*I thank John Ameriks, John Beshears, Guillermo Carranza, Marcos Catao, James Choi, Taha Choukhmane, Jeff Clark, Paulo Costa, Tom De Luca, Malena de la Fuente, Peter Ganong, Fiona Greig, Kelly Hahn, David Laibson, Spencer Look, Anna Madamba, Marsella Martino, Pascal Noel, Cormac O'Dea, David Pakula, Erich Pingel, Andy Reed, Lawrence Schmidt, Fu Tan, Jack VanDerhei, Xiao Xu, Nicky Zhang, and colleagues at Vanguard's Investment Strategy Group for valuable feedback. The views expressed here are those of the author and not necessarily those of the Vanguard Group, Inc. **This material is provided for informational purposes only and is not intended to be investment advice or a recommendation to take any particular investment action.**

1 Introduction

A significant share of assets in defined-contribution retirement accounts are withdrawn before retirement. The implications of this fact – for public policy, for the design of employer-sponsored retirement plans, and for analysis of household saving behavior more broadly – depend crucially on the reasons for early withdrawals. If retirement savings leakage arises solely from present bias, then early-withdrawal penalties raise welfare. Alternatively, if households tap retirement accounts because they lack the liquid resources to meet current consumption needs, then they benefit from policies that facilitate liquid wealth formation and make retirement assets available during emergencies.

This paper presents the strongest evidence to date that liquidity constraints are a key explanation for early withdrawals. The empirical analysis centers on a novel paycheck-level income panel covering 3.4 million 401(k) plan participants during a 16-year period. Because I construct the paycheck panel from administrative 401(k) recordkeeping data, I can relate granular features of participants' income histories to their saving and withdrawal decisions. When I do so, a consistent finding emerges: participants with greater sub-annual income volatility are significantly more likely to take penalized early withdrawals from 401(k) accounts. The relationship between earnings volatility and withdrawal rates is conditional on annual income, most pronounced among low- and middle-income participants, and driven by both mean-zero and asymmetric negative earnings shocks. These empirical results are consistent with a conceptual framework in which workers take early 401(k) withdrawals when income shocks create liquidity shortages.

To formalize the theoretical framework and discipline the empirical analysis, I develop a model of retirement saving and withdrawal decisions that reflects the institutional reality of the U.S. defined-contribution (DC) system. In the model, a worker earns stochastic labor income over multiple periods and saves for retirement in a 401(k) account. The worker may withdraw from the 401(k) account before retirement, but early withdrawals incur a penalty and are only permitted in a designated subset of periods during working life. The designated early-withdrawal periods represent times when the worker changes jobs and thus becomes eligible to take a penalized cash distribution from her 401(k) account. Because withdrawals during employment are subject to relatively strict regulation, the post-separation cashouts on which the model focuses are the most common form of 401(k) leakage: in 2023, 33% of all separators in Vanguard-administered plans cashed out (Clark, 2025). The modeled worker also has access to a lower-yielding liquid savings account,

from which withdrawals can be taken at any time without penalty.

The model makes three core predictions that structure the empirical analysis and shape my interpretation of the empirical results. First, negative income shocks cause cashouts. When the worker receives sufficiently poor draws from the stochastic income process, the consumption-smoothing benefit of cashing out becomes larger than the early-withdrawal penalty. Second, mean-zero income volatility causes cashouts. Even when negative income shocks are accompanied by equally sized positive shocks during the same employment spell, the worker uses her liquid wealth to smooth consumption, has less liquidity at separation, and may cash out in order to replenish the liquid account before beginning her next job. Third, the relationship between income volatility and cashout propensity holds in a baseline case where the worker is a standard exponential discounter, but becomes stronger when the worker is present-biased (Laibson, 1997). This is because liquidity is partly endogenous: present-biased workers build smaller precautionary buffers in the liquid account, have less liquidity on hand when income shocks arrive, and must resort to penalized 401(k) withdrawals more often.

Motivated by the model of withdrawal behavior, I build a dataset uniquely suited to test its predictions. High-frequency income observations are crucial: even short-lived negative shocks can deplete liquidity buffers, and mean-zero volatility within a given year would be completely undetectable in annual income data. Using administrative 401(k) recordkeeping data from Vanguard, I rely on the fact that for most of the plans it administers, Vanguard tracks both the dollar amounts of employee contribution transactions and employees' underlying contribution rates. Combining the information on contribution amounts and rates allows me to infer individual paychecks. For example, a participant who made a $50 contribution with a 5% contribution rate on January 1 likely received a $1,000 paycheck on January 1. Focusing on cases where I can verify that the annual sum of inferred paychecks matches official W-2 income figures, I obtain complete paycheck-level income histories for 10.6 million participant-years across 1,259 plans during the 2008-2023 period.

The paycheck data confirm the model's theoretical predictions. I first divide the sample of participants into those who are likely hourly workers and those who are likely salaried, based on the frequency with which their paycheck amounts change by at least $1 relative to the lagged paycheck amount.[1] Among those who separate from employment during the sample period, workers in the likely hourly group have cashout rates 10-15

[1]I thank Peter Ganong, Pascal Noel, and their coauthors in Ganong et al. (2024) for providing statistics from external payroll data that help make this hourly-salaried classification as accurate as possible.

percentage points higher than salaried workers with similar annual incomes. To measure volatility quantitatively, I work at the monthly level and consider monthly earnings fluctuations *within* a given participant-year. In regressions that control for annual income, age, employment tenure, and plan-level fixed effects, I find that a 10% increase in monthly income volatility raises post-separation cashout probability by 8 percentage points for middle-income workers (a 24% increase from the baseline cashout rate of 34%). When I control separately for the second moment and the third moment (i.e., skewness) of monthly income, I find that asymmetric negative shocks have even stronger effects on cashout probability than mean-zero volatility. Additionally, leveraging a small subset of workers who participate in two different Vanguard-administered plans during the sample period, I show the results continue to hold in regressions that control for worker-level fixed effects, with identification coming from variation in monthly income volatility between distinct employment spells.

Finally, to verify the theoretical link between liquidity and 401(k) withdrawals, I use a 2024 survey of participants in Vanguard-administered plans, with responses linked to the administrative recordkeeping data. Among a sample of roughly 4,000 survey respondents, 28% report being less than fully confident in their ability to meet a $2,000 emergency expense. Those who lack liquidity are overwhelmingly hourly workers with volatile incomes, and self-reported liquidity strongly predicts 401(k) withdrawal rates. Over half of low-liquidity participants contribute above their employer's match cap, suggesting that their lack of liquid savings may in some cases be suboptimal.

I contribute to an empirical literature studying pre-retirement leakage from retirement accounts. Most prior work in this literature computes leakage rates using federal tax-return data.[2] Goodman et al. (2021) find that from 2003 to 2015, 22% of contributions to DC and Individual Retirement Arrangement (IRA) accounts by working-age individuals leaked out as pre-retirement withdrawals.[3] Argento et al. (2015) show that each year during the 2004-2010 period, roughly 8% of households with DC accounts took penalized distributions.[4] Choukhmane et al. (2024) document that early withdrawals are significantly more common among minorities and lower-income workers and contribute to racial and income gaps in DC wealth accumulation. Coyne et al. (2024) use penalized early withdrawals from

[2] Choi et al. (2024) is an exception: using recordkeeping data covering nine firms between 2005 and 2011, they find that 42% of 401(k) balances are cashed out upon separation.

[3] I share a last name with the first-listed author on this paper but we are not related. I was not invited to collaborate on Goodman et al. (2015).

[4] Amromin and Smith (2003) compute similar leakage rates using less recent data from the 1990s.

retirement accounts as a revealed-preference measure of liquidity valuation.

In addition to studying average leakage rates, Goodman et al. (2021), Argento et al. (2015), and Coyne et al. (2024) find evidence that households are more likely to take early withdrawals in years where their annual income drops by a large amount or they receive unemployment benefits. By using recordkeeping data to study the effect of income volatility, I provide a more thorough analysis that overcomes several fundamental limitations of annual tax-return data. First, the correlation between income shocks and withdrawals in tax-return data is partly mechanical: job changes make workers eligible to take 401(k) distributions and may simultaneously reduce annual income if there is a transition or unemployment period between jobs. I *condition* on separation, then relate cashout decisions to income volatility *during* the preceding employment spell.[5] Second, the sub-annual volatility I study is fundamentally different (and likely more common) than large shocks to annual income. I can detect the short-lived negative earnings shocks and mean-zero, within-year volatility that matter for workers' liquidity positions but are not observable in annual income data. Finally, 401(k) recordkeeping data alleviates some aggregation issues in tax-return data, like the fact that distributions from defined-contribution and defined-benefit accounts are not separately reported on Form 1099-R and must be inferred using other information on the return (Goodman et al. 2021 discuss this issue in detail).

I show that liquidity constraints are an important explanation for early withdrawals, but policymakers generally face a tradeoff between mitigating behavioral biases and providing flexibility to constrained households. Beshears et al. (2025) study this tradeoff in a generalized planning problem (building on the modeling framework of Amador et al. 2006). They find that the optimal retirement savings system is well approximated by a two-account structure: one account is completely illiquid and mitigates present bias by forcing a high level of mandatory savings, while a second liquid account provides limited flexibility to patient households with marginal-utility shocks. The optimality of mandating high illiquid savings levels depends in part on an assumption that present bias is sufficiently heterogeneous relative to the distribution of marginal-utility shocks. The substantial monthly income volatility I document among 401(k) participants provides a micro-foundation for the Beshears et al. (2025) marginal-utility shocks and shows that these shocks have wide empirical variance.[6] An important avenue for future work is to estimate

[5] For completeness, I provide an additional analysis of layoffs in Appendix C.

[6] Checking and credit card account data (Farrell and Greig, 2015; Farrell et al., 2019) and household survey data (Fulford and Low, 2024) suggest expense shocks are an important determinant of liquidity constraints and that monthly expenses are even more volatile than monthly income. The income volatility I document

precisely the empirical distribution of present bias,[7] allowing for data-driven evaluations of the commitment-versus-flexibility tradeoff.

I also contribute to a literature studying the optimal design of employer-sponsored retirement plans. Since the seminal contribution of Madrian and Shea (2001), a substantial body of empirical work has shown that automatic enrollment increases participation and saving rates in 401(k) plans (Choi et al., 2002, 2004; Beshears et al., 2009). More recent studies show that the beneficial effects of automatic enrollment partially dissipate over time (Choukhmane, 2025) as workers reduce non-retirement saving (Choukhmane and Palmer, 2024), take on debt (Beshears et al., 2024a), and cash out at separation (Derby et al., 2023; Choi et al., 2024).[8] The link between income volatility and cashouts suggests liquidity constraints are a partial explanation for attenuated long-run savings effects. Theoretically, the model I develop demonstrates the value of plan-design choices that account for workers' liquidity needs: to the extent that behavioral biases inhibit retirement saving, they also cause workers to undersave in precautionary liquid accounts (Ameriks et al., 2007; Ganong and Noel, 2019). Promising interventions include those that enroll workers in employer-sponsored short-run savings accounts (Berk et al., 2023) and allow limited amounts of emergency 401(k) liquidity during employment (Beshears et al., 2024b).[9]

Finally, I contribute to a growing literature on heterogeneous labor income risk (Guvenen et al., 2014, 2021; Busch et al., 2022; Meeuwis et al., 2025). Whereas this literature relies primarily on annual income data and studies earnings risk that arises largely from job changes and unemployment spells, I show that sub-annual volatility within a given employment spell can have important household financial implications. In using paycheck-level data and analyzing monthly earnings volatility, I follow Ganong et al. (2024), who are the first to do so with U.S. administrative data. Relative to the payroll-processing data in Ganong et al. (2024), the earnings information I construct from 401(k) recordkeeping data is valuable because it allows me to relate income volatility to workers' retirement saving and withdrawal decisions. To my knowledge, this paper is the first to link paycheck-level income data in the U.S. with worker-level financial behaviors.

The remainder of the paper is organized as follows. Section 2 develops the model of

could therefore understate the total variance of marginal-utility shocks.

[7] See the literature reviews in Lockwood (2020) and Cohen et al. (2020). Of primary importance is research that estimates the distribution of present bias within a given sample (Paserman 2008 and Meier and Sprenger 2015 are examples), rather than relying on cross-study variance in average estimates.

[8] Earlier work on automatic enrollment found smaller effects in terms of saving crowd-out (Chetty et al., 2014) and debt accumulation (Beshears et al., 2022a).

[9] See the discussion of 401(k) liquidity rules in Section 7.

retirement saving and withdrawal decisions. Section 3 describes the administrative 401(k) recordkeeping data and the process by which I infer paychecks. Section 4 presents the main empirical results linking sub-annual income volatility to early 401(k) withdrawals. Section 5 studies cashouts using an alternative within-person identification strategy. Section 6 analyzes a linked survey gauging participants' liquid savings. Section 7 discusses policy implications and Section 8 concludes.

2 Model

2.1 Setup

To motivate the empirical analysis, I introduce a simple model of retirement saving and withdrawal decisions. Taking important features of the U.S. system as given, the model describes the partial-equilibrium behavior of a worker who saves in a 401(k) account and seeks to maximize discounted lifetime utility. As in prior literature, I allow the worker to be naively present-biased (Laibson, 1997; Beshears et al., 2022b). The distinguishing feature of the model is labor income volatility, which can shift the marginal utility of consumption during working life and potentially induce early withdrawals from the 401(k) account.[10]

The worker's life consists of T periods. In each period t, the worker's objective is to maximize discounted utility, which is given by

$$u(c_t) + \beta \sum_{x=t+1}^{T} \delta^{x-t} u(c_x) \tag{1}$$

The exponential discount factor is $\delta < 1$ and $\beta \in (0, 1]$ controls the degree of present bias; the benchmark case of $\beta = 1$ represents a worker who is not present-biased.

For $t < t_R$, the worker is employed and earns stochastic labor income y_t that is independently distributed across periods. For $t \geq t_R$, the worker is retired and receives fixed Social Security income y_R. Consumption may differ from current income because the worker has access to two savings accounts. The first account, with balance at the beginning of period t denoted by a_t^L, is a completely liquid account from which the worker can withdraw at any

[10] In similar dynamic settings, Beshears et al. (2025) and Amador et al. (2006) allow for time-varying taste shocks θ that shift marginal utility. The random income shocks here can be seen as a micro-foundation for such taste shocks (though with a different functional form: Beshears et al. 2025 and Amador et al. 2006 consider multiplicative shocks $\theta u(c)$, while I consider shocks to the consumption argument c). Moser and de Souza e Silva (2019) consider persistent heterogeneity in earnings ability but not time-varying, within-person shocks.

time without penalty. The only constraint applying to the liquid account is that its balance may not fall below a fixed floor, $a_t^L \geq \underline{a^L}$. I consider a simple no-borrowing case, so $\underline{a^L} = 0$. The return on the liquid account is normalized to zero.

The second savings account is a partially illiquid 401(k) account, with balance at the beginning of period t denoted by a_t^I. The 401(k) account must always have a weakly positive balance and earns a return of r between periods. The worker can only contribute to the account during working life for $t < t_R$. Contributions to the account are matched by the worker's employer at rate m up to a per-period cap of $\bar{m}$. During the retirement period for $t \geq t_R$, the worker may withdraw from the account without penalty. Additionally, for a subset of periods during working life $t \in W$, the worker may withdraw from the 401(k) account but must pay a proportional early-withdrawal penalty of τ.[11] The pre-retirement withdrawal periods W capture instances where the worker changes jobs and thus becomes eligible to take a penalized cash distribution.[12] Limiting early withdrawals to periods with job changes reflects the institutional reality of the DC system, where all workers are eligible to take withdrawals upon separation but in-service withdrawals are more restricted and much less common.[13]

Let s_t^I be the worker's 401(k) saving at time t, with positive values indicating contributions and negative values indicating withdrawals. Additionally, let $V_t(a^L, a^I)$ be the period-t value function representing the value of optimal behavior from period t onward, taking the two state variables (liquid and illiquid account balances) as arguments. Because any present bias with $\beta < 1$ is naive, the value function is computed assuming that the worker will make optimal choices according to $\beta = 1$ from period t onward. The fully stated problem for the worker at any time t is thus:

$$\max_{c_t, s_t^L, s_t^I} \; u(c_t) + \beta\delta V_{t+1}(a_{t+1}^L, a_{t+1}^I), \tag{2}$$

with state evolution given by:

$$a_{t+1}^L = a_t^L + s_t^L, \tag{3}$$

$$a_{t+1}^I = (1 + r)(a_t^I + s_t^I + \max(0, m * \min(\bar{m}, s_t^I))), \tag{4}$$

[11] I do not formally model taxes; the early-withdrawal penalty τ could be interpreted as representing both the nominal withdrawal penalty and the cost of forgoing tax-deferred investment returns.

[12] A simplifying assumption is that job changes occur in predetermined periods rather than stochastically.

[13] In the empirical sample, 34.4% of separators take a cash distribution but in-service hardship withdrawals occur in only 1.9% of participant-years. See the discussion in Section 4.1.

and constraints given by:

$$c_t + s_t^L + s_t^I + p_t = y_t, \tag{5}$$

$$s_t^L \geq -a_t^L, \tag{6}$$

$$s_t^I \begin{cases} \in [-a_t^I, 0] & \text{if } t \geq t_r \\ \geq -a_t^I & \text{if } t \in W \\ \geq 0 & \text{otherwise,} \end{cases} \tag{7}$$

$$p_t = \begin{cases} \tau s_t^I & \text{if } s_t^I < 0 \text{ and } t \in W \\ 0 & \text{otherwise.} \end{cases} \tag{8}$$

The worker chooses consumption c_t and 401(k) saving s_t^I; these two choices, along with the budget constraint in (5), imply liquid saving s_t^L. The 401(k) account earns a matching contribution if $s_t^I > 0$ and grows with net return r. The constraints in (6)-(8) require that neither account balance fall below zero, that 401(k) withdrawals occur only during retirement or during the designated pre-retirement periods W, and that the worker pay a penalty for any pre-retirement 401(k) withdrawals.

2.2 Simple three-period case

To build intuition for the model's key results, I first consider the simplest meaningful case: two working periods, one retirement period, and one job change. Formally, this corresponds to $T = 3$, $t_R = 3$, and $W = \{2\}$. At $t = 1$, the worker earns labor income from distribution $F_1(y)$. At $t = 2$, the worker changes jobs, earns labor income from distribution $F_2(y)$, and is allowed to cash out the 401(k) balance accumulated in the first job. Finally, at $t = 3$, the retired worker consumes Social Security income y_R and all remaining wealth.

2.2.1 Cashout decision at $t = 2$

Proceeding by backward induction, consider the worker's behavior at $t = 2$. Let a_2^L and a_2^I be the account balances entering $t = 2$ and y_2 be the realized income draw. Because 401(k) withdrawals are not penalized at $t = 3$, there is no reason to forgo the 401(k) return r by carrying a liquid account balance into $t = 3$. The worker therefore depletes the liquid

account and the budget constraint in (5) becomes $c_2 = y_2 + a_2^L - s_2^I - p_2$. A necessary[14] condition for the worker to choose $s_2^I < 0$ and take a penalized 401(k) withdrawal is:

$$u'\left(y_2 + a_2^L\right) > \beta\delta\left(\frac{1+r}{1-\tau}\right)u'\left(y_R + (1+r)a_2^I\right). \tag{9}$$

The left-hand side of (9) gives the marginal benefit of reducing s_2^I from zero to a small negative value. The right-hand side gives the corresponding marginal cost, which must be strictly smaller to justify a cashout. Inspecting (9), we can see that three key factors govern the cashout decision.

- *Current liquidity*: When liquid resources $y_2 + a_2^L$ are low, the marginal utility of consumption at $t = 2$ is high, incentivizing the worker to pull consumption forward in time by withdrawing from the 401(k) account. Low liquidity can arise from an exogenous shock to labor income y_2. It can also arise endogenously through a_2^L if the worker chose a low level of liquid saving at $t = 1$.

- *Future liquidity*: The worker's illiquid retirement wealth (which will become liquid at $t = 3$) is composed of Social Security income and the future value of the 401(k) balance, $y_R + (1+r)a_2^I$. Mirroring the logic from the prior bullet, when the stock of illiquid wealth is large, the worker anticipates relatively low marginal utility of consumption at $t = 3$ and has a stronger incentive to tap 401(k) wealth before retirement. When y_R replaces a larger share of the worker's labor income, the worker therefore has a stronger cashout incentive.

- *Discount rate*: Withdrawn 401(k) funds forgo the market return r and incur the early-withdrawal penalty τ; both of these considerations raise the marginal cost on the right-hand side of (9). Fixing r, τ, and the exponential discount factor δ, the effective discount rate is determined by the present-bias parameter: as β decreases below 1, the cashout condition becomes more likely to hold.

[14] By taking a 401(k) withdrawal (and therefore not contributing to the 401(k) account) at $t = 2$, the worker forgoes the employer match. This makes possible a case where (9) holds but the employer match is sufficiently generous to make a cashout suboptimal. As the match rate m or match cap $\bar{m}$ approach zero, (9) becomes both necessary and sufficient for a cashout to occur.

2.2.2 Numerical solutions

To make the theoretical considerations in the preceding section more concrete, I solve the three-period model numerically for an illustrative set of parameter values. To sharpen the intuition around the effect of income volatility, I use a simple binary income process. At $t = 1$, income is $y_1 = 1$ with certainty. At $t = 2$, income takes one of two values, occurring with equal probability: either $y_2 = 1 + \Delta$ or $y_2 = 1 - \Delta$ for some shock $\Delta \geq 0$.[15]

Appendix Figure A.1 shows the share of the 401(k) balance a_2^I that the worker withdraws at $t = 2$, conditional on the negative income shock $y_2 = 1 - \Delta$ being realized. Each panel shows behavior under a different value for the present-bias parameter ($\beta \in \{0.4, 0.6, 0.8, 1\}$) and lines within each panel represent a different value for Social Security replacement rates ($y_R \in \{0.3, 0.5, 0.7\}$). Consistent with the intuition from (9), cashouts in Figure A.1 are more likely when i) a larger negative income shock reduces current liquidity, ii) higher Social Security income raises future liquidity, and iii) a smaller present-bias parameter decreases the worker's discount rate.

An important feature of the model is that the worker's liquidity position at $t = 2$ is not purely exogenous. Labor income y_2 is a random draw, but the liquid account balance a_2^L is chosen by the worker at $t = 1$. Figure A.2 shows the endogenous accumulation of liquid savings at $t = 1$ (fixing Social Security income at the intermediate value of $y_R = 0.5$). For each β value, liquid saving rises with the size of the symmetric $t = 2$ income shock as the worker perceives a greater precautionary value of liquid wealth. The relationship between income volatility and liquid saving attenuates as β decreases; the most impatient worker with $\beta = 0.4$ never saves in the liquid account.

Two conclusions are worth drawing from Figure A.2. First, within the model, cashouts are more likely for lower β values both because the effective discount rate at $t = 2$ is lower *and* because liquid savings a_2^L are lower. Second, when moving to empirical and policy analysis, cashout behavior will be partially determined by liquid account balances that are not observable in 401(k) recordkeeping data.

[15] Other fixed parameters are as follows: utility is CRRA with coefficient of relatve risk aversion equal to 2, the exponential discount factor is $\delta = 0.97$, the 401(k) return is $r = 0.08$, the early withdrawal penalty is $\tau = 0.1$, the match rate is $m = 0.5$, and the match cap is $\bar{m} = 0.06$. Both the liquid account balance a_1^L and the 401(k) account balance a_1^I are initialized at zero.

2.3 Expanded case with many periods

2.3.1 Setup and cashout decision

The simple three-period case is sufficient to establish the essential intuition around cashout decisions. Even so, it is worth briefly considering an expanded case that aligns more closely with the empirical setting of Section 4. The empirical analysis will consider mid-career 401(k) participants who earn income over many sub-annual periods (months), then separate from employment and decide whether to cash out. Participants' income histories are no longer observable in the Vanguard data following separation, but after making their cashout decision they presumably begin another job and eventually retire.

