Retention Effects of Employee Stock Options: Evidence from Bunching at Vesting Dates*

Qing Gong† Hong Zhang‡ Li-An Zhou§

February 7, 2023
Latest Version Here

Abstract

We study the retention effect of broad-based employee stock options to rationalize their popularity despite the high granting costs. Leveraging the semi-random timing of option vesting dates, we estimate a bunching model on a novel employee-level panel dataset and find the quitting rate of option owners more than doubles shortly after the vesting dates. The retention effect is robust and unique to voluntary quitters, and accounts for a large share of the total benefit of options, which outweighs the granting cost by 8% even with the lower-bound estimates. Accounting for the retention effects thus avoids underestimating the benefit of options, thereby helping to explain many firms' decisions to grant options.

JEL classification: G30; J33; M50; M52

Keywords: stock options; employee turnover; bunching

*We thank Nicholas Bloom, François Gerard, Wojciech Kopczuk, Edward Lazear, Brian McManus, Paul Oyer, John Shoven, and Petra Todd for many helpful comments and suggestions. We also thank seminar participants at the Beyster Fellowship Symposium, Columbia University, and UNC Chapel Hill. Hong Zhang is grateful for the financial support of the National Natural Science Foundation of China (grants no. 71703054 and no. 72273050). All errors are our own.

†Gong: University of North Carolina at Chapel Hill, Department of Economics, qinggong@email.unc.edu

‡Zhang (corresponding author): Institute of Industrial Economics, Jinan University, hongzh@jnu.edu.cn

§Zhou: Guanghua School of Management, Peking University, zhoula@gsm.pku.edu.cn

1 Introduction

Equity compensation is now offered by 35% of private companies and 43% of public companies in the United States.[1] Among these, an estimated 9 million (or 7% of total private sector employees) hold stock options.[2] Such popularity is not unique to the US: over 800 of the 4,697 domestic public companies in China offered equity incentive plans in 2021, among which options are also commonly used.[3] Although employee stock options are popular, there is a lack of consensus on the rationale behind their popularity. Little is known about whether options trigger the firms' intended response from employees, and, if so, whether the resulting benefits outweigh the high granting costs.

An earlier theory proposes that options incentivize higher output by aligning the interests of employees with those of the firm. But free-riding in multiple-agent settings greatly limits such incentivizing effects, especially for broad-based stock options (BBSO). This is supported by both theoretical and empirical studies (e.g., Lazear (2004); Oyer and Schaefer (2005)), although the empirical evidence is mixed. Alternative theories have examined frictions in firm decisions or other benefits of options, including firm misperceptions (Hall and Murphy, 2003), firm peer effects (Kedia and Mozumdar, 2002), taking advantage of stock market volatility (Zhang, 2006), options as a screening device (Bergman and Jenter, 2007), cash conservation (Babenko et al., 2011), and retention effects (Core and Guay, 2001). Aldatmaz et al. (2018) find evidence supporting the *presence* of retention effects from establishment-level data. But what remains unresolved is whether the retention benefits of options are large enough to justify their costs.

In this paper, we answer two questions: (i) to what extent do BBSO have retention effects? and (ii) are the benefits large enough to rationalize the granting decision in the first place? We do so by first estimating the retention benefits of BBSO and then weighing them (together with other benefits) against the granting costs. Equipped with a unique employee-level panel dataset from a large, NASDAQ-listed firm, we can pinpoint the quitting decisions of individual BBSO owners. Moreover, we can identify the *causal* effect of BBSO on retaining employees with a bunching de-

[1] Morgan Stanley (2022).
[2] National Center for Employee Ownership (2021).
[3] According to Feng and Zhu (2022) and National Bureau of Statistics of China (2022).

sign that leverages discontinuities in the cost of quitting when options vest, the timing of which is exogenous and semi-random.

Our findings provide affirmative answers to both questions. First, BBSO does reduce turnover. The quitting rate of option owners increases by 0.2-0.7 percentage points immediately after options vest. The excess quitting is large relative to the average monthly quitting rate of less than 0.31% before vesting dates, and it is robust to tests of alternative explanations. Second, and more importantly, the retention effects are large enough so that the benefits outweigh the granting cost. The lower-bound estimates of total benefits exceed the total costs by 8% for all but the highest-level employees. Accounting for the retention benefits would thus avoid underestimating the benefit-cost ratio, thereby helping to reconcile the popularity of options and the high granting costs.

We contribute to the existing literature in two ways. First, we identify and quantify the *causal* effect of BBSO ownership on retaining employees with a bunching design. The endogeneity problem often plagues empirical studies in the literature: option owners and non-owners are likely to have unobserved differences that affect turnover and performance. Balsam et al. (2007) use data from one large firm and find lower turnover rates when options cannot be exercised. Recent work by Aldatmaz et al. (2018) examines whether turnover declines after large option grants using establishment-level panel data. They leverage exogenous variations in the value of unvested options that result from changing stock prices and market returns. This identification strategy, though well-suited for their research question, could not gauge the *magnitude* of the benefits, which is essential in answering our question of whether BBSO is worth the cost. Our bunching design, on the other hand, addresses the endogeneity problem and identifies the retention effects of options.[4]

Our second contribution is to provide new empirical evidence on the presence *and* magnitude of the effects of BBSO in general. More importantly, our findings shed light on the response of individual, non-executive employees. Using a novel dataset on all the employees within a firm, we can closely examine the effect of BBSO on the quitting decisions of individual employees. The

[4]Cowgill and Zitzewitz (2014) is another paper that examines the causal effects. Taking advantage of Google's unique equity pricing policy, they identify the incentivizing effects of equity compensation from exogenous variations in stock exposure. They find the incentives to be fairly weak. We take a different approach, using the discontinuous change in the opportunity cost of quitting to deal with unobserved employee heterogeneity. We also focus on the retention effects on employees who plan to quit instead of the effects on employees who choose to stay.

exact characteristics of options received by an employee and the detailed quitting times allow us to better gauge the retention benefits.

Most previous empirical studies either document firm-level effects or the response of executives. Oyer and Schaefer (2005) draw upon a combination of firm-level option grant data and survey data on salary and option packages of middle-level executives. They reject the incentive provision effect of options and find the sorting and retention theory more plausible. In a subsequent study, Oyer and Schaefer (2006) use the same survey data to estimate the firm's cost of granting options. They find that accounting concerns are unlikely to be the driving force behind option granting decisions, and that the empirical patterns are more consistent with the retention story. Carter and Lynch (2004) study option repricing and find it to be negatively correlated with overall employee turnover but not executive turnover. Dube and Freeman (2010) also use survey data and find that equity compensation and shared decision-making arrangements are jointly associated with lower turnover. Kim and Ouimet (2014) use firm-level data to document improved productivity as a result of granting employee options in smaller firms.

Existing evidence on the employee-level effects focuses on firm executives (e.g. Aggarwal and Samwick (1999); Mehran and Tracy (2001); Barron and Waddell (2003)). For instance, Gopalan et al. (2014) find that CEO turnover increases after a large stock or option grant vests. Ladika and Sautner (2019) and Jochem et al. (2018) both leveraged a large, near-random regulatory shock and show that CEO display short-termism behavior and are more likely to leave the firm after vesting periods are removed.

To the best of our knowledge, the only other papers using detailed, non-executive employee-level data to identify a causal relationship are Li and Zhang (2021) and Cowgill and Zitzewitz (2014). The former apply a DID framework to a subset of data used in this paper and found an absence of incentivization effects on the "rank-and-file" employees. The latter study the incentivizing effect of restricted stocks on Google employees. Balsam et al. (2007) and Vance (2013) also use single-firm employee data, but focus on the *association* between options and employee turnover instead.[5]

[5] Another (loosely) related paper using data on lower-level employees is Huddart and Lang (1996), where the authors examine the implications of option exercising on corporate debt and regulation.

Finally, it should also be noted that our findings shed useful light beyond the Chinese setting. Our data comes from a fast-growing, NASDAQ-listed firm straddling the technology and service industries, where the offering of BBSO is rather common. This firm resembles its U.S. peers in its management, compensation structure, and human resources policies. Because it is listed on NASDAQ, it also follows the same rules and regulations as U.S. listed companies. Its employees are primarily high-skilled, financially savvy, and have higher turnover rates, as is typical of this industry. Moreover, we identified a *local* treatment effect using the discontinuous drop in quitting costs. To the extent that the decision-making process of individual owners in our data is similar to that of owners in a more general setting of interest, the responses of the former are still informative of those of the latter.

The rest of the paper is organized as follows. In Section 2, we introduce the empirical background and the data. We discuss the vesting schedules and evolving values of options, as well as how they could affect turnover. We also highlight option owner heterogeneities, which guide the design of our empirical analysis that follows. In Section 3, we describe the bunching framework and the joint estimation of the counterfactual distribution and the affected range. At the end of this section, we discuss the advantages of bunching and how it differs from the closely related regression discontinuity design. We then present the baseline estimates of bunching in Section 4, followed by various robustness and falsification tests that corroborate the key findings. We design a benefit-cost analysis for BBSO in Section 5 and estimate the benefit-cost ratio under various scenarios. We conclude in Section 6.

2 Empirical Background and Data

2.1 Firm background and option grants

The firm and its employees. Our longitudinal administrative data comes from a leading online travel agency in China. The firm was founded in the late 1990s, listed in NASDAQ in the early 2000s, and has over 20,000 employees as of 2017. The firm categorizes its job positions, with the exception

of top executives, vertically into 10 levels.[6] Newly hired employees can start from any level upon entry and then move up over time. Levels 1 to 4 are junior employees, senior employees, heads of teams, and senior heads of teams, respectively. They receive options as an award for excellence in performance. Levels 5-10 are managers, senior managers, and higher-level positions, all the way up to the vice president.

BBSO granting at the firm. The firm granted BBSO at least twice a year since 2000 to a wide range of employees. Table 1 summarizes option ownership rates across departments and job levels. Option ownership rate is 31.13% even for level 2 employees, and is at least 63.34% for higher-level employees.[7] Neither the frequency nor the timing of option grants follow any pre-determined schedule.[8] This variation in timing is helpful to our identification strategy when estimating the retention effects of BBSO.

[Table 1 ABOUT HERE]

Types of option plans and vesting schedules. There are two types of plans: Plan 1 options were offered from 2000 to 2009, and Plan 2 has been offered from 2008 onward. The two types of option plans differ, among other things, in the vesting schedule. Plan 1 options vest in three equal batches every 12 months after an initial 12-month lock-in period. Suppose an employee receives options on July 1, 2011, which is defined as the beginning of month 0. Then one-third of her total granted options vest will vest on July 1 of 2012, 2013, and 2014, respectively. These three vesting dates mark the beginning of months 12, 24, and 36. Plan 2 options also vest in three equal batches, but have an initial lock-in period of 24 months. So the three vesting dates are at the beginnings of months 24, 36, and 48.

The owner has to give up any unvested options upon leaving the firm, but can keep and exercise the vested options within 90 days. After that, any vested options that remain unexercised will revert

[6] Examples of top executives not included in the 10 levels are the chief executive officer and the chief financial officer. We exclude these executives from our sample because of privacy concerns and because their option plans and turnovers differ substantially from other employees, who are our focus in studying broad-based employee stock options.

[7] The sales department stands out as an anomaly: it has no employees at level 5 or above and a large number of entry-level employees, who are customer service representatives working at the call centers.

[8] We show the detailed timing and size of each option grant from 2000 to 2011 in Figure A1 in the appendix.

to the firm. The value of foregone options becomes "money left on the table" and add to the cost of quitting. Hence option owners who would quit in the absence of options may want to postpone quitting until after the vesting dates, if they still want to quit at all.

[FIGURE 1 ABOUT HERE]

Figure 1 illustrates the vesting schedules. The horizontal axes in all three panels are months after the grant date. The top panel plots the fraction of vested Plan 1 (solid) and Plan 2 (dashed) options over time. The middle panel plots the expected value of vested options over time, as calculated on the granting date. This is defined as the Black-Scholes value (BSV) of granted options on Day 1 of Month 0, multiplied by the fraction that has vested. It captures the expected value of options from the perspective of new option owners. The bottom panel plots the contemporaneous BSV of vested options calculated at the beginning of each month after being granted.

All three panels show that vested options increase discontinuously on vesting dates, both in terms of quantity and dollar values. On the other hand, the opportunity cost of quitting drops discontinuously on vesting dates. [9] This discontinuity will make up the cornerstone of our bunching design.

2.2 Data and sample construction

The dataset we use comes from the Human Resources Department of the firm. It spans from April 1999 to December 2017, covering all of the over 50,000 employees who had ever worked in the firm by the end of 2012. The data contains detailed records on option ownership and transactions, itemized wages, promotion records, and other employee characteristics. It is rare for studies in this literature to observe who received how many options on what date, or whether/when they quit or exercised options. Such a level of detail is especially valuable to us: it is essential for both the causal identification of the retention effects of options and for weighing these retention benefits against the costs.

[9] Although the bottom panel shows an increase in Plan 1 option values in month 48 (after all options vest), it does not invalidate the uniqueness of discontinuity on vesting dates. Also note that the change around month 48 is continuous. In fact, it is no greater than the change in option values in other non-vesting months.

We restrict the main sample for our baseline analysis to include only option owners. Because some employees receive option grants more than once, we restructure the data so that each observation is a unique option grant-employee-month combination. Consider an option owner who received her first option grant in month 0 and then received a second batch in month 15. In our restructured data, her two option grants will be represented by two separate observations for each of the subsequent months. The restructured data distinguishes between different option grants owned by the same individual at a given point in time. The distinction is important because the options have different vesting dates and values, thereby potentially affecting quitting decisions in different ways.

For each option-employee combination, we observe the grant date, the vesting schedule, the strike price, the spot market prices, and the timing and price at which vested options are exercised. For each option owner, we observe her job level, department, promotions, itemized monthly wages, and the date and cause of leaving the firm if separation happens.[10] We also observe the option owner's characteristics such as age, gender, education, and prior work experience. In the end, we get an unbalanced panel of 222,675 observations from 2,959 distinct option-employee combinations.[11]

2.3 Descriptive statistics: valuating the options

Table 2 reports the summary statistics on option-employee characteristics. On average, option owners of Plan 1 receive 19,956 shares, and those of Plan 2 receive 14,409 shares.[12] Despite the greater number of shares, Plan 1 options tend to be relatively less valuable than Plan 2, although both can be worth several times the owner's annual base wage: the BSV of Plan 1 options when

[10] In the empirical analyses that follow, we define monthly "wage" as the monthly average of an employee's comprehensive annual cash compensation, which includes monthly salaries, cash bonuses (usually paid at the end of a year), and all other cash benefits. We use the comprehensive measure instead of only wages because the former may be more relevant to the employees when deciding whether and when to quit.

[11] The number of option owners might seem small relative to the total number of employees that have worked at the firm. But note that many of the 50,000 employees are entry-level customer service representatives, who have lower wages, higher turnover, and almost never receive stock options.

[12] Each option contract could give the holder the right to purchase multiple shares of stock (usually 100). We do not observe the number of *contracts* granted to each owner. Hence, throughout this paper, we will refer to the amount of options in terms of the *shares of stocks* that could be purchased with them.

granted is about 2.63 times the owner's annual base wage, and that of Plan 2 is 4.07 times. Moreover, the average monthly wage of Plan 1 owners is also lower than Plan 2 owners, which is consistent with Plan 1 options being granted in the earlier years of the firm.[13] Hence the difference in the two plans' relative values translates into even larger differences in absolute values, at least *in expectation* when the options were granted.

[TABLE 2 ABOUT HERE]

The realized value of the two plans, however, did evolve to reverse that difference. The bottom panel of Figure 1 shows that Plan 1 options ended up having higher *realized* values over time due to the firm's rapid growth. In Figure A2, we also show the growth rates of the two plans until they are exercised, expired, or reverted to the firm. Once again we see that Plan 1 options appreciate faster in value.

As for the equity portfolio held by option owners, more than 60% of option owners have other options on hand when they receive a newly granted batch of options. The distribution of owner job levels follows an inverse U-shaped pattern for both plans. Fewer option owners are at the lowest level, more are at intermediate levels, and fewer are at very high levels. The initial increase is driven by better chances of receiving options when an employee is at higher levels, and the decrease beyond level 5 is because of the drastic decline in the number of high-level positions. The distribution for Plan 1 owners is slightly skewed toward the lower end, with a larger fraction of level-3 and level-4 owners.

