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Qing Gong | James Liang | Hong Zhang | Li-An Zhou

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Center for International Development

John A. and Cynthia Fry Gunn Building | 366 Galvez Street, Stanford, CA 94305-6015

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Qing Gong*[1], James Liang†[2], Hong Zhang‡[2,3] and Li-An Zhou§[2]

[1]Department of Economics, University of Pennsylvania

[2]Guanghua School of Management, Peking University

[3]Stanford Center for International Development

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*Email: qinggong@sas.upenn.edu

†Email: jzliang@gsm.pku.edu.cn

‡Email: zhanghong2010@pku.edu.cn

§Email: zhoula@gsm.pku.edu.cn

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Abstract

Whether and how employee options work in favor of the company has been the center of much debate. Taking advantage of a detailed employee-level panel data set and the predetermined option vesting schedules, we use a regression discontinuity (RD) design to quantify the causal effect of options with a vesting schedule on employee turnover. We find strong retention effects, especially at the first and the last vesting days. Increases in exit rates immediately after option vesting are on the order of 1 percent per month, and are several times the average exit rates in months before option vesting. This retention effect is very robust to alternative econometric specifications, but do vary across different levels of employees. Comparing the RD coefficients that capture the retention effect on those who delay quitting (the delayed) with estimates of the total retention effect which also include impacts on those who give up quitting altogether (the retained), we gauge the unobservable fraction of the retained and find it fairly sizable and varies quite a bit by job levels.

1. Introduction

Stock options are popular tools to entice employees. An estimated 9 million employees in the U.S. were in stock option ownership plans in 2014, which accounted for about 24 percent of all employees in companies with stock.[1] Such popularity is not unique to the U.S. In China, roughly 8 to 16 percent of domestic companies offer stock options to their employees in 2012, despite a small and highly regulated derivatives market.[2] Understanding employee stock options has important implications for both the employers and policy makers, who often provide tax benefits to encourage employee option ownership (Dube and Freeman, 2010). Despite its popularity, though, there is still much debate about whether options work well enough to justify their cost.

At first sight, options seem to align the interests of executives with those of the company, thus inducing more effort to increase the company value. However, free-riding in a multiple-agent environment would greatly reduce the efficacy of options (Lazear, 2004), and empirical findings do not always support the incentive explanation, either (Barron and Waddell, 2003; Oyer and Schaefer, 2005). The incentive explanation is even less sensible in justifying the popularity of options among lower-level employees who have little impact on company profitability. Moreover, these "rank-and-file" employees are often risk-averse and hold ill-diversified portfolios, making them most likely to undervalue the options (Hall and Murphy, 2003).

The limited incentivizing effects and the popularity of options can be reconciled in two ways. One is to discard the optimality assumption on firm behavior, and view options as a mistake. As far as we know, the only paper suggesting this point is Hall and Murphy (2003), where the authors conjecture that options are popular because decision makers mistakenly see options as costing less than they actually do. The other approach is to explore other benefits of options that justify their considerable cost. For example, Oyer and Schaefer (2006) and Lazear (2004) propose that firms may grant options to lower-level employees because of their ability to attract and retain employees, or as a sorting tool that helps select people who are optimistic about firm prospects. However, micro-level evidence of these alternative explanations, especially that from lower-level employees, is at best tenuous at this point.

In this paper, we focus on testing and quantifying the retention effect of employee option plans with vesting schedules, and explore the composition of these retention benefits, if there are any. This would help with understanding the puzzling popularity of options by providing evidence on a potentially important, yet less well-studied type of benefits of options. To provide any meaningful result, however, it is crucial to be able to draw causal conclusions, instead of mere correlations, about owning options with a vesting schedule and the reduced turnover. That said, the endogeneity of option ownership imposes great challenge to the analysis. Those receiving options are already a highly selective sample of employees, making it hard to evaluate the effect of options without contamination of confounding factors such as unobserved heterogeneity in employee ability or belief.

We use the sharp regression discontinuity (RD) design to resolve the endogeneity problem that often prohibits causal inference in this literature. Using a novel longitudinal dataset with a plethora of information

[1] http://www.esop.org and http://www.nceo.org/articles/widespread-employee-ownership-us

[2] http://www.china-briefing.com/news/2011/05/12/using-stock-options-to-attract-and-retain-key-employees-in-china.html

on individual characteristics, monthly wage, option ownership, and other human resources records from Ctrip.com ("Ctrip" hereafter), a leading on-line travel agency from China and listed on NASDAQ, we provide detailed empirical evidence at the individual employee level on the retention effect of options with vesting schedules. Most important, we take advantage of the predetermined option vesting schedules and use sharp RD to draw causal conclusions about the retention effects. After Ctrip grants options to its employees, the options vest in three equal batches every 12 months. In case an option-holding employee decides to quit, she has to give up all unvested shares, but can still exercise vested shares within three months. Therefore the options forgone decrease discontinuously on the vesting day, thus reducing the cost of quitting. We use this discontinuous reduction to study the existence and magnitude of retention effect of employee option plans with vesting schedules. The RD method, coupled with detailed employee-level longitudinal data, enables us to draw causal conclusions from the estimates.

Figure 1 plots the monthly exit rates since the grant date for a particular plan that Ctrip offers, which vest in three equal batches in months 12, 24, and 36. Note that the exit rate increases discontinuously upon vesting. For example, the monthly exit rate is usually between 0.1 and 0.5 percent in the first 11 months, but is as high as almost 1 percent in month 12. In addition, the gap is statistically significant, as shown by the non-overlapping 95 percent confidence intervals. The same is true for the other two vesting points, although the gap is smaller around month 24 and bigger around month 36. Interestingly, no such discontinuity exists around months 48 or 60, by which time all the options have vested already. Thus the previous discontinuous jumps in exit rates are more likely to be the result of option vesting, instead of mere seasonality. The increases in exit rates immediately after option vesting are suggestive of considerable retention effects.

[FIGURE 1 ABOUT HERE]

Formally, we find using RD regressions that employee option plans with vesting schedules have strong retention effects, especially at the first and the last vesting days. Monthly exit rates increase by about 0.74 to 1.21 percent immediately after option vesting, which is several times the average exit rates in months preceding the vesting. The retention effects of option ownership is very robust to alternative econometric specifications. Moreover, falsification tests show that no discontinuity is present around months when the options have fully vested, and that confounding factors such as time trend and contract year cyclicality are unlikely to be driving the results.

The RD estimates only capture the retention effects on those who delayed quitting till after the vesting days ("the delayed"), while the total retention effects also come from those who give up quitting because of options ("the retained"). Hence we use non-owners to estimate the elasticity of turnover rates to a series of important factors. Then we predict the hypothetical turnover of option owners should they have not received options, and get a simple estimate of the total retention effects, which, jointly with the RD estimates, help to gauge the unobservable fraction of the retained. We find that the retained contribute the majority of the retention effects, ranging from 53.84 percent for junior managers to more than 90 percent for lower-level workers. We further show that the retained employees do have better performance, making the retention effects beneficial to the company, and do a simple cost-benefit analysis to find the conditions under which

the benefit of options just offsets their costs.

Our study is related to a large body of literature. The first strand of literature is on profit sharing in general. Among other important studies, Kruse (1993) examined a panel of 500 U.S. firms over 21 years and argued that profit sharing plans helped boost productivity, and, more importantly, improve economic stability by lowering unemployment rate; Lazear (2000) also showed that variable pay schemes can partially resolve asymmetric information regarding effort, and serve as incentive provision tools.

A second strand of literature is specifically on employee stock option ownership. As discussed above, the popularity of options leads researchers to study why a lot of employers decide to bestow stock options on their employees and whether these options work as expected. Because of the aforementioned intuitiveness of the incentive justification, many earlier studies focus on the performance of executives after receiving options. Aggarwal and Samwick (1999) conduct a cross-firm comparison and find that the pay-performance sensitivity of option-holding executives does respond to the variance of firm performance, but provide no direct justification for the executive incentive plans in the first place. Core and Guay (2001) found that firms with lower monitoring cost had smaller fractions of option-holding employees, which sheds light more on why firms award options. Other studies have found no significant effect. For example, Kedia and Mozumdar (2002) find no increase on firm ROA from offering stock options.

Given the incentive effects are insufficient to justify the popularity of options, later researchers begin to explore other explanations, among which sorting and retention effects attract more attention. Oyer (2004) and Lazear (2004) provided theoretical foundation to this view. Oyer and Schaefer (2006) and Oyer and Schaefer (2005) evaluate the three competing explanations, namely incentive, sorting, and retention using firm-level data. They reject the incentive justification, and find the latter two to be more plausible. Carter and Lynch (2004) study the specific case of option repricing, which they find to be negatively correlated with overall employee turnover but have no effect on executive turnover. Dube and Freeman (2010) use survey data and find that shared compensation contracts, when coupled with shared decision-making arrangements, are associated with lower turnover, although the lack of exogenous variation prohibits them to draw causal conclusions about the effects of shared compensation alone. Overall, empirical evidence on the retention effects are fairly limited and mixed, let alone causal inference.

Our paper contributes to the existing literature first by providing concrete employee-side evidence on the effectiveness of stock options with a vesting schedule on retaining workers. Despite the wide variety of this literature, all aforementioned empirical studies are from the firms' perspective. They use firm-level data to analyze why firms offer options to many employees, and how that affects firm output, financing, and accounting. On the contrary, empirical evidence on individual employee response to stock options is minimal. This is to a large extent due to the lack of suitable data: existing papers taking the employees' perspective are almost all using data on company executives (e.g. Aggarwal and Samwick (1999); Mehran and Tracy (2001); Barron and Waddell (2003)). Using high-quality administrative data on employee compensation, turnover, and other characteristics, we are able to closely examine any effect stock option ownership may have on employee turnover. To the best of our knowledge, the only other paper using such detailed employee-side data in this literature is Cowgill and Zitzewitz (2014), which studies the effects of restricted stocks on Google

employees.[3]

More importantly, the RD design enables us to look into the causal effects of stock options with a vesting schedule on employee turnover without picking up the underlying effects of confounding factors. Comparing the turnover rates of employees who are option owners and those who are not directly suffers from strong endogeneity of option ownership, because option granting decisions are merit-based, leaving researchers with two highly different groups of people. The RD design, on the contrary, is known for its high validity, and any discontinuity at treatment-determining thresholds captures the treatment effect. To the best of our knowledge, Cowgill and Zitzewitz (2014) is the only paper that examines the causal effect of equity compensation. Using data from Google, they identify incentivizing effects from exogenous variation in stock exposure coming from Google's unique pricing policy, but find the incentives to be fairly weak. We take a different approach, using the discontinuity in option vesting schedules to take care of unobserved employee heterogeneity, and focus on retention effects instead of the incentivizing effects of those employees conditional on their choices to stay.

The rest of the paper is organized as follows. Section 2 describes the background of the Ctrip data, and discusses our sample construction. Section 3 introduces our empirical strategy, and conducts tests of sorting in light of our RD regression model. Section 4 presents the baseline results and tests alternative explanations. Section 5 discusses the interpretation of the RD estimates, quantifies the composition of the retention effect, and look into the costs and benefits of options. Section 6 explores the characteristics of the curious group of workers who quit right before option vesting. Section 7 concludes

2. Background and data

2.1. Ctrip and its option offering

The data set we use comes from Ctrip, the leading online travel agency in China. Ctrip was founded in 1999 and listed in NASDAQ in 2003. Now it has more than 20,000 employees all around China. Bloom et al. (2014) provide a more detailed description of the company background of Ctrip.

Ctrip classifies job positions vertically into 10 levels.[4] Newly hired employees can start out in any level depending on their seniority, and then move to higher levels over time. Levels 1 to 4 are junior employee, senior employee, head of team, and senior head of team, respectively. Level 5 and above are managers, senior managers, and higher-level positions all the way to vice president, almost all of whom receive stock options.