In an expanded but computationally tractable case, I set $T = 15$, $t_R = 11$, and $W = \{5\}$. The periods preceding the cashout decision $t \leq 5$ represent the observable income history during a worker's employment spell at a Vanguard-administered plan. The worker separates and makes a cashout decision at $t = 5$, earns income at a different job for $6 \leq t \leq 10$, then retires at $t = 11$. I make retirement five periods long so that the relative lengths of working life and retirement are the same as in the three-period case. Finally, I let labor income y_t be independent across periods and follow a lognormal distribution with mean zero and standard deviation σ. I consider a range of σ values to illustrate comparative statics with respect to income volatility.[16]

The cashout decision at $t = 5$ is driven by the same factors from the three-period case (current liquidity, future retirement liquidity, present bias) but has two important differences. First, the worker's current liquidity position at $t = 5$ represents the cumulative effects of many random income draws during the first job. Second, because working periods remain before retirement, cashouts need not fund consumption exclusively in the current $t = 5$ period. If the worker has a low liquid account balance a_5^L and finds the continuation value of marginal liquid wealth to be high, she may cash out some of her 401(k) balance to deposit in the liquid account as protection against income shocks during the second job.

2.3.2 Numerical simulations

Table 1 summarizes workers' cashout decisions at $t = 5$ in the many-period case. For each present-bias value $\beta \in \{0.4, 0.6, 0.8, 1.0\}$ and each $\sigma \in \{0, 0.1, 0.2, 0.3, 0.4, 0.5\}$,

[16] All other parameters remain the same as in the three-period case. I fix Social Security income y_R at 0.5.

Table 1: Cashout determinants in the many-period case

	Present-bias parameter			
	$\beta = 0.4$	$\beta = 0.6$	$\beta = 0.8$	$\beta = 1$
Intercept	2.43	1.55	1.32	1.02
	(0.02)	(0.02)	(0.01)	(0.01)
Std. dev. of income process (σ)	-0.54	1.01	0.76	0.55
	(0.01)	(0.01)	(0.01)	(0.01)
Realized avg. income before job change	-1.58	-1.51	-1.32	-1.04
	(0.02)	(0.02)	(0.01)	(0.01)

Note: This table summarizes cashout decisions made by workers in the expanded case of the model with $T = 15$. See Sections 2.1-2.3 for a description of the model. Income in each period is independently and lognormally distributed with mean 0 and standard deviation σ. For each $\sigma \in \{0, 0.1, 0.2, 0.3, 0.4, 0.5\}$ and each present-bias parameter $\beta \in \{0.4, 0.6, 0.8, 1\}$, I conduct 10,000 simulations. The table shows regression results where the observation is a simulation and the dependent variable is an indicator equal to 1 if the worker takes a penalized 401(k) withdrawal when switching jobs at $t = 5$. The independent variables are the standard deviation of the income process σ and the worker's realized average income before the job change (i.e., $\frac{1}{5}\sum_{t=1}^{5} y_t$). I estimate the regression separately for each of the β values. Standard errors are in parentheses.

I conduct 10,000 simulations. With individual simulations as observations, I estimate regressions (separately for each β value) where the dependent variable is an indicator equal to 1 if the worker takes a penalized 401(k) withdrawal at $t = 5$. The independent variables are σ (representing the volatility of the underlying income process) and realized average income through $t = 5$ (representing the exogenous realizations of the income process).

The results in Table 1 are intuitive. First, the regression intercepts decline monotonically with β, indicating that more patient workers have lower baseline cashout rates regardless of income volatility. Second, the coefficients on the average income realization are negative and decline in magnitude with β. This is a generalization of the simpler three-period logic: workers with lower average income during their first job have less liquidity at $t = 5$ and are more likely to cash out. The relationship attenuates for higher β values because patient workers build larger precautionary buffers in the liquid account that provide better insurance against negative shocks.

The many-period case is conceptually valuable because it allows us to estimate the remaining regression coefficient on σ. This coefficient is positive for $\beta \geq 0.6$,[17] indicating that cashouts are more likely when the underlying data-generating process is more volatile (*conditional* on the average income realization through $t = 5$). This relationship holds for two reasons. The first is backward-looking: fixing realized average income, workers facing

[17]The $\beta = 0.4$ case is outlying because the worker is sufficiently impatient to cash out in *every* simulation with $\sigma = 0$. Moving to nonzero σ values introduces the possibility of large positive income shocks at $t = 5$ that deter the worker from cashing out. As a result, the regression coefficient on σ is negative.

higher σ values experience greater mean-zero volatility through $t = 5$ and are therefore more likely to deplete their liquid wealth by intertemporally smoothing consumption during the first job. The second reason is forward-looking: conditional on realized income through $t = 5$, workers facing higher σ values anticipate greater volatility during $6 \leq t \leq 10$ and thus perceive a higher continuation value for liquid wealth. The coefficient magnitudes again decline with β because patient workers build larger liquidity buffers that are more likely to withstand volatility during the first job and remain sufficient for the second job.

The ceteris paribus relationships in Table 1 are instructive for the empirical analysis. The essential intuition from the three-period case persists, with negative income shocks reducing liquidity and raising cashout rates. Additionally, even if negative shocks are accompanied by positive shocks in other periods and the first-job income draws are symmetrically distributed around their expected value, the resulting mean-zero volatility can itself cause cashouts. For example, in the baseline $\beta = 1$ case with $\sigma = 0.3$, Table 1 implies that the cashout rate is 10% even in simulations where realized average income through $t = 5$ is exactly equal to its expectation.[18] The regression specifications in Section 4 will aim to capture both effects by controlling separately for the second moment (representing mean-zero volatility) and the third moment (representing asymmetric negative shocks) of participants' monthly income histories.

2.4 Key conclusions and empirical predictions

The model demonstrates that stochastic labor income can induce early 401(k) withdrawals, even in the absence of behavioral biases. When the worker receives negative income shocks, her current liquid resources can become small relative to her illiquid retirement wealth, making her willing to tap 401(k) accounts (and pay early-withdrawal penalties) in order to support current consumption. Additionally, even when negative shocks are offset by equally-sized positive shocks, the resulting mean-zero volatility can deplete the worker's liquidity buffer and prompt an early withdrawal. By assembling a paycheck-level income panel for 401(k) participants, I can test these empirical predictions and evaluate the effects of both symmetric and asymmetric shocks on cashout probability.

The model also accommodates behavioral biases, but allows them to play a more nuanced role in shaping withdrawal decisions. In standard models of naive β-δ preferences, the worker saves in a retirement account at $t = 1$, overestimates her future patience, and

[18] With $\mathbb{E}[y_t] = e^{(1/2)*(0.3^2)}$, the cashout rate is $1.02 + (0.55 * 0.3) - (1.04 * \mathbb{E}[y_t]) = 0.097$.

unambiguously lowers her lifetime utility by taking an early withdrawal at $t = 2$. Here, a present-biased worker's decision to cash out at $t = 2$ can be optimal *ex post* if an income shock raises her marginal utility of consumption at $t = 2$. But the same withdrawal can be suboptimal in an *ex ante* sense if a non-present-biased worker would have saved enough in the liquid account at $t = 1$ to provide insurance against the income shock. This distinction between ex post and ex ante optimality is not present in models with deterministic income (Beshears et al., 2022b) or in models where workers do not make ex ante liquid saving decisions (Beshears et al., 2025). To behave optimally, the worker must have the foresight to save in a lower-yielding, unmatched liquid account so that future income shocks do not necessitate costly early withdrawals from her retirement account. If some workers lack this foresight, then constructive policy responses include interventions that facilitate liquid wealth formation (e.g., automatic enrollment into employer-sponsored short-run savings accounts) and are not limited to the traditional policy implications of present-bias models (i.e., higher early-withdrawal penalties or complete illiquidity of retirement assets).

3 Data and summary statistics

3.1 Vanguard administrative data

I use administrative 401(k) recordkeeping data from Vanguard. At the plan level, I observe characteristics of the plan sponsor and plan rules. At the participant level, I observe all financial transactions (both contributions and withdrawals), account balances, hire and separation dates, and age.

Almost all plans in the recordkeeping data are 401(k) plans; the small remainder are highly similar but technically distinct defined-contribution plans, such as 403(b) plans. For brevity, I refer throughout the paper to the sample as being composed of "401(k) plans." I focus on a subset of plans in which I can accurately infer participants' paychecks, using the methodology described below.

3.2 Income inference

Employees contribute to 401(k) plans by deferring money from their paychecks. Usually, these payroll deferrals are specified in proportional terms: at any given time, a 401(k) participant has an "elected contribution rate" C such that C% of their next paycheck will be

Table 2: Income inference examples

Likely hourly worker

Date	Contribution amount	Contribution rate	Inferred income
2022-01-07	56.54	0.06	942.33
2022-01-14	73.42	0.06	1,223.67
2022-01-21	47.60	0.06	793.33
2022-01-28	57.91	0.06	965.17
2022-02-04	60.40	0.06	1,006.67

Likely salaried worker with bonus

Date	Contribution amount	Contribution rate	Inferred income
2022-10-28	153.42	0.06	2,557.00
2022-11-11	153.42	0.06	2,557.00
2022-11-25	885.42	0.06	14,757.00
2022-12-09	178.99	0.07	2,557.00
2022-12-23	178.99	0.07	2,557.00

Note: These fictional examples are for illustrative purposes only; they do not represent any particular participant in the data.

contributed to their 401(k) account.[19] If we observe both the dollar amount of an employee contribution transaction and the underlying contribution rate, we can infer the size of the corresponding paycheck. For example, if a participant with a 5% contribution rate made a $50 contribution on January 1, we can infer that they received a $1,000 paycheck on January 1. Because participants specify contribution rates as a percentage of *gross* pay, the $1,000 figure is a gross amount representing pay before tax withholding and other deductions.[20]

Table 2 illustrates the simple logic of the income inference process (using fictional transactions that do not represent any particular participant in the data). The left-hand panel shows a participant whose contribution amounts vary substantially from paycheck to paycheck while the contribution rate remains constant; the resulting variance in inferred income values likely indicates that this participant is an hourly worker. The right-hand panel shows a participant whose inferred income amounts are usually the same down to the cent, except for one large outlying transaction; this participant is likely a salaried worker who receives infrequent bonuses.

There are two important limitations to consider when inferring income from contribution activity. First, employees' paychecks are not observable unless they contribute to their 401(k) accounts; the empirical sample is therefore limited to 401(k) *participants* and does not include non-participating employees. Even if an employee is participating in the plan, their contribution transactions may cease abruptly if they separate from employment, voluntarily set their contribution rate to zero, or hit the IRS limit on annual employee

[19]The contribution rate can be set by active participant choice or by default in automatic-enrollment plans. I exclude the very small number of plans whose participants specify contributions in flat dollar terms (e.g., $50 is deferred from each paycheck, no matter the paycheck amount).

[20]This logic holds for both traditional tax-deferred and Roth contributions. In the above example, a 5% contribution rate implies $1,000 in gross pay regardless of whether the contribution is traditional or Roth. A participant making Roth contributions would have $1,000 in taxable income (rather than $950 for a participant making traditional contributions) and would therefore have higher tax withholding and lower net pay.

contribution amounts.[21] As discussed in more detail below, I generally restrict attention to participant-years in which contributions occur in both January and December, which helps ensure that the resulting sequence of paychecks represents the participant's complete income history for the corresponding calendar year. The main consequence of limiting to full calendar years is that I drop some high earners who hit the IRS contribution limit and therefore have unobservable paychecks for some portion of the year. I control for annual income nonparametrically throughout the empirical analysis, so dropping these high earners does not affect the estimates I obtain for low- and middle-income participants.[22]

The other limitation is that inferred paychecks do not contain information about hours worked. As a result, I cannot directly observe the hourly versus salaried nature of the participant's employment contract and cannot decompose earnings changes into wage and hours components. To make the binary hourly-salaried classification, I rely on statistics from the payroll-processing data used by Ganong et al. (2024), in which workers are explicitly identified as hourly or salaried. The optimal binary-classification rule in the Ganong et al. (2024) data is to classify a worker as hourly if at least 45% of their paychecks change by at least $1 relative to the lagged paycheck value; this rule accurately classifies about 84% of workers in their data.[23] I adopt this rule throughout the empirical analysis, classifying a participant-year as hourly if at least 45% of the participant's paychecks in the corresponding year change by at least $1. Similarly, I classify a participant as hourly if at least 45% of all of the participant's observable paychecks change by at least $1. In many cases I refer to participants as being "likely hourly" or "likely salaried" to emphasize that contract status is not directly observable in the recordkeeping data. In cases where I simply say "hourly" and "salaried," I do so only for brevity; the classification is always based on the same 45% cutoff.[24]

While imperfect, I show that this simple binary classification produces two groups

[21] For example, the IRS limit on employee contributions in 2023 was $22,500. If a participant reached $22,500 in total contributions in 2023, their contributions were automatically suspended and did not restart until January 2024. Across all Vanguard-administered plans in 2024, 14% of participants reached the contribution limit. See Clark (2025).

[22] Among Vanguard-administered plans, less than 2% of participants with annual income below $100,000 hit the IRS contribution limit. See Clark (2025). Dropping participant-years where the IRS limit is reached is likely conservative, since the excluded observations are consistent with my hypothesis: high earners are disproportionately salaried with low intra-year volatility (see Table 4) and have lower cashout rates (Table 6).

[23] I thank Peter Ganong, Pascal Noel, and coauthors for generously sharing these statistics with me.

[24] Additionally, Ganong et al. (2024) show that the vast majority of within-year earnings volatility for hourly workers is driven by variation in hours. It therefore seems safe to interpret earnings fluctuations for hourly participants within a given year as arising primarily from volatility in hours worked, with relatively stable wage rates.

with very different monthly income volatility (see Table 4) and roughly matches the share of hourly versus salaried workers reported in external datasets (see Table 5 and the surrounding discussion in Section 3.4). I also obtain similar results when using continuous volatility measures that do not require cutoff assumptions (see Section 4.2).

Even though contract status is not directly observable, the recordkeeping data do allow some independent checks of my hourly-salaried classifications. First, in a small number of cases, plans' official legal names give an explicit indication of their participants' contract types (e.g., "XYZ 401(k) Plan for Hourly Employees"). I show in Appendix Table A.1 that among plans with explicit hourly names, the 45% rule reassuringly classifies 99.5% of participant-years as hourly. In plans with explicit salaried names, the 45% rule classifies only 21.4% of participant-years as hourly.[25] The plan-name test is useful but limited in scope because less than 2% of participant-years in the final sample are in plans with an explicit name (the fact that most plans contain a mix of hourly and salaried workers permits the inclusion of plan fixed effects in the main regression specifications in Section 4). Second, I show in Appendix Table A.2 that participant-years with weekly pay schedules are classified as hourly 90.9% of the time. The hourly classification share decreases to 55.3% among biweekly and semimonthly pay schedules, and to 32.5% among monthly pay schedules. Since hourly workers tend to have more frequent pay schedules, and some states require hourly workers to be paid on a weekly basis (ADP, 2019), this heterogeneity by pay schedule is to be expected of an accurate classification rule.

3.3 Sample filtering

I study the 2008-2023 period, which is the longest time period for which all necessary components of the administrative data are available. As part of its recordkeeping responsibilities, Vanguard maintains information about the dollar amount of every contribution transaction for every plan. As discussed above, I must also observe the underlying contribution rate in order to infer paychecks. Vanguard tracks participants' contribution rates for about two-thirds of the plans it administers. During the 2008-2023 period, Vanguard tracked contribution rates for 1,655 distinct plans.

For a distinct but highly overlapping subset of about two-thirds of plans, Vanguard performs annual nondiscrimination testing mandated under ERISA. As part of the testing

[25]The 21.4% figure likely overstates the frequency of incorrect hourly classifications, as some plans with "salaried" in their official legal names include non-unionized hourly workers as well as salaried workers.

Table 3: Sample filtering steps

Filtering step	Plans	Participant-years	
		Number	Share of original
Contribution rate and W-2 income observable	1,298	26,051,745	100.0%
Dropped: hire or separation during the year		4,779,859	18.3%
Dropped: hit IRS limit or voluntary contribution cessation		3,874,152	14.9%
Contributions in Jan., Dec., and $\geq$ 9 distinct months	1,266	17,397,734	66.8%
Dropped: non-deferrable income $>$ 3% of total		3,252,052	12.5%
Dropped: remainder, including unobservable non-deferrable income		3,571,722	13.7%
Inferred annual income within 3% of W-2 value	1,259	10,573,960	40.6%

Note: The sample period is 2008-2023. I restrict to plans with observable contribution rates so I can infer paychecks according to the methodology discussed in Section 3.2. I further restrict to plans with W-2 income data so I can verify the sum of inferred paychecks against the annual W-2 figure. Non-deferrable income is income from which participants are not allowed to make 401(k) contributions (and is thus impossible to infer from contribution activity).

process, these plans provide Vanguard with official annual income figures for all employees (those reported on IRS form W-2). Although the empirical analysis focuses on *sub*-annual income volatility, the annual W-2 figures are useful as a final check on the income inference process: for each participant-year, I can sum the inferred paychecks and verify that the inferred annual income amount is sufficiently close to the official W-2 figure. I therefore restrict to the 1,298 distinct plans for which Vanguard tracked contribution rates and collected W-2 income figures during the 2008-2023 period.

Table 3 shows the steps I take to arrive at the final sample of participant-years. The starting point is the universe of 26 million participant-years for the 1,298 eligible plans across the 2008-2023 period. Requiring employee contribution transactions in January, December, and at least nine distinct months drops partial years with hire or separation activity (18.3% of the initial 26 million participant-years) and cases where participants hit the IRS limit or voluntarily cease contributions (14.9%).[26] Since roughly 14% of Vanguard participants hit the IRS limit in a given year (Clark, 2025), contribution limits account for most of the latter set of dropped observations and voluntary cessations are rare.

From the intermediate sample of 17.4 million complete calendar years, I restrict further to participant-years where the sum of inferred paychecks is within 3% of the official W-2 value. Observations dropped at this final filtering step fall into two groups. First, in roughly 3.3 million cases, the participant receives non-deferrable income that is at least 3% of their total W-2 income. Non-deferrable income is compensation (often annual

[26] A small number of participants are not paid during the summer, so I use a 9-month cutoff instead of a 12-month cutoff for the purpose of identifying full calendar years.

bonuses) from which participants are not permitted to make 401(k) contributions. Since it is impossible to infer non-deferrable income from contribution activity, dropping these 3.3 million participant-years is a mechanical result of the 3% W-2 accuracy filter. Some plan sponsors do not separately report deferrable and non-deferrable income to Vanguard, instead providing only a single total W-2 figure. The remaining 3.6 million observations dropped at the final filtering step thus likely contain many additional (but not explicitly identifiable) cases where non-deferrable income creates a mechanical difference between the inferred and W-2 values.[27]

The final sample of 10.6 million participant-years are cases where I can be highly confident that the sequence of inferred paychecks is a complete and accurate representation of the participant's income history. To alleviate concerns that the observations dropped at the final filtering step (primarily due to non-deferrable income) are selected, I show in the appendix that the main empirical results hold when removing the W-2 accuracy condition and considering the larger sample of 17.4 million complete years.

3.4 Summary statistics

3.4.1 Monthly income volatility

Table 4 shows summary statistics for the final sample of roughly 10.6 million participant-years. The classification rule is based on a paycheck-change cutoff of 45%, but most participant-years classified as hourly have a substantially higher share of paychecks change by at least $1: the 10th percentile among the hourly group is 73%. The average hourly participant sees 92% of their paychecks in a given year change by at least $1; the corresponding average for the salaried group is 16%. Unsurprisingly, annual income is significantly lower for hourly participants: the median is $66,300 for the hourly group and $107,700 for the salaried group (in 2023 dollars). Contribution rates are more similar, averaging 7.1% for hourly participants and 8% for salaried participants.

To measure sub-annual income volatility, I follow Ganong et al. (2024) in focusing on monthly earnings per paycheck. For a given participant-year, I aggregate earnings within each month and compute volatility measures (standard deviation, skewness, median absolute first difference) across the resulting sequence of 12 monthly values. Normalizing

[27] Additionally, participants who hit the IRS limit in December may be dropped at the final filtering step. For example, if a participant receives two paychecks in December and hits the IRS limit on the first, missing the second paycheck may cause me to understate income by at least 3% (even though this participant makes a December contribution and thus qualifies for the intermediate sample of 17.4 million complete years).

Table 4: Summary statistics by participant-year

	Likely hourly	Likely salaried
Participant-years	7,069,907	3,504,053
Annual income (2023 $)		
10th	33,612	63,328
50th	66,341	107,667
90th	125,854	184,287
Mean	74,640	117,920
Share of paychecks with $\geq$ $1 change		
10th	73.1%	4.0%
50th	97.5%	13.0%
90th	100.0%	33.7%
Mean	92.1%	16.1%
Median absolute first difference of monthly earnings/paycheck		
10th	3.1%	0.0%
50th	9.3%	0.0%
90th	21.7%	4.6%
Mean	11.5%	2.1%
Standard deviation of log monthly earnings/paycheck		
10th	0.05	0.01
50th	0.12	0.09
90th	0.27	0.29
Mean	0.15	0.13
Skewness of log monthly earnings/paycheck		
10th	-1.12	-1.58
50th	0.25	1.90
90th	1.71	3.01
Mean	0.26	1.24
Share negative	39.6%	26.8%
Annual contribution rate		
10th	2.9%	3.2%
50th	6.1%	6.6%
90th	12.3%	14.2%
Mean	7.1%	8.0%
Share with plan loan origination	13.0%	8.8%
Share with hardship withdrawal	2.5%	0.8%
Hardship withdrawal amount (cond. nonzero)		
Mean	7,648	11,776
Median	3,687	5,481

Note: Participant-years are classified as "likely hourly" if at least 45% of the participant's paychecks in the relevant year change by at least $1 relative to the lagged paycheck value. The remaining participant-years where fewer than 45% of paychecks change by at least $1 are classified as "likely salaried." See Section 3.2 for a discussion of this classification rule. Monthly income figures are normalized by the number of paychecks to eliminate mechanical variation (e.g., two- versus three-paycheck months). Annual income figures are given in real 2023 terms.

each monthly earnings total by the number of paychecks eliminates mechanical variation arising from common pay schedules (e.g., participants paid on biweekly schedules receive three paychecks in two months per calendar year).[28] For the standard deviation and skewness measures, I apply a log transformation and consider the log of monthly earnings per paycheck. This allows convenient proportional interpretations, especially when volatility measures appear in regression specifications in Section 4.

Table 4 confirms that the typical hourly participant experiences greater monthly income volatility than the typical salaried participant. For the average participant-year in the hourly group, the standard deviation of log monthly earnings per paycheck is 0.15, indicating that monthly volatility is roughly 15%. For the salaried group, the corresponding average figure is 0.13. The hourly and salaried figures are more similar than one might expect because roughly a third of the salaried group receive annual bonuses, creating a right tail of salaried participants with large monthly standard deviations (see the discussion of the bonus-identification algorithm in Appendix B). The difference between hourly and salaried volatility becomes clearer when looking at the third moment, i.e., the skewness of log monthly earnings per paycheck. The average hourly skewness value is 0.26, while infrequent positive bonus shocks push the corresponding salaried value up to 1.24. Hourly participants are also 1.5 times more likely to have a negative skewness value (40% of participant-years) than salaried participants (27%).[29]

The median first difference measure creates a cleaner division between the hourly and salaried groups, capturing the fact that fluctuations for salaried workers are rare (though large conditional on occurrence). Letting $e_{i,t}$ be monthly earnings per paycheck for participant i in month t, I compute $\frac{|e_{i,t}-e_{i,t-1}|}{e_{i,t-1}}$ for each month and take the median value within a participant-year. The resulting within-year medians represent participants' typical monthly earnings changes and are not affected by annual bonuses paid in one month per year. The average value for the hourly group is 11.5%, versus 2.1% for the salaried group (and the median salaried participant has a typical monthly earnings change of 0%). Appendix Figures A.3-A.6 show the full distributions of the standard deviation, skewness, and median absolute first difference measures.