The statistics highlight the heterogeneities between the two option plans, which call for separate examinations of the two plans when gauging their retention effects. Moreover, option owner heterogeneity re-emphasizes the need for a robust identification strategy to pin down any causal relationship.

[13] We have converted all money values to real 2014 RMB yuan using province-level urban CPIs.

3 Empirical Strategy

3.1 Option vesting and the bunching design

Timing, treatment, and bunching effects. Now we formalize the bunching design that identifies the retention effect of options. We first define time t for each option-employee combination as months elapsed since the grant date. Take the Plan 1 owner who receives options on July 1, 2011, as an example. For this option-employee combination, $t = 0$ in July 2011; $t = 1$ in August 2011; and so forth. The first one-third of the granted options vest at the beginning of $t = 12$, on July 1, 2012. The second and third one-third vest at the beginnings of $t = 24$ and $t = 36$, respectively. This relative notion of time, t, is the *running variable* in the bunching framework, whereas the vesting dates are the treatment-determining thresholds. Treatment in our empirical setting is the decrease in the cost of quitting due to options vesting. An option owner is subject to the treatment if t is immediately to the right of a vesting date.

We use the bunching design to quantify the bunching effects, if any. The value of unvested options foregone due to quitting drops discontinuously on the vesting date. But once the vesting date passes, the marginal cost to delay quitting remains unchanged, although the average cost continues to decrease. Option owners are thus more likely to quit immediately after vesting dates and less likely to quit before vesting dates. The *excess* mass in the distribution of quitting after vesting dates gives rise to bunching with a "notch," while the the corresponding *missing* mass before vesting dates forms a "hole." In theory, the excess mass in the notch and missing mass in the hole will offset each other, assuming no uncertainty and no optimization frictions.

Quantifying the bunching effects empirically. Pinning down the excess and missing masses requires knowing the counterfactual distribution and the affected range. They are the two pillars of estimating bunching effects. The counterfactual distribution in our context is the distribution of option owners' quitting times in the absence of options. The standard approach is to first exclude the data points in months affected by the bunching (i.e. the *affected range*); then fit a flexible function, often a polynomial, to the remaining sample; and finally, interpolate over the previously excluded affected range to get the entire distribution (Kleven, 2016).

The affected range is the months in which option owners alter their behavior in response to option vesting. It includes months before the vesting date with reduced quitting (missing mass) and months after the vesting date with increased quitting (excess mass). In our setting, each option-employee combination is subject to three potential vesting dates. Therefore, we define treatment *locally* as being affected by a nearby vesting date that is at most twelve months away. That is, the above-noted Plan 1 option owner in September 2006 might be affected by the treatment effects of the first vesting date (July 20, 2006). But we assume her quitting decision cannot be influenced by months in the affected range of the second vesting date, which is farther away. This assumption ensures that an option-employee combination is subject to at most one treatment at any given time. Empirically, it rules out potential overlap between the affected ranges of two adjacent vesting dates.

We do not impose other assumptions on the affected ranges. In particular, we do not assume they are the same across different vesting dates, or that they are symmetric before and after each vesting date. Instead, we estimate the affected range jointly with the counterfactual distribution and let the equality between the excess and missing masses govern the affected range.[14]

3.2 Joint estimation of counterfactual distributions and affected ranges

We follow Kleven and Waseem (2013) and take an iterative approach to jointly estimate the counterfactual distribution and the affected ranges. We start with an initial guess of the affected range, $[z_-^0, z_+^0]$, and run the regression:

$$Q_t = \sum_{j=0}^{p} \beta_j^0 \cdot (t)^j + \sum_{\tilde{t} \in [z_-^0, z_+^0]} \gamma_{\tilde{t}}^0 \mathbf{1}\{t = \tilde{t}\} + \varepsilon_t, \tag{1}$$

where Q_t is the percentage of employees who quit in period t and p is the order of the polynomial.[15] We use $p = 5$ for all relevant specifications throughout this paper. $\gamma_{\tilde{t}}^0$ captures the effect of the

[14]The equality between the missing mass and the excess mass requires the absence of extensive margin responses, which we discuss in Section 3.2. Apart from the missing and the excess masses, the distribution farther away from the vesting date might also shift because of the treatment. But Kleven (2016) shows that, for estimation purposes, the potential shift can be disregarded and the equality can still be considered as holding.

[15]We normalize the vesting dates, z^*, to $t = 0$ to improve the performance of linear regressions with high-order polynomials.

notch on quitting decisions in the affected range $\tilde{t} \in [z_-^0, z_+^0]$. The counterfactual distribution is then obtained from the predicted values:

$$\hat{Q}_t^0 = \sum_{j=0}^{p} \hat{\beta}_j^0 \cdot (t)^j, \tag{2}$$

where the effects from the affected range, $\hat{\gamma}_{\tilde{t}}^0$, are omitted because of the counterfactual absence of option ownership. Given the counterfactual distribution, we calculate the missing mass before the vesting date z^*:

$$\hat{M}^0 = \sum_{t \in [z_-^0, z^*)} (Q_t - \hat{Q}_t^0). \tag{3}$$

Similarly, we calculate the excess mass (i.e. bunching) after the vesting date:

$$\hat{B}^0 = \sum_{t \in [z^*, z_+^0]} (\hat{Q}_t^0 - Q_t). \tag{4}$$

The missing mass should be equal to the excess mass, $M = B$, *if no extensive margin responses exist*. The absence of extensive margin responses implies that option owners only adjust *when* to quit but not *whether* to quit. Options might also retain employees by making them give up quitting entirely, which, while likely, is not identifiable: the employees who give up quitting are observationally equivalent to those who never intended to quit.

We use the equality $M = B$ to adjust the initial guess on the affected range and iterate until the distance between $\hat{M}$ and $\hat{B}$ is minimized. We denote the optimal estimates of distribution parameters as $\hat{\boldsymbol{\theta}}^* \equiv \left(\{\hat{\beta}_j^*\}_{j=0}^p, \{\hat{\gamma}_{\tilde{t}}^*\}_{\tilde{t} \in [z_-^*, z_+^*]} \right)$ and the bunching effect as $\hat{B}^*$. We then use parametric bootstrapping to calculate the standard errors of bunching, $\hat{B}^*$. We describe the details in Appendix B.1.

3.3 Discussion of the bunching design

Taking the bunching approach is a natural result of the empirical setting we are examining. It is also advantageous in at least two ways. First, it identifies the causal relationship between stock option ownership and employee retention in the context of a vesting schedule. As Kleven (2016) points out, simply plotting the observed distribution often reveals bunching and causality. We show the plots in Section 4. Second, the bunching approach explicitly allows for imperfections of agents' decision-making process, such as errors and frictions of optimization. We will exploit this feature and look into the option owners who quit right before option vesting in Section 5.

A seemingly close alternative strategy is the regression discontinuity (RD) design. RD is another powerful tool to identify causal effects with strong internal validity.[16] For example, Dube et al. (2019) use peer-wage discontinuities to show how peer effects affect quitting. It is also not uncommon to use time as the running variable in RD applications (RD in Time, or RDiT), although Hausman and Rapson (2017) point out several potential fallacies of RDiT. RDiT does not suit our empirical context, however, where the time of quitting is precisely the *choice* of interest. Hence we adopt the bunching framework instead of RDiT.

4 Estimation Results on Bunching and Retention Effects

4.1 Preliminary evidence of bunching

We first plot the observed distribution of when option owners quit as preliminary evidence of bunching. We restrict the attention to employees who leave the firm voluntarily ("quitters"). Those who leave involuntarily are excluded from the main sample and only used later in the falsification test.

In Figure 2, the horizontal axes show months after option granting. Month 0 begins on the grant date. The top and bottom panels plot the fraction of quitters among initial option owners of Plan 1 and Plan 2, respectively. Vesting dates are marked by dotted vertical lines with labels showing the cumulative fraction of options vested so far.

[16] See Imbens and Lemieux (2008) for a survey of the literature.

The figure highlights the decrease in quitting in the months leading up to the vesting dates and the sharp increase afterward. For example, the average fraction of quitters per month among Plan 1 owners is 0.237% in the first 6 months. It drops below 0.126% in $t = 11$ and then jumps to 0.569% in $t = 12$, once the first one-third of options vest. The drop and jump in quitting rates correspond to the missing and excess masses in the bunching design. The same pattern of quitting also arises in months 24 and 36. Moreover, no discernible bunching exists around months 48 or 60, by which time all options have vested. The bottom panel for Plan 2 owners shows a similar pattern (with the exception of the first vesting date).

[FIGURE 2 ABOUT HERE]

Figure 2 provides preliminary evidence for the retention effects of options. To reiterate the point by Kleven (2016), plotting the raw data in a bunching framework is already informative of the causal relationship. The bunching effects in the figure do differ across plans and vesting dates, both in magnitude and statistical significance. We defer the discussion until we present the formal estimation results.

4.2 Baseline estimates of bunching

Estimation results. We present the baseline bunching estimation results in Table 3. Column (1) lists the vesting date whose treatment effect is being estimated. Column (2) quantifies the bunching effect, measured by the excess mass in quitting after the vesting date. Column (3) shows the expected length of retention due to options vesting. Columns (4)-(5) report the estimated affected range: z_-^* and z_+^* are the number of affected months before and after the vesting date, respectively. Columns (6)-(8) report the sample size used in the estimation for each threshold.[17] Bootstrapped standard errors are reported in parentheses.

[TABLE 3 ABOUT HERE]

[17] The number of observations fluctuate over time because of the varying number of months used in estimating bunching around each threshold. The numbers of unique employees and option-employee combinations both decrease over time because of quitting.

For Plan 1 owners, excess quitting after the vesting dates ranges from 0.20 to 0.54 percentage point. The bunching effects are statistically significant for the first two vesting dates, even though graphically there's also increased quitting after the third vesting date. The average monthly quitting rate of Plan 1 owners is 0.226% before vesting dates. Hence the bunching effects are also economically significant. The affected range estimates remain fairly stable across the thresholds: the missing mass appears 3-5 months before option vesting; the excess quitting concentrates in the 1-3 months after vesting. The asymmetry of affected ranges is quite intuitive: those who delay quitting because of the vesting schedule are likely planning it months ahead of time and would want to quit as soon as their options vest.

Plan 2 owners see more variation in the size of bunching effects across different vesting dates. The excess quitting after $t = 24$ is statistically indistinguishable from zero; those after $t = 36$ and $t = 48$ are 0.74 and 0.28 percentage points, respectively. The affected ranges are slightly smaller than those for Plan 1. One possible reason is the lower *realized* value of Plan 2 options (as shown in the bottom panel of Figure 1 and Figure A2). Option owners delay quitting in order to reap the gain of options that will soon vest. Thus lower-value options provide weaker incentives for such delay; underwater options provide no (short-term) incentive at all. That Plan 2 options are granted from 2008 onward exposes them to greater shocks in the 2008-09 financial crisis. We explore this mechanism further in Section 4.3.2.

Graphical representation of estimated bunching effects. Figures 3 and 4 illustrate the baseline estimates. The three plots in each figure correspond to the three vesting dates. Take the top panel in Figure 3, for example. The estimated counterfactual distribution of quitters (dashed line) closely resembles the observed one (solid line), especially considering the small number of data points used in its estimation. The estimated affected range runs from months 9 to 14. The figure highlights a missing mass of quitters before option vesting (months 9-11) and excess mass after option vesting (months 12-14).

[FIGURES 3 and 4 ABOUT HERE]

Discussion of the baseline estimates. Before proceeding to explore the mechanisms of retention effects and test for confounding factors, we discuss the baseline estimates and their interpretation.

First, recall that the treatment is defined as the *local* effect of the closest vesting date. In reality, vesting dates farther into the future might also have an impact if option owners are sufficiently forward-looking. By estimating bunching at the 3 vesting dates separately, we already allow the responses to vary by the remaining planning horizon (or frictions of optimization regarding different vesting dates). That said, decision-making over a longer horizon faces increasingly more complications, such as the the evolving cost of staying in the current firm, the time sensitivity and uncertainty of outside job offers, etc. Empirically, the estimated affected ranges are only 2-6 months. It is thus not clear whether vesting dates beyond the maximum affected range of 12 months have any first-order impacts. For this reason, we continue to interpret the bunching effects as capturing primarily the local effects of the closest vesting dates.

Second, the bunching effects we find are most likely a *lower-bound* estimate of the total retention effects. Some owners could have given up quitting because of the options but are observationally equivalent to those who never considered quitting even without options. As a crude measure of such extensive margin responses, we compare the raw turnover rates of owners and non-owners in Table A2. For employees at all levels, option owners display substantially lower turnover than non-owners. The endogeneity of option ownership can generate the same difference. However, endogeneity alone is hard to explain why the gap is more pronounced shortly after options are granted (within 1-3 years).[18] Thus, there is room for sizable extensive margin responses by option owners who give up plans to quit altogether.

Third, the counterfactual distributions are estimated using a small number of data points, resulting in some noisy estimates of the bunching effects. The small sample is due to the necessary aggregation of the raw data to the month level. That we estimate bunching at different notches separately further shrinks the sample size. To address this issue, an alternative is to pool data from all the months and jointly estimate the bunching at the three notches. Doing so moderately increases the sample size, but requires the assumption that there is one common counterfactual distribution

[18] If, for example, employees who are better matches with the firm are more likely to receive options, then the match quality would have to *decline* over time in order for the turnover gap to shrink.

over all months after the grant date. We report the estimates using this approach in the Appendix (Figure A3) and find them to be qualitatively similar to the baseline results.

4.3 Mechanism and heterogeneous responses

The baseline estimates showed significant bunching effects at the vesting dates. We now test whether the bunching reflects the retention effects or are driven by confounding factors that coincide with options vesting. We first examine the option exercising patterns and discuss the channels through which vesting dates affect turnover. We then examine how the retention effect varies with the value of options. We then show that bunching is not the artifact of "reference point" effects such as certain months of the year or the end of contract years. We also do a falsification test on the sample of option owners who leave the firm involuntarily and thus have little control over the timing of exits. We conclude the section with an exploratory discussion of option owners who quit "too early," shortly before the vesting dates.

4.3.1 Mechanism of retention: quitting, vesting, and option exercising

In our bunching framework, option owners delay quitting because of the discontinuous drop in the opportunity cost of quitting once options vest. It is thus essential that option owners do want to exercise the options when they can. Without the intention of exercising options, the vesting dates will not affect quitting decisions at all. We verify this assumption with data on option transactions. Figure 5 tracks the percentage of owners who exercise *all* of their newly vested options. The top panel shows discontinuous increases immediately after the vesting date. That is, not only do Plan 1 owners want to exercise the options, they tend to do so as soon as possible. The bottom panel shows a similar pattern for Plan 2, except for a slight delay in the timing of exercising.[19]

[FIGURE 5 ABOUT HERE]

[19] Even when an option owner quits, her vested options can still be exercised within 90 days. This delay is perhaps driven by the slower and more uncertain appreciation of Plan 2 options, as shown in Figure A2. Option owners might have to wait for a better spot market before exercising their vested shares.

Figure 6 corroborates the above with the exercising patterns of option owners who do quit. The horizontal axes show time relative to a quit. Month 0 is when quitting happens; month s is the sth month after quitting. We track how the cumulative fraction of options exercised change over time. First, the exercising patterns are consistent with the firm's policy, reassuringly. The exercised fraction is well below 100%, since the unvested options revert to the firm when the owner quits. It also stays unchanged after $s = 2$ because the vested options can only be exercised within 90 days of quitting. Second, the fraction of options that are exercised remains small, until about half a year before quitting. Then it increases rapidly and approximately doubles over months $s \in [-7, 3]$. This rapid increase, coupled with Figure 5, indicates that option exercising tends to concentrate in months before quitting, and that vesting dates have substantial impacts on the extent of option exercising. Hence, it is more likely the vesting of options, rather than other factors, that drives the bunching effects we document.

[FIGURE 6 ABOUT HERE]

4.3.2 Option value and the magnitude of retention effects

Our hypothesized mechanism suggest that bunching arises because option owners alter the quitting time to minimize the cost of foregone unvested options. An immediate corollary is that the magnitude of bunching will vary with the value of options: pushing off quitting until options vest is more profitable when the options are more valuable. We now test this prediction formally.