Ctrip started to grant options to its employees since 2000. There are two types of plans with different vesting schedules. Plan 1 options are offered from 2000 to 2009, and Plan 2 from 2008 onward. Plan 1 options vest in three equal batches every 12 months after the grant date. That is, if an employee receives her options in month 0, then one-third of her options vest in month 12, an additional one-third in month 24, and the final one-third in month 36. Plan 2 options follow a similar schedule in that they also vest in three

[3]Another remotely related paper using lower-level employee data is Huddart and Lang (1996), which studies the timing of option execution, and its implication on the compensation of corporate debt and regulation from an accounting perspective.

[4]There is in fact a Level 11, which are the highest executives like the CEO and the CFO. We exclude this level from our sample.

equal batches, but there is an initial lock-in period of two years. Therefore the first vesting point is in month 24, the second in month 36, and the third in month 48. When an employee quits Ctrip, she immediately loses the unvested options, but can keep the already vested shares for three months. After three months, any unexercised vested shares revert to the company aw well.

One notable difference between the two plans besides the vesting schedule is the strike price. Plan 1 options have significantly lower whose prices than Plan 2 options, whose strike-to-market ratios are about 2.5 times as high as those of Plan 1 options. Because of this difference in option value, we will study Plan 1 and Plan 2 options separately in the paper.

To see how representative the Ctrip employees are of the general Chinese workforce, we compare the basic characteristics of the former to those reported in the Chinese Urban Household Survey (UHS) during our sample period. Table 1 reports the age, gender, monthly wage, years of work experience, and years of schooling of the working population in UHS, the whole sample of Ctrip employees, and the Ctrip option owners, respectively. We divide the samples into three time periods to reflect possible aggregate time trends. Ctrip employees differ from the general workforce in China especially in that they earn significantly higher wages, are 10 to 15 years younger, and have less work experience as a consequence. They do share almost the same level of education, and have moderately different gender ratios. Note that the UHS is a representative sample of the entire urban population, many of whom come from small, less developed cities and work in low-paying, traditional industries. On the other hand, Ctrip branches are often in cities like Shanghai and Beijing, which explains why its employees are somewhat different from the UHS sample. Overall we feel that the Ctrip data still shed some light on the Chinese working population, although they would be more representative of those working in better-paying jobs in bigger cities of the country.

[TABLE 1 ABOUT HERE]

2.2. Sample description

The dataset we use spans over 1999 and 2014, covering all employees that joined Ctrip by the end of 2012, totaling more than 50,000 distinct individuals. It contains a rich set of information that the human resources department keeps track of, notably very detailed stock option information, monthly wage information, promotion records, along with other individual characteristics. We use province-level CPI for urban districts to convert all monetary variables to real 2014 yuan in order to adjust for inflation.

We restrict our sample for baseline analyses to option-holding employees only. Because some employees are granted options more than once, we restructure the data so that each observation is an option grant instead of an individual. This way we are able to distinguish between different batches of options owned by the same person, which may belong to different plans and/or have different vesting days. For each option grant, we observe option characteristics like the grant date, vesting days, strike price, spot market price, and the number of shares on hand; individual personnel information like job level, department, promotion records, detailed wage information, and date leaving Ctrip if the employee quits; and other employee characteristics like age, gender, education, and years of experience in Ctrip. In the end, we get an unbalanced panel of

235,559 monthly observations from 1,796 distinct employees, each staying in the sample for an average of 47.64 months.

[TABLE 2 ABOUT HERE]

Table 2 shows summary statistics on option characteristics. On average, option-holding employees receive 9275 shares of Plan 1, or 6405 shares of Plan 2, although the figure varies substantially across individuals. As we discussed previously, Plan 1 options also have a lower strike-to-market ratio of 1.06, whereas that of Plan 2 options is 2.45. The potential value is about 9.43 times the annual base wage of their owners for Plan 1 options, and is 13.48 times for Plan 2 options. The average monthly wage of Plan 1 owners is slightly lower than Plan 2 owners, which reflects the fact that Plan 1 options were granted at a much earlier time than Plan 2. Hence Plan 2 options have higher value on average in both relative and absolute terms despite having higher strike-to-market ratios. This is to a large extent due to Ctrip's much faster growth rate when offering Plan 2 options.

As for the equity portfolio held by option owners, more than 60 percent of owners of both plans have other options on hand when they receive the current batch of options. The majority of them own other Plan 1 options, which is again consistent with the longer history of Plan 1 options. None of Plan 1 owners had restricted stocks when they received the options, as Ctrip started offering restricted stocks only since 2010. About 3 percent of Plan 2 owners had restricted stocks, although the value of these stocks is merely 5 percent of the annual wage on average.

In terms of the distribution of option owners' levels, both plans have an inverse U-shaped pattern, with fewer owners in very low levels when they receive the options, more in intermediate levels, peaking at level 5, and fewer in very high levels. The initial increase is due to the fact that higher level managers are more likely to get options; and the decrease beyond level 5 is mainly driven by the drastic decline in the number of higher level positions. Compared with Plan 2, the distribution of owner levels for Plan 1 is slightly skewed toward the lower end, with a larger fraction of owners in Levels 3 and 4.

Overall, the summary statistics indicate that there is a lot of heterogeneity across option plans, which requires estimating the treatment effects of different plans separately. Moreover, the heterogeneity in option owners once again suggests the need for a robust identification strategy of the causal effect to mitigate the confounding effects of endogenous ownership.

3. Empirical Strategy

In this section, we formally introduce the sharp RD design that we use to identify the retention effect of employee option plans with a vesting schedule. We first define relative date t as time elapsed since the grant date. For instance, if an employee receives Plan 1 options on March 15, 2008, then t is 0 from March 15 to April 14, is 1 from April 15 to May 14, is 12 from March 15, 2009 (on which day the first one-third of Plan 1 options vests) to April 14, 2009, and so on. We t as the forcing variable, and the pre-specified option vesting days as the treatment-determining thresholds. Note, however, that there are generically multiple

treatments for a given option holder because of the vesting schedule[5], with each individual undergoing up to three vesting days before the options fully vest. Therefore we define treatment locally as being subject to a vesting day no more than six months ahead. We will only use data within at most six months before and after each vesting day in our RD regressions. This and the definition of treatment jointly ensure all individuals in our sample are either under the treatment, or are recently "freed" from the treatment.

3.1. Baseline specification

In the baseline specification, we use i to index a unique individual, j a batch of options, and t months elapsed since the grant date of (i, j). We run RD regressions around the first, second, and third vesting days separately, as they may have different retention effects. For a given vesting point $c \in \{12, 24, 36, 48\}$, we run the following regression for observations (i, j, t) with $t \in \{c-h, \ldots, c-1, c, c+1, \ldots, c+h-1\}$

$$Y_{ijt} = \gamma_0 + \gamma_1 \cdot D_{ijt} + f(t;\theta) + X_{it}^{'}\gamma_2 + \alpha_i + \varepsilon_{ijt} \quad (1)$$

where the outcome of interest, Y_{ijt}, is an indicator for i leaving Ctrip in the t'th month after receiving her j'th batch of options; $D_{ijt} = 1\{t \geq c\}$ is the indicator for being shortly after vesting point c; $f(t;\theta)$ is a function of t with parameter θ; X_{it} is a vector of covariates; and α_i is the individual fixed effect to control for unobserved, time-invariant heterogeneity among the employees.

We run local linear regressions with a rectangular kernel and bandwidth h. Because of the discreteness of time in this specific context, we will not use general optimal bandwidth selection methods in the literature, such as Imbens and Lemieux (2008) or Hahn et al. (2001). Instead, we choose $h = 3$, i.e. using observations three months before and after the vesting day to estimate the above RD model; and we will show that the results are robust to alternative bandwidths.

Note that the binary outcome variable, Y_{ijt}, necessarily introduces heteroskedasticity and requires more robust standard errors than the plain vanilla ones. In addition, there is potential autocorrelation between the errors of a given individual across time, which may have significant impacts given our short panel of length $2h$. Hence we allow for clustering at the individual level in the regressions.[6]

The regression coefficient of interest, γ_1, captures the discontinuity at the vesting day, thus the (reversed) retention effect. The function $f(t;\theta)$ captures any underlying continuous relationship between time and the outcome. According to Imbens and Lemieux (2008), RD regressions with the above two components alone produce consistent estimators, but the inclusion of covariates nonetheless improves precision. We will show results both with and without covariates, X_{it}, which include the option holder's age, years of work experience in Ctrip, a promotion dummy, as well as job category and level dummies. More importantly, we control for the effect of other financial assets she owns at the same time by including as covariates the value of other options, the value of restricted stocks, and the number of months before the nearest vesting points of other options and restricted stocks, respectively. Because we have been using the relatively defined time, t, we also include a set of year dummies to capture any potential calendar time trend.

[5] Unless, of course, she quits halfway during the process.

[6] As a robustness check, we also use bootstrapping and find very similar standard errors.

3.2. Tests of sorting

A critical assumption of the RD design is that the forcing variable is not subject to manipulation, i.e. there is no sorting into either side of the treatment-determining threshold. While it is common for researchers to test this by examining the distribution of the forcing variable, such a test is not necessary in our context. This is because the forcing variable is months elapsed since the grant date. And both the grant date and time lapse are free from manipulation of individuals. Quitting is the only way an employee can avoid being in a certain month (and thereafter), but that is exactly the outcome we are interested in. Therefore the manipulation-free assumption on the forcing variable naturally holds.

Yet another type of sorting we need to rule out is that on the predetermined variables, so that the discontinuity at the threshold captures the effect of treatment, and not that of other variables. To show this, we first plot the predetermined variables against the forcing variable in Figure 2. Panels A1 and A2 show the average years of schooling for Plan 1 and Plan 2 owners, respectively, and neither has discontinuity on vesting days. The only two large jumps are around months 42 and 54 for Plan 2 owners, which does not invalidate the RD design but is still worth discussing. This is mainly because Ctrip started offering Plan 2 options since 2008, and our data only tracks employees up to mid-2014. Therefore the later cohorts of Plan 2 owners are in the sample for fewer months. This problem is absent for Plan 1 owners since Ctrip stopped granting Plan 1 options in 2009, leaving us with enough months afterwards in the data. In fact, Figure 3 plots the evolution of the number of individuals over relative time and confirms the point above. The number of observations declines steadily for Plan 1 and for the majority part of Plan 2, but drops abruptly around months 42 and 54. As we will show momentarily in the plot of employee exit rates, these drops are not driven by employee turnover. Hence it is only the change in sample size and, consequently, in the composition of cohorts that generate the discontinuity, though not at the vesting days. The other plots in Figure 2 show the same absence of discontinuity in predetermined variables at the treatment-determining thresholds.

[FIGURE 2 ABOUT HERE]

[FIGURE 3 ABOUT HERE]

Figure 4 plots the closing spot market prices against relative time t and over calendar time. Once again, there is no apparent discontinuity, which is consistent with the common belief that stock price variations can be seen as exogenous to individual behaviors.

[FIGURE 4 ABOUT HERE]

To formally test the absence of discontinuity in predetermined variables, we estimate the baseline RD regression without covariates, X_{ijt}, but use the covariates, years of schooling, age, years of experience in Ctrip, and job level as dependent variables. The estimated RD coefficients will then capture any discontinuity in the value of these predetermined variables at the threshold. In case such discontinuity exists, the baseline specification in Equation (1) might be picking up the effect of covariates on employee turnover along with the true treatment effect, which will invalidate our RD design.

[TABLE 3 ABOUT HERE]

Table 3 reports the RD regression results using the predetermined variables as dependent variables. Panel A1 examines any potential discontinuity at the first vesting day of Plan 1 options. The RD coefficients are both tiny relative to the mean and statistically insignificant even at 10% level for all the variables we are interested in. Panels A2 through B3 show that the same is true for the other vesting days of Plan 1 and Plan 2. These results indicate that there is no detectable discontinuity in our covariates at the treatment-determining thresholds. Hence one can be reasonably assured that the RD coefficients in our baseline specification will be capturing the treatment effect instead of confounding factors.