Additionally, Appendix Table A.4 gives summary statistics for the participant-years

[28] About 3% of participant-years in the final sample have fewer than 12 months with earnings, due to the small number of participants who are not paid over the summer. The sample filtering discussed in Section 3.3 ensures that all participant-years in the final sample have at least 9 months with earnings.

[29] Salaried participants can have negative skewness values if they do not receive an annual bonus but do receive an annual raise early in the year.

dropped at the final filtering step (i.e., cases where I observe a complete year of contribution history but where the inferred annual income amount is not within 3% of the W-2 value). The participant-years dropped at this step have higher incomes and are more likely to be classified as salaried, and the salaried observations have lower standard deviation and skewness measures than the baseline sample. These patterns are all consistent with non-deferrable income creating mechanical gaps between the inferred and W-2 values: non-deferrable payments are more likely to be made as bonuses to higher-income salaried participants, and missing these bonus payments causes me to understate the volatility and positive skewness of the dropped participants' paycheck histories.

3.4.2 Cashout rates

Table 5 summarizes separation and cashout outcomes for the roughly 3.4 million distinct participants in the final sample (i.e., those with at least one yearly observation in the final sample of 10.6 million participant-years). Using the binary-classification rule discussed in Section 3.2, 65% of participants are classified as likely hourly. Because salaried participants are more likely to hit the IRS contribution limit and to receive non-deferrable bonuses, this slightly overstates the share of the broader recordkeeping population that is hourly. Among the initial unfiltered sample (i.e., the distinct participants comprising the 26 million yearly observations in the first row of Table 3), I classify 62% of participants as hourly. This share is comparable to, though still slightly higher than, the Ganong et al. (2024) dataset (where 60% of workers are hourly) and nationally representative government statistics (where 56% are hourly; see U.S. Bureau of Labor Statistics 2024).

For both the hourly and salaried groups, about half of participants separate from employment during the sample period. Separating participants can do one of three things with their 401(k) balance: they can leave the balance in the plan, roll it over to an IRA or a new employer's 401(k) plan, or take a cash distribution. They can also combine these options, for example by taking a partial cash distribution and rolling the remaining balance to an IRA. I consider a separated participant to have cashed out if they take any nonzero cash distribution at any point after their separation date.[30]

[30]The sample period is 2008-2023 but the distribution transaction data are complete through August 2025, so I have an extra 20 months of data to observe cashout transactions for the latest cohort of separators. When identifying cashouts, I only consider total distributions (i.e., cases where the participant's entire balance is distributed through a cash withdrawal, rollover, or combination) and ignore partial withdrawals (e.g.,

Table 5: Summary statistics by distinct participant

	Likely hourly	Likely salaried
Distinct participants	2,243,079	1,186,705
Share auto-enrolled	45.4%	36.8%
Share separated	51.4%	53.9%
With complete spell observed	17.3%	12.4%
Age at separation		
10th	27	30
50th	44	47
90th	64	64
Tenure at separation		
10th	1	2
50th	5	6
90th	22	24
Share with cashout (cond. separation)	41.8%	21.0%
Cashout amount (cond. nonzero)		
Mean	12,704	17,109
Median	3,849	5,224
Cashout share of balance (cond. nonzero)		
Mean	91.0%	75.9%
Median	100.0%	100.0%
Share of cashouts for entire balance	88.2%	71.5%

Note: Participants are classified as "likely hourly" if at least 45% of their observable paychecks change by at least $1 relative to the lagged paycheck value. The remaining participants for whom fewer than 45% of paychecks change by at least $1 are classified as "likely salaried." See Section 3.2 for a discussion of this classification rule.

The baseline cashout rates in Table 5 already give evidence of the model's main empirical prediction: conditional on separating, 42% of hourly participants cash out, compared to 21% of salaried participants. The large majority of cashouts are for the participant's entire vested balance, so cashout amounts are substantial in dollar terms: the median value is $3,849 for the hourly group and $5,224 for the salaried group. Of course, hourly participants have lower annual incomes, so the difference in baseline cashout rates is driven both by the level of income and the volatility of income. Table 6, which again shows cashout rates conditional on separation, disentangles these two effects. Cashout rates decline with annual income in both the hourly and salaried groups, but for each income level, hourly cashout rates are significantly higher. In the $25,000-$150,000 range with the most overlap between the hourly and salaried income distributions, hourly cashout rates are 10-15 percentage points higher.

installment payments taken by retirees). Cash withdrawals and rollovers are separately identified in the recordkeeping data.

Table 6: Cashout rates by annual income

	Cashout rate		Separated participants	
Annual income (2023 $)	Likely hourly	Likely salaried	Likely hourly	Likely salaried
$\leq$ 25,000	45.8%	38.3%	95,183	1,836
25,001 – 50,000	49.4%	37.7%	367,053	25,676
50,001 – 75,000	43.1%	28.1%	339,693	106,738
75,001 – 100,000	35.6%	22.8%	177,869	146,570
100,001 – 125,000	31.0%	19.3%	82,493	123,906
125,001 – 150,000	26.6%	16.9%	42,477	89,834
$>$ 150,000	22.4%	14.7%	48,840	145,472

Note: This table considers participants who separate during the sample period. Participants are classified by their average annual income across the sample period. Participants are classified as "likely hourly" if at least 45% of their observable paychecks change by at least $1 relative to the lagged paycheck value. The remaining participants for whom fewer than 45% of paychecks change by at least $1 are classified as "likely salaried." See Section 3.2 for a discussion of this classification rule. Annual income figures are in real 2023 terms.

Appendix Tables A.5-A.6 show that cashout rates remain very similar when I remove the W-2 accuracy condition and consider the larger sample of participants with at least one year of complete contribution history. In the next section, I turn to more formal tests of the volatility-cashout relationship.

4 The effect of income volatility on early withdrawals

4.1 Empirical specification

In this section, I present formal regression-based evidence on the relationship between income volatility and early 401(k) withdrawals. I consider two different withdrawal outcomes: one varies at the participant-year level and the other at the participant level.

First, at the participant-year level, I consider the probability that a given participant takes a hardship withdrawal in a given year. Hardship withdrawals, which are generally subject to the 10% early-withdrawal penalty, allow participants experiencing financial distress to withdraw 401(k) savings during employment. Because hardship withdrawals are subject to relatively strict regulation – participants must provide documentation of a specific financial hardship and may only withdraw an amount necessary to cover the documented hardship – they are rarely used.[31] As shown in Table 4, hardship withdrawals occur in only 2.5% of hourly participant-years and 0.8% of salaried participant-years. Although they are rare, hardship withdrawals are still an informative empirical outcome because they allow me to draw a contemporaneous link between income volatility experienced in a

[31]See this IRS summary of hardship withdrawal rules.

given year and penalized 401(k) withdrawals taken in the *same* year.

The second and more important outcome is cashout probability at separation. Theoretically, post-separation cashout decisions are of primary interest because they are the explicit subject of the model discussed in Section 2. Empirically, cashouts are the most important form of leakage because they are the most common form of leakage. Again referencing Table 4, 42% of separating participants in the hourly group and 21% of separating participants in the salaried group take a cash distribution. Cashouts are more prevalent than hardship withdrawals in part because they are less regulated: any participant who has separated from their employer is eligible to take a cash distribution from their 401(k) balance, without providing a specific reason or documentation of financial hardship. Cashouts are only subject to the 10% early-withdrawal penalty if the participant is younger than 55 at the end of the calendar year in which the separation occurs; I therefore restrict the baseline cashout analysis to participants who are younger than 55 at the end of their separation year.[32] Since cashouts only occur after separation, the analysis will condition on separation and relate cashout probability to measures of income volatility that are averaged across the participant's employment spell.

Let i index participants, j index plans, and t index years. Additionally, let h_{ijt} be an indicator equal to one if participant i in plan j takes a hardship withdrawal in year t. The regression specification I use to study hardship withdrawals is:

$$h_{ijt} = \sum_k \gamma_k * \mathbb{1}\{e_{ijt} \in b_k\} + \sum_k \beta_k * \mathbb{1}\{e_{ijt} \in b_k\} * v_{ijt} + \rho X_{ijt} + \alpha_j + \theta_t, \tag{10}$$

where e_{ijt} is annual income, b_k are buckets for annual income, v_{ijt} is a measure of sub-annual income volatility, X_{ijt} are covariates, α_j are plan fixed effects, and θ_t are year fixed effects. The coefficients of interest are β_k, which give the effect of sub-annual income volatility on hardship-withdrawal probability. The estimated effect of income volatility is conditional on nonparametric controls for annual income (via the b_k buckets), quadratics of age and plan tenure (via X_{ijt}), and the plan fixed effects (which absorb time-invariant characteristics like employer industry and plan rules). Interacting the income-volatility measure v_{ijt} with the income buckets b_k allows me to estimate heterogeneous effects across the annual income distribution. For the hardship-withdrawal analysis, I use buckets b_k that are \$5,000 wide (using annual income figures converted to real 2023 dollars).

The regression specification for the cashout analysis has the same basic structure but is

[32] IRS rules on post-separation distributions are discussed here.

specified at the participant level. Let c_{ij} be an indicator equal to one if participant i in plan j takes a nonzero cash distribution after separation. Considering only participants who separate during the sample period, the specification is:

$$c_{ij} = \sum_k \gamma_k * \mathbb{1}\{\bar{e}_{ij} \in b_k\} + \sum_k \beta_k * \mathbb{1}\{\bar{e}_{ij} \in b_k\} * \bar{v}_{ij} + \rho X_{ij} + \alpha_j + \theta_t. \tag{11}$$

Annual income $\bar{e}_{ij}$ is now averaged across all observable years for participant i in plan j, as is the volatility measure $\bar{v}_{ij}$. The age and tenure values in X_{ij} are taken as of the separation year. Because the sample size for (11) is significantly smaller (participant-years are collapsed to participants and non-separating participants are dropped), I expand the income buckets b_k to be \$10,000 wide.

A final consideration for the cashout regression specification is that many plans execute automatic distribution transactions for separating participants with small balances. Most Vanguard-administered plans execute automatic cashouts for balances smaller than \$1,000 and automatic IRA rollover transactions for balances that are between \$1,000 and \$5,000 (Clark, 2025).[33] In the X_{ij} vector in (11), I therefore include dummy variables controlling for balances less than \$1,000 and for balances between \$1,000 and \$5,000 at separation. The proper econometric treatment of automatic distributions is not obvious, but I include the dummy variables because doing so is conservative: balance size correlates negatively with income volatility and negatively with cashout probability. In any event, the cashout regression results are not sensitive to the inclusion or exclusion of these dummy variables. Requiring at least one year of complete contribution history (see Table 3) ensures that most balances are above the \$1,000 automatic-cashout threshold, and the controls for annual income, age, tenure, and plan fixed effects explain much of the remaining variation in balance size.[34]

As v_{ijt}, I use the volatility measures introduced in Section 3.4. In some specifications, I let v_{ijt} be simpler, one-dimensional measures of income volatility. The one-dimensional measures I consider are i) a dummy variable for the participant-year being classified as hourly (according to the binary cutoff rule from Section 3.2), and ii) the median absolute first difference of monthly earnings per paycheck (representing participants' typical monthly

[33] Separating participants are notified of impending automatic distributions and have a time window (usually 30-60 days) to elect a different distribution option before the automatic transaction. The cashout rate is therefore less than 100% for balances below \$1,000 and greater than 0% for balances between \$1,000 and \$5,000. See Appendix Table A.10.

[34] Fewer than 3% of participants considered in the cashout analysis have balances below \$1,000 at separation.

earnings fluctuations). These measures are useful primarily in capturing extensive-margin differences between hourly and salaried participants.

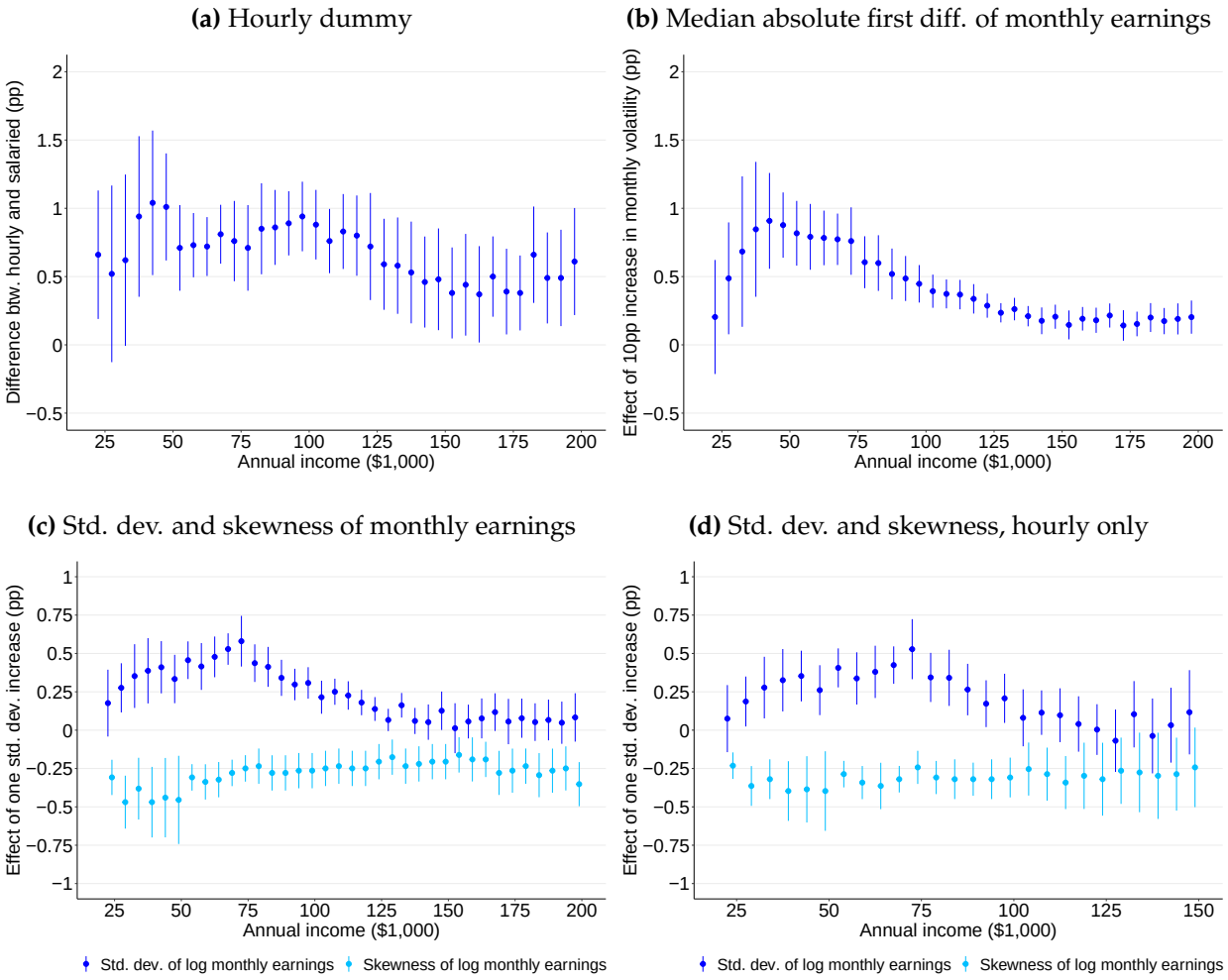
In other specifications, v_{ijt} is two-dimensional and includes both the standard deviation and the skewness of log monthly earnings per paycheck. The two-dimensional case allows me to distinguish the effects of mean-zero and asymmetric volatility. Conditional on skewness, an increase in standard deviation occurs through larger mean-zero shocks; conditional on standard deviation, a decrease in skewness results from an asymmetric shift toward larger negative shocks. Finally, it is worth emphasizing for the cashout regressions that the standard deviation and skewness values are computed within participant-year and then averaged across years to reach $\bar{v}_{ij}$. The $\bar{v}_{ij}$ values therefore represent the average level of within-year volatility, not annual income growth across years.[35]

4.2 Results

Figure 1 shows the results of the hardship-withdrawal regressions from (10). The plotted coefficients are the β_k, which give the heterogeneous effects of income volatility within each annual income bucket. Panel (a) considers the entire sample of participant-years and adopts the simpler specification where the independent variable of interest is a dummy for participant-years classified as likely hourly. The coefficients are positive throughout the annual income distribution and indicate that hourly participants (conditional on annual income, age, tenure, and plan fixed effects) have hardship withdrawal rates that are 0.5–1.0 percentage points higher than salaried participants. Given the low baseline hardship-withdrawal rate (1.9% for the overall sample), the coefficients represent substantial proportional effects of 26–53%. In panel (b), I replace the hourly dummy with the median absolute first difference of monthly earnings per paycheck. The coefficients in panel (b) are scaled to give the effect of a 10-percentage-point increase, which roughly corresponds to the average difference between the hourly and salaried groups (see again Table 5). The estimates are therefore similar in magnitude to those in panel (a), but using a more precise quantitative measure reduces standard errors. The heterogeneity by annual income is also clearer for the median first difference measure, with the effect of monthly volatility peaking in the \$50,000-\$75,000 range before decreasing steadily toward zero.

[35]The median absolute first difference measure compares earnings in consecutive months and is thus minimally affected by annual growth. For the cashout regressions, I simply take the median absolute first difference across all of the participant's observable months (rather than taking the median within each year and averaging across years).

Figure 1: The effect of income volatility on hardship-withdrawal probability



Note: Observations are participant-years and the monthly income volatility measures are as defined in Section 3.4. Participant-years are classified as hourly if at least 45% of the participant's paychecks in the relevant year change by at least $1 relative to the lagged paycheck value. The remaining participant-years where fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. The underlying regression specification in (10) includes annual income buckets (interacted with the volatility measures), a quadratic of age, a quadratic of plan tenure, plan fixed effects, and year fixed effects. The coefficient estimates in panels (c) and (d) are scaled by the standard deviation of the corresponding volatility measure. Annual income buckets on the horizontal axis are $5,000 wide and based on values in real 2023 dollars. Standard errors are clustered by plan.

Panel (c) of Figure 1 adopts the fuller specification with both the standard deviation (second moment) and the skewness (third moment) of log monthly earnings per paycheck included on the right-hand side. To put the standard deviation and skewness measures on equal footing, I scale the coefficients so that they each represent the effect of a one-standard-deviation increase in the corresponding measure.[36] The second-moment effects

[36]This requires an unfortunate composition of standard deviations. The dark-blue dots are scaled by the standard deviation of the standard deviation of log monthly earnings per paycheck; the light-blue dots are scaled by the standard deviation of the skewness of log monthly earnings per paycheck. I compute these

are slightly smaller than those in panel (b) because they now represent the effect of mean-zero volatility conditional on the symmetry of the monthly earnings history. The new third-moment effects are consistently negative, indicating that negative monthly earnings shocks (which decrease the skewness measure) have the strongest effect on hardship-withdrawal probability. When I restrict in panel (d) to the likely hourly group – and therefore consider only intensive-margin variation in monthly volatility among hourly workers – the results are largely the same.

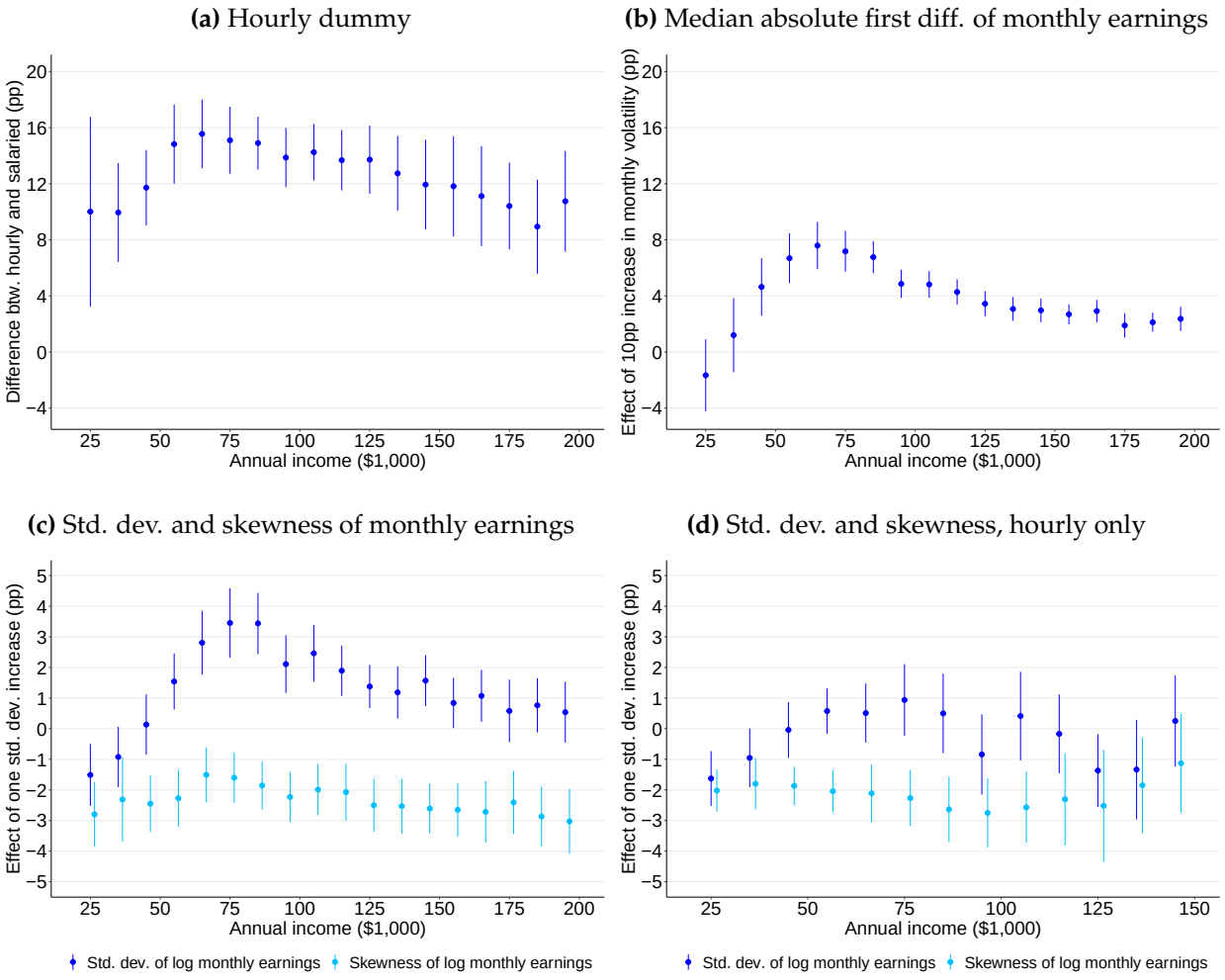
Figure 2 has the same structure but reports the results of the cashout regressions from (11). In the simpler specification with the hourly dummy, participants classified as hourly are consistently 10-15 percentage points more likely to cash out (echoing the cashout rates summarized in Table 6). The median absolute first difference coefficients in panel (b) again peak in the \$50,000-\$75,000 range, where a 10 percentage-point increase in typical monthly volatility (measured across the participant's entire employment spell) raises cashout probability by about 8 percentage points. Given the sample-wide average cashout rate of 34% among separating participants, this is a proportional increase of 24%.

Moving to panels (c) and (d) of Figure 2, the coefficients show that negative income shocks have the largest effect on cashout probability. Considering the full sample in panel (c) and focusing on the peak effect at the \$70,000-\$80,000 bucket, a symmetric perturbation that raised the second moment by one standard deviation but held fixed the third moment would increase cashout probability by about 3.5 percentage points. An asymmetric perturbation that raised the second moment by one standard deviation *and* lowered the third moment by one standard deviation would instead raise cashout probability by about 5 percentage points. When restricting to hourly participants (and therefore conditioning on the higher level of mean-zero volatility among this group), the second-moment effects shrink in magnitude and are at most marginally significant. Among hourly participants, the intensive-margin relationship between income volatility and cashout probability is thus primarily a third-moment effect driven by asymmetric negative shocks.