[TABLE 4 ABOUT HERE]

We compare the monthly quitting rate of option owners and matched non-owners in Table 4. Each observation is an employee-month. The dependent variable is whether employee i quits in month t. The variables of interest are four measures of option value: column (1) examines the value of options that are vested *and* exercised; column (2) that of options vested but not yet exercised; column (3) that of options not vested yet; column (4) that of options that are not exercised, which include unvested options and vested but not exercised options.

Column (1) shows a positive correlation between the value of *exercised* options and quitting. That is, option owners who have already pocketed larger gains (either in the current month or in the past) are more likely to quit. Whether the high-value options are exercised in the month of quitting or prior to that, it is consistent with our hypothesis that employee quitting decisions are made under the goal of minimizing money left on the table. In contrast, column (2) shows that higher values of vested options that have not been exercised discourages quitting. Similarly, column (3) shows an even larger effect for the value of unvested options. Both results support our hypothesis that it is the value not yet reaped by option owners that motivate them to delay quitting, thereby keeping them in the firm for longer. Finally, column (4) uses a measure of option value that is the combination of those in columns (2) and (3) and documents a similar effect.

Overall, the results in Table 4 show that the retention effects are larger when the value not yet captured by the option owners is higher, whereas the value of options already exercised have the opposite effect.

4.3.3 Potential reference point effects

We now test for confounding factors of bunching, with a focus on "reference point" effects. Reference points are times when quitting increases systematically for reasons other than option vesting. They could arise for various reasons, such as seasonality, institutional rules, or social norms and behavioral factors (Kleven, 2016). Reference points, if present and coinciding with the vesting dates, can also generate excess quitting and thus result in the overestimation of retention effects.

Of particular relevance to our setting is the possibility of quitting at contract termination. The firm's employment contract terms are usually multiples of 12 months (i.e., 1 year, 2 year, 3 years, etc.). If quit is more likely to happen upon contract termination, then the end of contract years will become a reference point.

To test whether contract termination is driving the results, we compare the distribution of option owners' and non-owners' quitting over the contract year in the top panel of Figure 7. While non-owners tend to quit shortly after completing a contract year, option owners do not show such

tendencies. In fact, the latter are almost equally likely to quit in any month of the contract year. This contrast rejects the hypothesis that bunching is driven by contract termination. More importantly, it suggests that the baseline bunching estimates could be understated because the retention effects are partially offset by the reference point effects.

[FIGURE 7 ABOUT HERE]

To test for other types of potential reference points, we also compare the quitting distribution over the calendar year in the bottom panel of Figure 7. Seasonality is essentially nonexistent in the quitting time of non-owners. But some months do see slightly more quitting of option owners. We formally test reference point effects by conditioning the empirical distribution of quitters on fixed effects of months that are potential reference points.[20]

We re-estimate the bunching model based on the adjusted empirical distribution. Results are reported in Table 5 and plotted in Figure 8. We find that Plan 1 affected range estimates do not change qualitatively after controlling for potential reference points. Estimates on the excess mass do change slightly in size but remain significant. Estimates on Plan 2 owners are similar, except that the bunching effect at the first vesting date (month 24) remains absent. One possibility is that the realized value of Plan 2 options have a lower mean and larger variance. When the option values are too low, the latent bunching effects might be more than offset by the contract year-end effects.

[TABLE 5 AND FIGURE 8 ABOUT HERE]

Taken together, we find that the bunching effects mostly persist after accounting for various types of potential reference points. This shows that the documented bunching is mainly driven by the retention effect of options rather than reference points.

[20] We include month fixed effects for February, May, June, and September. June is included because it concludes the first half of the year and precedes the entry of new graduates. We also include fixed effects of the first, second, and third months after contract year ends to counteract the potential underestimation bias. Alternatively, we can include fixed effects for *each* calendar month and/or contract year month. Doing so will produce qualitatively similar, but noisier results due to the loss of statistical power.

4.3.4 Falsification test: absence of bunching for involuntary exits

We now falsify the identification strategy using involuntary exits, which include being fired or laid off. If bunching is driven by confounding factors that are orthogonal to option vesting, then it will likely show up among involuntary exits as well. If bunching is indeed due to the retention effect of options, however, then it would not be present in the distribution of involuntary exits.

We plot the raw distribution of involuntary exits of option owners over time in Figure A4 and find no discernible bunching around the vesting dates. We then formally estimate the bunching model and plot the estimates in Figure 9. Due to the small number of involuntary exits, we set the affected ranges to be the same as those in the baseline estimates. The estimated counterfactual distributions fit the overall distribution fairly well. But once again, we find no bunching at the vesting dates. The estimated excess mass is either not significantly different from 0 or negative. The absence of bunching among involuntary exits further rejects the hypothesis that the baseline bunching effects are driven by confounding factors.

[FIGURE 9 ABOUT HERE]

4.3.5 Heterogeneous responses: "early" and delayed quitting

We conclude this section by going beyond the bunching framework and examining the small but non-negligible group of option owners who quit shortly *before* the vesting dates. Because all unvested options revert to the firm, quitting early effectively leaves money on the table—large amounts of money if the option values are high. We study the characteristics of early quitters and explore what drives the curious timing of their exits. In particular, we are interested in the possibility of optimization frictions, which could limit the retention benefit of options.

We define "early quitters" as those who quit within the affected range but before options vest, i.e., up to z_{-}^{*} months before the vesting date. Of all early quitters, we pay special attention to a subgroup who quit in the month right before option vesting and compare their characteristics

with those who quit within in z_+^* months after the same vesting date ("delayed quitters").[21]

[TABLE 6 ABOUT HERE]

Table 6 reveals two major differences between early quitters and delayed quitters. First, early quitters hold options of lower relative value. The relative value of options is 5-6 times their annual base wage, whereas it is over 7 times the annual base wage of delayed quitters. Early quitters also earn moderately higher wages and hold higher-level positions. This is especially true for early quitters who leave within one month of option vesting. Second, although the overall years of experience are similar, early quitters have less *firm-specific* experience. Other than these, early quitters do not differ in gender, age, or education attainment.

Taken together, it is unlikely that early quitting is driven by a financial illiteracy or optimization frictions. Instead, a likely rationale for quitting early is better outside offers and/or a greater inclination to leave. The higher wages and job levels of early quitters reflect stronger abilities; the shorter in-firm experience suggests either more frequent job switching or weaker attachment. Either way, there is more likely to be an outside offer attractive enough for the early quitters to give up their unvested options. Note that although the estimates in Table 6 are noisy due to the very small sample size, they could be a helpful first step in exploring reasons behind seemingly irrational quitting decisions.

5 Benefit-Cost Analyses

In this section, we then gauge whether the benefit of retention effects is economically important for the firm. We answer this question in two steps. We first evaluate the quality of retained option owners, because the options' retention effect is only valuable to the firm if option owner perform better than the non-owners.[22] We then do a simple analysis of the benefit and cost of options.

[21] Recall that z_-^* and z_+^* mark the beginning and the end of the affected range, respectively.

[22] In general, retaining employees alone is valuable, since it reduces turnover, which has been known to be costly. If one ignores the opportunity costs, a firm may want to retain an employee with lower performance, as long as the performance is not so low that it becomes cheaper to fire her and recruit a replacement. But if options are not granted to everyone, doing so is not optimal: if an option owner performs worse than a non-owner, then the firm could have

5.1 Performance of option owners

Retaining employees does not generate value in itself. We need to examine whether the retained option owners are indeed valuable to the firm. To this end, we compare the performance review outcomes of option owners between 2004 and 2017 in Figure 10. Many employees at the firm are reviewed by their supervisors quarterly and receive a performance score. Employees on piece-rate wages, such as customer service representatives at the call center, do not have performance scores and are thus excluded from the sample. The performance score is a number between 0 and 1, which does not translate into a concrete or objective measure of productivity. Instead, it reflects an employee's relative performance among her peers at the same level. Thus we normalize the scores by quarter and job level.

[FIGURE 10 ABOUT HERE]

The top two panels of Figure 10 compare the performance among subgroups of option owners. For both Plan 1 and Plan 2 owners who never quit, their performance is at least as good as quitters up to 48 months after receiving options. After 48 months, never-quitters have significantly superior performance than quitters. Both the never-quitters' and quitters' performance dominate that of option owners who are fired.

The bottom panel further compares the performance of option owners and non-owners. Option-owning never-quitters and quitters outperform non-owners throughout most of the 14 years in our data. Note that the average normalized performance scores of non-owners stay below zero, which indicates subpar performance relative to others at the same job level.

[FIGURE 11 ABOUT HERE]

The above compares the performance of different groups of employees, all of whom are still in the firm and thus have the performance scores. Figure 11 further shows the difference in performance between those who quit and those who stay. We plot the average performance evaluation

generated more value by granting the options to the non-owner instead. Thus we argue that the benchmark should be the performance of non-owners when evaluating the retention benefits of BBSO.

scores over time, specifically before and after option vesting. Within each 12-month cycle, we observe that the average performance (normalized and adjusted for seasonality) *drops* systematically after option vesting. Recall that option owners tend to quit shortly after the vesting day. Thus the drop implies that those who quit have *better* performance than those who stay in the firm. That is, without the options, the highest-performing employees would have quit sooner. All else equal, granting options to these employees (instead of others) generates the most value to the firm, which seems to be optimizing already.

The comparisons above show that the option owners, in particular those who delayed quitting due to option vesting, are in general more valuable to the company, whether they are retained for the short term or for longer. This result establishes our premise for assessing the retention benefit of BBSO.

5.2 Gauging BBSO benefits

We now evaluate whether BBSO generates enough benefits to justify its cost. The cost of granting BBSO to employee i, c_i, is simply measured by the Black-Scholes value (BSV). The benefit of BBSO, on the other hand, consists of at least three components: the savings in the wages paid to option owners, the improvement in output by option owners, and the value of retaining productive option owners. The first two have received more attention in the literature, whereas the magnitude of retention benefits are less well understood.

Wage savings. The first benefit of BBSO comes from its role as an alternative form of remuneration. Without options, the firm might pay higher wages to option owners, who we have shown to have superior performance. Estimating this counterfactual wage is not a straightforward exercise given the endogeneity of wages. We propose an informative, lower-bound estimate of the counterfactual wage using option owners' wages before receiving BBSO and the wage trajectory of non-owners who are otherwise similar.[23] The wage savings is then defined as the difference between option owners' counterfactual and observed wages. Note that assuming the same wage

[23] See Appendix C.2 for details.

trajectory for owners and non-owners *under-estimates* the counterfactual wage and therefore the wage savings. This is because the unobserved characteristics that increase the probability of receiving BBSO are also likely to lead to faster wage growth *in the absence of BBSO*. For example, Table A3 shows that option owners are more likely to be promoted and therefore receive higher wages.

Improved productivity. The second type of BBSO benefits is the improvement in option owners' output. To quantify worker output, we start by regressing non-owner wages on their performance scores and a rich set of observable characteristics. We then predict "fair wages" for option owners, assuming that firms set wages to compensate for employee output. The improvement in productivity is then defined as the difference between option owners' output and the average output of an otherwise similar non-owner.[24]

Retention benefits. The retention benefits of BBSO also come in various forms. First and foremost, the firm receives additional output from option owners who *delayed* quitting because of option vesting. The output during the few months close to the vesting date would have been lost without BBSO. As a lower-bound measure of this benefit, we only consider the output within the affected range by option owners who *delayed* quitting.[25]

But among the option owners who never quit, some could have given up quitting only because of BBSO. To account for the benefits of keeping these employees, we propose an upper-bound measure of the retention benefits. It includes the lower-bound measure, plus the output of option owners after they show an intention to quit (but have not yet done so). We identify the intention to quit from the exercising of a large fraction of one's vested options, in light of the findings in Figure 6.[26] Note that even the upper-bound measure only accounts for a small fraction of retained option owners' output, namely that produced after a clear intention to quit emerges. In the absence of

[24] See details in Appendix C.3.

[25] That is, option owners who quit after an option vesting date but still within the affected range. They are also the group captured by the bunching effects.

[26] The rationale for this measure is that after an intention to leave emerges, it is the vested but not yet exercised options that are keeping an owner in the firm. We thus regard her output during this time as part of the retention benefit of BBSO. See Appendix C.4 for details.

BBSO, these employees could have quit long before.

5.3 BBSO benefit-cost analysis

Now we compare the cost and various types of benefits of BBSO by job level in Table 7. For Plan 1 owners, the average cost of granting options is 317.26 thousand yuan per person. This cost is hardly offset when only considering the wage savings, which are 51.10 thousand yuan and 98% of the granting cost for the average option owner. This reflects a strong role of Plan 1 options as a substitute for wage compensation, which is again related to the plan's being offered when the company was rapidly growing.

[TABLE 7 HERE]

In addition to the wage savings, there are also substantial retention benefits. The estimated average retention benefits range from 170.35 to 362.43 thousand yuan, depending on which measure of benefit is used. The total benefit of BBSO is 2.33-4.59 times the cost of granting it to employees. Plan 1 owners in lower-level jobs have a substantially higher benefit-cost ratio, which is largely the artifact of high wage savings. The benefit-cost ratio is lower for level 5-10 owners, but it is still in the range of 1.40 to 2.81.[27]

The benefit-cost comparison for Plan 2 owners is qualitatively similar. For Plan 2 owners as a whole, wages savings compensate for a smaller fraction of granting cost than for Plan 1 owners. For the average owner, the wage savings are about 70% of the granting cost. Once retention benefits are accounted for, the total benefits outweigh the cost, as is the case with Plan 1 options. The ratio of total benefits to the cost is 1.08-3.27, which is still much higher for lower-level owners.

The benefit-cost comparison shows that the benefit of granting options is sufficient to justify the cost *only if the retention effects are considered*. In fact, the retention benefits make up a sizable fraction of total benefits to the firm. For options granted at the early stage of the firm (Plan 1), the retention benefits *alone* can be large enough to justify the granting cost. Hence, accounting for the retention effects of options can avoid the potential underestimation of the benefit-cost ratio, thereby helping

[27] We report the detailed breakdown of BBSO costs and benefits by job levels in Table A4.

to reconcile the popularity of options and the high granting costs.

6 Concluding Remarks

In this paper, we use the bunching design to identify the causal effect of employee stock options on turnover. We leverage unique, granular panel data and exploit the discontinuous changes in the cost of quitting around option vesting dates. We find significant and robust bunching after option vesting dates, which quantifies the retention effect of option ownership. We also show that the retention effects are nonexistent for involuntary exits, and that they are unlikely to be driven by confounding factors such as reference points.

The immediately relevant question stemming from the findings is whether the benefit of options is large enough to justify their costs. To that end, we discuss the performance of option owners and find that the retained employees are indeed valuable to the company. We then do a benefit-cost analysis and show that the retention benefits contribute greatly to the total benefit of options, which more than justifies the granting cost only when the retention benefits are accounted for.

The bunching design that we employ in this paper is known for its strong internal validity that identifies the treatment effect. While testing the external validity is outside the scope of this paper, there are several reasons why we believe the results shed useful light on the retention benefits of BBSO in a more general setting. First, we are using data on a fast-growing, NASDAQ-listed firm in the high-tech/service industry, which is representative of firms that are more likely to include stock options in their remuneration packages. Second, the firm's workforce is primarily high-skilled and financially savvy. Thus the retention effects identified using this group of workers are perhaps close to the upper end of the spectrum. For option owners who give up quitting due to BBSO (but are not separately identifiable), we have shown the disparities in turnover rates between option owners and non-owners to gauge the extent of such an "extensive margin" response. This response is also likely to be on the larger side considering the higher turnover rate in this industry.

Overall, our findings contribute to the understanding of the puzzling popularity of broad-based employee stock options in spite of high granting costs. Significant retention benefits provide an additional reason for the prevalence of options, which is not adequately explained by the incen-

tivization hypothesis. Bunching at the vesting dates also suggests why there is a vesting schedule in the first place. Given that options hardly induce long-term efforts, having a vesting schedule can at least retain option owners for the short term.

References

Aggarwal, R. K. and A. A. Samwick (1999). The Other Side of the Trade-off: The Impact of Risk on Executive Compensation. *Journal of Political Economy 107*(1), 65–105.

Aldatmaz, S., P. Ouimet, and E. D. Van Wesep (2018). The option to quit: The effect of employee stock options on turnover. *Journal of Financial Economics 127*(1), 136–151.