4. Results

4.1. Baseline results

Before presenting the RD regression results, we first plot the monthly exit rates for owners of both plans in Figure 5 as a preliminary result. Panel A is the same as Figure 1, which shows that the exit rates of Plan 1 owners increase discontinuously at the vesting points in months 12, 24, and 36, but do not have such significant differences in months 48 and 60, where the options have fully vested. Panel B shows similar patterns for Plan 2 owners, with jumps in exit rates only in months 24, 36, and 48, but not in earlier or later months.[7]

[FIGURE 5 ABOUT HERE]

Now we present the regression results of the baseline RD specification in Table 4. Panel A reports the estimated coefficients for Plan 1 option owners under various bandwidths and at different thresholds, without any covariates. Taking bandwidth $h = 3$ as an example, the exit rate increases by 0.74 percent at the first vesting point in month 12. This is almost 6 times as high as the exit rate in the month immediately before vesting, which is merely 0.13 percent. Notice from Figure 5, however, that turnover is somewhat suppressed immediately before vesting. Thus comparing the RD coefficient with the mean at month $c - 1$ alone may overstate the relative size of the retention effect. So we also compare the coefficient with the average exit rates over the preceding six and twelve months, and find it still 2.6 and 2.3 times as high, respectively. Similarly at the second vesting point in Column (2), there is also an increase of 0.45 percent, the magnitude of which is comparable to the mean in earlier months but not statistically significant. At the last vesting point in Column (3), there is an even larger jump in exit rates of 1.14 percent, which is 15.8 times the mean in the previous month, or 4 (2.74) times that over the previous six (twelve) months. Both the magnitudes and statistical significance of these estimated coefficients are stable under alternative bandwidth choices.

[TABLE 4 ABOUT HERE]

[7] Granted, not all jumps are statistically significant, which we will discuss later when showing the regression results.

In Panel B, we include the full set of covariates, and the results are very similar.[8] Some of the estimates do become smaller in magnitude, although still on the same order. The same is true if we fit a fourth-order polynomial instead of a linear trend on both sides of the thresholds.[9] Note that in both panels, we also run the same regressions for months 48 and 60 in Columns (4) and (5) as a falsification test. Recall that Plan 1 options would have vested completely in month 36, beyond which point the vesting schedule should have no effects. Indeed, none of the coefficients are significantly positive, either economically or statistically, which is consistent with the retention effect story.

That the retention effects are only significant at the first and the last vesting days is somewhat curious. We conjecture that it may be the result of behavioral responses from the option owners. For example, the first and the last vesting days could be more "salient" than the vesting day halfway through, thus having stronger retention effects. We leave it to future work to explore this conjecture.

Table 5 reports the same set of RD regression estimates on Plan 2 option owners.[10] Recall that Plan 2 options vest in three equal batches in months 24, 36, and 48. We find strong retention effect at the last vesting point on the order of 1 percent. This is about 3 to 6 times the average exit rates in the preceding month, or 1.3 to 2.7 times those in the preceding six months. Moreover, this effect is robust to alternative bandwidth choices and regression specifications. Similarly to Plan 1, however, there are no significant effects at the first two vesting points.

[TABLE 5 ABOUT HERE]

We proceed to explore the heterogeneity in retention effects across employees at different levels. Table 6 shows the RD regression results using bandwidth $h = 3$ by three groups of employees: those in levels 1 to 4, who are junior employees up to team leaders; those in 5 and 6, who are junior and senior managers; and those in 7 to 10, who are the higher level managers and vice presidents. Among Plan 1 owners, retention effects are the strongest for levels 5 and 6, and almost nonexistent for lower- or higher-levels. Nonetheless, there is some retention effect at the last vesting point for levels 7 to 10, too. Similarly for Plan 2, it is also the intermediate levels that display the most retention effects. The drastic reduction in sample size makes the estimation here very noisy, which partially explains the lack of significance.

[TABLE 6 ABOUT HERE]

4.2. Tests of alternative hypotheses

The jump upward in exit rates in 5 and the significantly positive RD coefficients in Tables 4 and 5 indicate that there exist strong and substantial dicontinuities at the vesting days. Now we use a series of tests to show that they are indeed the result of option retention effects and not driven by other factors that coincide with option vesting.

[8] In the interest of space, we only report here the RD coefficients, $\hat{\gamma}_1$. Estimated coefficients on the covariates are mostly insignificant and are presented in the Appendix.

[9] Results are reported in the Appendix, too.

[10] The coefficients on the covariates in Panel B are also reported in the Appendix.

We first plot in Figure 6 the percentage of option owners that exercise all vested shares on hand over time as a first step to disentangling the confounding factors. The figure shows that a lot more Plan 1 owners choose to exercise all the options they could in the vesting months. [11]

[FIGURE 6 ABOUT HERE]

In addition, Figure 7 plots the exercise decisions of those who quit. The vertical axis shows the cumulative fraction of options exercised, and the horizontal axis shows time relative to the month in which the owner quits. For both Plan 1 and Plan 2 owners that decide to leave, the fraction of shares exercised remains fairly flat until about four months prior to quitting, and starts to grow rapidly after that. This, combined with Figure 6, indicates that option vesting is indeed associated with more exercises, which the quitting employees are more inclined to do before leaving the company. Hence the driving force of the exit rate discontinuities is more likely to be options than other factors.

[FIGURE 7 ABOUT HERE]

Next we examine the patterns of non-owner quitting times as a falsification test. If the discontinuity we found is the result of confounding factors that are irrelevant to options, then one would expect to see similar discontinuities on non-owner exit rates over time as well. The top panel of Figure 8 plots the monthly exit rates of non-owners from the month of entry.[12] Despite small ups and downs, there is no significant discontinuity within the first five years of an employee's career in Ctrip.

[FIGURE 8 ABOUT HERE]

[FIGURE 8 ABOUT HERE]

We also test one especially plausible confounding factor, namely the cyclicality of contract years. Employees usually sign contracts for 1-5 full years when they enter Ctrip. Suppose they only quit after completing full contract years of service, and that the option grant dates coincide with the beginning of contract years, then the jumps we observe would be the result of contract year completion instead of option vesting. To test this alternative explanation, we plot in the bottom panel of Figure 8 the distribution of quitting months in contract years for option owners and non-owners. The horizontal axis show months after full contract years, and the vertical axis the fraction of turnover that happened in each month of the contract year. First, note that the non-owners are indeed more likely to quit in the first three months after completing a contract year. This is consistent with the cyclicality of contract years that we just proposed. However, there is no such tendency for option owners, who are almost equally likely to quit in any month of the contract year. The difference in the distribution of quitting time between option owners and non-owners rejects the cyclicality hypothesis, and further supports that the discontinuities are the result of option vesting.

[11] There is some evidence for Plan 2 owners, but the pattern is much noisier.

[12] The hike in exit rates between 0 and 6 months is due to probation periods in some positions.

5. Interpreting the baseline results

In this section, we take the baseline results and look into their implications on the cost and benefit of granting options. We first discuss the interpretation of the RD coefficients, especially how it relates to and differs from the total retention effects of options. We decompose the total retention effects into the effect of delaying quits and the effect of keeping employees for good, and quantify these two fractions via inference. Then we look at the performance of option owners as a first step to see whether it is worth the cost to keep them by granting options. Finally, we do a simple, back-of-the-envelop cost-benefit analysis of options, and discuss the conditions under which Ctrip would break even.

5.1. RD estimates and the retention effect

The baseline RD estimates show that option plans with a vesting schedule have strong causal effects on retaining employees. But the retention effect is not straightforwardly the RD coefficient, which is the local average treatment effects (LATE) on compliers. Recall the treatment-determining variable, $D = 1\{t \geq c\}$, and define option-owner dummy $W = 1\{\text{option owner}\}$ and outcome dummy $Z_d^w = 1\{\text{quit when } W = w \text{ and } D = d\}$.[13] The fractions of always-takers, compliers, and never-takers in our setting are defined as

$$\pi_A := \Pr(\text{always-taker}) = \Pr(Z_0^1 = 1, Z_1^1 = 1) \tag{2}$$

$$\pi_N := \Pr(\text{never-taker}) = \Pr(Z_0^1 = 0, Z_1^1 = 0) \tag{3}$$

$$\pi_C := \Pr(\text{complier}) = \Pr(Z_0^1 = 0, Z_1^1 = 1) \tag{4}$$

respectively. Note that there are no defiers with $(Z_0^1 = 1, Z_1^1 = 0)$ because quitting when $t < c$ necessarily lead to $Z_1^1 = 1$ in our context.

Our setting differs from the conventional setup for LATE in that the never-takers and the compliers have been in both the no-treatment state and the treatment state, as they are in the sample both when $t < c$ and when $t \geq c$. Hence, unlike most LATE analyses, we can identify the always-takers, never-takers, and compliers from their observed quitting behavior. The RD regression captures the LATE on compliers alone, i.e. π_C. Now we show how this relates to the total retention effect of options that we are interested in.

The ideal measure of the retention effect is the difference between the turnover rate among option owners and the counterfactual turnover rate among the same group of employees without options. Note that the turnover rates are necessarily defined on a group of employees, which consists in theory of the following

[13] If $Z_0^W = 1$, i.e. the individual has quit when $t < c$, then we denote $Z_1^W = 1$, too.

subgroups:

$$\begin{aligned}
\pi_l &:= \Pr(\text{the loyal}) = \Pr(Z_0^0 = 0, Z_1^0 = 0, Z_0^1 = 0, Z_1^1 = 0) && (5)\\
\pi_q &:= \Pr(\text{the quitter}) = \Pr(\max(Z_0^0, Z_1^0) = 1, Z_D^1 = Z_D^0,\ \forall D) && (6)\\
&= \Pr(Z_0^0 = 0, Z_1^0 = 1, Z_0^1 = 0, Z_1^1 = 1)\\
&\quad + \Pr(Z_0^0 = 1, Z_1^0 = 1, Z_0^1 = 1, Z_1^1 = 1)\\
\pi_r &:= \Pr(\text{the retained}) = \Pr(\max(Z_0^0, Z_1^0) = 1, Z_0^1 = 0, Z_1^1 = 0) && (7)\\
\pi_d &:= \Pr(\text{the delayed}) = \Pr(Z_0^0 = 1, Z_1^0 = 1, Z_0^1 = 0, Z_1^1 = 1) && (8)
\end{aligned}$$

The loyal are those who won't quit regardless of options; *the quitters* are those who plan to quit (either before or after c) and whose decisions are not affected by options in any way (including the time to quit); *the retained* are those who plan to quit if without options, but chose to stay after becoming option owners; *the delayed* are those who plan to quit if without options, but would push back the quitting date till after some vesting day if granted options.

Among these subgroups, options play no role on the loyal and the determined quitters, but do reduce turnover by affecting both the delayed and the retained. Therefore the total retention effect is the reduction in turnover among the delayed plus the fraction of the retained among all option owners. Under innocuous assumptions, we find the following relationship between these four subgroups and the always-takers, never-takers, and compliers discussed earlier in this subsection:[14]

$$\pi_d = \pi_C - \pi_A, \quad \pi_q = 2\pi_A, \quad \pi_l + \pi_r = \pi_N \tag{9}$$

It is clear from the first equation that the RD coefficient, π_C, only captures the retention effect on the delayed. Despite their initial plan to quit, none of those in the retained subgroup actually quit in our sample. Thus the RD estimates do not include the reduced turnover coming from them. In this sense, the RD estimates net of π_A provide a fairly conservative lower bound of the total retention effects.

Another implication of Equation (9) is that it is impossible to separately identify the fraction of the retained from the loyal, because the only information we have is $\pi_l + \pi_r = \pi_N$. Yet the retained are potentially important contributors of the total retention effect, and could be the primary target group of firms when granting options. Hence we resort to alternative methods to estimate the total retention effect, use it together with the RD estimates to infer the unobservable fraction of the retained, π_r, and discuss their importance relative to the delayed.