Taken together, the estimates in Figures 1 and 2 are consistent with the key model predictions from Section 2. For both in-service hardship withdrawals and post-separation cashouts, participants with more volatile incomes are more likely to take penalized early distributions from their 401(k) accounts. The hourly dummy variable and the median

standard deviations over all participant-years for panel (c) and over hourly participant-years for panel (d). The same logic applies to panels (c) and (d) of Figure 2.

Figure 2: The effect of income volatility on post-separation cashout probability



Note: Observations are participants and the sample is restricted to participants who separate from employment during the sample period. I further restrict to participants for whom cashouts are penalized (i.e., those who are younger than 55 at the end of the calendar year in which they separate). The monthly income volatility measures are as defined in Section 3.4 and are taken across all observable years for a given participant. Participants are classified as hourly if at least 45% of their observable paychecks change by at least $1 relative to the lagged paycheck value. The remaining participants for whom fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. The underlying regression specification in (11) includes buckets for average annual income during the employment spell (interacted with the volatility measures), a quadratic of age at separation, a quadratic of plan tenure at separation, plan fixed effects, and year fixed effects. The coefficient estimates in panels (c) and (d) are scaled by the standard deviation of the corresponding volatility measure. Annual income buckets on the horizontal axis are $10,000 wide and are based on income values converted to real 2023 dollars. Standard errors are clustered by plan.

absolute first difference measure capture the extensive-margin difference in mean-zero volatility between the hourly and salaried groups. When I allow for both second- and third-moment effects, negative shocks that decrease the skewness of monthly income have the strongest effect on withdrawal probability. Finally, the effect of income volatility on withdrawal probability is strongest for low- and middle-income participants.

I include additional empirical results in Appendix A. Figures A.7-A.8 show that the

main results hold when dropping the W-2 accuracy condition and considering the larger sample of complete calendar years. Figure A.9 uses an indicator for 401(k) loan origination as the dependent variable; the results are qualitatively similar to the hardship-withdrawal regressions and demonstrate that participants experiencing income volatility also use plan loans as a source of liquidity. Figure A.10 shows that the cashout results hold for the subset of participants whose entire employment spells are observable in the final sample. Figure A.12 uses as the dependent variable the share of the participant's final balance taken as a cash distribution. Because most cashouts are for the entire balance (see Table 5), the coefficient estimates are nearly identical to the baseline analysis. Finally, Figure A.13 shows regressions where the employee contribution rate is the dependent variable. In contrast to the strong withdrawal effects documented above, income volatility has muted effects on contribution behavior: contribution rates are about 0.25 percentage points lower for middle-income hourly participants than for similar salaried participants.

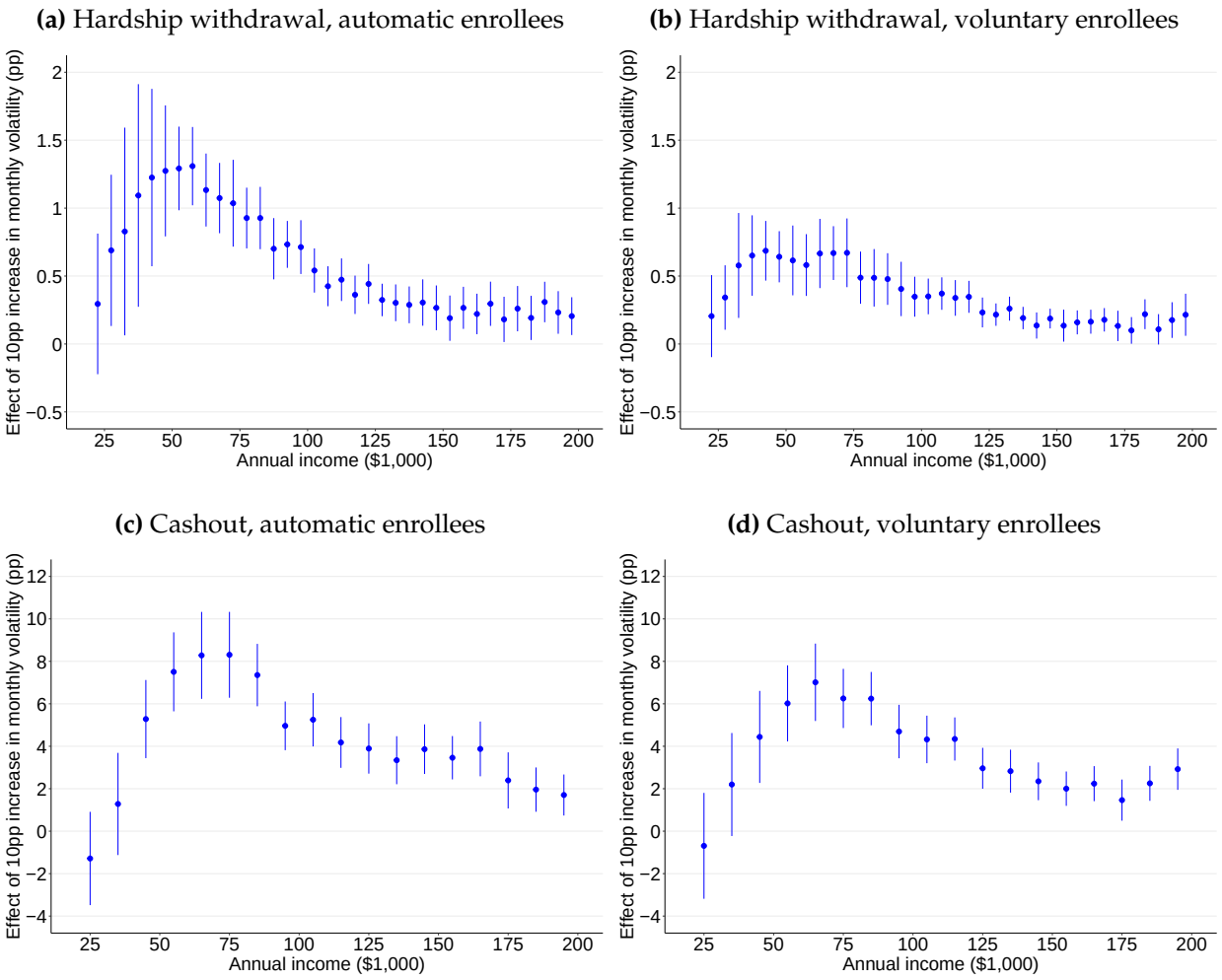
4.3 Heterogeneity by enrollment policy

The effect of income volatility on 401(k) leakage may differ between automatic and voluntary enrollees. Because Vanguard administers both automatic- and voluntary-enrollment plans, my sample contains both types of participants: about 40% of participants in the final sample were automatically enrolled (Table 5).[37] The set of automatic enrollees likely includes some workers who would not not have contributed to their employer's 401(k) plan under a voluntary enrollment scheme. If 401(k) contributions crowd out liquid saving among these automatic-enrollment compliers (Choukhmane and Palmer, 2024), we might expect income volatility to produce stronger withdrawal effects for automatic enrollees.

Figure 3 reproduces the baseline hardship-withdrawal and cashout analysis separately for automatic and voluntary enrollees. I focus on the quantitative median absolute first difference measure of volatility; similar results for the simple hourly dummy are in Appendix Figure A.14. With the annual hardship withdrawal rate as the dependent variable, automatic enrollees do show stronger withdrawal effects: a 10-percentage-point increase in monthly volatility raises hardship-withdrawal probability by about 1.25 percentage points for middle-income automatic enrollees, versus an effect size of about 0.6 percentage points for similar voluntary enrollees. The cashout results in panels (c)-(d) also show stronger

[37]The share of Vanguard-administered plans using automatic enrollment grew from 20% in 2008 to 59% in 2023. See Clark (2025).

Figure 3: The effect of monthly income volatility on withdrawal probability, automatic versus voluntary enrollees



Note: The monthly income volatility measure (interacted with the annual income buckets) is the median absolute first difference of monthly earnings per paycheck, as defined in Section 3.4. In panels (a)-(b), observations are participant-years and the annual income buckets are $5,000 wide (in real 2023 dollars). The underlying regression specification in (10) includes annual income buckets, a quadratic of age, a quadratic of plan tenure, plan fixed effects, and year fixed effects. In panels (c)-(d), observations are participants and the annual income buckets (based on average annual income during the employment spell) are $10,000 wide. The sample is restricted to participants who separate from employment during the sample period and are younger than 55 at the end of the calendar year in which they separate. The underlying regression specification in (11) includes buckets for average annual income during the employment spell, a quadratic of age at separation, a quadratic of plan tenure at separation, plan fixed effects, and year fixed effects. Panels (a) and (c) consider only participants who were automatically enrolled and panels (b) and (d) consider only those who were voluntarily enrolled. Standard errors are clustered by plan.

effects for automatic enrollees but the differences are much smaller in proportional terms, with effect sizes for automatic enrollees only about 1-2 percentage points higher. Appendix Figure A.15 explicitly plots the differences between the automatic- and voluntary-enrollment effects; the differences in hardship-withdrawal estimates are statistically significant for middle-income participants but none of the cashout differences are statistically

different from zero.

Segmenting the sample by enrollment policy thus yields stronger volatility effects among automatic enrollees, particularly when hardship-withdrawal probability is the dependent variable. But perhaps the relevant conclusion to draw from Figure 3 is that the estimated effects remain large and statistically significant among voluntary enrollees, and indeed the more important cashout estimates are statistically indistinguishable between the two groups. The relationship between monthly income volatility and early 401(k) withdrawals is not simply a story of automatic enrollment crowding out liquid saving among marginal participants: whatever the underlying mechanism, it is also present for workers who actively choose to participate in their 401(k) plans.

5 Within-person evidence from multiple employment spells

5.1 Within-person identification

The main empirical analysis in Section 4 establishes the relationship between monthly income volatility and early withdrawals, controlling for annual income, age, employment tenure, and plan fixed effects. The conclusion is that income volatility correlates with significantly higher cashout rates, even among workers who have the same annual earnings, are at the same point in the lifecycle, have worked at the same firm for the same period of time, and are subject to the same 401(k) plan rules. The remaining threat to identification is the possibility that unobservable worker-level determinants of cashout probability are correlated with monthly earnings volatility.

The source of potential omitted variable bias is not obvious. Possible identification threats include:

- *Low-attachment workers*. Paycheck volatility could be higher among part-time workers or second earners, and these less-attached workers may have higher withdrawal rates. But Figures 1-2 show that the effect of volatility on withdrawal probability persists across a wide range of annual income levels, including many middle- and upper-income brackets where we would expect job attachment to be high.

- *Correlation between income and expense shocks*. Earnings volatility could arise from worker-level circumstances (e.g., illness, family emergencies) that simultaneously cause hourly workers to miss shifts and create expenses that raise the liquidity value

of 401(k) withdrawals (e.g., medical bills). But while correlated expense shocks could explain intensive-margin effects among hourly workers with flexible shifts, they would have a harder time explaining the extensive-margin differences between hourly and non-shift salaried workers – unless hourly workers are somehow more likely to have expense shocks than salaried workers with the same annual income.

- *Correlation between income volatility and present bias*. It could simply be that hourly workers have higher withdrawal rates because they have higher levels of present bias. While prior work suggests present bias is higher among lower-income households (Lawrance, 1991; Paserman, 2008; Fang and Silverman, 2009; Meier and Sprenger, 2015), there is no existing evidence suggesting that present bias correlates with income *volatility*. Again, the identification threat would only arise if hourly workers had different time preferences than salaried workers with the same annual income.

To help address potential identification threats, I exploit the fact that a small share of workers in the main sample participate in multiple 401(k) plans during the sample period. These are cases in which a worker separates from a job at one Vanguard-administered plan and eventually moves to another job at a different Vanguard-administered plan. The sample of observable job switchers allows me to estimate cashout regressions with participant-level fixed effects, using differences in monthly income volatility between distinct employment spells to identify the coefficient of interest. Present bias – which evidence suggests is a temporally stable individual characteristic (Meier and Sprenger, 2015) – is the leading example of an unobservable time-invariant factor that would be absorbed by participant fixed effects.

Concretely, I construct the job-switcher sample by identifying the participants who meet all of three conditions. First, they must appear in the main analysis sample (bottom row of Table 3) in at least two distinct plans. Second, they must have separated from each plan-level employment spell, so that cashout decisions are observable. Finally, hire dates in the second and subsequent spells must occur after the separation date of the preceding spell, so that the spells are nonoverlapping.

Table 7 summarizes the subsample of participants who meet these three conditions. Because it is a relatively rare occurrence to begin and end employment spells at two different Vanguard-administered plans within a fixed time period (and meet all of the additional sample filtering conditions in Table 3 in each spell), the set of observable job switchers is relatively small: it contains 10,850 participants, or 0.32% of the 3.4 million

Table 7: Summary statistics for subsample with multiple employment spells

	All participants with multiple spells	Participants with hourly and salaried spells
Number of participants	10,850	2,164
Within-participant distribution of spells		
Two spells	98%	97%
More than two spells	2%	3%
Hourly spells precede salaried spells		54%
Median annual income		
First spells	80,918	74,824
Second spells	90,130	83,732
Hourly spells	60,723	72,200
Salaried spells	108,362	86,113
Within-participant cashout patterns		
Cashout in all spells	15%	13%
Cashout in no spells	59%	60%
Cashout in at least one but not all spells	26%	27%
In hourly spells only		15%
In salaried spells only		12%

Note: The subsample of participants considered in this table meet all of three conditions. First, they appear in the main analysis sample (bottom row of Table 3) in at least two distinct plans. Second, they have separated from each plan-level employment spell, so cashout decisions are observable. Finally, hire dates in the second and subsequent spells always occur after the separation date of the preceding spell, so the spells are nonoverlapping. Spells are classified as hourly if at least 45% of the participant's observable paychecks in that spell change by at least $1 relative to the lagged paycheck value. The remaining spells in which fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. The rightmost column summarizes the smaller subset of participants for whom at least one spell is classified as hourly and at least one spell is classified as salaried. Annual income figures are in real 2023 terms.

distinct participants in the main sample. The vast majority of participants are observable in exactly two spells. For a subset of 2,164 participants, one spell is classified as hourly and one spell is classified as salaried (again using the 45% rule discussed in Section 3.2). This smaller subset is of particular interest, since it contains within-person variation in the hourly-salaried classification that matters most for cashout probability in Section 4.

5.2 Results

Table 8 presents the results of the job-switcher analysis. I estimate regressions where observations are employment spells and the dependent variable is an indicator equal to one if the participant cashed out after separating. I control for participant fixed effects, year-of-separation fixed effects, the log of annual income (averaged across the employment spell), and age at separation. As the independent variable of interest, I consider the hourly dummy (again, equal to one if at least 45% of paychecks during the spell change by at least $1) and the median absolute first difference of monthly earnings (see again Section 3.4).

Table 8: Regression analysis of subsample with multiple employment spells

	Dependent variable: cashout probability					
	All participants with multiple spells		Participants with hourly and salaried spells			
	(1)	(2)	(3)	(4)	(5)	(6)
Hourly dummy	0.029		0.032	0.076		
	(0.016)		(0.016)	(0.037)		
* **1**{Hourly spell first}				-0.086		
				(0.063)		
Median abs. first diff. of monthly earnings		0.033			0.212	0.256
		(0.060)			(0.098)	(0.143)
* **1**{Hourly spell first}						-0.102
						(0.210)
Controls						
Participant FE	X	X	X	X	X	X
Year-of-separation FE	X	X	X	X	X	X
Annual income	X	X	X	X	X	X
Age at separation	X	X	X	X	X	X
Observations (spells)	21,941	21,941	4,396	4,396	4,396	4,396
Participants	10,850	10,850	2,164	2,164	2,164	2,164

Note: The subsample of participants considered in this table meet all of three conditions. First, they appear in the main analysis sample (bottom row of Table 3) in at least two distinct plans. Second, they have separated from each plan-level employment spell, so cashout decisions are observable. Finally, hire dates in the second and subsequent spells always occur after the separation date of the preceding spell, so the spells are nonoverlapping. Spells are classified as hourly if at least 45% of the participant's observable paychecks in that spell change by at least $1 relative to the lagged paycheck value. The remaining spells in which fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. Observations are employment spells and the dependent variable is an indicator equal to one if the participant took a cash distribution after separating from the employment spell. The regression specification controls for participant fixed effects, year-of-separation fixed effects, the log of average annual income during the employment spell, and age at separation. See Section 3.4 for a discussion of the median absolute first difference measure. Standard errors are clustered by participant.

Focusing on the subset with both hourly and salaried spells, the positive relationship between income volatility and cashout probability persists even when using more restrictive within-person variation. The hourly dummy in column (3) implies that cashout rates are 3.2 percentage points higher in hourly spells; the median first difference coefficient in column (5) implies that a 10-percentage-point increase in monthly income volatility raises cashout rates by 2.1 percentage points. In columns (4) and (6), I include an additional interaction term allowing the coefficient to depend on whether the hourly spell or salaried spell occurs first. Interestingly, the coefficients are driven primarily by cases where the salaried spell precedes the hourly spell (though adding the interaction term to the already small sample size makes the estimates statistically imprecise). This pattern can be rationalized by the model: workers who move from an hourly to a salaried job anticipate a lower need for

future liquidity as their income becomes less volatile and therefore have a weaker cashout incentive (see again the discussion in Section 2.3).

Using an alternative within-person identification strategy, the effect of monthly income volatility on cashout probability remains. The sample is much smaller than the main analysis in Section 4, and I no longer have the statistical power to estimate heterogeneous effects across the annual income distribution. Even so, the estimates in Table 8 help rule out the possibility that the main estimates are driven by a correlation between income volatility and unobservable characteristics like time preferences.

6 Survey evidence on liquid savings

6.1 Survey sample

Theoretically, the link between income volatility and 401(k) leakage runs through liquid savings: when workers do not have sufficient liquid wealth to smooth income shocks, they resort to penalized retirement withdrawals. If the model from Section 2 explains the empirical results, then it should be the case that some 401(k) participants lack liquid savings, and those with low liquidity should be more likely to take penalized withdrawals. Because liquid assets are not observable in 401(k) recorkeeping data, I cannot test these predictions systematically using the main analysis sample.

To evaluate participants' liquidity buffers, I rely instead on survey evidence. The limitations of such an approach should be made clear. First, survey responses may be less reliable than administrative records. Second, surveys yield smaller sample sizes than the main empirical analysis. Finally, survey responses may be nonrandom. Even if survey invitations are randomized, those who choose to respond may be observably or unobservably different than non-respondents.

With these limitations in mind, I leverage a large survey of Vanguard clients, conducted with randomized email invitations sent in July 2024. The survey posed a broad array of questions about household balance sheets and financial well-being, including questions specifically gauging clients' emergency savings. Invitations were sent to both retail brokerage clients and participants in Vanguard-administered 401(k) plans; I analyze only the subset of 401(k) participants and only the questions concerning emergency savings. Costa et al. (2025) provide a detailed discussion of the survey design and a more extensive analysis of the complete set of survey questions.

Mirroring the sample-selection criteria from Section 3.3, I consider the set of survey respondents who made employee contributions to their 401(k) accounts in all 12 months of 2024.[38] This yields a sample size of 4,041 respondents, whose survey responses I analyze jointly with 2024 information from the administrative recordkeeping data.[39]

6.2 Results

The survey's primary measure of emergency savings is a question that asked, "How confident are you that you could find the money to pay for an emergency that costs about $2,000?" The answer choices were "entirely confident," "somewhat confident," and "not at all confident." Participants could also choose to leave their answer blank (the sample of 4,041 respondents includes those who answered *any* question in the broader survey). The question wording is adapted from a leading industry survey on household financial well-being (FINRA Investor Education Foundation, 2024) and gauges participants' general ability to meet unexpected expenses. If some participants based their answer on sources of liquidity other than cash or bank accounts (e.g., short-term loans or family help), then the responses conservatively overstate participants' own liquid savings. Appendix Table A.7 shows similar results for a secondary question that had a lower response rate but asked directly how many months' worth of expenses participants held as emergency savings.

Table 9 summarizes responses to the $2,000 question and linked information from the administrative data. Only 31% of participants reported being "entirely confident" in their ability to meet a $2,000 emergency expense; 14% chose "somewhat confident" and another 14% chose "not all confident." A plurality of 42% left their answer to this question blank. Because the group with blank responses is dissimilar to the "entirely confident" group (in terms of income, hourly share, and withdrawal rates), it appears that many participants with low liquidity preferred not to state this explicitly. The share who are less than fully confident in their ability to access $2,000 is thus at least 28%, but observable characteristics of the missing-response group suggest that 28% is a conservative lower bound.[40]

[38] Survey invitations were restricted to the set of plans with W-2 income data, so the plan population for the survey is the same as the plan population I use to construct the main sample in Section 3. However, W-2 income data are not yet available for 2024, so I cannot impose the W-2 accuracy condition as I do for the main analysis sample (which ends in 2023).

[39] The overall response rate for the survey was 1.66%.

[40] This result is not out of line with nationally representative surveys: for example, the Federal Reserve finds that 37% of American adults do not have enough cash on hand to cover even a $400 emergency expense (Federal Reserve Board of Governors, 2025).

Table 9: Participant demographics and behavior by survey response

		Confidence in meeting $2,000 emergency expense			
	Overall	Entirely confident	Somewhat confident	Not at all confident	Did not answer
Number of survey respondents	4,041	1,233 (30.5%)	569 (14.0%)	554 (13.7%)	1,685 (41.7%)
Annual income in 2024					
10th	41,957	53,345	42,997	37,310	40,630
50th	81,609	103,807	77,741	60,332	76,377
90th	156,302	187,206	138,745	108,992	150,023
Median age	45	46	46	43	45
Median tenure	6	8	6	5	6
Likely hourly	62.6%	42.7%	67.8%	80.5%	69.4%
401(k) behavior in 2024					
Median contribution rate	8.0%	9.9%	7.9%	6.6%	7.7%
Contributed above match cap	67.4%	75.2%	68.5%	57.6%	64.6%
Plan loan origination	21.1%	9.2%	26.5%	35.2%	23.3%
Hardship withdrawal	15.6%	3.6%	15.6%	33.2%	18.5%
Separated in 2025	8.1%	7.9%	8.6%	9.9%	7.4%
Share with cashout (cond. separation)	30.1%	11.3%	40.8%	45.5%	33.6%

Note: This table considers 4,041 participants in Vanguard-administered 401(k) plans who responded to a survey in July 2024 and made employee contributions to their plan in all 12 months in 2024. The particular survey question analyzed in this table (which participants could leave blank) asked, "How confident are you that you could find the money to pay for an emergency that costs about $2,000?" Participants are classified as hourly if at least 45% of their paychecks in 2024 change by at least $1 relative to the lagged paycheck value. The remaining participants for whom fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. Aside from the column groupings, all information in this table comes directly from the administrative recordkeeping data (i.e., I do not rely on self-reporting in the survey). Separations are observable through August 2025.

The low liquidity levels documented in Table 9 are consequential, as many low-liquidity participants have volatile incomes. Of those who chose "somewhat confident" or "not at all confident," 74% are classified as likely hourly based on their 2024 paycheck history. Indeed, Appendix Table A.8 shows that hourly workers (conditional on annual income, age, tenure, and plan fixed effects) were about 16 percentage points less likely to choose "entirely confident." Another important fact about low-liquidity participants is that their marginal 401(k) contributions are often unmatched. Among the "somewhat confident" and "not at all confident" groups, 63% of participants had a contribution rate in 2024 that was strictly higher than their employer's match cap.

Hourly workers could have lower liquidity because they have different preferences (though the within-person evidence in Section 5 provides a counterpoint to the preference hypothesis) or because paycheck volatility itself makes it difficult to accumulate and maintain a liquidity buffer. Either way, liquidity appears to matter for withdrawal behavior.

The 2024 hardship withdrawal rate rises from 3.6% in the "entirely confident" group to 33.2% in the "not at all confident" group. Among those who separated in 2025, 11.3% of the "entirely confident" group cashed out, versus 45.5% of the "not at all confident" group. Appendix Table A.9 shows that self-reported liquidity continues to predict withdrawal rates after controlling for observable characteristics and plan fixed effects.