Babenko, I., M. Lemmon, and Y. Tserlukevich (2011). Employee stock options and investment. *The Journal of Finance 66*(3), 981–1009.

Balsam, S., R. Gifford, and S. Kim (2007). The effect of stock option grants on voluntary employee turnover. *Review of Accounting and Finance 6*(1), 5–14.

Barron, J. M. and R. Waddell (2003). Hard Work Versus Good Intentions: Stock Options as Compensation. *Purdue University and University of Oregon Working Paper*.

Bergman, N. and D. Jenter (2007, June). Employee Sentiment and Stock Option Compensation. *Journal of Financial Economics 84*(3), 667–712.

Carter, M. E. and L. J. Lynch (2004, February). The Effect of Stock Option Repricing on Employee Turnover. *Journal of Accounting and Economics 37*(1), 91–112.

Core, J. E. and W. R. Guay (2001). Stock Option Plans for Non-executive Employees. *Journal of Financial Economics 61*, 253–287.

Cowgill, B. and E. Zitzewitz (2014). Incentive Effects of Equity Compensation: Employee-level Evidence from Google. *Dartmouth Department of Economics Working Paper*.

Dube, A. and R. B. Freeman (2010). Complementarity of Shared Compensation and Decision-Making Systems: Evidence from the American Labor Market. In D. Kruse, R. B. Freeman, and J. Blasi (Eds.), *Shared Capitalism at Work: Employee Ownership, Profit and Gain Sharing, and Broad-based Stock Options*, Chapter 5, pp. 167–199. University of Chicago Press.

Dube, A., L. Giuliano, and J. S. Leonard (2019). Fairness and frictions: The impact of unequal raises on quit behavior. *American Economic Review 109*(2), 620–63.

Feng, G. and P. Zhu (2022, October). Employee Share Plans in China: Regulatory Overview. *Thomson Reuters Practical Law* (Retrieved December 20, 2022, from https://uk.practicallaw.thomsonreuters.com/9-507-4557).

Gopalan, R., S. Huang, and J. Maharjan (2014). The role of deferred pay in retaining managerial talent. *Working Paper, Washington University in St. Louis*.

Hall, B. J. and K. J. Murphy (2003). The Trouble with Stock Options. *Journal of Economic Perspectives 17*(3), 49–70.

Hausman, C. and D. S. Rapson (2017). Regression Discontinuity in Time: Considerations for Empirical Applications. *NBER Working Paper 23602*.

Huddart, S. and M. Lang (1996, February). Employee Stock Option Exercises an Empirical Analysis. *Journal of Accounting and Economics 21*(1), 5–43.

Imbens, G. and T. Lemieux (2008). Regression Discontinuity Designs: A Guide to Practice. *Journal of Econometrics 142*(2), 615–635.

Jochem, T., T. Ladika, and Z. Sautner (2018). The retention effects of unvested equity: Evidence from accelerated option vesting. *The Review of Financial Studies 31*(11), 4142–4186.

Kedia, S. and A. Mozumdar (2002). Performance Impact of Employee Stock Options. *Rutgers Business School Working Paper*.

Kim, E. H. and P. Ouimet (2014). Broad-based employee stock ownership: Motives and outcomes. *The Journal of Finance 69*(3), 1273–1319.

Kleven, H. J. (2016). Bunching. *Annual Review of Economics 8*, 435–464.

Kleven, H. J. and M. Waseem (2013). Using Notches to Uncover Optimization Frictions and Structural Elasticities: Theory and Evidence from Pakistan. *The Quarterly Journal of Economics*, 669–723.

Ladika, T. and Z. Sautner (2019, 07). Managerial Short-Termism and Investment: Evidence from Accelerated Option Vesting*. *Review of Finance 24*(2), 305–344.

Lazear, E. P. (2004). Output-Based Pay: Incentives, Retention or Sorting? *Research in Labor Economics 23*, 1–25.

Li, Q. and H. Zhang (2021). The causal effect of option ownership on employee performance: Empirical evidence from personnel data. *Labour Economics 69*(C), S0927537121000014.

Mehran, H. and J. Tracy (2001). The Impact of Employee Stock Options on the Evolution of Compensation in the 1990s. *Economic Policy Review 3*, 17–34.

Morgan Stanley (Feb 17, 2022). New Morgan Stanley at Work Report Reveals Companies Reimagining Equity Compensation to Compete for Talent Amid Great Resignation. (Retrieved December 20, 2022 from https://www.morganstanley.com/press-releases/report-reveals-companies-reimagining-equity-compensation).

National Bureau of Statistics of China (2022, October). Number of companies listed on the chinese stock exchanges from 2011 to 2021 [graph]. *In Statistica.* (Retrieved December 20, 2022, from https://www-statista-com.libproxy.lib.unc.edu/statistics/225725/number-of-companies-listed-on-the-chinese-stock-exchange/).

National Center for Employee Ownership (December 2021). Employee Ownership by the Numbers. *NCEO Web Article* (Retrieved December 20, 2022 from https://www.nceo.org/articles/employee-ownership-by-the-numbers).

Oyer, P. and S. Schaefer (2005, April). Why Do Some Firms Give Stock Options to All Employees? An Empirical Examination of Alternative Theories. *Journal of Financial Economics 76*(1), 99–133.

Oyer, P. and S. Schaefer (2006). Costs of Broad-Based Stock Option Plans. *Journal of Financial Intermediation 15*(4), 511 – 534.

Vance, M. (2013). Broad-based equity compensation, employee turnover, and unit performance. *Working Paper, University of Pennsylvania*.

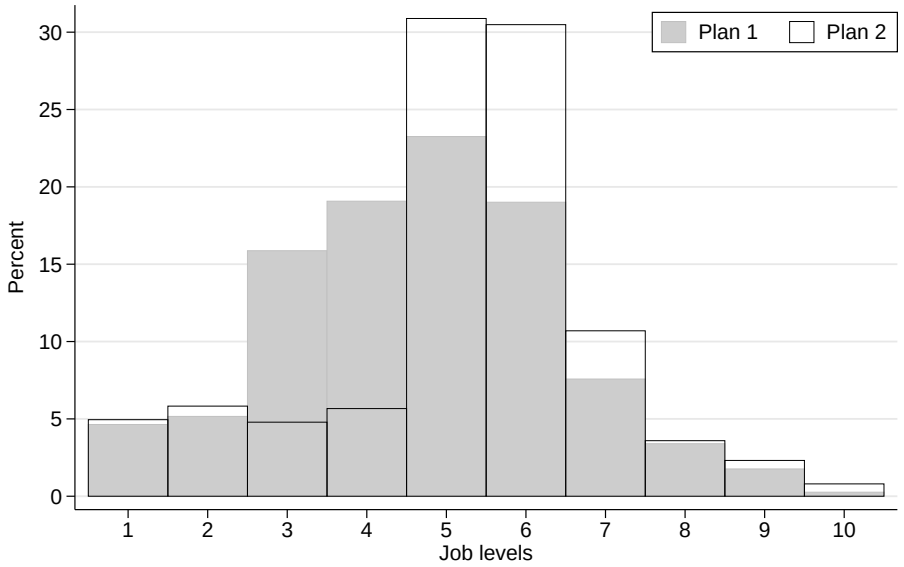
Zhang, G. (2006). Market valuation and employee stock options. *Management Science 52*(9), 1377–1393.

	Level 1		Level 2		Level 3		Level 4		Level 5		Level 6		Levels 7-10	
	N	%	N	%	N	%	N	%	N	%	N	%	N	%
Admin	199	10.33	349	47.56	221	91.79	134	95.14	159	99.31	75	97.77	32	89.02
Product	10818	4.16	1092	25.47	455	57.19	240	74.61	228	95.34	36	97.08	3	92.94
IT	139	1.61	115	5.44	229	17.94	131	41.68	130	94.14	31	98.72	2	100.00
Sales	12827	4.11	201	27.90	37	34.52	4	52.74	-	-	-	-	-	-
All	23983	4.24	1757	31.13	942	63.34	509	77.05	517	96.54	142	97.79	37	89.67

Table 1: Option ownership rates by job level and department

NOTES: This table reports the number of employees (N) and the fraction of option ownership $(\%)$ by job level and type of department (administrative, product management, sales and customer services, and IT). The rate of option ownership is weighted by the number of months an employee worked at the firm. Levels 7-10 are combined due to sample size and privacy concerns. There are no jobs at level 5 or above in the sales department, which mostly consists of entry-level employees working at the call center as customer service representatives.

	Plan 1				Plan 2			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Shares granted (in 1,000)	19.956	54.743	1.064	1344	14.409	53.455	0.016	1200
(Option value)/(annual base wage)	2.631	2.691	0.119	24.117	4.074	4.454	0.156	72.396
Monthly base wage (in 1,000 yuan)	6.686	5.016	0.5	49	9.832	6.796	0.629	60
1{have other options}	0.616	0.486	0	1	0.698	0.459	0	1
Distribution of job levels:								



Workers	625	674
Options	1,646	1,313

Table 2: Option value and owner characteristics on grant dates

Notes: All statistics are measured on the grant date. Option values are the Black-Scholes value, originally in US dollars and converted to real 2014 RMB yuan using the then-prevailing exchange rate and the province-level urban CPI.

Threshold month	Bunching (excess mass B^*)	Months retained ($\mathbb{E}(T)$)	Affected range		Sample size		
			Before (z_-^*)	After (z_+^*)	Observations	Employees	Option-employees
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Panel A. Plan 1				
12	0.36	2.06	3.00	2.00	35,138	625	1,647
	(0.10)	(0.61)	(1.46)	(0.79)			
24	0.20	0.28	2.00	3.00	25,796	588	1,569
	(0.11)	(0.95)	(1.63)	(0.93)			
36	0.54	2.61	5.00	1.00	46,473	550	1,488
	(1.94)	(0.51)	(0.82)	(0.31)			
			Panel B. Plan 2				
24	-0.07	1.00	1.00	1.00	39,098	674	1,313
	(0.13)	(1.05)	(1.34)	(0.68)			
36	0.74	2.96	4.00	2.00	24,421	581	1,180
	(0.15)	(0.51)	(1.23)	(0.76)			
48	0.28	1.00	1.00	1.00	22,924	544	1,097
	(0.09)	(0.48)	(1.64)	(0.80)			

Table 3: Joint estimates of bunching and the affected range: baseline specification

NOTES: Column (1) shows the threshold months (those with a vesting date) for which the results in other columns are estimated. Column (2) quantifies the magnitude of bunching, measured by the excess mass in the quitting distribution to the right of the threshold. Column (3) shows the expected length of retention due to option vesting in the threshold month. Columns (4)-(5) report the estimated affected range. z_-^* and z_+^* is the number of affected months before and after the vesting date, respectively. Columns (6)-(8) report the sample size used in the estimation for each threshold: the number of observations, the number of unique employees, and the number of unique option-employee combinations. Bootstrapped standard errors are in parentheses.

	Dependent variable: 1(Exit)			
	(1)	(2)	(3)	(4)
Vested and exercised value	.0027			
	(.0014)			
Vested but unexercised value		-.0034		
		(.0007)		
Unvested value			-.0048	
			(.0011)	
Unexercised value				-.0033
				(.0005)
Year-month FE	Yes	Yes	Yes	Yes
Location FE	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes
Job level FE	Yes	Yes	Yes	Yes
Job category FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Observations	373439	373439	373439	373439
Adjusted R^2	.0673	.0672	.0672	.0672

Table 4: Option value and retention effects

NOTES: The table examines the impact of various measures of option value on employee quitting. The sample is restricted to option owners and matched non-owners. Each observation is an employee-month. The variables of interest in columns (1)-(4) are: the value of options that are vested *and* exercised, that of options vested but not yet exercised, that of unvested options, and that of unexercised options (including both unvested options and vested but unexercised options). All option values are measured in millions of 2014 RMB yuan. The option value for non-owners are normalized to 0. All columns also included year-month, location, department, job level, job category, and individual employee fixed effects.

Threshold month	Bunching (excess mass B^*)	Months retained ($\mathbb{E}(T)$)	Affected range		Sample size		
			Before (z_-^*)	After (z_+^*)	Observations	Employees	Option-employees
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Panel A. Plan 1				
12	0.53	3.00	5.00	3.00	33,615	625	1,647
	(0.08)	(0.79)	(1.40)	(0.68)			
24	0.47	2.87	3.00	2.00	25,687	586	1,563
	(0.07)	(0.52)	(1.28)	(0.41)			
36	0.44	3.09	4.00	3.00	47,969	556	1,496
	(1.04)	(0.52)	(0.89)	(0.28)			
			Panel B. Plan 2				
24	-0.09	1.00	1.00	1.00	41,372	674	1,313
	(0.04)	(1.27)	(1.21)	(0.76)			
36	0.41	1.55	2.00	1.00	20,267	581	1,180
	(0.11)	(0.75)	(1.62)	(0.83)			
48	0.83	3.01	5.00	2.00	24,033	547	1,109
	(0.18)	(0.98)	(1.98)	(0.82)			

Table 5: Testing for reference points: bunching estimates with month fixed effects

NOTES: Column (1) shows the threshold months (those with a vesting date) for which the results in other columns are estimated. Column (2) quantifies the magnitude of bunching, measured by the excess mass in the quitting distribution to the right of the threshold. Column (3) shows the expected length of retention due to option vesting in the threshold month. Columns (4)-(5) report the estimated affected range. z_-^* and z_+^* is the number of affected months before and after the vesting date, respectively. Columns (6)-(8) report the sample size used in the estimation for each threshold: the number of observations, the number of unique employees, and the number of unique option-employee combinations. The econometric specification is the same as the baseline model, except we add fixed effects for February, May, June, and September, which are potential reference points in the calendar year; we also add fixed effects for the first, second, and third month after the end of contract years to avoid underestimating bunching, because part of the retention effects may be offset by the reference point effects *in the absence of options* in these months. Bootstrapped standard errors are in parentheses.

	(1) Early quitters $[Z^*-1, Z^*)$	(2) Early quitters $[Z^*-Z_l^*, Z^*)$	(3) Delayed quitters $[Z^*, Z^*+Z_u^*)$	(4) Difference (3)-(2)
(Option value)/(annual base wage)	5.821	5.301	7.239	1.938
	(1.066)	(0.840)	(1.865)	(2.796)
Monthly base wage (1,000 yuan)	13.065	10.570	10.807	0.238
	(1.681)	(1.126)	(0.909)	(1.518)
Job level	6.211	5.714	5.363	-0.352
	(0.355)	(0.290)	(0.210)	(0.358)
Experience	13.284	11.864	12.481	0.618
	(1.688)	(1.200)	(0.825)	(1.419)
Firm-specific experience	4.563	4.324	5.046	0.722
	(0.550)	(0.379)	(0.318)	(0.516)
Male	0.611	0.474	0.606	0.132
	(0.118)	(0.082)	(0.058)	(0.100)
Age	36.954	35.393	35.172	-0.221
	(1.643)	(1.158)	(0.720)	(1.299)
Years of schooling	16.000	15.914	16.017	0.103
	(0.364)	(0.270)	(0.190)	(0.322)
Observations	21	44	83	

Table 6: Testing for optimization friction: comparison of early quitters and other quitters

NOTES: Column (1) shows statistics of option owners who quit in the month preceding a vesting date, i.e. between month (z^*-1) and z^*. Column (2) shows statistics of option owners who quit within the affected range and before the vesting date, i.e. between month $(z^*-z_-^*)$ and z^*. Column (3) shows statistics of option owners who quit within the affected range but after the vesting date, i.e. those who are bunching in $[z^*, z^*+z_+^*)$. Column (4) reports the differences between (3) and (2). All standard errors are in parentheses. Option value is the Black-Scholes value of options in months before the vesting date and within the affected range. Option values are originally in US dollars and converted to real 2014 RMB yuan using the then-prevailing exchange rate. All money variables are adjusted for inflation using province-level urban CPI in China. Experience is estimated from employee age and education attainment. Firm-specific experience is the number of years an employee worked in the firm when she quits, which is readily available in the data.