Before we proceed to this exercise in the next subsection, note that the RD estimates still shed important light. They identify the causal effect of options on reduced turnover, which shows strong evidence that retention effects do exist. They also serve as a first step in quantifying the magnitude of the retention effects by providing a conservative lower-bound estimate.

[14] The complete proof is in the appendix.

5.2. Quantifying the fraction of the retained

To quantify the fraction of the retained, we first estimate the total retention effect, $\pi_r + \pi_d$, by predicting the hypothetical exit rates of option owners should they have no options. One way to do that is to use the general non-owners to estimate the elasticity of turnover to various factors, and use them to predict the turnover of option owners in the absence of options.

We estimate the following probit model on the sample of non-owners:

$$1\{\text{exit}\}_i = \Phi\left(\beta_0 + \beta_1 \log(\text{wage}_i) + X_i'\beta_2\right)$$

where β_1 captures the elasticity of turnover to wages, and β_2 captures the elasticities to other factors, X_i, which include worker age, gender, schooling, and firm-specific work experience. We also include a full set of department dummies and groups of job level dummies. The probit results are reported in Table A4, where all coefficients have expected signs.

We then use the elasticity estimates to predict the one-year turnover rate of option owners. Note that plugging in the actual wage of option owners would lead to biased results, because the counterfactual wage in the absence of options is most likely higher. Hence we first predict the would-be wage of option owners using their wages before receiving options and individual-specific wage growth rates. Then we use the predicted wage, along with other observable covariates, X, and the corresponding elasticities to get the predicted exit rates of option owners should they have no options. Granted, option owners and non-owners can still be somewhat different. Nonetheless, the predicted results are still informative as long as the *elasticities* of turnover to wages and other factors are the same between the two groups.

[TABLE 7 ABOUT HERE]

The first row of Table 7 reports the predicted one-year exit rates of option owners by level. Level 1-4 workers have the highest exit rate of 53.82 percent. This is not surprising because labor markets for the rank-and-file workers are usually the most fluid. That lower-level workers have accumulated less firm-specific human capital could also contribute to their high turnover. Level 5-6 managers, on the contrary, have a lower exit rate of 32.73 percent. And Level 7-10 executives have a high exit rate of 68.53 percent, too, should they have no options. Given the popularity of option awards for executives, those without options are either more likely to find a position elsewhere with a better compensation package, or are different in observable characteristics such as ability or seniority from option-owning executives. Both could explain the high hypothetical exit rate of executives in the absence of options.

Then second row of Table 7 shows the actual one-year exit rates of option owners. First, the actual exit rates are substantially lower than predicted for all levels. This is not surprising given the significant retention effects, although only on the delayed, found in RD regressions. Taking the difference gives the total retention effect, $\pi_r + \pi_d$, shown in the third row of the table. Option ownership greatly reduces one-year exit rates by 49.89 percentage points for Level 1-4 workers, 28.67 percentage points for Level 5-6 managers, and 66.08 percentage points for Level 7-10 executives. These total retention effects are large both in absolute terms

and relative to the predicted exit rates.

The total retention effect consists of that on the delayed, π_d, and that on the retained, π_r. Recall that $\pi_d = \pi_C - \pi_A$, where π_C is the RD coefficient and π_A is the fraction of employees who quit regardless of option vesting.[15] Annualized retention effects on the delayed, shown in the fourth row of Table 7, turn out to be considerable smaller than the total retention effect. For example, only 2.83 percentage points of the reduced exit rates are from the delayed for Level 1-4 workers. And although the retention effect on the delayed contributes 13.23 percentage points for Level 5-6 managers, it is still less than one-half of the total retention effect. Consequently, the implied retention effects on the retained make up the larger share, as shown in the fifth row of the table.

For easy comparison of the relative shares of the two sources of retention effects, the last two rows of Table 7 shows the fraction of retention on the delayed and the retained, respectively. For the lowest and the highest levels, options mainly reduce turnover by retaining people for good instead of merely making them delay quitting for a few months. The retained contribute to at least 80 percent of the total effect for these two levels. For Level 5-6 managers, however, a larger fraction of the total retention effects comes from the delayed, with the retained contributing a modest 53.84 percent.

Despite the heterogeneity across job levels, the fractions of the retained are overall fairly large. This further supports the results of Equation (9) that looking at π_d alone would result in considerable underestimation of the total retention effects. By combining the causal effects found in RD regressions and simple estimates of the total retention effects, we manage to gauge the unobservable fraction of the retained. And this is very important in evaluating the cost and benefit of options, as keeping workers for a longer time is more valuable to the firm than keeping workers for a few more months till the options vest. [TBC]

5.3. Performance of option owners who quit and who do not

Before evaluating the cost and benefit of options, we look at the performance of option owners, because retaining employees does not generate value in itself unless those retained have better than average performance. Figure 9 plots the performance of Ctrip employees between 2004 and 2012 as measured by their performance score, which is a number between 0 and 1 assigned by a director at the end of each quarter.[16] The scores do not translate into a concrete measure of performance like revenues or attendance, and should be interpreted in relative terms. The two plots in Panel A shows the average scores up to two years before leaving for Plan 1 and Plan 2 owners who quit within our sample period, respectively. Although there is a modest downward trend one year prior to quitting, the decrease is not often significant between 12 and 24 months. We look further at the performance scores of option owners who have not quit within our sample period, i.e. the retained, in Panel B. The scores of the retained option owners remained steady up to 5.5 years after the grant date for Plan 1, and even increased for Plan 2. Finally, Panel C compares the performance scores of option owners and non-owners. The average option owner has consistently higher scores than non-owners. These results show that the retained option owners are indeed more valuable to the company,

[15] We use RD coefficients from our preferred specification with bandwidth $h = 3$, and estimate π_A as the annualized exit rates of employees three months before the first vesting day to be consistent with the bandwidth choice.

[16] Employees who receive piece-rate pays do not have performance scores, thus not included in the sample.

even if some of them may have declining performances before quitting.

[FIGURE 9 ABOUT HERE]

One potential problem with performance scores is that they reflect the subjective opinion of an employee's director, which may very likely be adversely affected if the director knows the employee is to quit in a few months. To help mitigate this problem, we look at sales department employees, for whom sales revenues are an important dimension of monthly evaluation and make an objective measure of performance. We restrict our sample to 2006 to 2012, when we have individual-level monthly sales data. Panel A in Figure 10 plots the log of monthly sales made by option owners who quit in our sample period. Unlike performance scores, the sales revenues have no significant drop up to 24 months prior to leaving. Panel B plots the log sales of option owners who have not quit, and again find no significant downward trend after the grant date. Panel C shows that the option owners in general also make consistently higher sales than non-owners. Overall, these results show that option owners, quitting or not, do create more value for the company. This sheds light on why companies grant options to certain employees in the first place, and is the first step to justify the substantial cost of granting using the retention effect of options.

[FIGURE 10 ABOUT HERE]

5.4. A simple cost-benefit analysis

Given that the retained option owners do create more value for the company, we now do a simple, back-of-the-envelop cost-benefit analysis to see whether the retention effect of options is enough to offset the cost of options to the company.

[TABLE 8 HERE]

The first row of Table 8 shows the hypothetical wage, W, that an option owner would get should she have no options, which is estimated using her past wages and annual wage growth rates. The second row shows the actual wages of option owners, w, and the third row takes the difference between the two and shows an estimate of the amount of wage cost that Ctrip have saved on option owners, s. The fourth row shows the Black-Scholes value (BSV) of options in the grant year, C, estimated using parameters disclosed in the Ctrip annual reports. Then the fifth row calculates the expected BSV of options that have vested and not reverted to Ctrip because of owner turnover one year after granting, $c = C \times (1-e)/3$. Finally, the sixth row estimates the benefit of options from retaining an employee, $b = r \times W$, where r is the total retention rate and W is a proxy for the per-employee annual output.

In a simplified framework, the company breaks even if the cost of options (c) equates the total benefit of options, including both saved wages (s) and retention benefits ($\tau \times b$):

$$c = s + \tau \times b$$

where τ is a turnover cost coefficient that captures how costly it is to lose, or how valuable it is to keep, an employee, measured in multiples of that employee's annual output.

Instead of giving a binary answer that benefits exceed costs or not, we calculate the τ value such that the company just breaks even. For Level 1-4, turnover costs need to be 2.5 times the quitting employee's annual output in order to justify the cost of retaining her via option grants. The figures are even larger for higher levels due to the high value of their option awards. As a reference, Carter and Lynch (2004) find that the replacement cost of a quitting employee is 50 to 200 percent of the annual wage. Thus the break-even τ values seem relatively high, making it insufficient to justify the cost of options with saved wage costs and retention effects alone. Note that we have not taken into account the incentivizing effect of options, which may further offset the cost. Nonetheless, the retention effect of options with a vesting schedule does help understand the popularity of options, especially at the lower level.

6. Quitting before the vesting day

Before concluding the paper, we look into the details of option owners' quitting behavior. Because all unvested options upon quitting revert to the company, postponing quits near the vesting day leads to a sharp decline in the option value foregone. In spite of this discontinuous benefit, there are still a small but non-negligible number of employees who chose to quit shortly *before* the vesting days, leaving considerable money on the table. We examine the characteristics of these "early quitters" and explore what drives their curious quitting decisions.

We focus on the subsample of employees who quit within a month on either side of the vesting day, and compare the characteristics of early quitters with those of the others. Table 9 reports the t-test results. For both Plan 1 and Plan 2 owners, we find that the early quitters and the delayed are highly similar in most aspects, including age, general and firm-specific work experience, and job level. Nonetheless, they do differ in education attainment: the early quitters with Plan 1 have on average about 1 year less schooling than the delayed. They also have higher monthly wages, although the difference is only marginally significant. Early quitter among Plan 2 owners, on the other hand, earn higher wages.

[TABLE 9 ABOUT HERE]

The rest of Table 9 compares the value of options to vest (one-third of total options) between early quitters and the delayed. We calculate the Black-Scholes value of options as the hypothetical gain from exercising the options in the month they quit, although the early quitters cannot exercise the unvested options. For Plan 1 owners, the value of options forgone by early quitters is much less than that of the delayed, although not statistically significant. This may partially explain why they are willing to give up the options and quit right before the vesting day. For Plan 2 owners, the difference is much smaller across all levels.

Granted, the above is merely a quick comparison based on a small subsample of employees, but it still sheds some light on the possible reasons why people quit right before the options vest. Note that the prominent difference of early quitters holding Plan 1 is their lower education. Therefore the early quitters

holding Plan 1 might have quit because of bounded rationality or misperception of option values. On the other hand, those quitting immediately before Plan 2 options vest seem to have a greater stock of human capital, as reflected by their higher wages. They may have quit early because of even better outside offers that could make up for the foregone options.

7. Concluding remarks

In this paper, we use RD regressions to quantify the causal effect of employee option plans with a vesting schedule on retaining employees. Taking advantage of the discontinuous change in foregone option values around the vesting days, we find that options help reduce employee turnover by about 1 percent per month using detailed individual-level panel data from China. We also show that the retention effects are strongest at the first and the last vesting days, which could be driven by behavioral responses of option owners. We evaluate alternative explanations of the discontinuities, and find that confounding factors such as time trend or cyclicality are unlikely to be driving the results.

The immediate policy-relevant question stemming from our findings is that whether the benefit of options is large enough to offset their costs. To that end we discuss how the RD estimates relate to the total retention effect of options, gauge the size of effects from different subgroups of option owners, and find that both short-term retention effects (delaying quits) and longer-term effects (keeping employees for good) are important, although the latter are usually larger. We also show that the retained employees are indeed valuable to the company, and use a very simple cost-benefit analysis to get some idea of the conditions under which the benefits of options offsets their costs. A thorough cost-benefit analysis, however, is likely to be complex and requires some restrictive assumptions without more detailed data. For example, the presence and magnitude of incentive effects alone could be hard to quantify.