Given the share of blank answers and the fact that survey respondents had higher baseline withdrawal rates in 2024 than the broader Vanguard recordkeeping population,[41] the exact magnitudes here should be interpreted with caution. Even so, the survey evidence seems sufficient to establish a few relevant findings. First, many 401(k) participants – particularly those with volatile incomes – lack liquid savings. Second, many low-liquidity workers nonetheless contribute regularly to 401(k) accounts (recall that all survey respondents analyzed here contributed in all 12 months in 2024) and are often above their employer's match cap. Finally, lower liquidity predicts higher 401(k) withdrawal rates.

7 Discussion and policy implications

Monthly income volatility – identified from both within-plan and within-person variation – raises the probability of penalized early withdrawals from 401(k) accounts. At the same time, survey responses indicate that many 401(k) participants lack liquid savings, even though they contribute regularly to illiquid retirement accounts. What are the implications of these findings for policy and plan design?

It is possible that these results reflect fully optimal behavior, as in Kaplan and Violante (2014). In this case (corresponding to $\beta = 1$ in the model), participants with volatile incomes understand that they may need to draw on savings to smooth income shocks, and intentionally choose to hold those savings in their 401(k) accounts instead of liquid accounts. The choice to use 401(k) funds as insurance against income shocks could be optimal if the opportunity cost of liquid savings (foregone employer matches, stock market returns, and tax advantages) outweighs the 10% early-withdrawal penalty. Participants might shift their savings toward liquid accounts if the early-withdrawal penalty were higher, but policy interventions would not be correcting any clear behavioral bias.

[41] The overall hardship-withdrawal rate in Table 9 is 15.6%, versus 4.8% for all Vanguard participants in 2024 (Clark, 2025). Participants who are more engaged with their plan tend to have higher survey response rates. It is possible that participants who recently took a withdrawal (and therefore interacted with Vanguard via website or phone) became more likely to respond to an emailed survey invitation. The survey sample is representative in terms of annual income, age, tenure, and hourly share (compare Table 9 to Tables 4 and 5).

Given that 401(k) funds are difficult to access (hardship withdrawals require documentation of financial need and cashouts can only occur after job separations), many low-liquidity participants contribute past their employer's match cap (see again Section 6), and tax-advantaged stock market returns can be earned in more liquid vehicles like Roth IRAs, the fully optimal hypothesis seems like an incomplete explanation of the empirical facts. And because the main empirical results hold for voluntary enrollees (see again Section 4.3), the potential crowd-out effect of automatic enrollment on liquid savings is not a sufficient explanation, either. This leaves us with a case where behavioral biases lead at least some participants to hold suboptimally low levels of liquid savings. The model stylistically represents behavioral biases as $\beta < 1$, but in reality many things besides pure myopia can depress liquid savings: inexperience with budgeting or financial planning, incomplete knowledge of 401(k) withdrawal rules, or difficulty arranging automated payroll deferrals (unlike the automatic payroll deferrals that always fund 401(k) accounts).

A promising intervention is for employers to offer workplace access to – or automatically enroll employees in – short-term savings accounts. Small but automatically reucrring payroll deferrals could be used to build liquidity buffers, and could then be redirected to 401(k) accounts once the liquid account reaches a target balance. This was precisely the motivation behind recent legislation allowing 401(k) plan sponsors to establish pension-linked emergency savings accounts (PLESAs), which are funded with Roth payroll deferrals and capped at a maximum balance of $2,500.[42] Because PLESAs have proven difficult to administer in practice, some employers have chosen instead to offer short-term savings options as distinct accounts outside of the 401(k) plan.[43] Beshears et al. (2020) discuss the practical considerations involved in designing employer-sponsored short-term savings accounts, and recent evidence from U.K. firms (Berk et al., 2023) indicates that automatic enrollment into these accounts significantly increases liquid wealth accumulation.

The policy discussion around retirement savings leakage often focuses on early withdrawal penalties and thus introduces a difficult tradeoff: higher penalties mitigate present bias but impose additional costs on liquidity-constrained households (Beshears et al., 2025; Amador et al., 2006; Beshears et al., 2015; Laibson et al., 1998). If the link between behavioral biases and 401(k) leakage operates at least in part through depressed liquid savings, then policies facilitating liquid wealth formation might help make this tradeoff less severe.

[42]The SECURE 2.0 legislation also allows plan sponsors to offer penalty-free emergency withdrawals of up to $1,000 per year. See Beshears et al. (2024b) for a detailed discussion of these new plan provisions.

[43]See, e.g., New York Times (2024).

8 Conclusion and opportunities for future work

Using paycheck-level income data for a large sample of 401(k) participants, I demonstrate a systematic relationship between workers' monthly income volatility and their propensity to take penalized early withdrawals. The empirical results are consistent with a simple model in which workers use (and present-biased workers *overuse*) retirement assets as insurance against income shocks. Survey evidence confirms that many 401(k) participants with volatile incomes hold low levels of liquid savings, an issue that might be addressed with employer-sponsored interventions facilitating liquid wealth accumulation.

There are several promising opportunities for future work. First, linking 401(k) data with other household financial information would permit a fuller analysis of withdrawal decisions. Data on liquid accounts would improve upon the point-in-time survey evidence here and allow a complete dynamic analysis of participants' short-term liquidity management. Similarly, linking 401(k) data with credit reports would allow us to determine whether income shocks lead to accumulation of costly credit card debt and whether some cashouts are used to pay off debt. Finally, policy experiments can test the potential benefit of short-term savings interventions. Prior research has shown that automatic enrollment into 401(k) accounts raises 401(k) saving, and that automatic enrollment into short-term savings accounts raises short-term saving. Future work should study the interaction effect and determine whether automatic enrollment into short-term savings accounts reduces 401(k) leakage.

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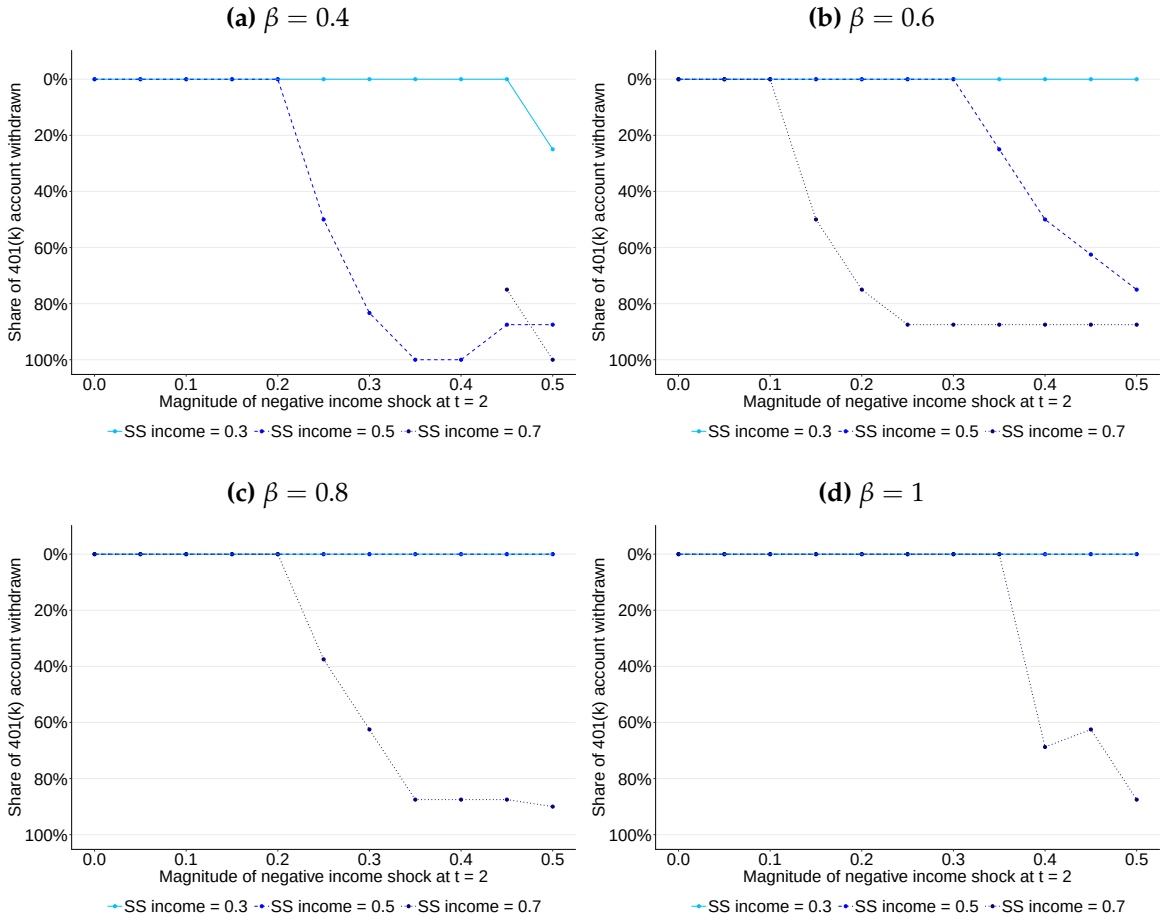
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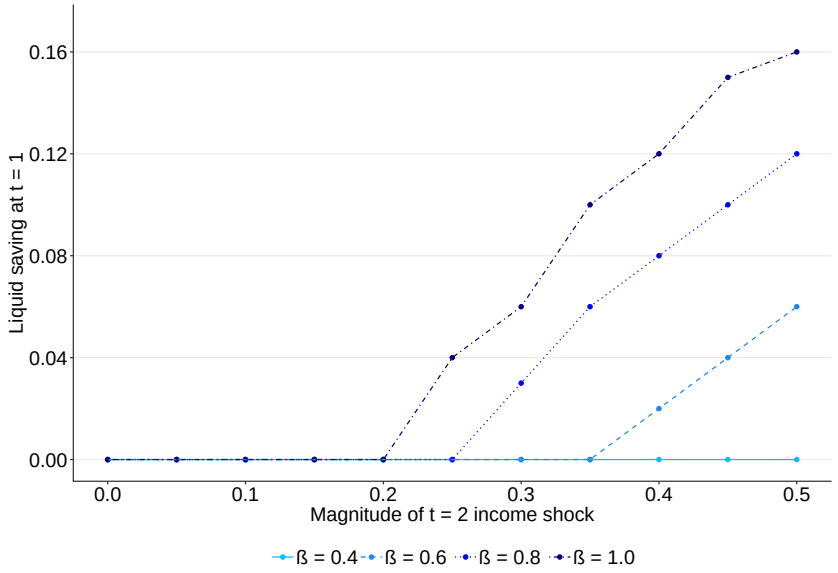
Appendix A Additional tables and figures (for online publication)

Figure A.1: Cashout choices at $t = 2$ in the three-period case, conditional on the negative income shock



Note: This figure shows cashout choices made by the worker at $t = 2$ when the negative income shock $y_2 = 1 - \Delta$ realizes. The x-axis shows the magnitude of the Δ shock and the y-axis shows the share of the 401(k) account balance a_2^I that the worker chooses to take as a penalized withdrawal. If the worker does not take a withdrawal and $s_2^I \geq 0$, the plotted value is 0. Missing values indicate cases where the worker does not contribute to the 401(k) account at $t = 1$ and $a_2^I = 0$. Fixed model parameters are as follows: the coefficient of relative risk aversion is 2, the exponential discount factor is $\delta = 0.97$, the 401(k) return is $r = 0.08$, the early withdrawal penalty is $\tau = 0.1$, the match rate is $m = 0.5$, the match cap is $\bar{m} = 0.06$, and first-period income is $y_1 = 1$. The worker's saving choice set is discretized with steps of 0.01 between grid points. See Sections 2.1-2.2 for a description of the model and the simplified three-period case.

Figure A.2: Liquid saving at $t = 1$ in the three-period case



Note: This figure shows liquid saving choices made by the worker at $t = 1$. The x-axis shows the magnitude of the symmetric $t = 2$ income shock Δ, with $y_2 = 1 + \Delta$ and $y_2 = 1 - \Delta$ occurring with equal probability. The y-axis shows the worker's contribution to the liquid account at $t = 1$. Fixed model parameters are as follows: the coefficient of relatve risk aversion is 2, the exponential discount factor is $\delta = 0.97$, the 401(k) return is $r = 0.08$, the early withdrawal penalty is $\tau = 0.1$, the match rate is $m = 0.5$, the match cap is $\bar{m} = 0.06$, first-period income is $y_1 = 1$, and Social Security income is $y_R = 0.5$. The worker's saving choice set is discretized with steps of 0.01 between grid points. See Sections 2.1-2.2 for a description of the model and the simplified three-period case.

Table A.1: Checking the hourly/salaried classification using explicit plan names

	Participant-years	
Type of plan name	Number	Share classified as hourly
Explicit hourly	121,502	99.5%
Explicit salaried	23,860	21.4%
Rest	10,428,598	66.6%

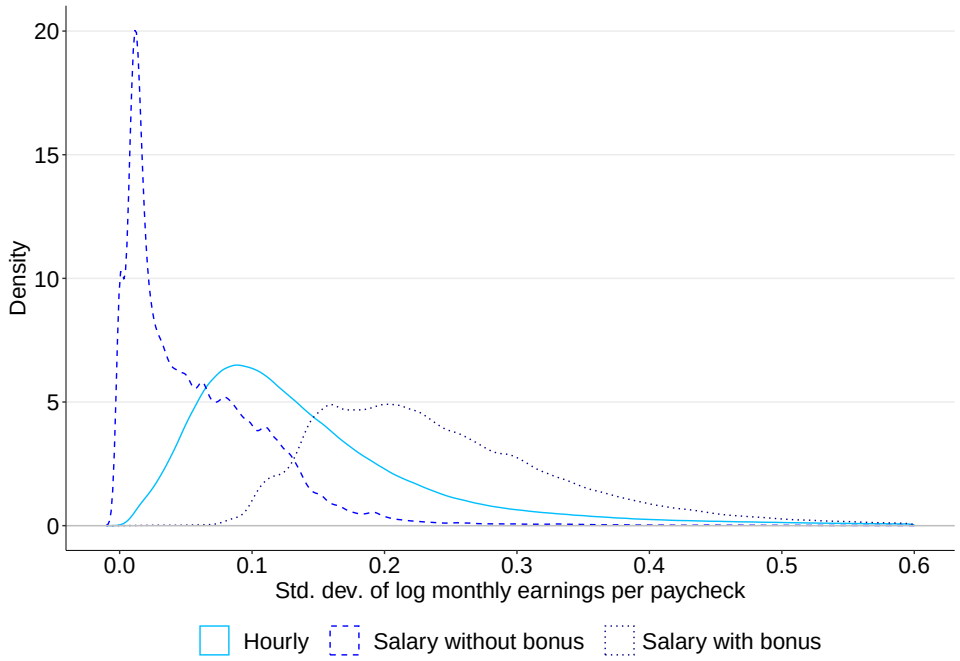
Note: This table considers the final sample of participant-years (bottom row of Table 3). Plans are classified as having an explicit hourly name if their official legal name contains "hourly" or "non-salaried." Plans are classified as having an explicit salaried name if their official legal name contains "salaried" and does not contain "non-salaried." Participant-years are classified as hourly if at least 45% of the participant's paychecks in the relevant year change by at least $1 relative to the lagged paycheck value. The remaining participant-years where fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule.

Table A.2: Checking the hourly/salaried classification using pay frequency

	Participant-years	
Pay frequency	Number	Share classified as hourly
Weekly ($\geq$ 50 paychecks)	2,856,260	90.9%
Change to/from weekly (29-49 paychecks)	526,509	92.5%
Biweekly/semi-monthly (24-28 paychecks)	6,894,280	55.3%
Monthly (12-13 paychecks)	122,019	32.5%
Rest	174,892	78.5%

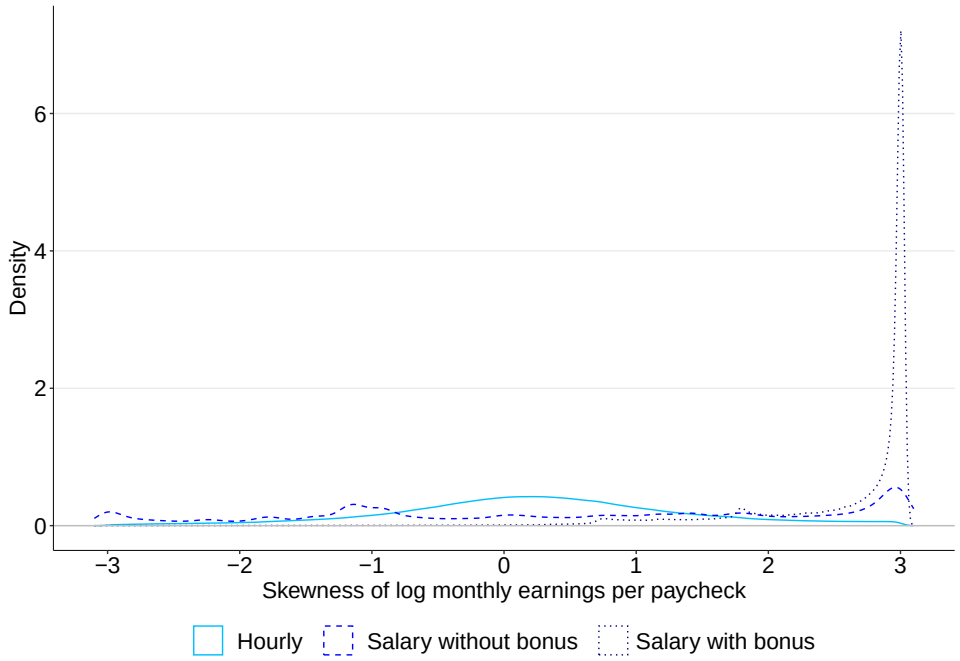
Note: This table considers the final sample of participant-years (bottom row of Table 3). The second row is primarily composed of cases in which the participant switched between a weekly pay schedule and some other pay schedule during the year. Participant-years are classified as hourly if at least 45% of the participant's paychecks in the relevant year change by at least $1 relative to the lagged paycheck value. The remaining participant-years where fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule.

Figure A.3: The standard deviation of monthly income, within participant-year



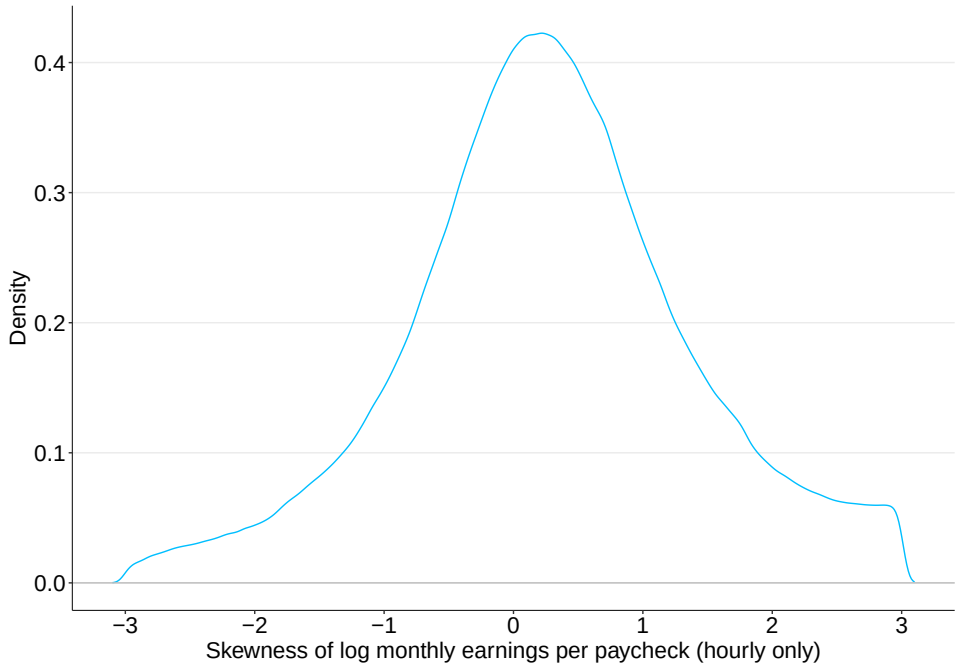
Note: Observations are participant-years; volatility measures computed over monthly values within a given participant-year. Participant-years are classified as hourly if at least 45% of the participant's paychecks in the relevant year change by at least $1 relative to the lagged paycheck value. The remaining participant-years where fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. Bonuses are identified using the algorithm discussed in Appendix B.

Figure A.4: The skewness of monthly income, within participant-year



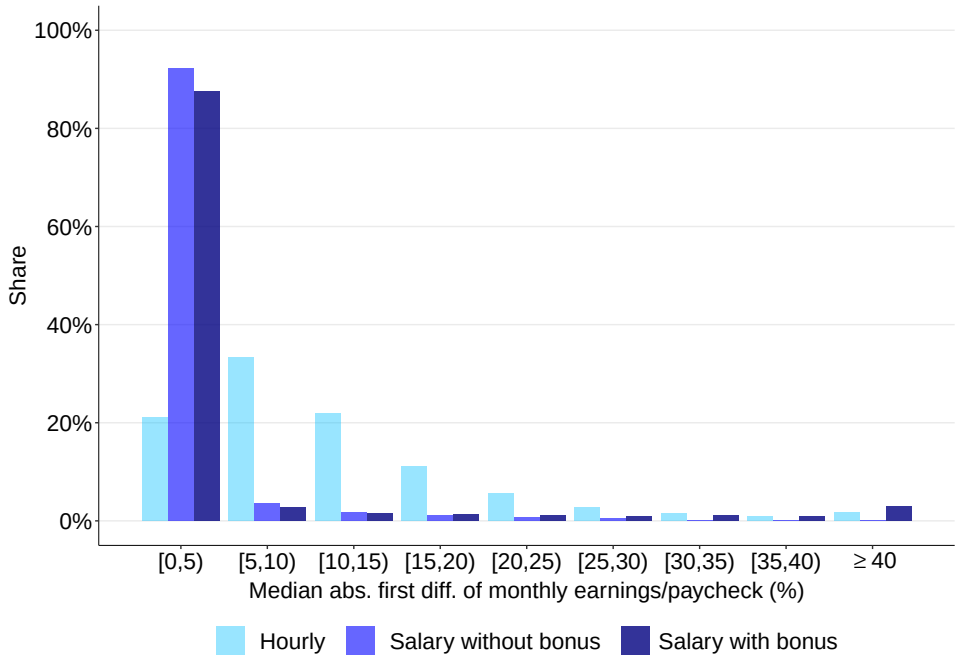
Note: Observations are participant-years; volatility measures computed over monthly values within a given participant-year. Participant-years are classified as hourly if at least 45% of the participant's paychecks in the relevant year change by at least $1 relative to the lagged paycheck value. The remaining participant-years where fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. Bonuses are identified using the algorithm discussed in Appendix B. If X is a vector of 12 values, 11 of which are identical and strictly less than the 12th, then the skewness of X is approximately 3. Salaried participants with bonuses (who often have 11 very similar monthly income values and an outlying 12th) therefore bunch around 3.

Figure A.5: The skewness of monthly income, within participant-year (hourly only)



Note: Observations are participant-years; volatility measures computed over monthly values within a given participant-year. Participant-years are classified as hourly if at least 45% of the participant's paychecks in the relevant year change by at least $1 relative to the lagged paycheck value. The remaining participant-years where fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule.

Figure A.6: Median absolute monthly earnings changes, within participant-year



Note: Observations are participant-years. Letting $e_{i,t}$ be monthly earnings per paycheck for participant i in month t, I compute $\frac{|e_{i,t}-e_{i,t-1}|}{e_{i,t-1}}$ for each month and take the median value within each participant-year. Participant-years are classified as hourly if at least 45% of the participant's paychecks in the relevant year change by at least \$1 relative to the lagged paycheck value. The remaining participant-years where fewer than 45% of paychecks change by at least \$1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. Bonuses are identified using the algorithm discussed in Appendix B.