	Benefits and cost (in $1,000)				Benefit-to-cost ratio				
		Wage	Retention benefits		Wage	Retention benefits		Total benefits	
	Cost	savings	Min.	Max.	savings	Min.	Max.	Min.	Max.
	(1)	(2)	(3a)	(3b)	$\frac{(2)}{(1)}$	$\frac{(3a)}{(1)}$	$\frac{(3b)}{(1)}$	$\frac{(2)+(3a)}{(1)}$	$\frac{(2)+(3b)}{(1)}$
Panel A. Plan 1									
All	317.26	51.10	170.35	362.43	0.98	1.35	3.61	2.33	4.59
Levels 1-4	90.83	123.67	81.41	265.03	2.38	1.31	4.81	3.69	7.20
Levels 5-10	465.56	3.57	228.59	426.22	0.03	1.37	2.79	1.40	2.81
Panel B. Plan 2									
All	538.72	82.98	124.52	487.77	0.70	0.38	2.57	1.08	3.27
Levels 1-4	131.23	334.18	-37.66	271.68	4.09	-0.67	3.94	3.42	8.02
Levels 5-10	611.64	38.12	153.49	526.35	0.10	0.56	2.33	0.66	2.42

Table 7: Benefit-cost analysis

NOTES: The left half of the table reports the benefits and costs estimates in thousand RMB yuan. "Cost" (1) is the average cost of granting BBSO to an employee. "Wage savings" (2) are the average estimated wage savings on the employee. (3a) and (3b) are the lower- and upper-bound estimates of the retention benefits, respectively. The right half of the table reports ratios of BBSO benefits (by type and in total) to the granting cost.

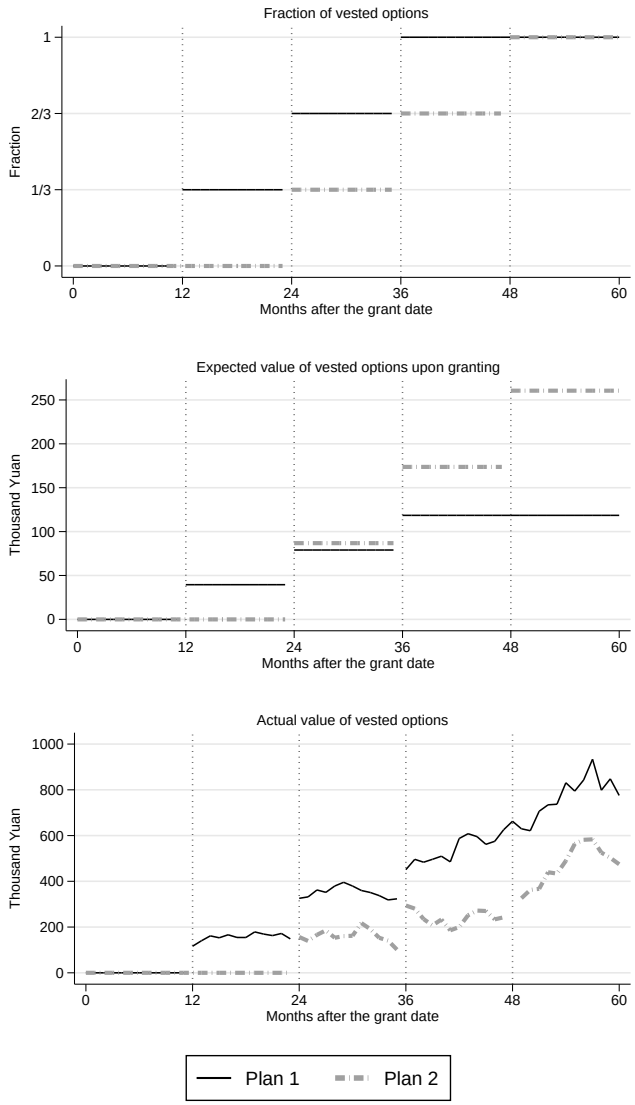


Figure 1: Option vesting schedules and values over time

NOTES: The horizontal axes show months after option granting. The grant dates are denoted as the beginning of month 0. Plan 1 options vest in three equal batches at the beginning of months 12, 24, and 36. Plan 2 options vest in three equal batches at the beginning of months 24, 36, and 48. Vesting dates are marked by dotted vertical lines. In all three panels, the solid (dash-dotted) line shows Plan 1 (Plan 2) averages. The top panel illustrates the vesting schedules by showing the fraction of vested options over time. The middle panel shows the expected value of vested shares, which is calculated by multiplying the number of vested shares at each point in time with the Black-Scholes value per share *on the grant date*. The bottom panel shows the actual value of vested shares by plotting their real-time Black-Scholes value in each month after granting.

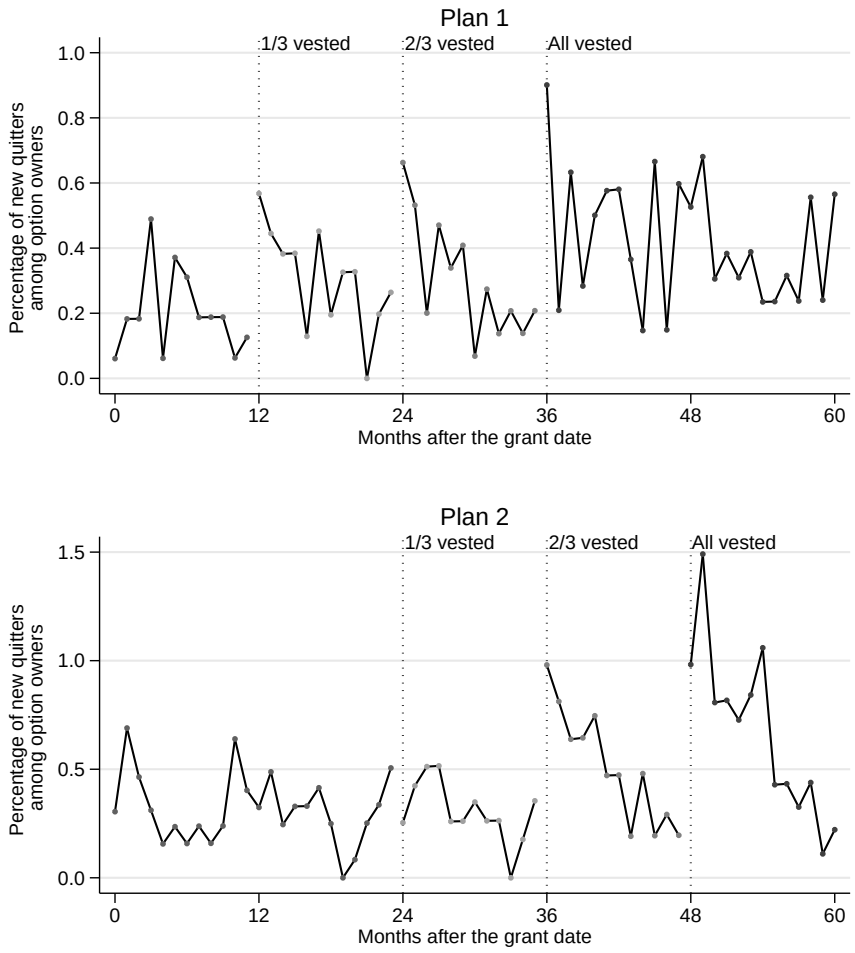


Figure 2: Preliminary evidence of bunching: monthly quitting rates of option owners

NOTES: The horizontal axes show months after option granting, with the grant date being the first day of month 0. Option vesting dates are marked by dotted vertical lines (months 12, 24, and 36 for Plan 1; months 24, 36, and 48 for Plan 2). The vertical axes show monthly quit rates (in %), calculated as the fraction of voluntary quitters in each month as a fraction of total initial option owners of that plan.

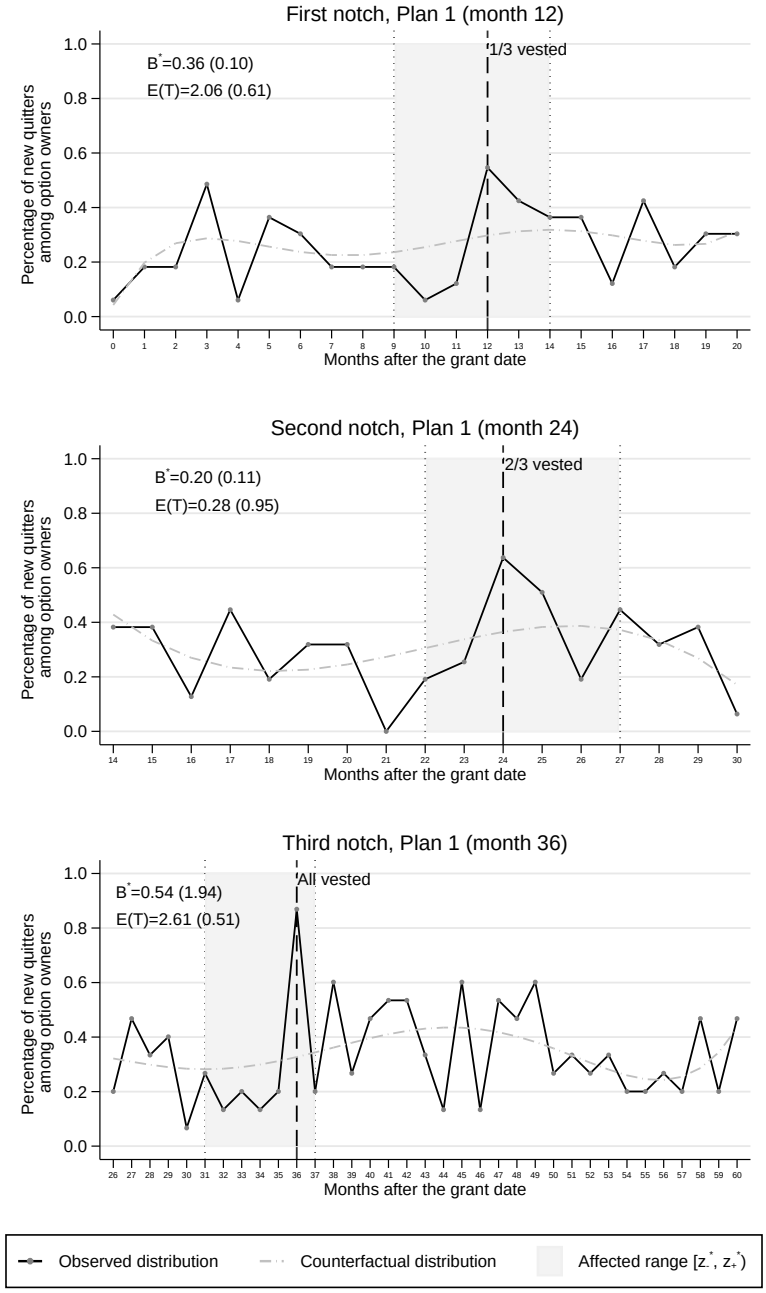


Figure 3: Baseline estimates of bunching: empirical and counterfactual distributions of quitters and the affected range (Plan 1)

NOTES: The horizontal axes show months after option granting, with the grant date being the first day of month 0. The three panels show bunching at the three vesting dates for Plan 1. The solid lines trace out the observed distribution of quitters. The dashed lines show the counterfactual distributions. The shaded area indicates the affected range. Sizes of bunching at the three vesting dates, measured by excess mass B^*, are reported at the top-left corner of each plot. The corresponding standard errors are in parentheses. Also reported are the expected number of months an option owner is retained, $E(T)$.

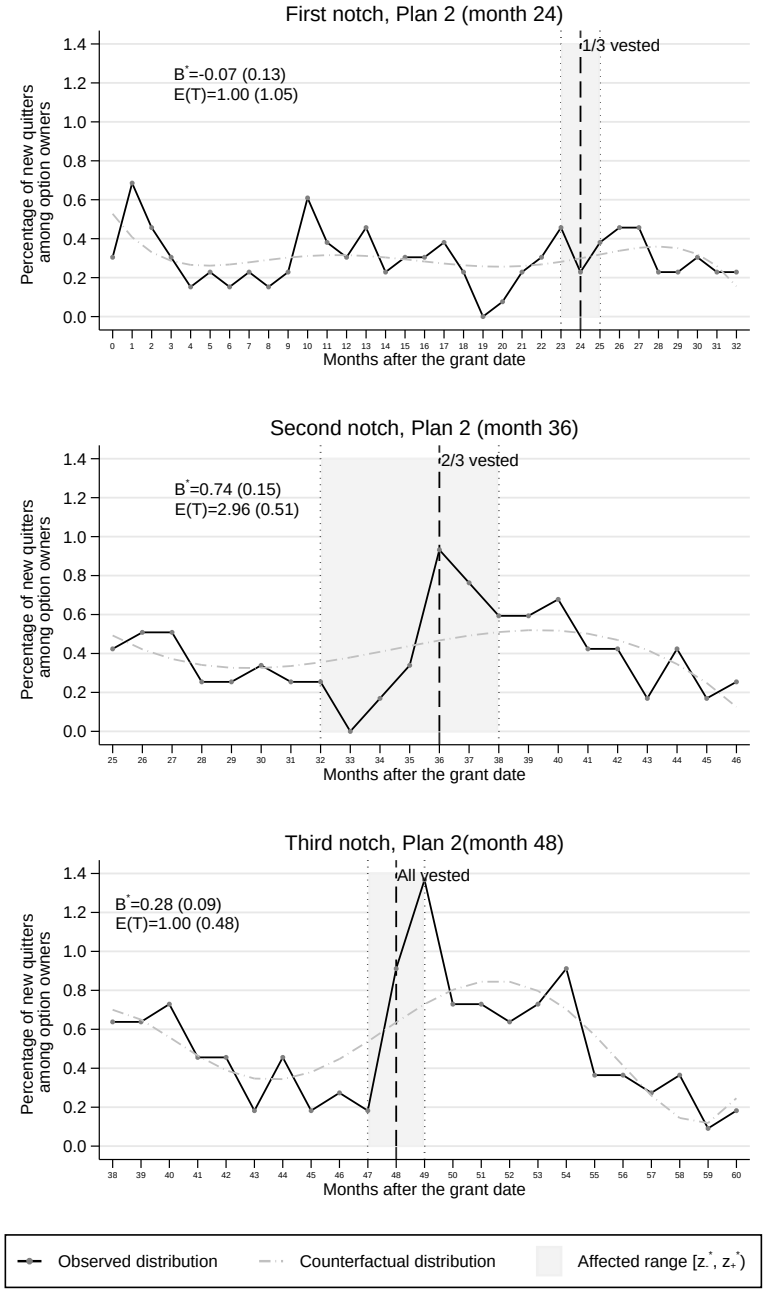


Figure 4: Baseline estimates of bunching: empirical and counterfactual distributions of quitters and the affected range (Plan 2)

NOTES: The horizontal axes show months after option granting, with the grant date being the first day of month 0. The three panels show bunching at the three vesting dates for Plan 2. The solid lines trace out the observed distribution of quitters. The dashed lines show the counterfactual distributions. The shaded area indicates the affected range. Sizes of bunching at the three vesting dates, measured by excess mass B^*, are reported at the top-left corner of each plot. The corresponding standard errors are in parentheses. Also reported are the expected number of months an option owner is retained, $E(T)$.

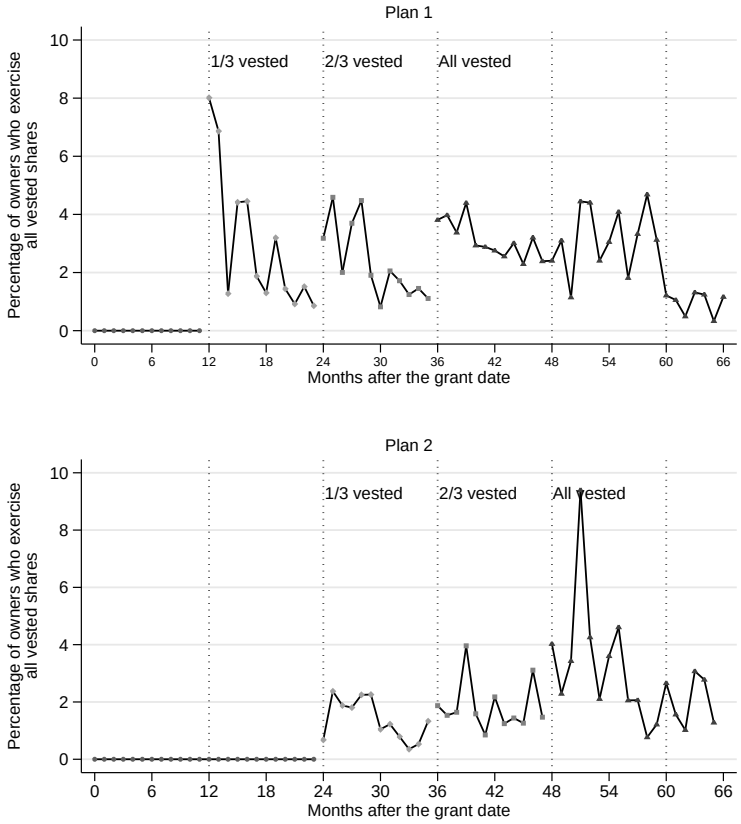


Figure 5: Percentage of option owners exercising all vested shares over time

NOTES: The solid lines plot the percentage of option owners exercising all vested options in each given month. It is the number of option owners who exercise at least the number of shares that have most recently vested, divided by the total number of option owners still in the firm in that month.