Our study sheds light on the puzzling popularity of options among all levels of employees. The significant and robust retention effects can be an important justification for the prevalence of options with a vesting schedule, which is hard to explain using traditional incentive-provision stories alone. That options have significant retention effects also suggest why there is a vesting schedule in the first place. Given that options hardly induce long-term efforts by aligning the interest of employees and that of the company, having a vesting schedule can at least retain option holders for the short term. On the other hand, the substantial retention effects from the retained suggests that it is not just the vesting schedule that is working, which should merely delay quitting over the short run. That a sizable fraction of the reduction in exit rates is from the retained shows that the option values are also playing an important role.

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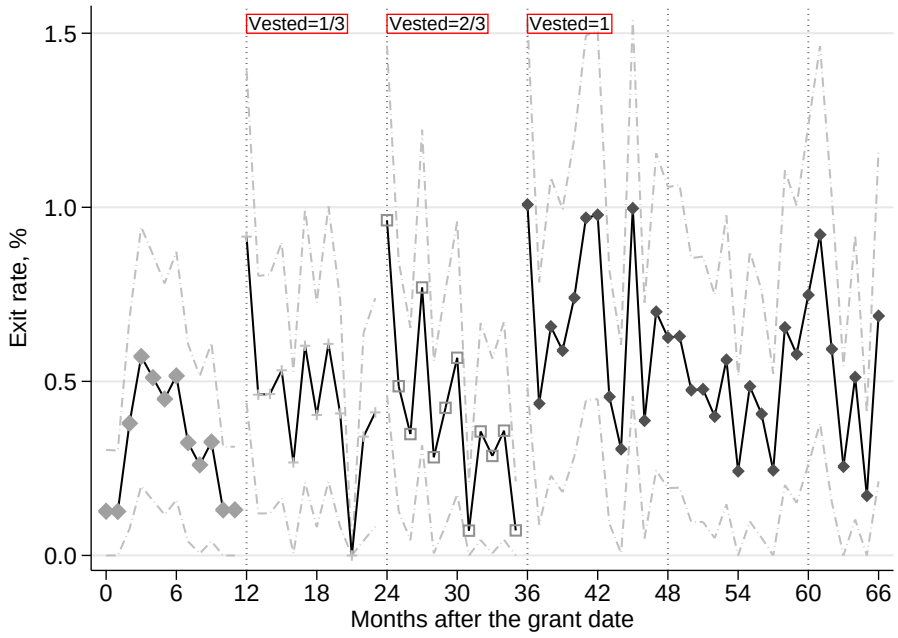

Figure 1: Monthly exit rates of Ctrip Plan 1 option owners

Notes: Ctrip Plan 1 options vest in three equal batches 12, 24, and 36 months after granting, at which time one-third, two-thirds, and all options have vested. The horizontal axis shows months after option granting, with the grant date being the first day of month 0. Vesting days are marked by dotted vertical lines with text labels showing the cumulative fraction of options vested so far. The vertical axis plots the monthly exit rates (in %). The solid line shows the average monthly exit rates, and the gray dashed lines two standard errors above and below the mean.

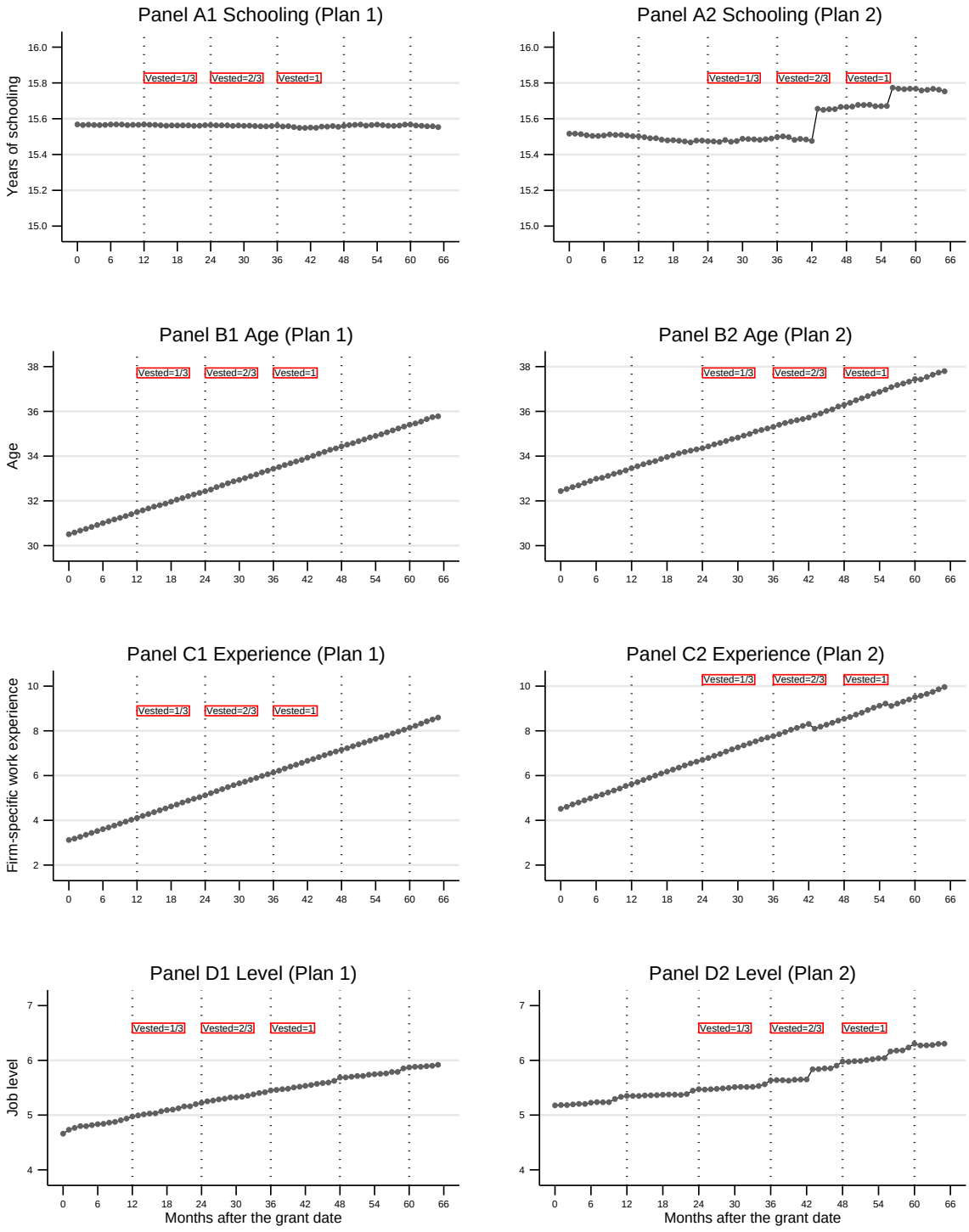

Figure 2: Predetermined variables over time

Notes: The eight panels plot the value of four predetermined variables, years of schooling, age, years of experience in Ctrip, and job level (1-10), for Plan 1 and Plan 2 option owners.

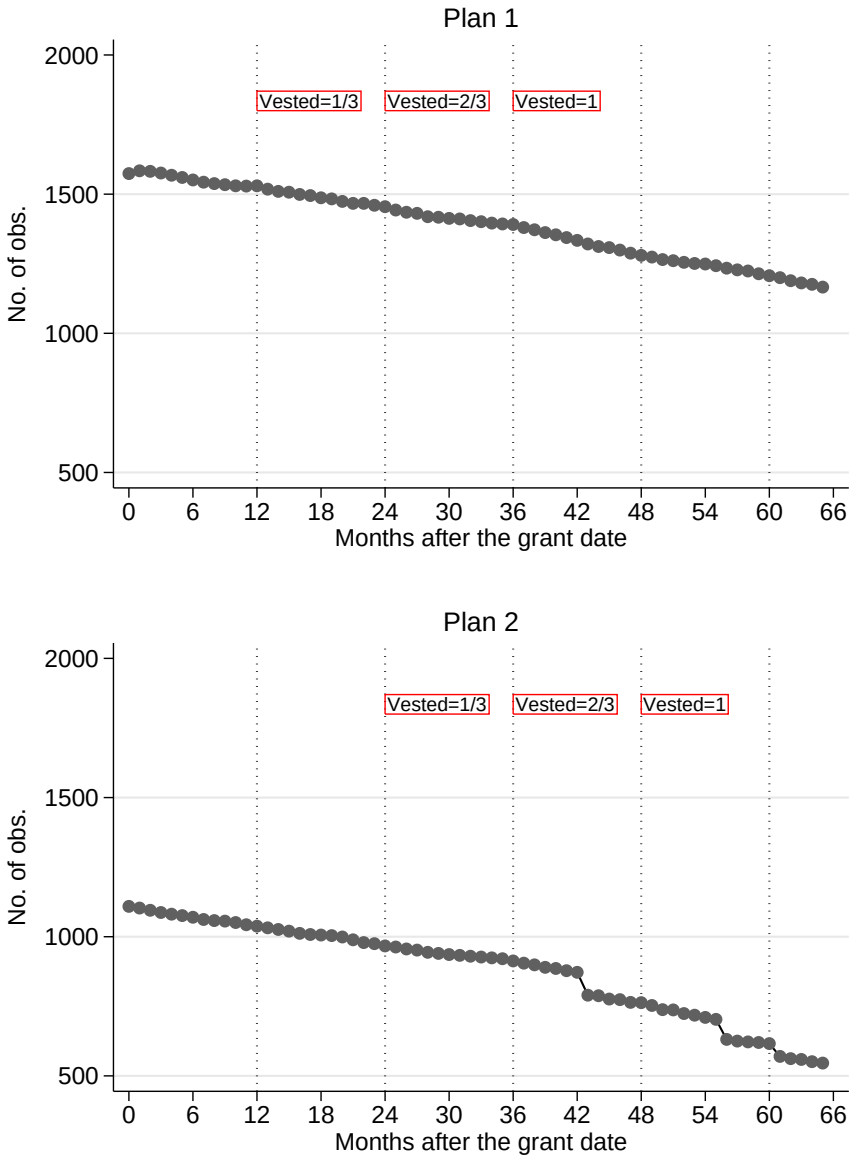

Figure 3: The number of observations over time

Notes: The two panels plot the evolution of sample sizes for Plan 1 and Plan 2 option owners, respectively. Each observation is an option grant.