Table A.3: Summary statistics by participant-year, removing the W-2 accuracy condition

	Likely hourly	Likely salaried
Participant-years	10,357,869	7,039,865
Annual income (2023 $)		
10th	33,515	65,354
50th	68,591	114,613
90th	134,558	205,506
Mean	78,842	130,426
Share of paychecks with $\geq$ $1 change		
10th	70.4%	3.9%
50th	97.3%	12.4%
90th	100.0%	32.2%
Mean	91.5%	15.1%
Median absolute first difference of monthly earnings/paycheck		
10th	3.1%	0.0%
50th	9.5%	0.0%
90th	23.1%	3.5%
Mean	12.0%	1.8%
Standard deviation of log monthly earnings/paycheck		
10th	0.06	0.01
50th	0.13	0.08
90th	0.31	0.30
Mean	0.17	0.12
Skewness of log monthly earnings/paycheck		
10th	-1.18	-2.01
50th	0.23	1.14
90th	1.75	3.00
Mean	0.25	0.78
Share negative	40.3%	37.3%
Annual contribution rate		
10th	2.8%	3.3%
50th	6.0%	7.0%
90th	13.1%	14.8%
Mean	7.3%	8.4%
Share with plan loan origination	13.3%	8.5%
Share with hardship withdrawal	2.5%	0.8%
Hardship withdrawal amount (cond. nonzero)		
Mean	7,743	12,581
Median	3,703	5,702

Note: This table shows the same summary statistics as Table 4, but considers all participant-years in which employee contributions occur in January, December, and at least 9 distinct months. The baseline sample summarized in Table 4 is subject to an additional filter requiring the inferred annual income amount to be within 3% of the official W-2 value. See Table 3 for an accounting of these filtering steps. Participant-years are classified as "likely hourly" if at least 45% of the participant's paychecks in the relevant year change by at least $1 relative to the lagged paycheck value. The remaining participant-years where fewer than 45% of paychecks change by at least $1 are classified as "likely salaried." See Section 3.2 for a discussion of this classification rule. Monthly income figures are normalized by the number of paychecks to eliminate mechanical variation (e.g., two- versus three-paycheck months). Annual income figures are given in real 2023 terms.

Table A.4: Summary statistics by participant-year, for those dropped by the W-2 accuracy condition

	Likely hourly	Likely salaried
Participant-years	3,287,962	3,535,812
Annual income (2023 $)		
10th	34,244	67,462
50th	72,376	122,157
90th	154,653	225,604
Mean	87,877	142,820
Share of paychecks with $\geq$ $1 change		
10th	65.1%	3.8%
50th	96.5%	11.7%
90th	100.0%	30.6%
Mean	90.1%	14.2%
Median absolute first difference of monthly earnings/paycheck		
10th	2.9%	0.0%
50th	10.2%	0.0%
90th	26.4%	2.4%
Mean	13.1%	1.5%
Standard deviation of log monthly earnings/paycheck		
10th	0.06	0.01
50th	0.15	0.06
90th	0.38	0.32
Mean	0.20	0.12
Skewness of log monthly earnings/paycheck		
10th	-1.37	-2.35
50th	0.22	0.14
90th	1.83	2.98
Mean	0.22	0.31
Share negative	41.8%	47.9%
Annual contribution rate		
10th	2.9%	3.4%
50th	6.1%	7.3%
90th	14.1%	15.3%
Mean	7.6%	8.8%
Share with plan loan origination	13.8%	8.2%
Share with hardship withdrawal	2.6%	0.9%
Hardship withdrawal amount (cond. nonzero)		
Mean	7,966	13,363
Median	3,702	5,980

Note: This table shows the same summary statistics as Table 4, but considers only participant-years in which: i) employee contributions occur in January, December, and at least 9 distinct months, but ii) the inferred annual income amount is not within 3% of the official W-2 value. These participant-years are dropped from the baseline sample by the final filtering step in Table 3. Participant-years are classified as "likely hourly" if at least 45% of the participant's paychecks in the relevant year change by at least $1 relative to the lagged paycheck value. The remaining participant-years where fewer than 45% of paychecks change by at least $1 are classified as "likely salaried." See Section 3.2 for a discussion of this classification rule. Monthly income figures are normalized by the number of paychecks to eliminate mechanical variation (e.g., two- versus three-paycheck months). Annual income figures are given in real 2023 terms.

Table A.5: Summary statistics by distinct participant, removing the W-2 accuracy condition

	Likely hourly	Likely salaried
Distinct participants	2,800,909	1,949,385
Share auto-enrolled	46.0%	36.4%
Share separated	53.0%	53.7%
With complete spell observed	23.9%	19.1%
Age at separation		
10th	27	29
50th	44	46
90th	64	64
Tenure at separation		
10th	1	1
50th	5	6
90th	21	23
Share with cashout (cond. separation)	41.5%	19.9%
Cashout amount (cond. nonzero)		
Mean	12,179	16,353
Median	3,597	4,935
Cashout share of balance (cond. nonzero)		
Mean	91.0%	74.5%
Median	100.0%	100.0%
Share of cashouts for entire balance	88.3%	70.5%

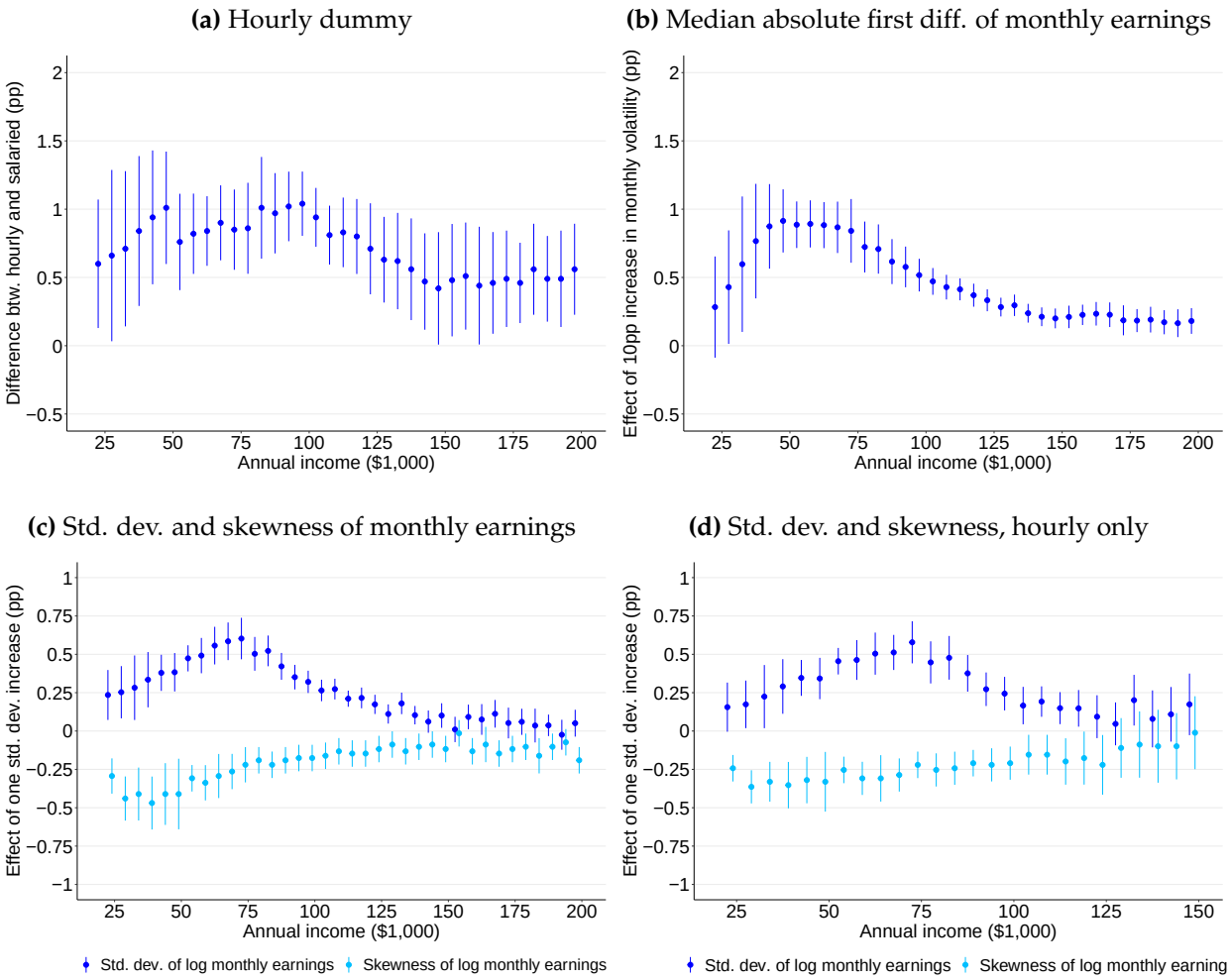
Note: This table shows the same summary statistics as Table 5, but considers all participants who have at least one year in which employee contributions occur in January, December, and at least 9 distinct months. The baseline sample summarized in Table 5 is subject to an additional filter requiring at least one year in which the inferred annual income amount is within 3% of the official W-2 value. See Table 3 for an accounting of these filtering steps. Participants are classified as "likely hourly" if at least 45% of their observable paychecks change by at least $1 relative to the lagged paycheck value. The remaining participants where fewer than 45% of paychecks change by at least $1 are classified as "likely salaried." See Section 3.2 for a discussion of this classification rule.

Table A.6: Cashout rates by annual income, removing the W-2 accuracy condition

	Cashout rate		Separated participants	
Annual income (2023 $)	Likely hourly	Likely salaried	Likely hourly	Likely salaried
$\leq$ 25,000	46.1%	38.3%	125,789	3,192
25,000 – 50,000	49.3%	38.4%	466,463	39,246
50,000 – 75,000	42.9%	28.4%	416,074	150,814
75,000 – 100,000	36.2%	22.6%	225,229	215,393
100,000 – 125,000	31.4%	19.2%	111,628	188,983
125,000 – 150,000	26.5%	16.3%	59,498	145,758
$>$ 150,000	21.6%	13.4%	81,175	303,242

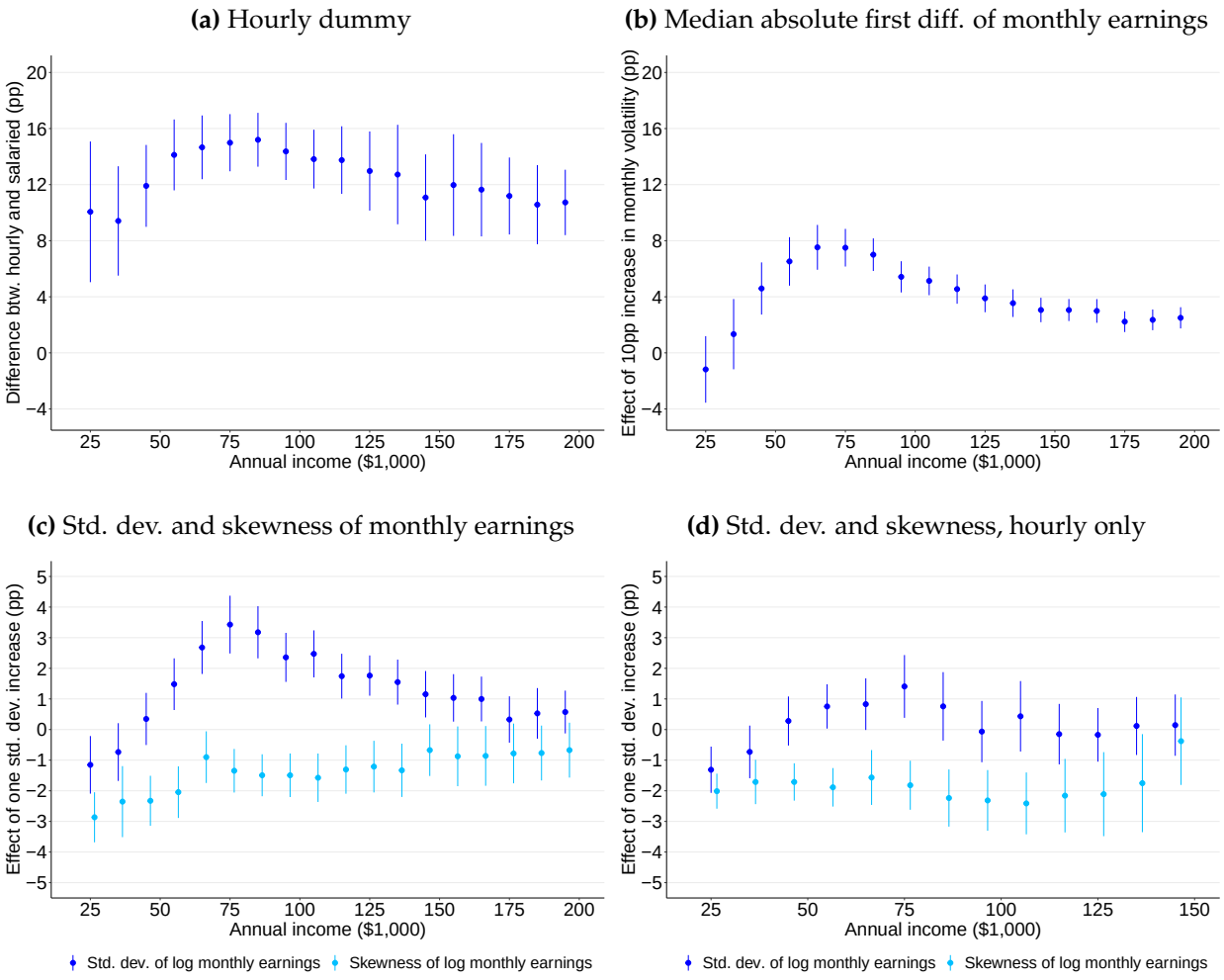
Note: This table shows the same summary statistics as Table 6, but considers all separated participants who have at least one year in which employee contributions occur in January, December, and at least 9 distinct months. The baseline sample summarized in Table 6 is subject to an additional filter requiring at least one year in which the inferred annual income amount is within 3% of the official W-2 value. See Table 3 for an accounting of these filtering steps. Participants are classified by their average annual income across the sample period. Participants are classified as "likely hourly" if at least 45% of their observable paychecks change by at least $1 relative to the lagged paycheck value. The remaining participants for whom fewer than 45% of paychecks change by at least $1 are classified as "likely salaried." See Section 3.2 for a discussion of this classification rule. Annual income figures are in real 2023 terms.

Figure A.7: The effect of income volatility on hardship-withdrawal probability, removing the W-2 accuracy condition



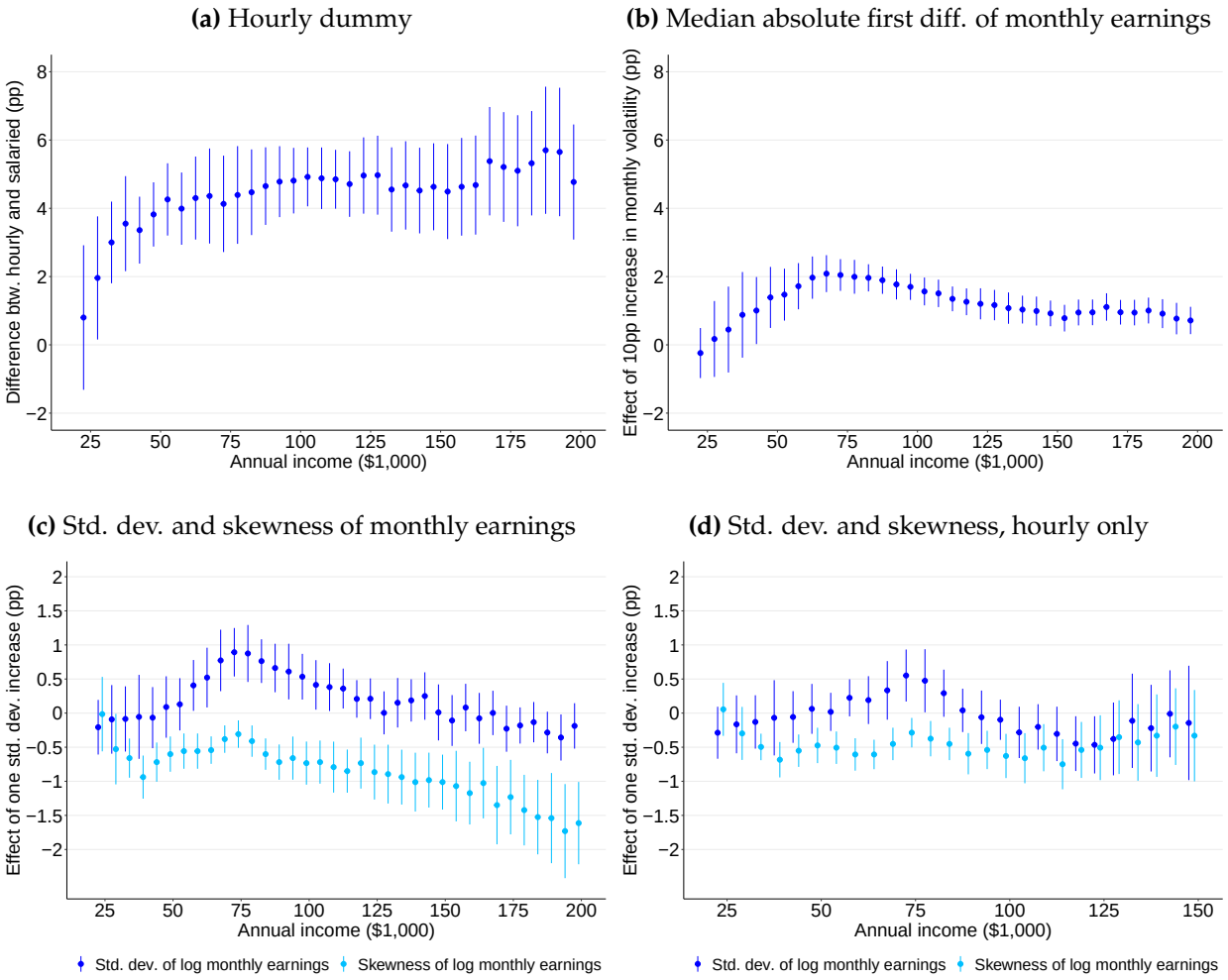
Note: Observations are participant-years and the monthly income volatility measures are as defined in Section 3.4. I consider all participant-years in which employee contributions occur in January, December, and at least 9 distinct months. The baseline analysis in Figure 1 imposes an additional filter requiring the inferred annual income amount to be within 3% of the official W-2 value. Participant-years are classified as hourly if at least 45% of the participant's paychecks in the relevant year change by at least $1 relative to the lagged paycheck value. The remaining participant-years where fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. The underlying regression specification in (10) includes annual income buckets (interacted with the volatility measures), a quadratic of age, a quadratic of plan tenure, plan fixed effects, and year fixed effects. The coefficient estimates in panels (c) and (d) are scaled by the standard deviation of the corresponding volatility measure. Annual income buckets on the horizontal axis are $5,000 wide and based on values in real 2023 dollars. Standard errors are clustered by plan.

Figure A.8: The effect of income volatility on post-separation cashout probability, removing the W-2 accuracy condition



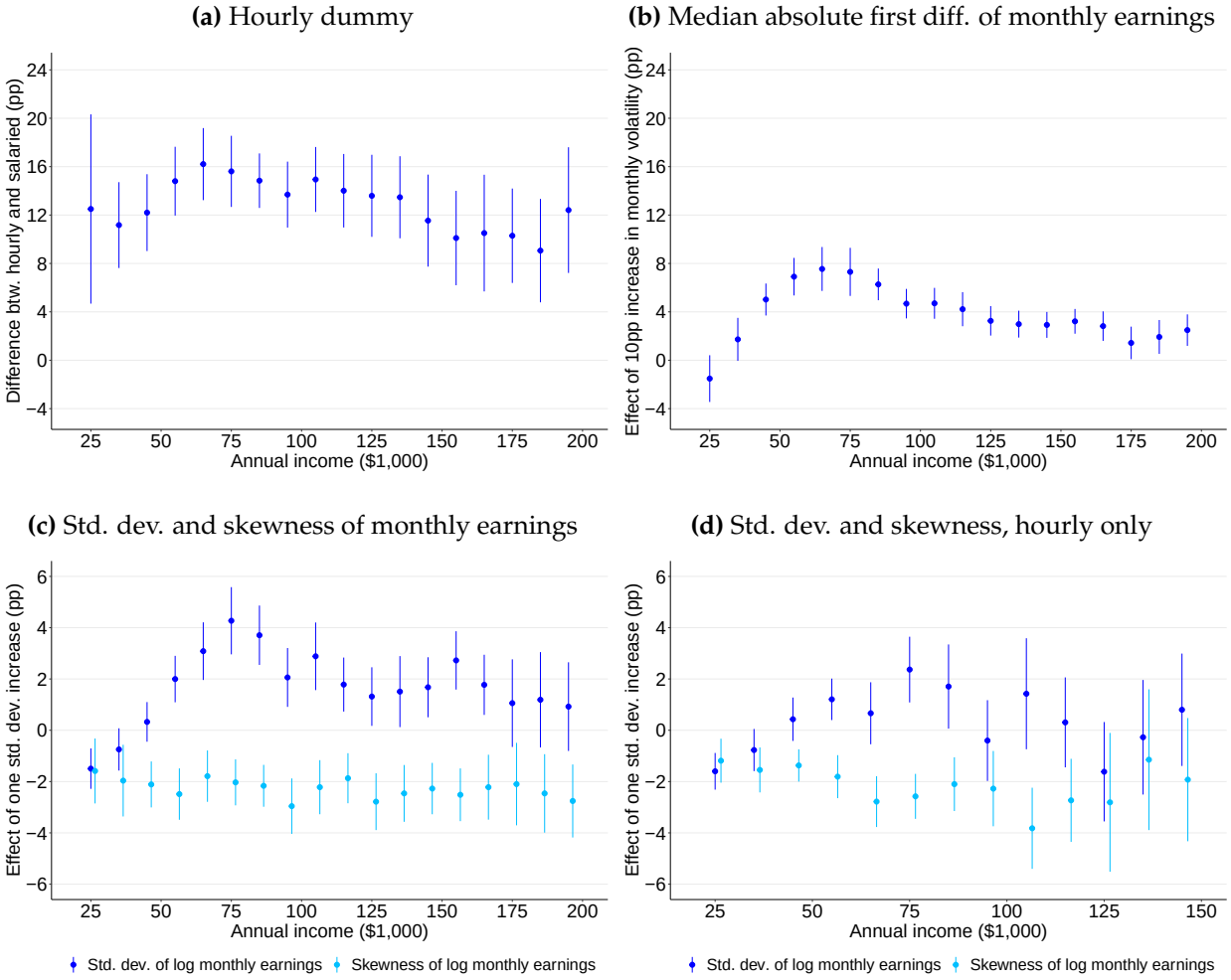
Note: Observations are participants and the sample is restricted to participants who separate from employment during the sample period. I consider all separated participants who are younger than 55 at separation and have at least one year in which employee contributions occur in January, December, and at least 9 distinct months. The baseline analysis in Figure 2 imposes an additional filter requiring at least one year in which the inferred annual income amount is within 3% of the official W-2 value. The monthly income volatility measures are as defined in Section 3.4 and are taken across all observable years for a given participant. Participants are classified as hourly if at least 45% of their observable paychecks change by at least $1 relative to the lagged paycheck value. The remaining participants for whom fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. The underlying regression specification in (11) includes buckets for average annual income during the employment spell (interacted with the volatility measures), a quadratic of age at separation, a quadratic of plan tenure at separation, plan fixed effects, and year fixed effects. The coefficient estimates in panels (c) and (d) are scaled by the standard deviation of the corresponding volatility measure. Annual income buckets on the horizontal axis are $10,000 wide and are based on income values converted to real 2023 dollars. Standard errors are clustered by plan.