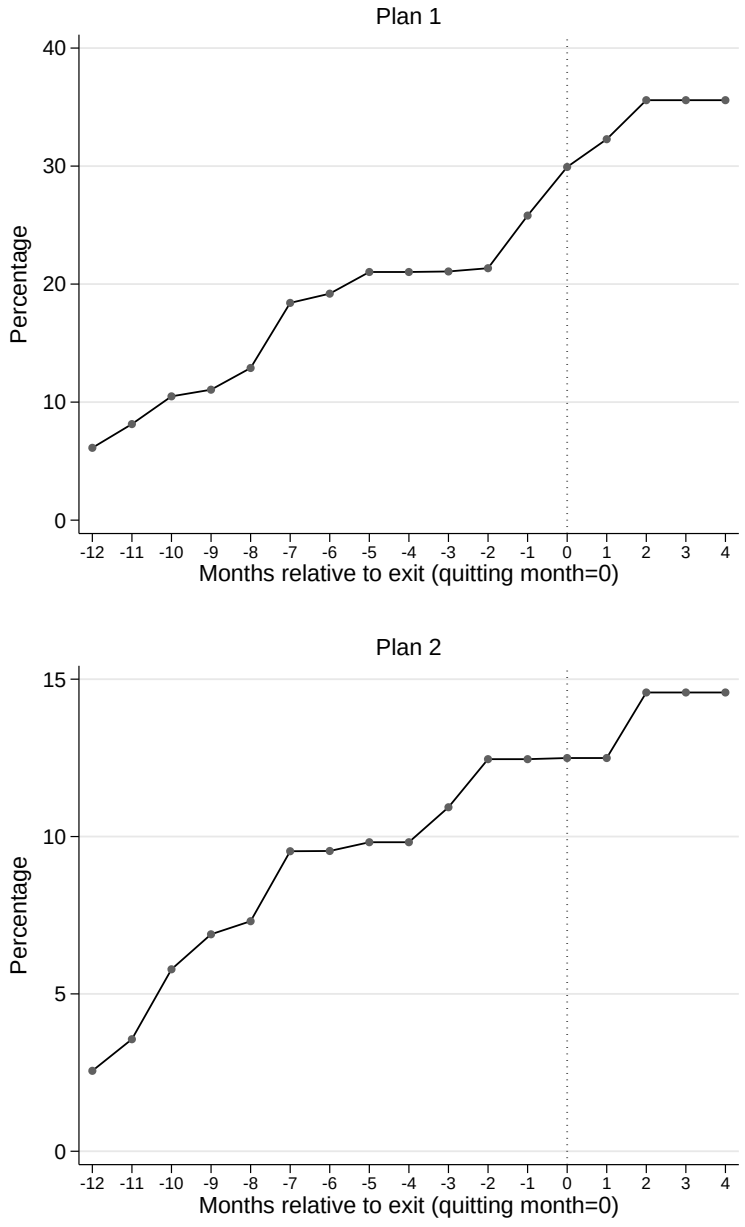


Figure 6: Cumulative fraction of exercised options by quitting owners

NOTES: The horizontal axes are months relative to an option owner's exit. Month 0 is the month of quitting; months with negative indices are those preceding the quit; months with positive indices are those after the quit. For option owners who quit, we track the cumulative number of options exercised as a fraction of total shares initially granted over time. The solid lines plot the fractions averaged over quitting owners. The fractions do not reach 100% because some option owners quit while still having unvested options, which revert to the firm immediately. The fractions remain unchanged after month 3 because quitting owners can exercise vested options only within 90 days after they quit.

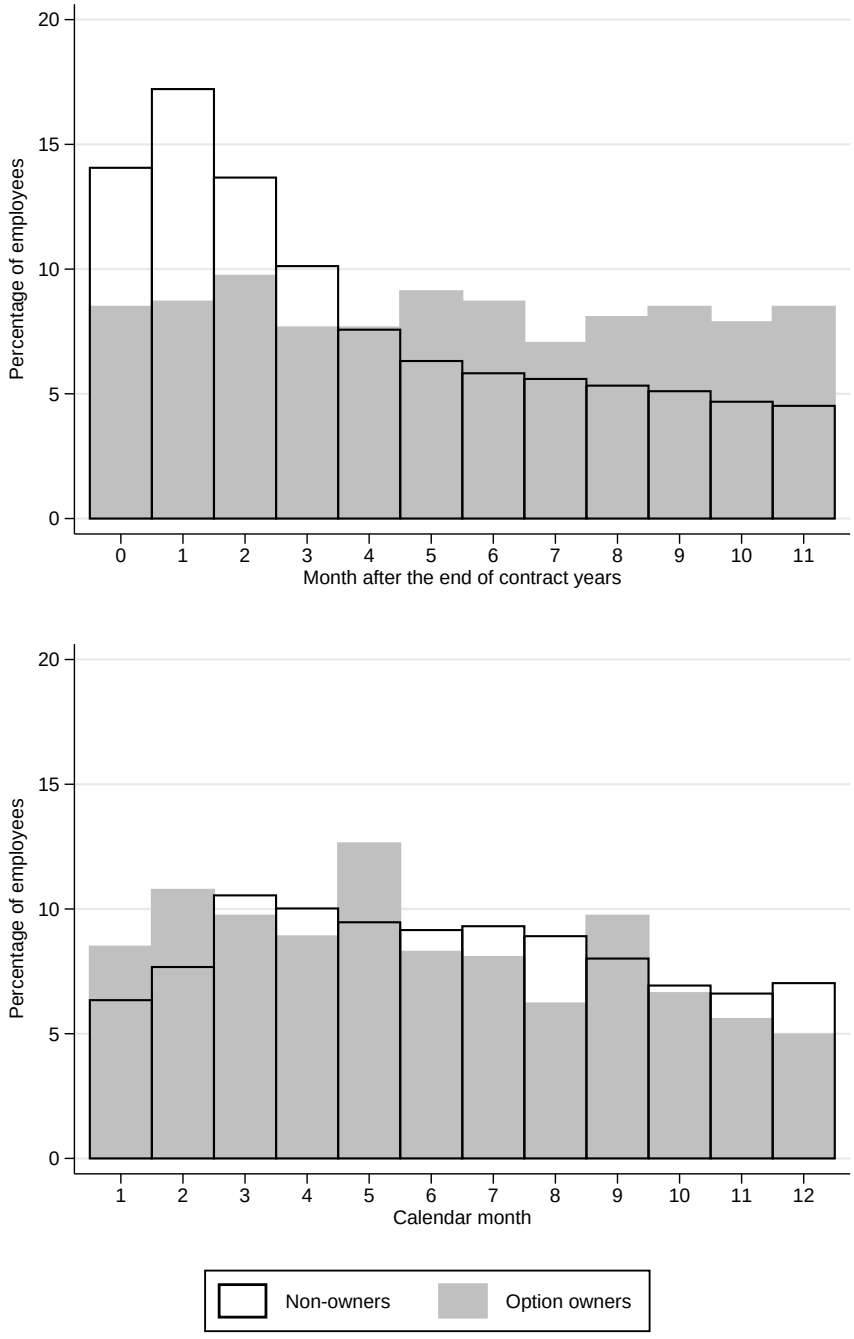


Figure 7: Testing for reference points: distribution of quitting within contract and calendar years

NOTES: The top panel plots the distribution of quitting time within a contract year, with the ending month of contract years being month 0, and the sth month of contract years being month s. Each shaded (unshaded) bar shows the fraction of option owners (non-owners) quitting in a given month among all owners (non-owners) who quit. Similarly, the bottom panel is the distribution of quitting time within a calendar year.

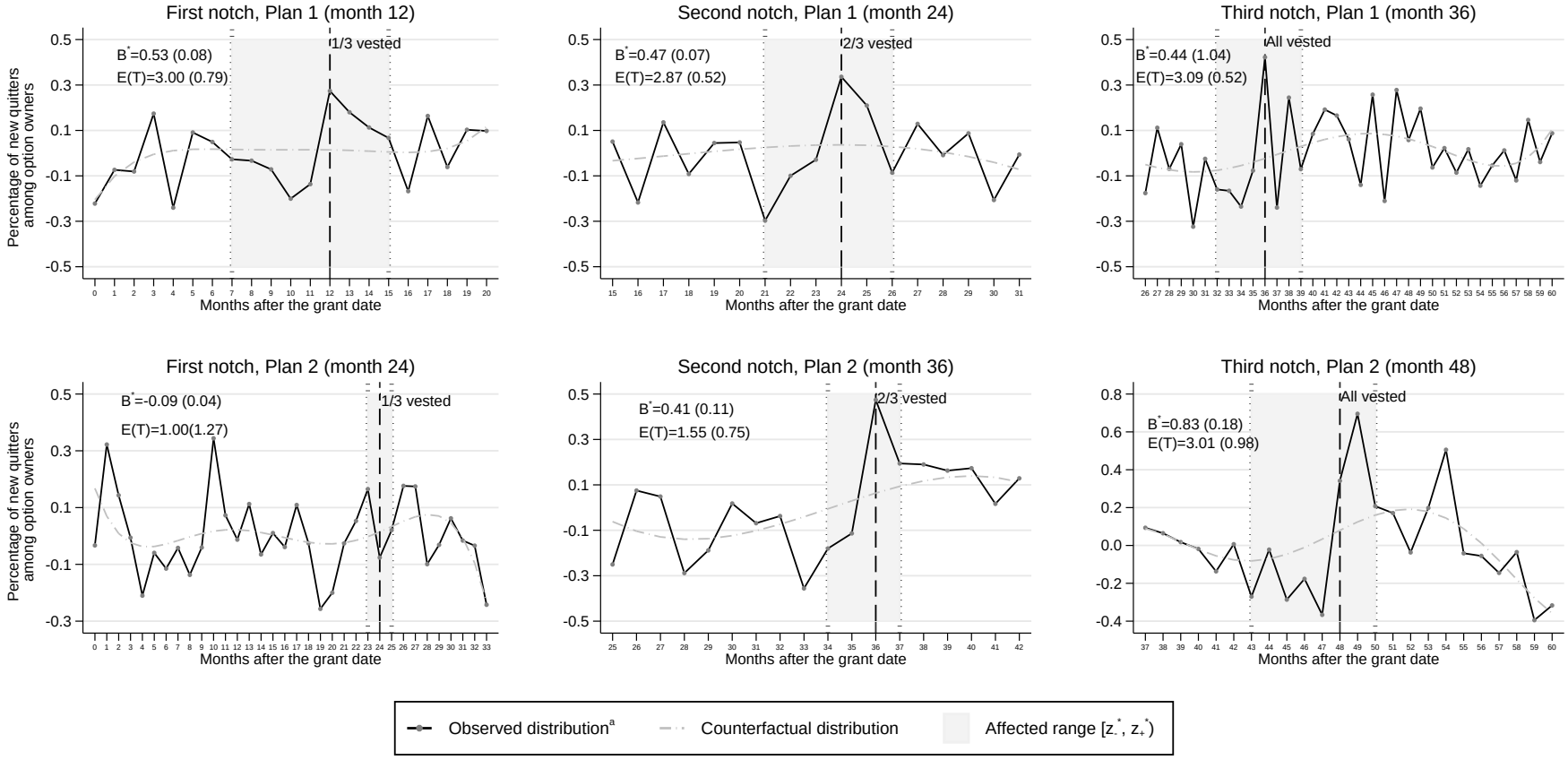


Figure 8: Testing for reference points: bunching estimates with month fixed effects

NOTES: *Observed distribution of quitters (solid line) is conditional on potential reference point effects (hence the negative values in some parts of the distribution). Potential reference points are February, May, June, September, and the first 3 months of a contract year. The horizontal axes show months after option granting, with the grant date being the first day of month 0. The dashed lines show the counterfactual distributions, estimated by fitting a flexible quintic polynomial. The shaded area indicates the affected range. The sizes of bunching, measured by excess mass B^*, are reported at the top-left corner of each plot. The corresponding standard errors are in parentheses. Also reported are the expected number of months an option owner is retained, $E(T)$. $E(T)$ for the first notch of Plan 2 (month 24) is not well defined due to the absence of bunching.

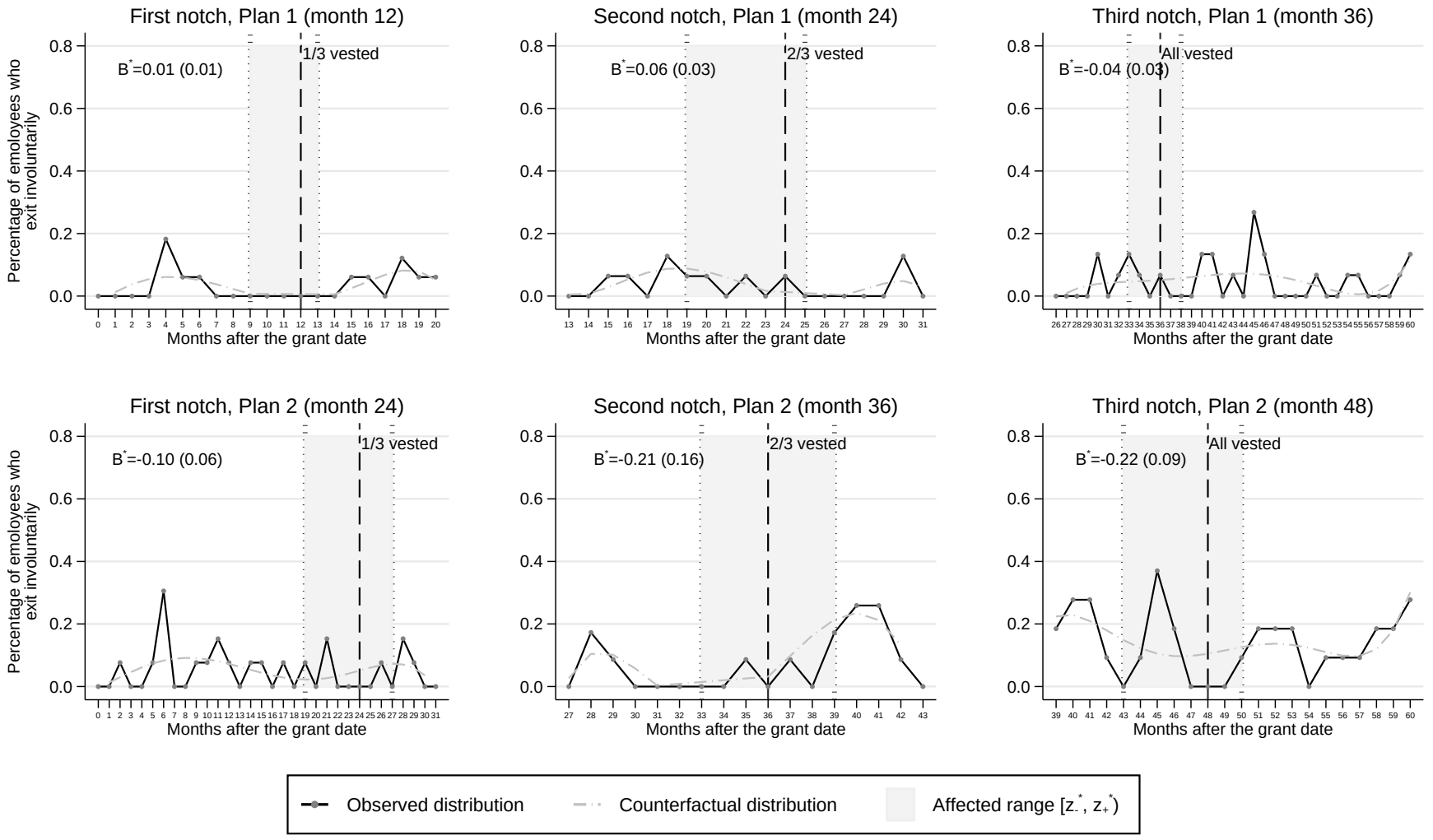


Figure 9: Falsification test: absence of bunching for involuntary exits

NOTES: The horizontal axes show months after option granting, with the grant date being the first day of month 0. The top, middle, and bottom plots to bunching at the first, second, and third vesting dates for Plan 1. The solid lines in the plots are the observed number of option owners who leave the firm involuntarily (e.g. fired or laid off) in each month as a fraction of all initial option owners. The dash-dotted lines are estimated counterfactual distributions of quitting rates over time in the absence of options, estimated by fitting a flexible quintic polynomial (using data points around the threshold, except those in the affected range). The shaded area indicates the affected range.