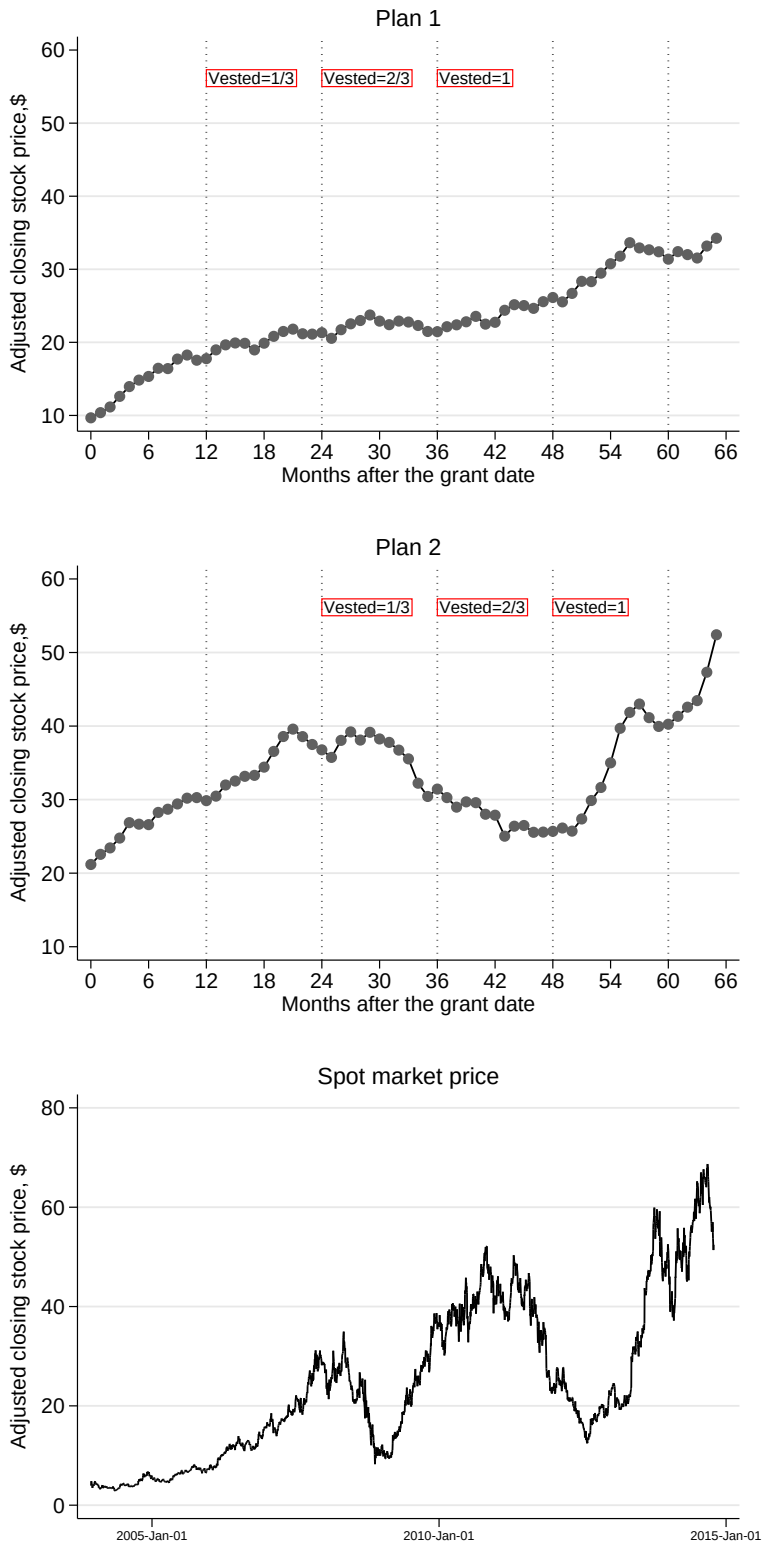

Figure 4: Ctrip stock prices over time

Notes: The first two plots show the evolution of average Ctrip stock closing prices (in nominal US dollars, adjusted for stock splits and other corporate actions) after granting Plan 1 and Plan 2 options, respectively. The bottom plot shows the adjusted closing prices over calendar time.

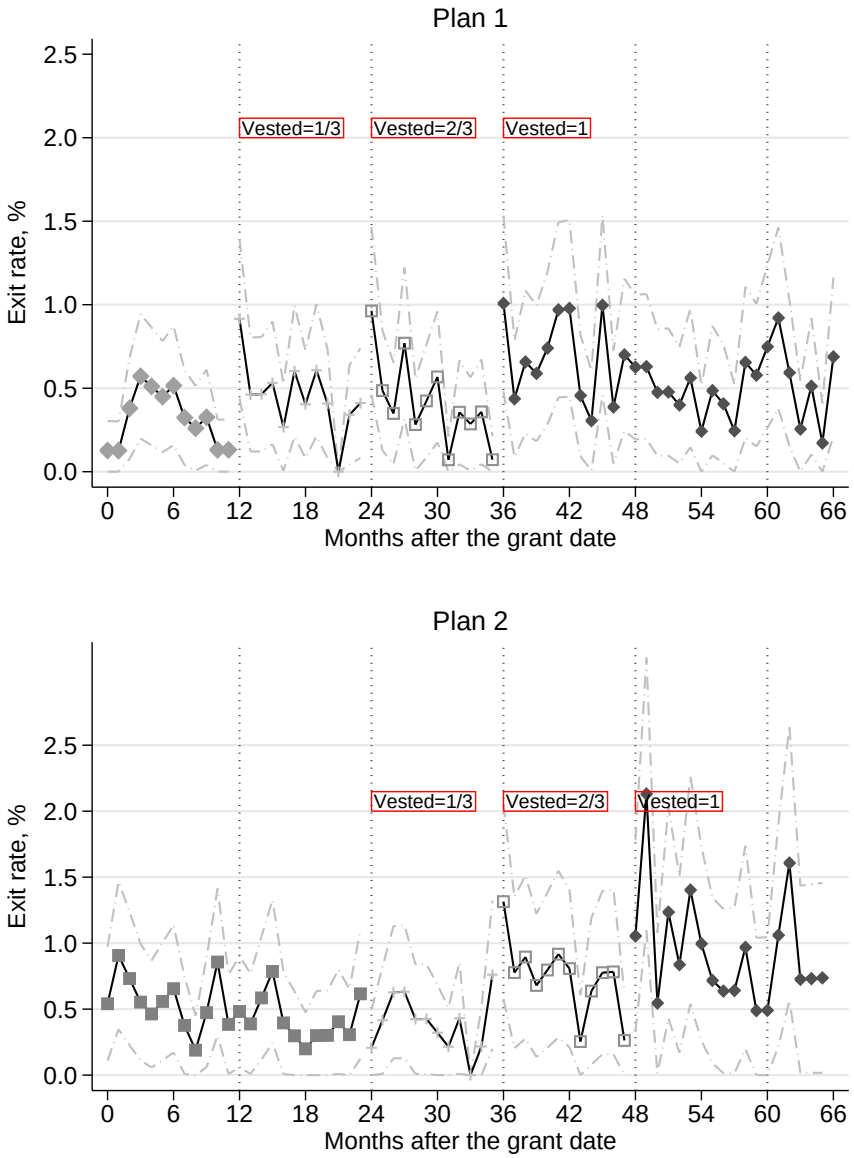

Figure 5: Monthly exit rates of option owners

Notes: The horizontal axis shows months after option granting, with the grant date being the first day of month 0. Plan 1 options vest in three equal batches 12, 24, and 36 months after granting, at which time one-third, two-thirds, and all options have vested. Plan 2 options vest in a similar fashion in months 24, 36, and 48. Vesting days are marked by dotted vertical lines with text labels showing the cumulative fraction of options vested so far. The vertical axis plots the monthly exit rates (in %). The solid line shows the average monthly exit rates, and the gray dashed lines two standard errors above and below the mean.

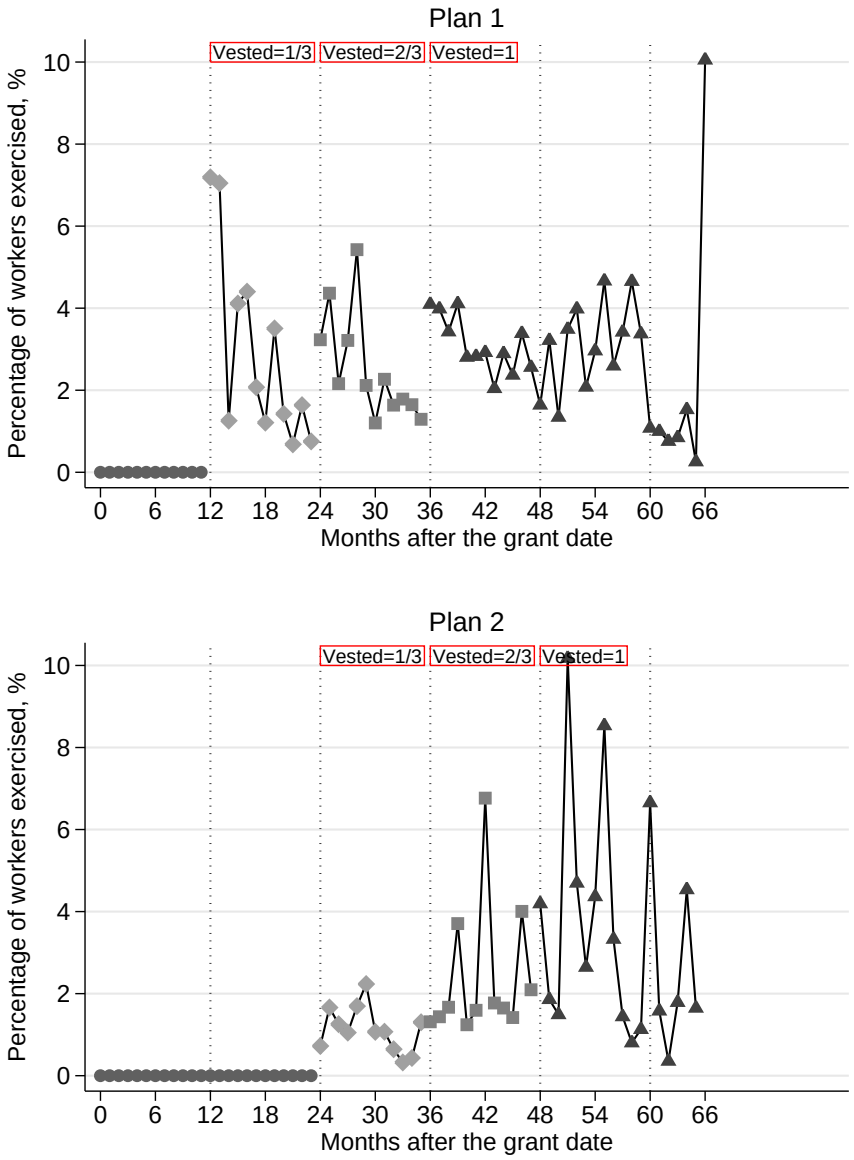

Figure 6: Percentage of option owners exercising all vested options

Notes: The percentage of option owners exercising all vested options in a given month (relative to the grant date) is the number of people who exercise at least the number of most recently vested shares in that month (i.e. one-third of total shares granted), divided by the total number of option owners in that month.

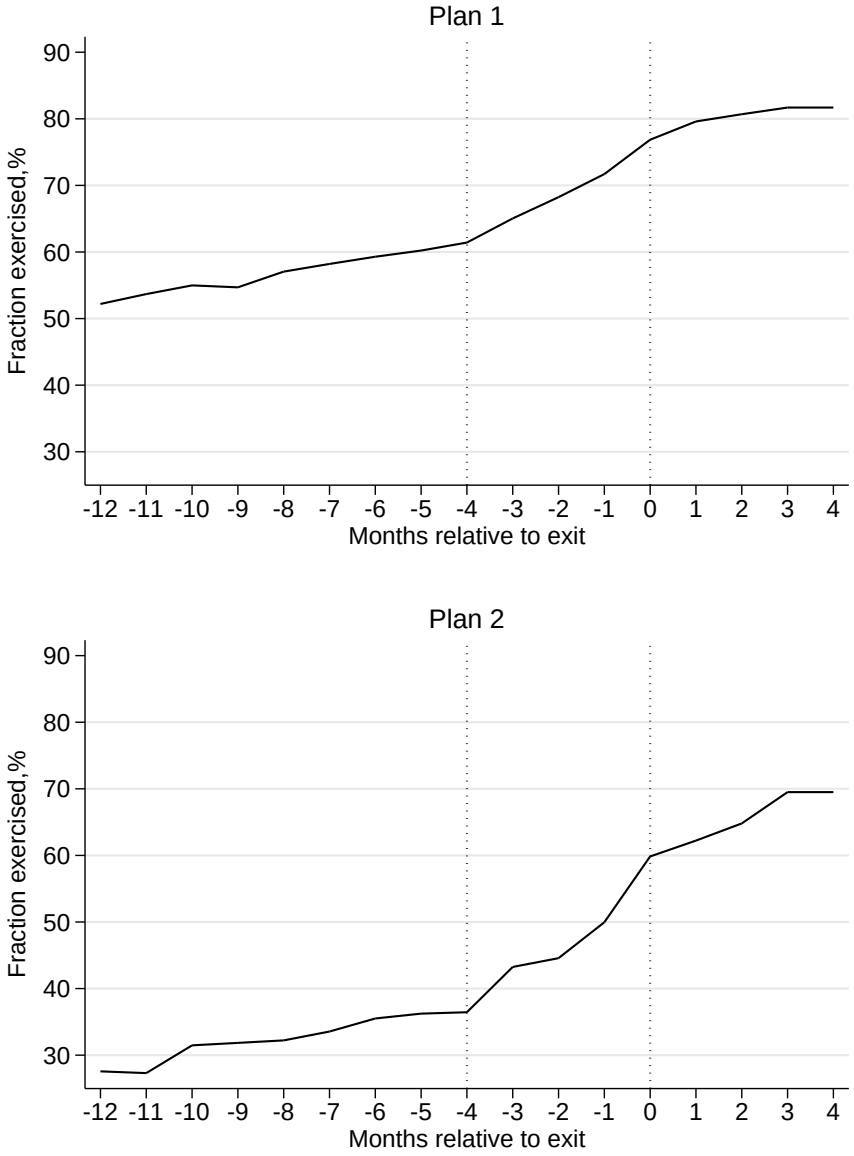

Figure 7: Cumulative fraction of shares exercised over time by quitting employees

Notes: A quitting employee is defined as an option owners who left Ctrip during our sample period. We track all such employees' option exercising behavior and calculate the cumulative fraction of shares exercised in month s as the number of shares they have exercised so far, divided by the total number of shares granted, where s is measured relative to the month in which they quit (denoted as month 0). We then take the average across all quitting employees. The fraction does not go up to 100% because some quit while still having unvested options, all of which will be returned to Ctrip upon leaving.