Figure A.9: The effect of income volatility on loan-origination probability



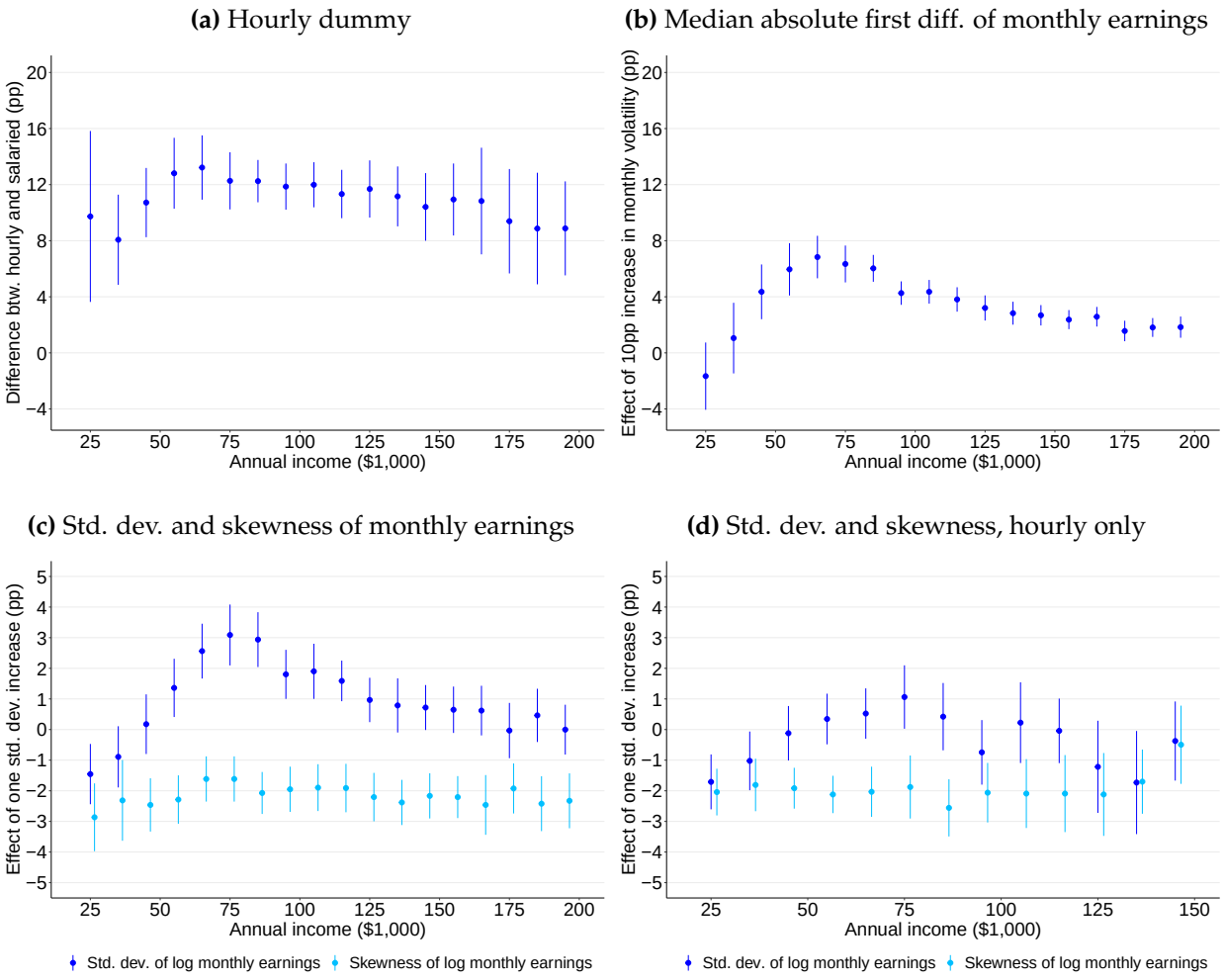
Note: Observations are participant-years and the monthly income volatility measures are as defined in Section 3.4. The dependent variable is an indicator equal to one if the participant originates a 401(k) plan loan in the relevant year. Plan loans allow participants to borrow against the vested balance in their 401(k) account, paying back the account by making additional payroll deferrals over a prespecified repayment period. If a participant fails to make the repayments on schedule, the remaining loan balance is considered a cash distribution on which the participant must pay income taxes and a 10% penalty (the penalty only applies to participants younger than 59.5). Participant-years are classified as hourly if at least 45% of the participant's paychecks in the relevant year change by at least $1 relative to the lagged paycheck value. The remaining participant-years where fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. The underlying regression specification in (10) includes annual income buckets (interacted with the volatility measures), a quadratic of age, a quadratic of plan tenure, plan fixed effects, and year fixed effects. The coefficient estimates in panels (c) and (d) are scaled by the standard deviation of the corresponding volatility measure. Annual income buckets on the horizontal axis are $5,000 wide and based on income values converted to real 2023 dollars. Standard errors are clustered by plan.

Figure A.10: The effect of income volatility on post-separation cashout probability, restricting to balanced panel of participant-years



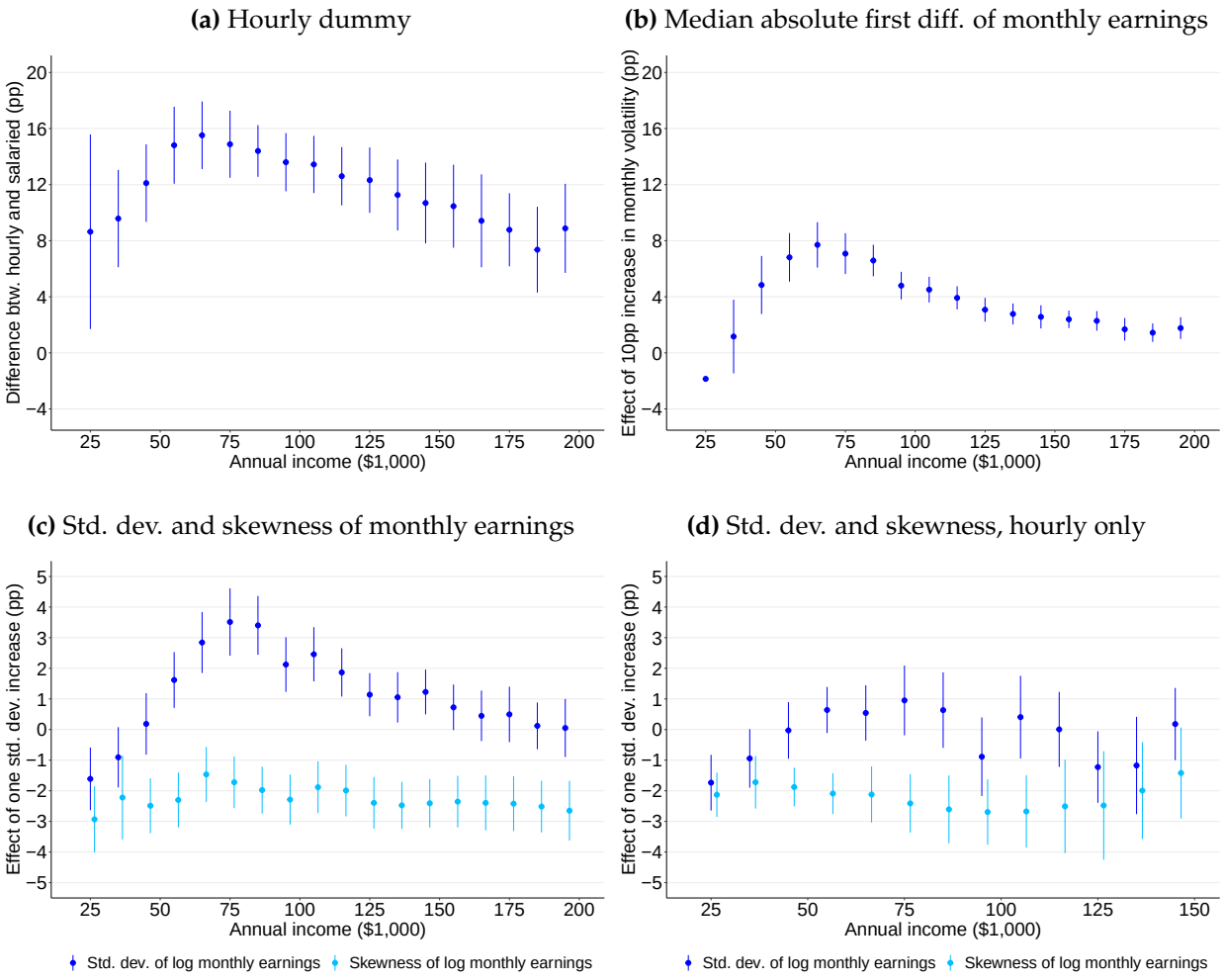
Note: Observations are participants and the sample is restricted to participants who separate from employment during the sample period. I further restrict to participants for whom cashouts are penalized (i.e., those who are younger than 55 at the end of the calendar year in which they separate). Unlike the baseline analysis in Figure 2, I also restrict to participants who appear in the final sample of participant-years for their entire employment spell (i.e., they begin and separate from employment within the sample period and their inferred annual income is within 3% of the corresponding W-2 value for each year of their employment spell). The monthly income volatility measures are as defined in Section 3.4 and are taken across all observable years for a given participant. Participants are classified as hourly if at least 45% of their observable paychecks change by at least $1 relative to the lagged paycheck value. The remaining participants for whom fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. The underlying regression specification in (11) includes buckets for average annual income during the employment spell (interacted with the volatility measures), a quadratic of age at separation, a quadratic of plan tenure at separation, plan fixed effects, and year fixed effects. The coefficient estimates in panels (c) and (d) are scaled by the standard deviation of the corresponding volatility measure. Annual income buckets on the horizontal axis are $10,000 wide and are based on income values converted to real 2023 dollars. Standard errors are clustered by plan.

Figure A.11: The effect of income volatility on post-separation cashout probability, including separators older than 55



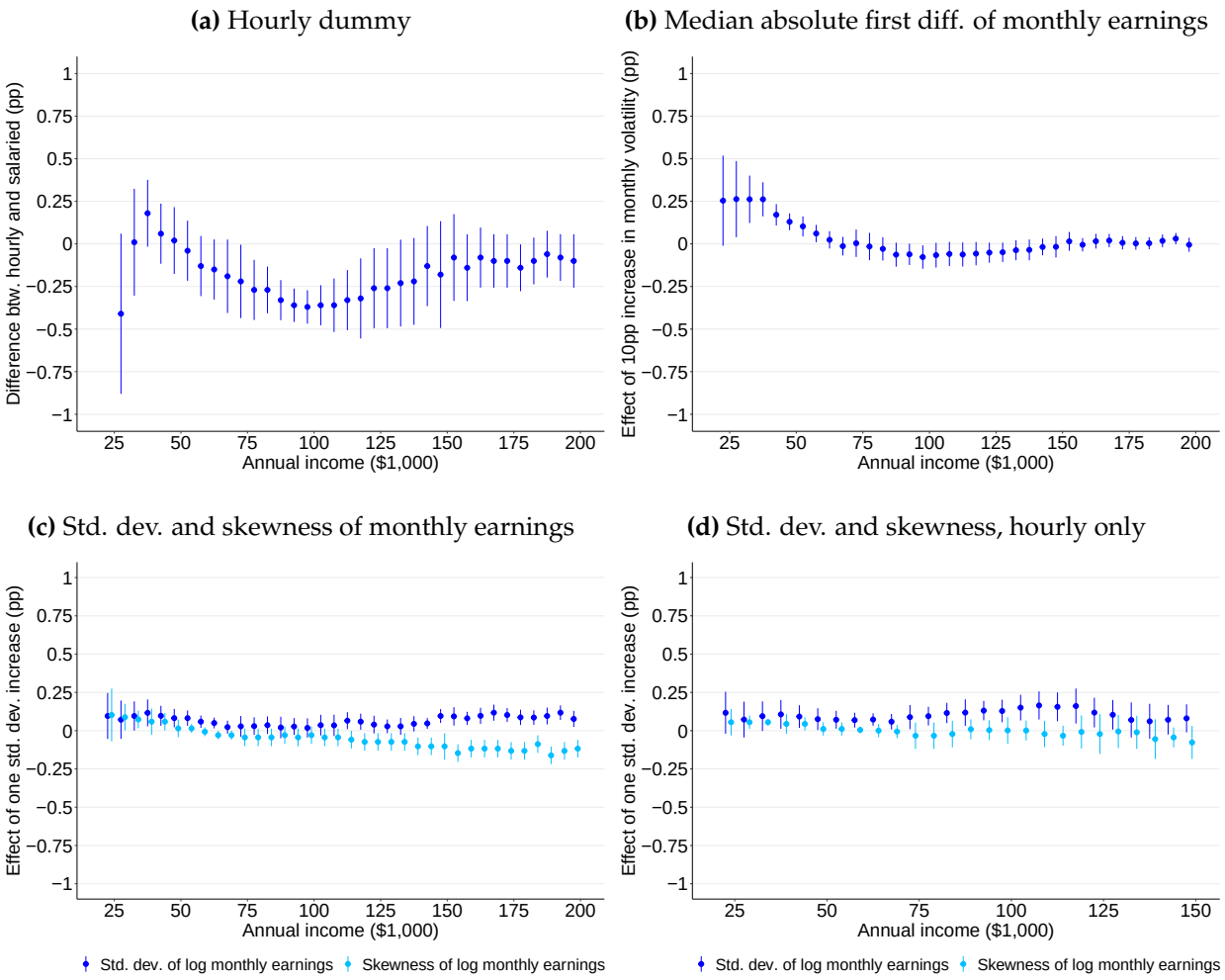
Note: Observations are participants and the sample is restricted to participants who separate from employment during the sample period. Unlike the baseline analysis in Figure 2, I consider all separators, even those who are 55 or older at the end of the calendar year in which they separate and for whom cashouts are not penalized. The monthly income volatility measures are as defined in Section 3.4 and are taken across all observable years for a given participant. Participants are classified as hourly if at least 45% of their observable paychecks change by at least $1 relative to the lagged paycheck value. The remaining participants for whom fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. The underlying regression specification in (11) includes buckets for average annual income during the employment spell (interacted with the volatility measures), a quadratic of age at separation, a quadratic of plan tenure at separation, plan fixed effects, and year fixed effects. The coefficient estimates in panels (c) and (d) are scaled by the standard deviation of the corresponding volatility measure. Annual income buckets on the horizontal axis are $10,000 wide and are based on income values converted to real 2023 dollars. Standard errors are clustered by plan.

Figure A.12: The effect of income volatility on post-separation cashouts, with share of balance cashed out as the dependent variable



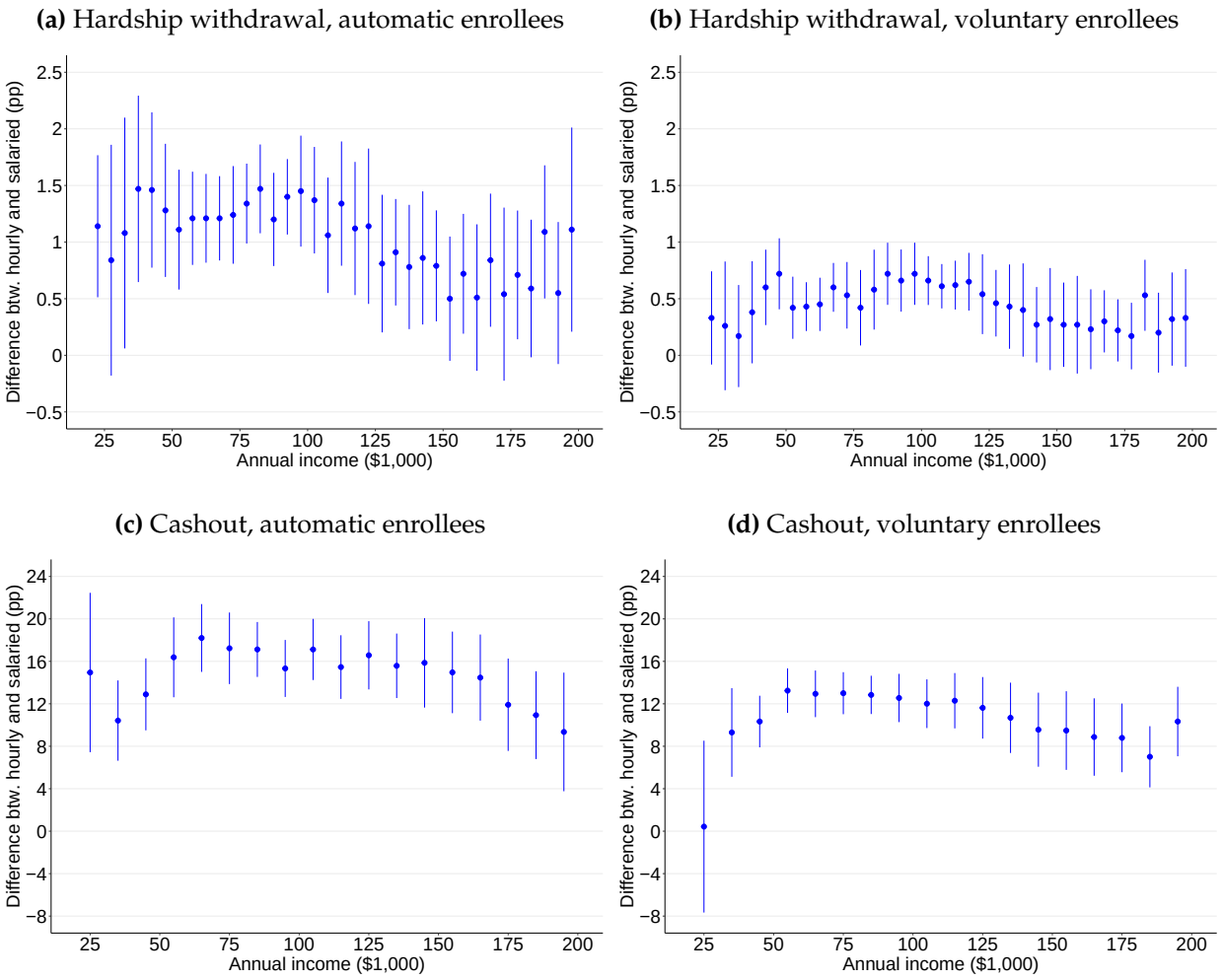
Note: Observations are participants and the sample is restricted to participants who separate from employment during the sample period. The dependent variable is the post-separation cashout amount, expressed as a share of the participant's final balance. If the participant does not take a post-separation cash distribution, the dependent variable is 0. The baseline analysis in Figure 2 uses a binary indicator (equal to 1 if the participant takes a post-separation cash distribution and 0 otherwise) as the dependent variable. The monthly income volatility measures are as defined in Section 3.4 and are taken across all observable years for a given participant. Participants are classified as hourly if at least 45% of their observable paychecks change by at least $1 relative to the lagged paycheck value. The remaining participants for whom fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. The underlying regression specification in (11) includes buckets for average annual income during the employment spell (interacted with the volatility measures), a quadratic of age at separation, a quadratic of plan tenure at separation, plan fixed effects, and year fixed effects. The coefficient estimates in panels (c) and (d) are scaled by the standard deviation of the corresponding volatility measure. Annual income buckets on the horizontal axis are $10,000 wide and are based on income values converted to real 2023 dollars. Standard errors are clustered by plan.

Figure A.13: The effect of income volatility on employee contribution rates



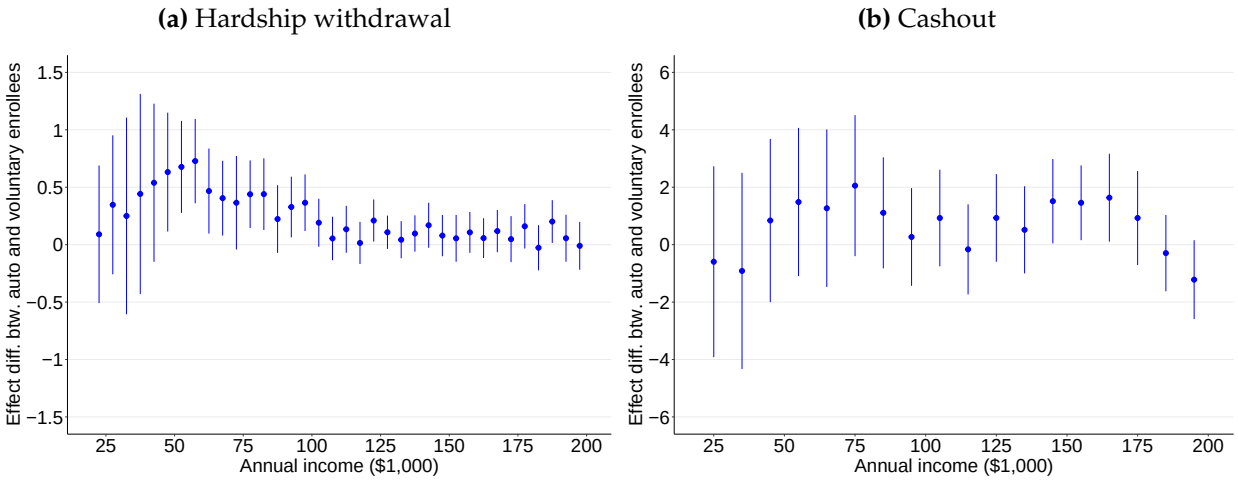
Note: Observations are participant-years and the monthly income volatility measures are as defined in Section 3.4. The dependent variable is the participant's annual contribution rate (i.e., total employee contributions divided by annual income). Participant-years are classified as hourly if at least 45% of the participant's paychecks in the relevant year change by at least $1 relative to the lagged paycheck value. The remaining participant-years where fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. The underlying regression specification in (10) includes annual income buckets (interacted with the volatility measures), a quadratic of age, a quadratic of plan tenure, plan fixed effects, and year fixed effects. The coefficient estimates in panels (c) and (d) are scaled by the standard deviation of the corresponding volatility measure. Annual income buckets on the horizontal axis are $5,000 wide and based on income values converted to real 2023 dollars. Standard errors are clustered by plan.

Figure A.14: Heterogeneity by enrollment policy,
using the hourly dummy to measure income volatility



Note: The income volatility measure (interacted with the annual income buckets) is a dummy variable for hourly participants. Participants are classified as hourly if at least 45% of their observable paychecks change by at least $1 relative to the lagged paycheck value. The remaining participants for whom fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. In panels (a)-(b), observations are participant-years and the annual income buckets are $5,000 wide (in real 2023 dollars). The underlying regression specification in (10) includes annual income buckets, a quadratic of age, a quadratic of plan tenure, plan fixed effects, and year fixed effects. In panels (c)-(d), observations are participants and the annual income buckets (based on average annual income during the employment spell) are $10,000 wide. The sample is restricted to participants who separate from employment during the sample period and are younger than 55 at the end of the calendar year in which they separate. The underlying regression specification in (11) includes buckets for average annual income during the employment spell, a quadratic of age at separation, a quadratic of plan tenure at separation, plan fixed effects, and year fixed effects. Panels (a) and (c) consider only participants who were automatically enrolled and panels (b) and (d) consider only those who were voluntarily enrolled. Standard errors are clustered by plan.

Figure A.15: Difference in volatility effects between automatic and voluntary enrollees



Note: This figure plots the differences in estimated coefficients (effect for automatic enrollees minus effect for voluntary enrollees) from Figure 3.

Table A.7: Participant demographics and behavior by survey response (alternate emergency savings question)

	Months of expenses in emergency savings				
	Less than 1 month	1-3 months	3-6 months	More than 6 months	Did not answer
Number of survey respondents	495 (12.2%)	578 (14.3%)	353 (8.7%)	475 (11.8%)	2,140 (53.0%)
Annual income					
10th	43,180	46,127	51,616	55,099	38,950
50th	74,393	89,968	101,834	108,520	72,397
90th	135,448	163,069	182,015	204,276	143,983
Median age	45	46	47	47	44
Median tenure	6	8	7	7	6
Likely hourly	65.7%	52.6%	41.6%	43.6%	72.2%
401(k) behavior in 2024					
Median contribution rate	7.4%	8.6%	10.0%	10.2%	7.3%
Contributed above match cap	62.8%	73.5%	78.2%	77.7%	62.8%
Plan loan origination in 2024	28.9%	16.3%	9.9%	5.1%	25.9%
Hardship withdrawal in 2024	19.6%	7.1%	2.6%	2.1%	22.1%
Separated in 2025	8.1%	8.1%	7.6%	7.4%	8.3%
Share with cashout (cond. separation)	40.0%	19.1%	7.4%	8.6%	38.4%

Note: This table considers 4,041 participants in Vanguard-administered 401(k) plans who responded to a survey in July 2024 and made employee contributions to their plan in all 12 months in 2024. The particular survey question analyzed in this table (which participants could leave blank) asked, "How much money do you have that could be used as emergency savings?" Participants are classified as hourly if at least 45% of their paychecks in 2024 change by at least $1 relative to the lagged paycheck value. The remaining participants for whom fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. Aside from the column groupings, all information in this table comes directly from the administrative recordkeeping data (i.e., I do not rely on self-reporting in the survey). Separations are observable through August 2025.