Figure 10: Performance evaluation scores of option owners and non-owners

NOTES: We restrict our sample to salaried option owners from 2004 to 2017, during which time performance scores are available. Raw performance scores are numbers between 0 and 1 assigned quarterly by each employee's director; the scores plotted here are normalized by quarter and job level. The horizontal axes in the two top panels are months after the granting date. These two panels plot the performance scores of option owners who stay (solid), quit (dashed), and leave involuntarily (dash-dotted). The bottom-left panel plots the performance scores of all employees over calendar time, including non-owners (solid with round marker) and the 3 types of options owners. The 95% confidence intervals are in shaded in gray for all plots.

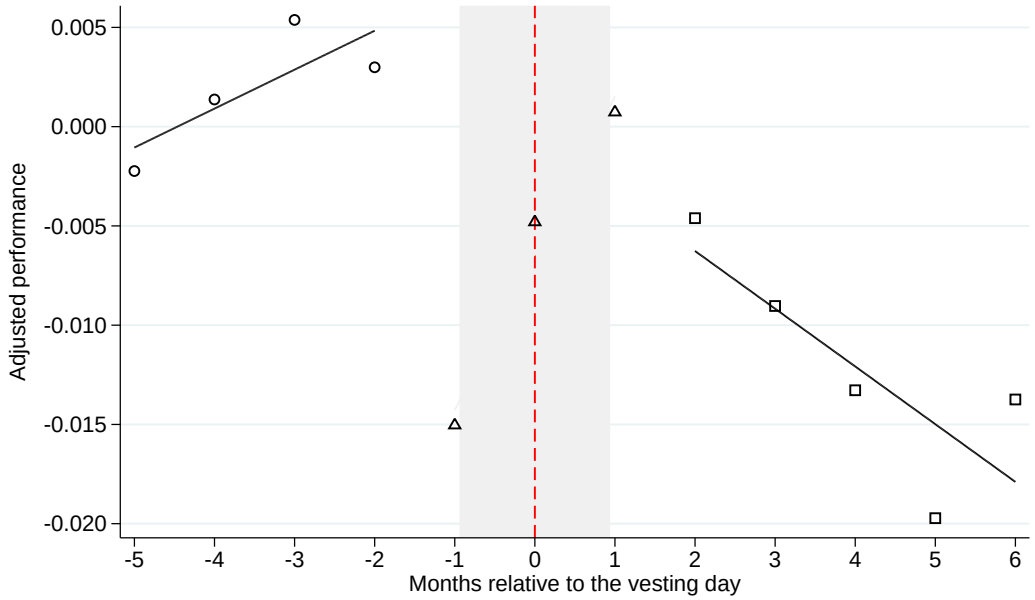


Figure 11: Changes in performance evaluation scores before and after option vesting

NOTES: We restrict our sample to salaried option owners from 2004 to 2017, during which time performance scores are available. The horizontal axis shows months relative to option vesting ($t = 0$, indicated by the dashed vertical line). The vertical axis shows normalized performance evaluation scores. Raw performance scores are numbers between 0 and 1, assigned quarterly by each employee's director; the scores plotted here are normalized by quarter and job level, and adjusted for seasonality. The figure is a binned scatter plot, which shows the average performance of employees who are present in each month.

Online Appendix for

Retention Effects of Employee Stock Options: Evidence from Bunching at Vesting Dates

A Supplemental Backgrounds and Descriptive Statistics

A.1 Additional information on option granting

The timing of option grants. Figure A1 summarizes when the firm granted Plan 1 (gray) and Plan 2 (white) options. The vertical axis represents the number of employees who received the granted options in each batch. There are typically 2-4 grants in a calendar year, but the specific timing of the grants does not follow any schedule and varies from year to year. We exploit the variation in the timing of option grants (within a calendar year) as part of our identification strategy.

Changes in the value of granted options. Figure A2 describes how the value of granted options changed over time. The top figure plots the distribution of the ratio of "ex-post" value to the Black-Scholes value (BSV) at the time of granting. The "ex-post" value of a batch of option is the BSV when it was exercised, expired, or reverted to the firm. The median ratio is 5.48 for Plan 1 options and 2.97 for Plan 2 options. This difference is at least partly driven by the fact that Plan 1 options were granted early on when the firm was experiencing more rapid growth.

Another possible explanation for the difference is that Plan 1 and Plan 2 options are held for different lengths of time. To account for this factor, we show calculate the *annual* growth rate of option values and plot its distribution in the bottom figure. Denote the ex-post value and the granting value as BSV_t and BSV_0, respectively, the annual growth rate is defined as $[(BSV_t)/(BSV_0)]^{1/t} - 1$. The median growth rate is 81% for Plan 1 options and 25% for Plan 2 options.

Regardless of the plan, however, the options granted to the firm's employees proved to be a valuable asset. The vast majority of options not only appreciated in value, but they also saw substantial annual growth rates. Even for the small fraction of options with a negative growth rate

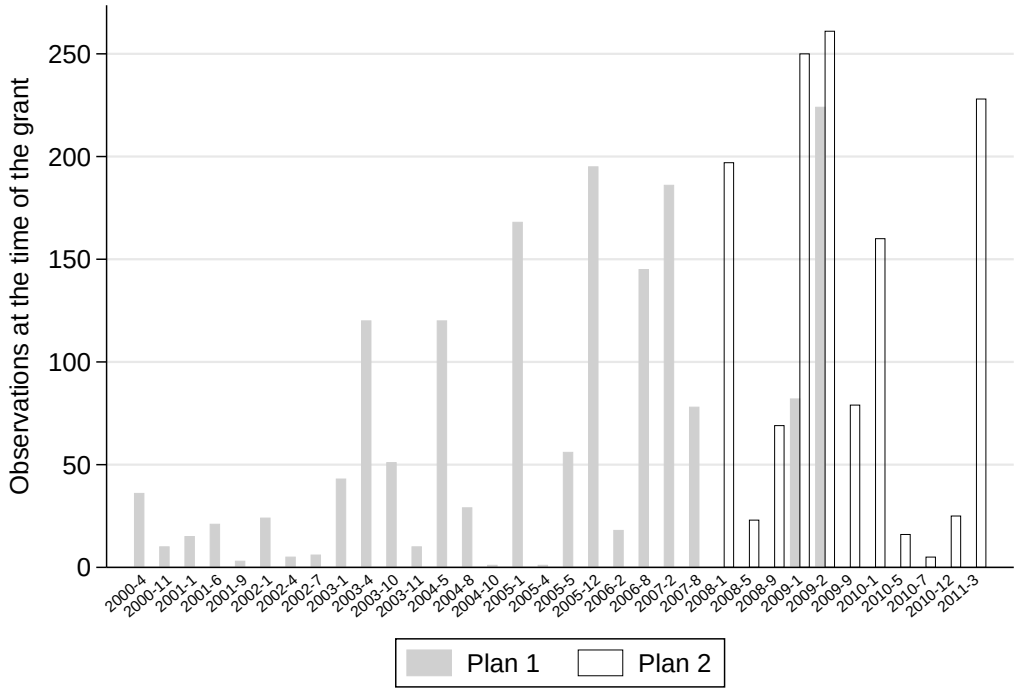
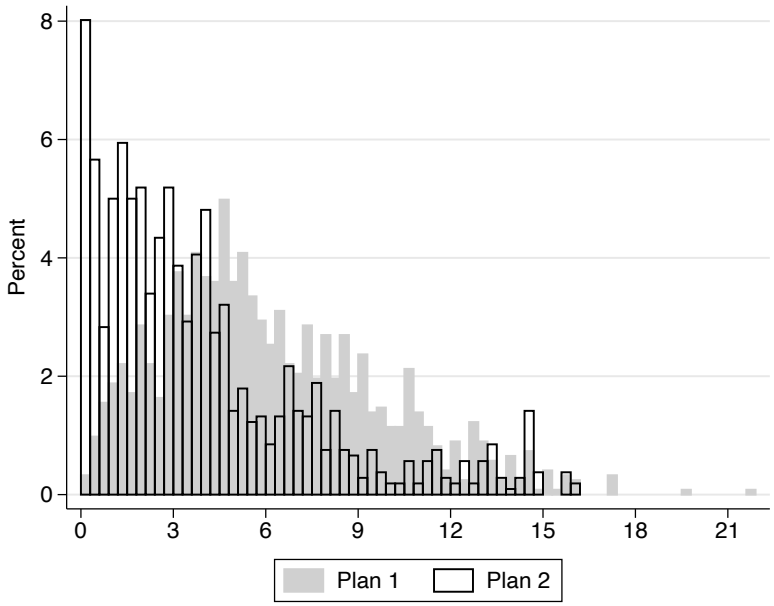


Figure A1: Timing of option grants, by type of plan

NOTES: The figure plots the timing and number of recipients of each batch of Plan 1 and Plan 2 options. Plan 1 options were granted from 2000 to 2009, whereas Plan 2 options were granted from 2008 to 2011.

at the time of exercise/expiration, their owners did not necessarily take a loss. This is because (i) negative changes in the BSV did not imply that the options were under water, and (ii) the owners were granted the options and did not purchase them at the market value.

Panel A. ("Ex-post" value) / (Granted value)



Panel B. Average annual growth rate of option value

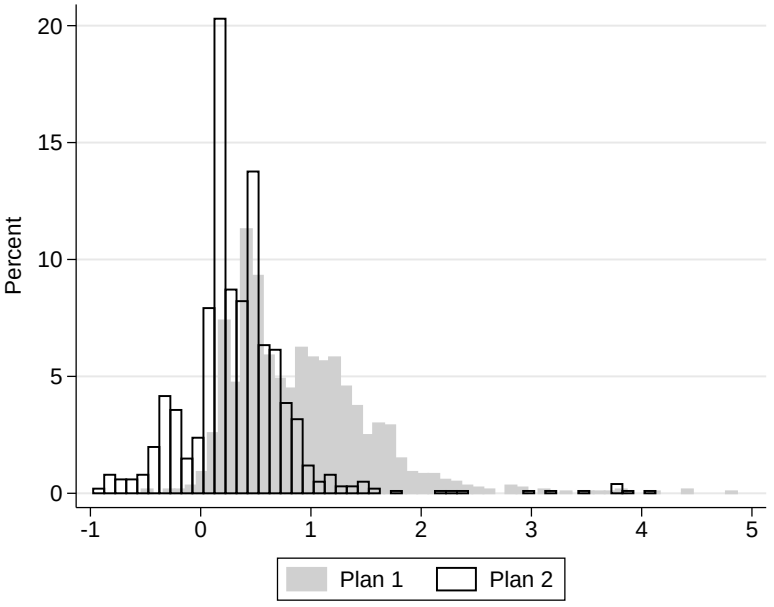


Figure A2: Changes in the value of granted options

NOTES: Both the ratios of option values (panel A) and the growth rates (panel B) are unit-less. That is, a growth rate of 1 means the value appreciated by 100%.

A.2 Additional information on turnovers at the firm

The turnover patterns of option owners Table A1 reports summary statistics on the turnover pattern of option owners in the firm. Panel A reports the fractions of option owners who quit during the sample period, 1999-2017. Among Plan 1 owners, 34.92% quit at some point; the figures are very similar between option owners in lower-level positions and those in higher-level ones. Plan 2 owners have higher turnover rates: the overall quit rate over the 18 years in the sample is 40.67%; lower-level owners are slightly less likely to quit (38.84%), while higher-level owners are more likely to do so (41.21%).

Panel A. Turnover rates (overall and by level)

	Plan 1		Plan 2	
	N	Quit%	N	Quit%
All	1,574	26.99	1,214	31.90
Level 1-4	663	26.24	226	30.09
Level 5-10	804	25.62	932	32.73

Panel B. Survival function over time

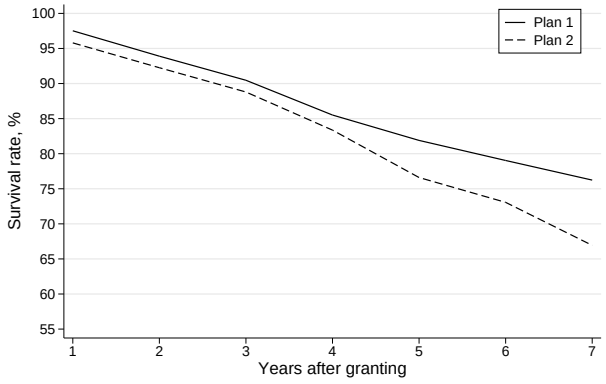


Table A1: Descriptive statistics on turnover patterns

Notes: Panel A reports the fraction of option owners who quit during the sample period, 1999-2017. Panel B plots the survival function (the fraction of option owners who remained in the firm) 1-7 years after option granting.

Owners of the two types of options also quit at different rates over time. Plan 1 owners quit at a stable annual rate of about 5% for most years. Plan 2 owners, on the other hand, have accelerating quit rates two years after options are granted. Recall that Plan 2 options were granted starting 2008.

Both the increased volatility in the stock market (which affects the option values) and the rapid growth of the industry the firm is in could have contributed to the accelerated turnover of Plan 2 owners.

Comparison: turnover rates of owners and non-owners Table A2 compares the cumulative turnover rates of option owners and non-owners at different job levels.

	Cumulative turnover rate (%)					
	In 1 year		In 3 years		In 5 years	
	Non-owners	Owners	Non-owners	Owners	Non-owners	Owners
Level 1	56.89	0.82	77.28	3.48	85.25	8.20
Level 2	17.41	0.00	42.38	0.52	58.44	1.39
Level 3	31.29	1.55	64.46	6.19	77.35	14.43
Level 4	31.80	3.21	64.92	16.04	77.05	24.60
Level 5	56.60	11.26	79.25	30.18	84.91	44.59
Level 6	68.18	6.25	90.91	23.75	90.91	40.00
Levels 7-10	40.00	0.00	86.67	21.21	86.67	39.39

Table A2: Cumulative turnover rates by option ownership and job level

NOTES: Non-owners are employees who never received options during our sample period. The cumulative turnover rate is defined as the fraction of employees that left the firm within s years of entry for non-owners, and within s years of option granting for owners. The job levels for non-owners are their levels upon entry, whereas those for owners are their levels when granted their first batch of options.

B Supplemental Details on the Bunch Estimation

B.1 Bootstrapping the standard errors of B^*

We use a parametric bootstrapping method to calculate the standard errors of bunching, $\hat{B}^*$. We follow the notations in Section 3.2.

For each bootstrap sample $k = 1, 2, \ldots, K$, we follow these steps:

1. Draw with replacement a vector of random "errors" $\boldsymbol{\nu}^{(k)}$ from the empirical distribution of the residuals from (1);
2. Generate a new vector of $\hat{Q}^{(k)} = X'\hat{\boldsymbol{\theta}}^* + \boldsymbol{\nu}^{(k)}$, with X being the matrix of regressors in (1);
3. Re-estimate (1) on $\hat{Q}^{(k)}$ to get $\hat{\boldsymbol{\theta}}^{(k)}$ and $\hat{B}^{(k)}$.