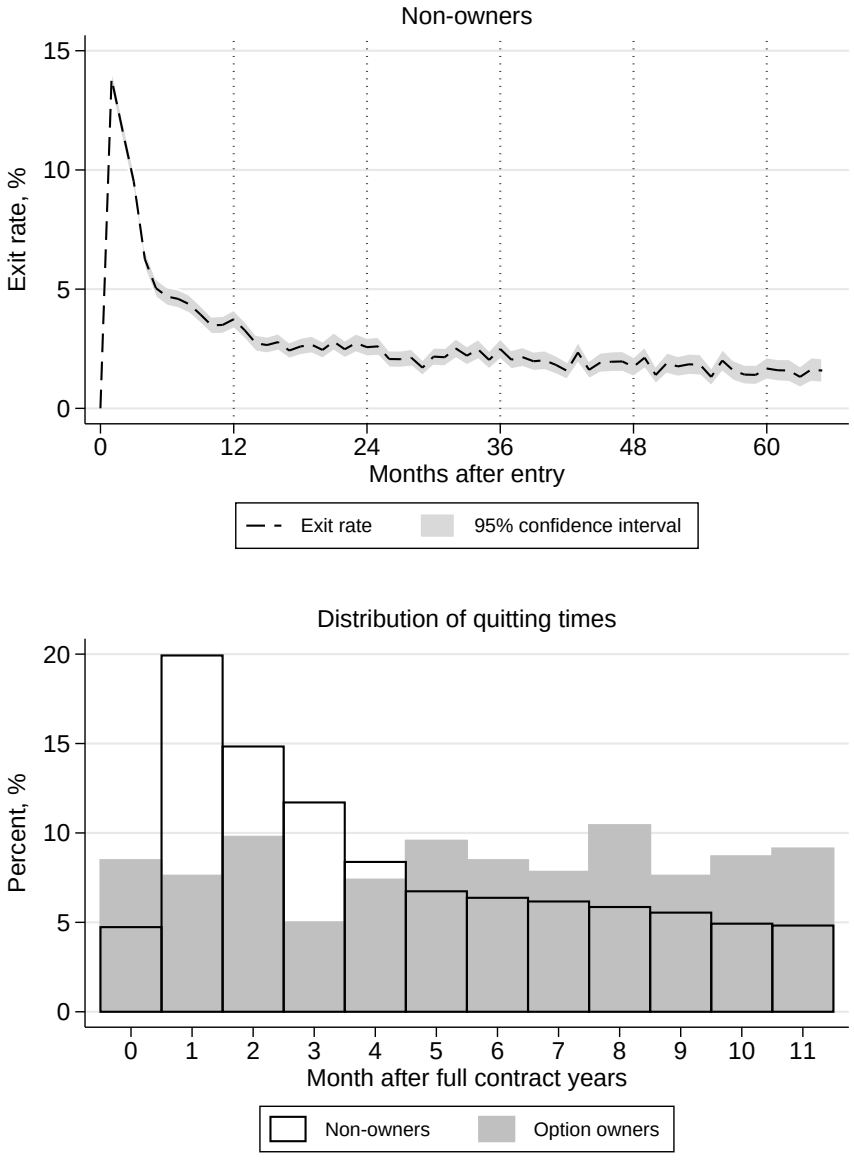

Figure 8: Patterns of quitting time: non-owners versus owners

Notes: We restrict the sample to non-owners and owners of Plan 1 options who joined Ctrip in 2000-2007 and in 2009, during which time Plan 1 options were offered. The top panel plots the monthly exit rates of non-owners after entry. The bottom panel shows the distribution of the quitting month in a contract year, which begins on the day-of-the-year of the entry date. For example, 0 means quitting in the last month of a contract year; and 3 means quitting in the third month after a full contract year.

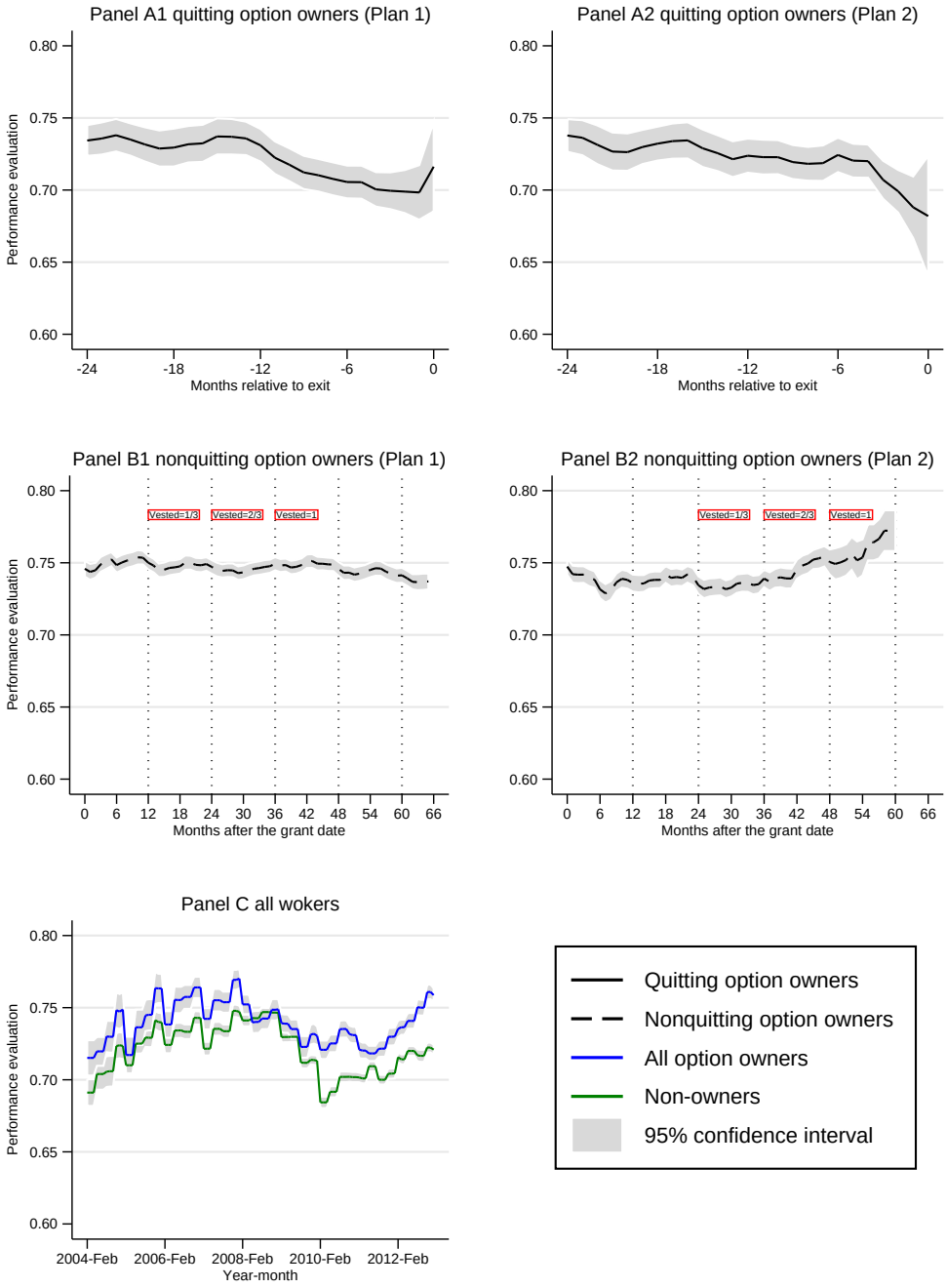

Figure 9: Performance evaluation scores of option owners and non-owners

Notes: We restrict our sample to employees who are not earning piece-rate pays from 2004 to 2012, during which time we have data on performance scores. Performance scores are numbers between 0 and 1 and are given by directors to reflect an employee's personal performance in each quarter. The numbers should be interpreted in relative terms. Panel A plots the performance score of option owners up to two years before quitting. Panel B plots the performance scores of option owners who have not quit within our sample period (up to 66 months after the grant date). Panel C compares the performance scores of option owners and non-owners.