Table A.8: Regression analysis of liquid savings survey responses

	Dependent variable			
	"Entirely confident" in meeting $2,000 expense		At least one month worth of expenses saved	
	All participants	Drop non-responses	All participants	Drop non-responses
	(1)	(2)	(3)	(4)
Likely hourly	-0.157	-0.175	-0.133	-0.079
	(0.022)	(0.031)	(0.023)	(0.028)
Log annual income	0.134	0.213	0.122	0.115
	(0.021)	(0.033)	(0.020)	(0.030)
Controls				
Age	X	X	X	X
Tenure	X	X	X	X
Plan FE	X	X	X	X
Number of participants	4,041	2,356	4,041	1,901

Note: This table considers 4,041 participants in Vanguard-administered 401(k) plans who responded to a survey in July 2024 and made employee contributions to their plan in all 12 months in 2024. The first survey question asked, "How confident are you that you could find the money to pay for an emergency that costs about $2,000?" The answer choices were "entirely confident," "somewhat confident," and "not at all confident" (participants could also leave their answers blank). The dependent variable in columns (1)-(2) is the probability that the participant chose "entirely confident." The second survey question asked, "How much money do you have that could be used as emergency savings?" The answer choices were "less than one month," "between one and three months," "between three and six months," and "more than six months" (participants could also leave their answers blank). The dependent variable in columns (3)-(4) is the probability that the participant chose "between one and three months," "between three and six months," or "more than six months." Columns (1) and (3) code the dependent variable as zero for participants who left their answers blank. Columns (2) and (4) drop participants who left their answers blank. Participants are classified as hourly if at least 45% of their paychecks in 2024 change by at least $1 relative to the lagged paycheck value. The remaining participants for whom fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. All columns control for quadratics of age and tenure and plan fixed effects. Standard errors are clustered by plan.

Table A.9: Regression analysis of withdrawal behavior among survey respondents

	Dependent variable		
	Hardship withdrawal in 2024	Loan origination in 2024	Cashout (cond. separation)
	(1)	(2)	(3)
Confidence in meeting $2,000 expense			
"Not at all confident"	0.220	0.197	0.304
	(0.027)	(0.045)	(0.128)
"Somewhat confident"	0.082	0.140	0.437
	(0.016)	(0.019)	(0.103)
Did not answer	0.105	0.107	0.249
	(0.013)	(0.015)	(0.063)
Likely hourly	0.069	0.084	0.162
	(0.014)	(0.021)	(0.129)
Log annual income	-0.051	-0.023	0.123
	(0.013)	(0.012)	(0.074)
Controls			
Age	X	X	X
Tenure	X	X	X
Plan FE	X	X	X
Number of participants	4,041	4,041	326

Note: This table considers 4,041 participants in Vanguard-administered 401(k) plans who responded to a survey in July 2024 and made employee contributions to their plan in all 12 months in 2024. The survey question asked, "How confident are you that you could find the money to pay for an emergency that costs about $2,000?" The answer choices were "entirely confident," "somewhat confident," and "not at all confident" (participants could also leave their answers blank). The omitted group is the participants who chose "entirely confident." Column (3) considers the smaller subset of 326 participants who separated from employment in 2025. Participants are classified as hourly if at least 45% of their paychecks in 2024 change by at least $1 relative to the lagged paycheck value. The remaining participants for whom fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. All columns control for quadratics of age and tenure and plan fixed effects. Standard errors are clustered by plan.

Table A.10: Cashout rates by age, annual income, balance, and hourly/salaried

		Ages 20-29		Ages 30-39	
Annual income	Balance at separation	Hourly cashout rate	Salaried cashout rate	Hourly cashout rate	Salaried cashout rate
$<$ 25k	$<$ 1,000	68%	–	71%	–
	1,000-4,999	34%	26%	44%	39%
	5,000-9,999	33%	–	41%	–
	10,000-49,999	23%	–	26%	–
	50,000-99,999	–	–	6%	–
	$\geq$ 100,000	–	–	–	–
25-50k	$<$ 1,000	74%	–	79%	–
	1,000-4,999	40%	27%	53%	43%
	5,000-9,999	45%	25%	55%	41%
	10,000-49,999	41%	22%	47%	33%
	50,000-99,999	29%	–	29%	25%
	$\geq$ 100,000	–	–	14%	–
50-75k	$<$ 1,000	70%	–	68%	70%
	1,000-4,999	32%	21%	45%	41%
	5,000-9,999	41%	20%	54%	36%
	10,000-49,999	39%	14%	47%	26%
	50,000-99,999	29%	9%	34%	16%
	$\geq$ 100,000	21%	–	24%	11%
75-100k	$<$ 1,000	59%	–	63%	–
	1,000-4,999	30%	20%	52%	35%
	5,000-9,999	35%	17%	47%	32%
	10,000-49,999	32%	12%	41%	22%
	50,000-99,999	21%	7%	31%	14%
	$\geq$ 100,000	16%	4%	19%	9%
100-125k	$<$ 1,000	–	–	60%	–
	1,000-4,999	–	–	47%	33%
	5,000-9,999	41%	14%	46%	27%
	10,000-49,999	31%	8%	40%	20%
	50,000-99,999	23%	7%	28%	12%
	$\geq$ 100,000	22%	6%	18%	7%
125-150k	$<$ 1,000	–	–	–	–
	1,000-4,999	–	–	–	–
	5,000-9,999	–	–	33%	33%
	10,000-49,999	24%	10%	32%	18%
	50,000-99,999	20%	6%	26%	11%
	$\geq$ 100,000	14%	7%	16%	7%
$\geq$ 150k	$<$ 1,000	–	–	–	–
	1,000-4,999	–	–	–	–
	5,000-9,999	–	–	–	20%
	10,000-49,999	18%	7%	26%	14%
	50,000-99,999	16%	6%	20%	9%
	$\geq$ 100,000	15%	4%	13%	7%

Note: This table shows detailed cashout rates for separated participants in the final sample (i.e., participants with at least one year of complete contribution history in which the inferred annual income amount is within 3% of the W-2 value). Annual income is averaged across participants' employment spells and is in real 2023 dollars. Age and balance are as of separation. Participants are classified as hourly if at least 45% of their observable paychecks change by at least \$1 relative to the lagged paycheck value. The remaining participants for whom fewer than 45% of paychecks change by at least \$1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. Cells are left blank if they contain less than 100 participants.

Table A.11: Cashout rates by age, annual income, balance, and hourly/salaried (continued)

		Ages 40-49		Ages 50-59	
Annual income	Balance at separation	Hourly cashout rate	Salaried cashout rate	Hourly cashout rate	Salaried cashout rate
$<$ 25k	$<$ 1,000	72%	–	71%	–
	1,000-4,999	47%	–	42%	–
	5,000-9,999	45%	–	40%	–
	10,000-49,999	32%	–	30%	–
	50,000-99,999	13%	–	10%	–
	$\geq$ 100,000	5%	–	6%	–
25-50k	$<$ 1,000	81%	–	73%	–
	1,000-4,999	55%	50%	51%	43%
	5,000-9,999	60%	49%	54%	44%
	10,000-49,999	52%	44%	47%	39%
	50,000-99,999	32%	26%	30%	25%
	$\geq$ 100,000	16%	18%	14%	19%
50-75k	$<$ 1,000	69%	–	55%	–
	1,000-4,999	48%	49%	44%	49%
	5,000-9,999	60%	47%	54%	43%
	10,000-49,999	54%	38%	49%	37%
	50,000-99,999	40%	27%	36%	26%
	$\geq$ 100,000	25%	16%	22%	17%
75-100k	$<$ 1,000	58%	–	51%	–
	1,000-4,999	48%	45%	45%	42%
	5,000-9,999	53%	43%	50%	42%
	10,000-49,999	50%	36%	46%	33%
	50,000-99,999	40%	24%	36%	24%
	$\geq$ 100,000	26%	14%	23%	16%
100-125k	$<$ 1,000	59%	–	43%	–
	1,000-4,999	48%	45%	43%	37%
	5,000-9,999	45%	38%	44%	35%
	10,000-49,999	44%	30%	43%	28%
	50,000-99,999	38%	21%	34%	21%
	$\geq$ 100,000	22%	12%	21%	14%
125-150k	$<$ 1,000	–	–	–	–
	1,000-4,999	–	–	–	–
	5,000-9,999	50%	35%	–	38%
	10,000-49,999	40%	25%	35%	25%
	50,000-99,999	32%	19%	32%	21%
	$\geq$ 100,000	18%	10%	19%	12%
$\geq$ 150k	$<$ 1,000	–	–	–	–
	1,000-4,999	–	–	–	–
	5,000-9,999	–	32%	–	25%
	10,000-49,999	34%	22%	32%	22%
	50,000-99,999	27%	15%	25%	16%
	$\geq$ 100,000	16%	9%	17%	11%

Note: This table shows detailed cashout rates for separated participants in the final sample (i.e., participants with at least one year of complete contribution history in which the inferred annual income amount is within 3% of the W-2 value). Annual income is averaged across participants' employment spells and is in real 2023 dollars. Age and balance are as of separation. Participants are classified as hourly if at least 45% of their observable paychecks change by at least $1 relative to the lagged paycheck value. The remaining participants for whom fewer than 45% of paychecks change by at least $1 are classified as salaried. See Section 3.2 for a discussion of this classification rule. Cells are left blank if they contain less than 100 participants.

Appendix B Bonus-identification algorithm (for online publication)

To identify annual bonuses among salaried participants, I use the following algorithm. I only apply the algorithm to participant-years that are classified as salaried (i.e., participant-years in which less than 45% of paychecks change by at least $1 relative to the lagged paycheck value; see the discussion in Section 3.2).

- Let g_{it} be annual base income for salaried participant i in year t. The typical paycheck is therefore $\frac{1}{n_{it}} g_{it}$, where n_{it} is the number of paychecks for participant i in year t.
- When an annual bonus is paid, the paycheck becomes $g_{it}\left(\frac{1}{n_{it}} + \theta\right)$, where θ is the bonus amount as a share of base income.
- Proxy for g_{it} with $n_{it} y^m_{it}$, where y^m_{it} is the median paycheck amount for participant i in year t. I use the median paycheck amount (rather than the mean) so that my estimate of base income is not affected by the annual bonus amount itself.
- Choose a minimum bonus-detection threshold $\underline{\theta}$. I set $\underline{\theta} = 0.05$, so I aim to detect bonuses that are at least 5% of base income.
- Classify a given paycheck y_{itp} as containing a bonus if:

$$\begin{aligned} y_{itp} &\geq n_{it} y^m_{it} \left(\frac{1}{n_{it}} + \underline{\theta}\right) \\ &= y^m_{it}(1 + n_{it}\underline{\theta}) \end{aligned}$$

- If the above condition is satisfied for a given paycheck y_{itp}, the inferred bonus amount is $y_{itp} - y^m_{it}$ and the inferred base amount is y^m_{it}.

Table B.1 shows summary statistics for the identified bonuses. About 35% of salaried participant-years have bonus income. In the vast majority of these cases, the bonus is identified in only one paycheck, consistent with the algorithm's goal of identifying annual bonuses. As a share of base income, bonus amounts range from 5.8% at the 10th percentile to 25.6% at the 90th percentile.

Table B.1: Inferred bonuses among likely salaried workers

	Likely hourly	Likely salaried
Participant-years	7,069,907	3,504,053
Share with inferred bonus	–	34.9%
# of paychecks with bonus income (cond. nonzero)		
Mean	–	1.2
10th	–	1
50th	–	1
90th	–	1
Bonus as share of base income (cond. nonzero)		
Mean	–	14.5%
10th	–	5.8%
50th	–	10.9%
90th	–	25.6%

Note: Observations are participant-years. Participant-years are classified as hourly if at least 45% of the participant's paychecks in the relevant year change by at least $1 relative to the lagged paycheck value. The remaining participant-years where fewer than 45% of paychecks change by at least $1 are classified as salaried.

Appendix C Additional evidence from layoffs (for online publication)

The main empirical analysis in Section 4 – enabled by the paycheck-level income data I construct – focuses on monthly earnings volatility *within* employment spells. Relating high-frequency, sub-annual earnings fluctuations to 401(k) withdrawals is the principal contribution of this paper. However, for completeness and comparison with prior literature, I also consider *between*-job income shocks caused by job loss.

Goodman et al. (2021) and Coyne et al. (2024) find that households are significantly more likely to take early withdrawals from retirement accounts in years in which they receive unemployment insurance (UI) benefits. These findings are consistent with households using retirement accounts as a source of liquidity after layoffs. Because UI receipt is not observable in 401(k) recordkeeping data, I must use other methods to identify layoffs. The recordkeeping data contain a field explicitly identifying layoffs, but plan sponsors do not always flag layoffs to Vanguard. I therefore use a combination of direct information and indirect inference. I consider the separation of participant i in plan j to be a layoff if i) the separation is explicitly identified as a layoff in the administrative data, or ii) at least L% of currently employed participants in plan j separate in the same month as participant i, for some threshold L. The logic behind the second condition is that tightly bunched separations typically indicate firm-wide layoffs (Jacobson et al., 1993).[44]

Figure C.1 shows the results of the layoff analysis. I begin with the cashout regression specification in (11), using the simple dummy for hourly participants as the volatility measure $\bar{v}_{ij}$. I then add two indicators, again interacted with the annual income buckets: the first indicator is equal to one for separations occurring during a bunched month (I set $L = 5\%$ in the baseline analysis and consider robustness in Figure C.2), and the second indicator is equal to one for separations that do not occur in bunched months but are explicitly coded as layoffs.[45]

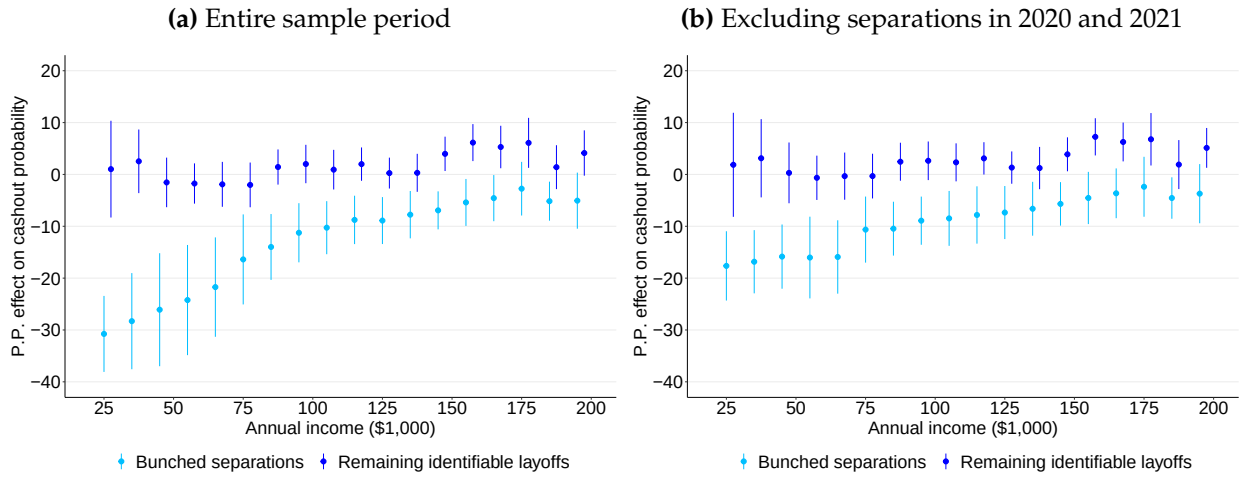
The indicators for explicitly coded layoffs are generally positive, especially when I exclude separations occurring in 2020 or 2021, when unusually generous pandemic-era UI benefits likely made laid-off workers less dependent on retirement accounts for liquidity.[46]

[44] I only apply the second condition to plans with at least 1,000 participants.

[45] With $L = 5\%$, 10.7% of separations occur in bunched months and an additional 2.3% of separations are explicitly coded as layoffs.

[46] See Ganong et al. 2020 regarding the pandemic-era UI expansion. The empirical samples for both Goodman et al. (2021) and Coyne et al. (2024) end before 2020.

Figure C.1: The effect of layoffs on post-separation cashout probability



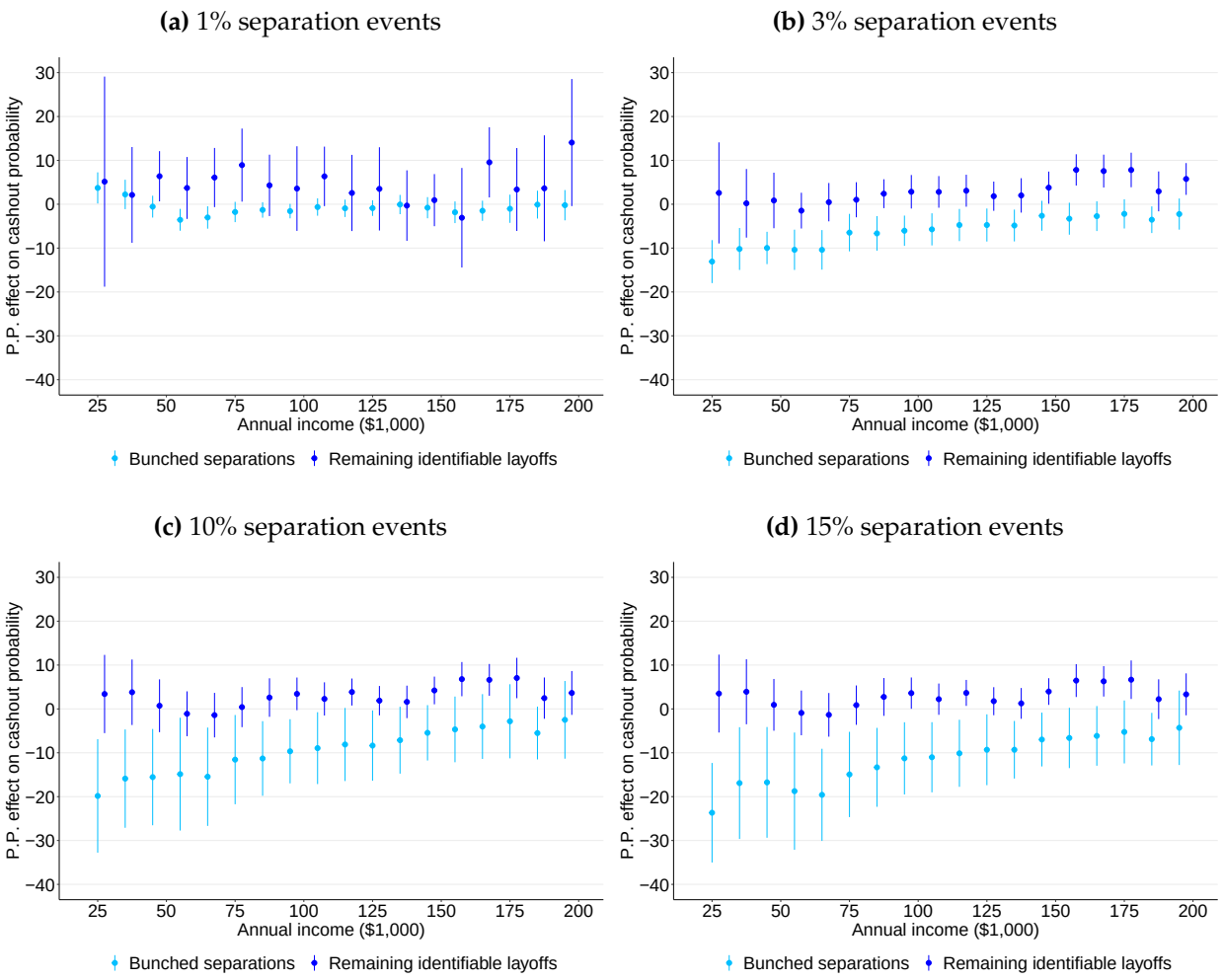
Note: Observations are participants and the sample is restricted to participants who separate from employment during the sample period. I further restrict to participants for whom cashouts are penalized (i.e., those who are younger than 55 at the end of the calendar year in which they separate). I begin with the regression specification in (11) and add two indicators (each interacted with the annual income buckets). The first indicator is equal to one if participant i in plan j separates in a month in which at least L% of currently employed participants in plan j also separate (I set $L = 5\%$ in the baseline analysis and consider robustness in Figure C.2). The second indicator is equal to one if participant i does not separate in a bunched month but their separation is explicitly coded as a layoff in the administrative recordkeeping data. In panel (b), I exclude separations occurring in 2020 or 2021. The underlying regression specification in (11) includes buckets for average annual income during the employment spell, a dummy for participants classified as hourly workers (interacted with the annual income buckets), a quadratic of age at separation, a quadratic of plan tenure at separation, plan fixed effects, and year fixed effects. Annual income buckets on the horizontal axis are $10,000 wide and are based on income values converted to real 2023 dollars. Standard errors are clustered by plan.

The estimates for middle- and higher-income workers (in the 5-10 percentage point range) are roughly consistent with Goodman et al. (2021), who find UI recipients are 11 percentage points more likely to take penalized withdrawals than other job separators. Interestingly, the indicators for bunched separations are uniformly negative and quite large at the bottom of the income distribution, though they again shift upward when I exclude the 2020-2021 period. The recordkeeping data do not allow me to confirm this, but it is possible that employers are more likely to give advance notice (allowing workers to accumulate liquid savings) or award severance payments (which provide post-separation liquidity) during mass layoff events, reducing the affected workers' dependence on 401(k) withdrawals.[47]

When excluding mass-layoff events (which appear to have unique implications for cashout behavior), I thus find modest positive cashout effects that are reconcilable with prior literature. The explicit layoff effects in Figure C.1 are nonetheless smaller than the effects of recurring monthly volatility in Figure 2, underscoring the importance of *within*-job income shocks in explaining early 401(k) withdrawals.

[47]The federal WARN Act requires employers to give 60 days' notice before certain types of mass layoffs, or to award back pay to laid-off workers if notice is not given.

Figure C.2: The effect of layoffs on post-separation cashout probability, using alternative separation bunching thresholds



Note: Observations are participants and the sample is restricted to participants who separate from employment during the sample period. I further restrict to participants for whom cashouts are penalized (i.e., those who are younger than 55 at the end of the calendar year in which they separate). I also exclude layoffs occurring in 2020 or 2021, when unemployment insurance benefits were unusually generous (the baseline analysis in Figure C.1 shows results including the 2020-2021 period). I begin with the regression specification in (11) and add two indicators (each interacted with the annual income buckets). The first indicator is equal to one if participant i in plan j separates in a month in which at least L% of currently employed participants in plan j also separate (I consider $L \in \{1\%, 3\%, 10\%, 15\%\}$ to compare with baseline analysis using $L = 5\%$ in Figure C.1). The second indicator is equal to one if participant i does not separate in a bunched month but their separation is explicitly coded as a layoff in the administrative recordkeeping data. In panel (b), I exclude separations occurring in 2020 or 2021. The underlying regression specification in (11) includes buckets for average annual income during the employment spell, a dummy for participants classified as hourly workers (interacted with the annual income buckets), a quadratic of age at separation, a quadratic of plan tenure at separation, plan fixed effects, and year fixed effects. Annual income buckets on the horizontal axis are $10,000 wide and are based on income values converted to real 2023 dollars. Standard errors are clustered by plan.