Finally, we calculate the sample standard deviation of $\hat{B}^{(k)}$ and use it as an estimate of the standard error of $\hat{B}^*$:

$$\hat{\sigma}_{\hat{B}^*} = \sqrt{\frac{1}{K-1}\sum_{k=1}^{K}\left(\hat{B}^{(k)} - \bar{\hat{B}}\right)^2}, \tag{5}$$

where $\bar{\hat{B}} = \frac{1}{K}\sum_{k=1}^{K}\hat{B}^{(k)}$ is the average of $\hat{B}^{(k)}$ across the K bootstrap samples.

B.2 Baseline estimates with larger samples

In light of the limited sample sizes issue in the main analysis, we use an alternative approach to estimate the bunching effects. Instead of separately estimating the bunching behavior at each of the three vesting dates, we pool data from *all months* and jointly estimate the bunching effects at those three points. Doing so assumes that there is one single counterfactual distribution of quitting times in the absence of options, as we discussed in the main text. The stronger assumption is the cost of a moderate increase in the sample size.

We summarize the estimates for both plans in Figure A3. The baseline findings remain unchanged: there is reduced quitting shortly before options vest and excess quitting afterward. In addition, the estimated magnitude of the bunching effect is similar to that in our baseline analysis. The bunching effects range from 0.69 to 0.83 percentage points for Plan 1 owners and from 0.11 to 1.26 for Plan 2 owners.

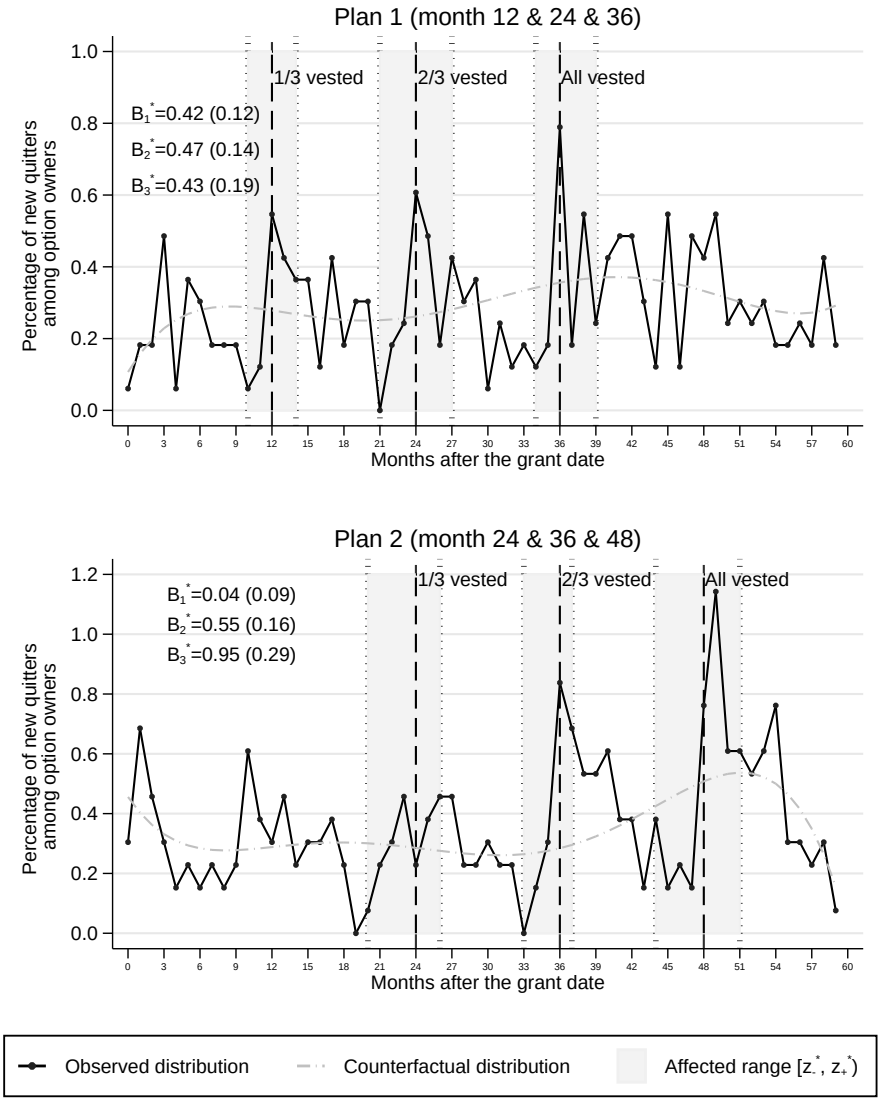


Figure A3: Pooled estimation of bunching at all three notches

NOTES: The horizontal axes show months after option granting, with the grant date being the first day of month 0. For each option plan, we assume all months after the grant date share the same counterfactual distribution. The solid lines trace out the observed distribution of quitters. The dashed lines show the counterfactual distributions. The shaded area indicates the affected range. Sizes of bunching at the three vesting dates, measured by excess mass B^*, are reported at the top-left corner of each plot. The corresponding standard errors are in parentheses. Also reported are the expected number of months an option owner is retained, $E(T)$.

B.3 Distribution of involuntary turnover of option owners

Figure A4 plots the raw distribution of involuntary exits by option owners, which is used in the falsification test in Section 4.3.4.

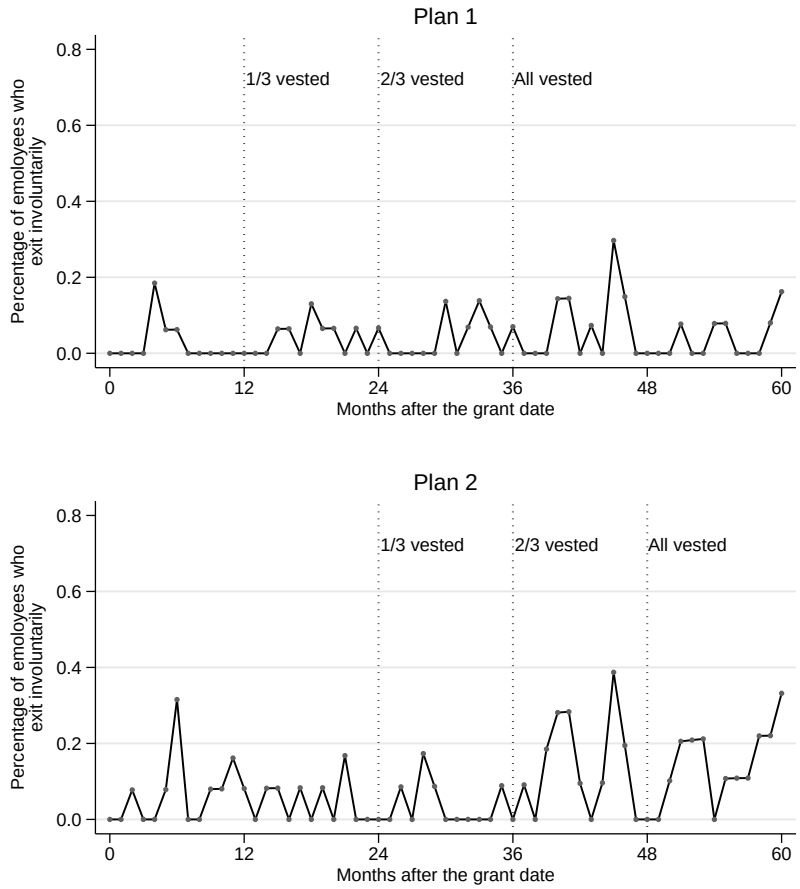


Figure A4: Monthly involuntary exit rates of option owners

NOTES: The horizontal axis shows months after option granting, with the grant date being the first day of month 0. Vesting dates are marked by dotted vertical lines. The vertical axes show monthly rates of involuntary separations (in %). The solid lines plot the number of option owners who leave the firm involuntarily (e.g. fired or laid off) in each month as a fraction of all initial option owners.

C Supplemental Details on Benefit-Cost Analyses

C.1 Promotion patterns of option owners and non-owners

Table A3 compares the patterns of promotion among option owners and non-owners. The top panel shows the average probability of being promoted. The bottom panel shows the average job level in 3 and 5 years, respectively. The "average level" is a simple way to summarize the joint effect of higher probabilities of promotion and bigger promotions (to higher levels).

The table shows that option owners are more likely to be promoted. The only exceptions happen among levels 6-10. The reasons are two-fold: (i) there isn't as much room for promotion for higher-level employees; and (ii) there are a small number of employees to begin with (see Table 1) and even fewer 3-5 years later.

	Pr(promoted in s years)			
Starting	$s = 3$ years		$s = 5$ years	
level	Non-owners	Owners	Non-owners	Owners
Level 1	34.38	73.95	48.57	75.38
Level 2	51.74	89.90	59.66	90.32
Level 3	65.97	95.16	86.30	96.07
Level 4	56.43	90.59	80.72	96.71
Level 5	31.03	69.94	44.44	83.69
Level 6	75.00	58.33	100.00	70.18
Levels 7-10	50.00	38.71	0.00	54.55
	Job level s years later			
Starting	$s = 3$ years		$s = 5$ years	
level	Non-owners	Owners	Non-owners	Owners
Level 1	1.24	1.55	1.51	1.92
Level 2	2.32	2.55	2.62	3.08
Level 3	3.59	4.04	4.04	4.71
Level 4	4.37	4.78	4.83	5.25
Level 5	5.11	5.40	5.44	5.83
Level 6	6.00	6.45	7.00	6.82
Levels 7-10	8.00	7.86	8.00	8.19

Table A3: Promotion patterns by job level and option ownership

NOTES: Non-owners are employees who never received options during our sample period. The job levels for non-owners are their levels upon entry, whereas those for owners are their levels when granted their first batch of options. The top panel shows the average probability of being promote; the bottom panel shows the simple average job levels in 3 and 5 years. A larger "average level" implies a greater probability of being promoted and/or promotions to higher levers.

C.2 Calculating wage savings

When estimating option owners' counterfactual wage, we first calculate the average monthly wage growth rate of non-owners at the same job level in the same month, $\bar{g}_t$. We then extrapolate option owner i's counterfactual wage $\hat{w}_{it}$ as

$$\hat{w}_{it} = \begin{cases} (1+\bar{g}_{t-1})\hat{w}_{i,t-1}, & t = 1, 2, \ldots, \min(T_i, 60) \\ w_{i0}, & t = 0. \end{cases} \tag{6}$$

That is, we start with the owner's observed wage in the month when BBSO is granted $(t = 0)$. We assume a constant wage growth for the owner at the same rate as an otherwise similar non-owner. We then continue until i leaves the firm (in month T_i) or month 60, whichever comes earlier.

Denote the observed and counterfactual wages of option owner i in month t as w_{it} and $\hat{w}_{it}$, respectively. The wage savings on owner i is then

$$S_i = \sum_{t=0}^{\min(T_i,60)} (\hat{w}_{it} - w_{it}), \tag{7}$$

where the monthly savings are summed up until either i quits (in month T_i) or $t = 60$, whichever comes first.[28]

C.3 Calculating improved output

The first step of quantifying the value of improved output by option owners is to construct a pecuniary measure of output, which is not easily available for firms in the service sector. We start by regressing non-owner wages on their performance scores, along with employee age, education attainment, work experience, type and level of job, and year-month fixed effects. Then we take the estimated coefficients to predict owner i's "fair wages," $\hat{y}_{it}$. Finally, we take the difference between the fair wage $\hat{y}_{it}$ of option-owners and the average wage of similar non-owners, $\bar{y}_t$. The

[28] We cap the summation at $t = 60$ to avoid extrapolating over too long a horizon.

improvement in productivity of option owner i as a result of receiving BBSO is then measured by

$$P_i = \sum_{t=0}^{T_i} (\hat{y}_{it} - \bar{y}_t). \tag{8}$$

C.4 Calculating retention benefits

The lower-bound measure of retention benefits, B_i^L, is defined as

$$B_i^L = \mathbf{1}\{T_i \in [z^*, z^* + z_+^*]\} \times \sum_{t=z^*-z_-^*}^{T_i} \bar{y}_{it}. \tag{9}$$

where only option owners who quit between $[z^*, z^* + z_+^*]$ (after a vesting date and within the affected range) are considered, and only their output within the affected range is accounted for. Because a quitting option owner might have quit any time in the absence of vesting dates, we consider the additional output during her extended stay as part of the firm's retention benefit. Hence we sum the owner's output from the beginning of the affected range until the actual time of quitting.

We also propose an upper-bound measure of the retention benefit, B_i^U:

$$B_i^U = \mathbf{1}\{T_i \in [z^*, z^* + z_+^*]\} \times \sum_{t=z^*-z_-^*}^{T_i} \bar{y}_{it} + \mathbf{1}\{\text{Retained}\} \times \mathbf{1}\{\text{Exercised } x\%\} \times \left(\sum_{t=z^x}^{T_i} \bar{y}_{it} \right), \tag{10}$$

where $\mathbf{1}\{\text{Retained}\}$ is an indicator that i is retained throughout the entire sample period, $\mathbf{1}\{\text{Exercised } x\%\}$ is an indicator that i has exercised at least $x\%$ of her newly vested options since the most recent vesting date, and z^x is the point in time that she reaches the $x\%$ threshold.[29] The rationale for this measure is that exercising large fractions of vested options is a signal for the intention to leave the firm, as shown in Figure 6. We count the output from that point onward into the firm's retention benefit, because the option owner could quit at any time now (after the intention to leave emerges). It is the vested but not yet exercised options that are keeping her in the firm and generating out-

[29] If an option owner never exercised more than $x\%$ of her vested options, then her output is not accounted for in this measure of retention benefits.

put as a result. In light of the empirical patterns of option exercising before quitting, we set the threshold at 70% (i.e., $x = 70$). The benefit-cost analysis results are robust to alternative choices of x.

C.5 Additional details on the benefit-cost comparison

Table A4 provides a more detailed version of Table 7, with finer breakdowns by job level. Levels 7-10 are still grouped together due to the small number of observations.

	Benefits and cost (in $1,000)				Benefit-to-cost ratio				
		Wage	Retention benefits		Wage	Retention benefits		Total benefits	
	Cost	savings	Min.	Max.	savings	Min.	Max.	Min.	Max.
	(1)	(2)	(3a)	(3b)	$\frac{(2)}{(1)}$	$\frac{(3a)}{(1)}$	$\frac{(3b)}{(1)}$	$\frac{(2)+(3a)}{(1)}$	$\frac{(2)+(3b)}{(1)}$
Panel A. Plan 1									
All	317.26	51.10	170.35	362.43	0.98	1.35	3.61	2.33	4.59
Level 1	114.78	55.49	-6.38	69.33	0.80	-0.13	1.31	0.67	2.11
Level 2	45.98	137.53	15.15	195.35	7.04	0.89	7.92	7.93	14.96
Level 3	66.77	149.60	77.77	253.51	2.94	1.30	4.75	4.24	7.69
Level 4	111.76	108.47	98.47	294.95	1.50	1.46	4.67	2.96	6.17
Level 5	238.82	1.04	116.77	276.91	-0.01	1.11	2.73	1.10	2.72
Level 6	298.62	14.27	223.59	409.45	0.12	1.57	2.97	1.69	3.09
Levels 7-10	1183.80	-10.63	446.12	734.49	-0.09	1.53	2.58	1.45	2.49
Panel B. Plan 2									
All	538.72	82.98	124.52	487.77	0.70	0.38	2.57	1.08	3.27
Level 1	57.95	223.56	-67.07	73.46	5.10	-1.69	2.21	3.41	7.32
Level 2	76.66	188.19	-55.49	107.23	3.10	-1.33	2.22	1.77	5.32
Level 3	116.46	392.78	-78.95	255.88	4.51	-0.95	3.73	3.56	8.24
Level 4	192.00	384.80	18.55	422.53	4.02	0.14	5.37	4.16	9.39
Level 5	414.62	121.27	80.69	432.24	0.31	0.52	2.89	0.83	3.20
Level 6	436.16	0.44	111.43	494.02	0.00	0.48	2.11	0.48	2.11
Levels 7-10	1281.00	-26.54	355.69	745.01	-0.08	0.82	1.78	0.74	1.71

Table A4: Benefit-cost analysis by job level

NOTES: The left half of the table reports the benefits and costs estimates in thousand RMB yuan. "Cost" (1) is the average cost of granting BBSO to an employee. "Wage savings" (2) are the average estimated wage savings on the employee. (3a) and (3b) are the lower- and upper-bound estimates of the retention benefits, respectively. The right half of the table reports ratios of BBSO benefits (by type and in total) to the granting cost.