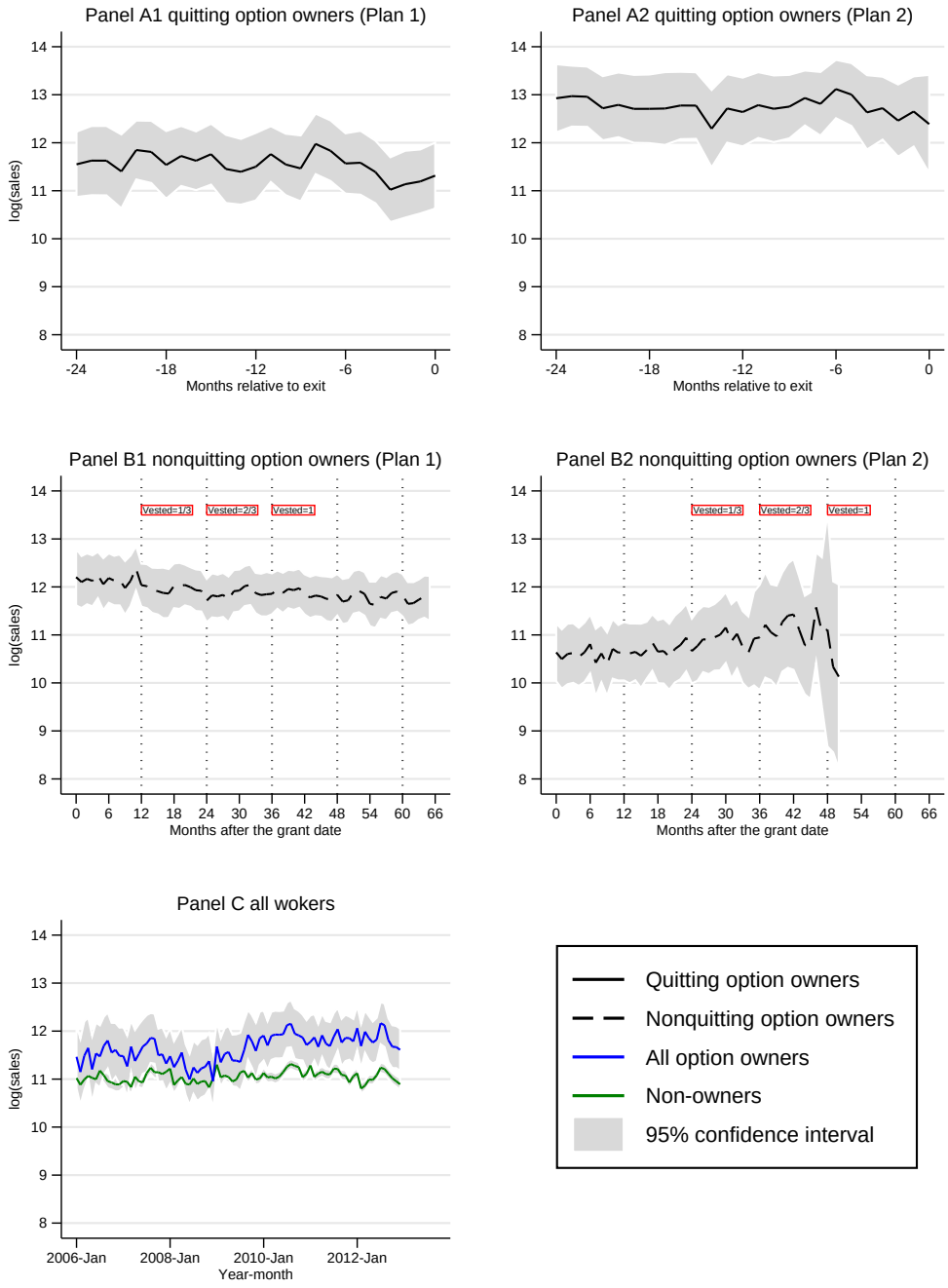

Figure 10: Sales made by option owners and non-owners

Notes: We restrict the sample to sales department employees from 2006 to 2012, when we have data on individual-level monthly sales. Sales are measured in 2014 yuan. Panel A plots the log of sales revenues of option owners up to 24 months before quitting. Panel B plots the sales revenues of option owners who have not quit within our sample period (up to 66 months after the grant date). Panel C compares the sales made by option owners and non-owners between 2006 and 2012.

Table 1: The Representativeness of Ctrip data of China's working population

	UHS		All in Ctrip		P-value	Option owners in Ctrip		P-value
	Mean	Std.Dev.	Mean	Std.Dev.	of (3)-(1)	Mean	Std.Dev.	of (6)-(1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. 2002-2004								
Monthly wage	1406.80	1085.79	3683.22	6862.05	0.00	12799.03	14814.52	0.00
Age	40.78	9.21	25.66	7.17	0.00	29.14	5.36	0.00
Work experience	20.67	3.89	4.01	8.36	0.00	4.49	6.76	0.00
Years of schooling	12.54	2.89	13.24	1.99	0.00	15.29	1.51	0.00
1{male}	0.54	0.50	0.38	0.49	0.00	0.45	0.50	0.00
Obs.	123175		7302			537		
Panel B. 2005-2009								
Monthly wage	2080.16	1694.98	4263.38	5257.04	0.00	15148.60	16396.64	0.00
Age	41.13	9.28	25.00	5.81	0.00	31.24	5.35	0.00
Work experience	20.28	8.46	4.01	6.05	0.00	6.99	6.34	0.00
Years of schooling	12.92	2.98	13.79	1.93	0.00	15.39	1.64	0.00
1{male}	0.55	0.50	0.36	0.48	0.00	0.50	0.50	0.00
Obs.	226619		47556			2556		
Panel C. 2010-2014								
Monthly wage	3244.78	2778.07	6076.54	7660.59	0.00	18126.65	17803.59	0.00
Age	40.79	9.41	25.82	5.61	0.00	33.76	5.30	0.00
Work experience	19.45	10.77	4.61	5.35	0.00	10.48	5.91	0.00
Years of schooling	13.04	3.01	14.16	1.86	0.00	14.94	1.95	0.00
1{male}	0.56	0.50	0.34	0.47	0.00	0.46	0.50	0.00
Obs.	45507		91700			5970		

Notes: The UHS sample is restricted to those currently working but excludes those in a paid job after retirement. The monthly wages are in real 2014 yuan. Work experience in UHS is defined as the number of years since the first job, but in Ctrip data it is the number of years since the most recent graduation.

Table 2: Summary statistics on option-related variables

	Plan 1		Plan 2	
	Mean	Std.Dev.	Mean	Std.Dev.
Shares granted	9274.51	24900.86	6405.46	9987.55
Strike price/spot market price	1.06	.64	2.45	.84
Relative value	9.43	7.94	13.48	11.88
Monthly base wage (yuan)	8345.10	6096.76	10885.13	8041.15
1{have other options}	.62	.49	.66	.47
1{have other options in Plan 1}	.61	.49	.56	.50
1{have other options in Plan 2}	.10	.30	.39	.49
Relative value of other options	18.12	26.53	41.64	62.96
1{have restricted stocks}	.00	.00	.03	.17
Relative value of restricted stocks	.00	.00	.05	.43
1{level 1}	.05	.21	.05	.22
1{level 2}	.05	.22	.06	.25
1{level 3}	.16	.37	.05	.22
1{level 4}	.19	.39	.05	.21
1{level 5}	.23	.42	.35	.48
1{level 6}	.19	.39	.25	.44
1{level 7}	.07	.26	.10	.30
1{level 8}	.04	.19	.04	.21
1{level 9}	.02	.13	.03	.16
1{level 10}	.00	.05	.01	.09
Observations	1609		1110	

Notes: All statistics are measured on the grant date. Relative value is defined as total option value divided by annual base wage, where total option value is Black-scholes value per share$\times$(shares granted)$\times$(exchange rate), and annual base wage is monthly base wage$\times$12. Relative value of other options and restricted stocks are defined likewise.

Table 3: RD on covariates

	Years of schooling	Age	Experience in firm	Level
Panel 1b: Plan 1, c=12				
RD coef.	0.002	0.009	-0.003	0.007
	(0.096)	(0.338)	(0.156)	(0.106)
Controls	No	No	No	No
Mean at c-1	15.566	31.405	4.026	4.936
N	8601	8810	9151	9139
Panel 1b: Plan 1, c=24				
RD coef.	-0.002	0.000	0.012	-0.016
	(0.097)	(0.340)	(0.160)	(0.106)
Controls	No	No	No	No
Mean at c-1	15.564	32.356	5.036	5.200
N	8392	8537	8727	8724
Panel 1c: Plan 1, c=36				
RD coef.	0.000	0.008	0.001	0.019
	(0.099)	(0.346)	(0.164)	(0.107)
Controls	No	No	No	No
Mean at c-1	15.559	33.350	6.060	5.417
N	8131	8222	8333	8333
Panel 2a: Plan 2, c=24				
RD coef.	-0.003	-0.002	0.004	-0.036
	(0.131)	(0.408)	(0.228)	(0.143)
Controls	No	No	No	No
Mean at c-1	15.478	34.302	6.622	5.446
N	5829	5829	5829	5829
Panel 2b: Plan 2, c=36				
RD coef.	0.006	0.002	-0.009	0.039
	(0.134)	(0.418)	(0.233)	(0.146)
Controls	No	No	No	No
Mean at c-1	15.490	35.237	7.698	5.564
N	5489	5489	5489	5489
Panel 2c: Plan 2, c=48				
RD coef.	-0.014	-0.052	-0.015	0.023
	(0.134)	(0.452)	(0.248)	(0.149)
Controls	No	No	No	No
Mean at c-1	15.666	36.217	8.457	5.903
N	4568	4568	4568	4568

Notes: RD estimation using local linear regression on both sides of the threshold, c, with rectangular kernels and bandwidth h= 3. Control variables are not included in all specifications. N is the number of observations. Mean at (c-1) reports the mean of the dependent variable immediately preceding the threshold, i.e. in month (c-1). Standard errors are in parentheses.

Table 4: RD regression on exit rates, Plan 1

	(1) c=12	(2) c=24	(3) c=36	(4) c=48	(5) c=60
A: Without Covariates					
h=2	0.00785***	0.00552*	0.00936***	-0.00074	0.00170
	(0.00261)	(0.00307)	(0.00276)	(0.00321)	(0.00331)
N	6101	5819	5550	5126	4830
h=3	0.00739**	0.00449	0.01144**	-0.00371	0.00296
	(0.00304)	(0.00437)	(0.00327)	(0.00540)	(0.00547)
N	9144	8717	8318	7693	7236
h=4	0.00785***	0.00224	0.00914***	0.00092	0.00103
	(0.00256)	(0.00331)	(0.00261)	(0.00427)	(0.00405)
N	12186	11617	11081	10259	9641
h=5	0.00746**	0.00345	0.00800**	-0.00023	0.00123
	(0.00222)	(0.00290)	(0.00241)	(0.00351)	(0.00344)
N	15227	14516	13836	12827	12049
h=6	0.00692***	0.00450*	0.00648***	-0.00076	0.00184
	(0.00202)	(0.00261)	(0.00221)	(0.00308)	(0.00304)
N	18272	17418	16586	15402	14453
Mean at c-1	0.00131	0.00411	0.00072	0.00700	0.00578
Mean at [c-6,c-1]	0.00282	0.00362	0.00286	0.00638	0.00435
Mean at [c-12,c-1]	0.00322	0.00453	0.00418	0.00687	0.00482
B: With Covariates					
h=2	0.00640**	0.00214	0.00410	-0.00323	-0.00134
	(0.00285)	(0.00426)	(0.00256)	(0.00390)	(0.00425)
N	5152	5195	5178	5020	4764
h=3	0.00746**	0.00067	0.00724**	-0.00456	-0.00200
	(0.00271)	(0.00377)	(0.00309)	(0.00414)	(0.00449)
N	7728	7785	7770	7531	7138
h=4	0.00666***	-0.00003	0.00663***	-0.00004	-0.00067
	(0.00233)	(0.00309)	(0.00244)	(0.00335)	(0.00358)
N	10301	10373	10358	10040	9508
h=5	0.00573**	0.00104	0.00604**	0.00021	0.00010
	(0.00211)	(0.00271)	(0.00216)	(0.00284)	(0.00307)
N	12777	12959	12942	12529	11879
h=6	0.00555***	0.00182	0.00516**	0.00005	0.00119
	(0.00201)	(0.00248)	(0.00201)	(0.00257)	(0.00276)
N	15252	15549	15503	15018	14244
Mean at c-1	0.00000	0.00309	0.00077	0.00556	0.00503
Mean at [c-6,c-1]	0.00081	0.00206	0.00167	0.00500	0.00345
Mean at [c-12,c-1]	0.00179	0.00231	0.00264	0.00541	0.00430

Notes: RD estimation using local linear regression on both sides of the threshold, c, with rectangular kernels and varying bandwidths. The dependent variable is an indicator for whether employee i exits the company in month t. RD coefficients are reported in rows labeled "h=...", and their standard errors are reported in the rows below. N is the number of observations. Mean at (c-1) reports the mean exit rates in months immediately preceding the threshold, i.e. month (c-1). Coefficients on covariates in Panel B are reported in the Appendix. Standard errors clustered at the individual level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 5: RD regression on exit rates, Plan 2

	(1) c=12	(2) c=24	(3) c=36	(4) c=48	(5) c=60
A: Without Covariates					
h=2	0.00098	-0.00409	0.00553	0.00791*	0.00002
	(0.00289)	(0.00251)	(0.00497)	(0.00454)	(0.00462)
N	4159	3881	3657	3038	2411
h=3	0.00542	-0.00719	-0.00055	0.01572**	0.00484
	(0.00522)	(0.00472)	(0.00678)	(0.00628)	(0.00768)
N	6241	5826	5476	4544	3595
h=4	0.00083	-0.00494	0.00077	0.01349***	0.00213
	(0.00364)	(0.00380)	(0.00564)	(0.00500)	(0.00555)
N	8318	7773	7287	6058	4774
h=5	-0.00121	-0.00388	0.00329	0.01180**	0.00172
	(0.00312)	(0.00322)	(0.00498)	(0.00455)	(0.00492)
N	10391	9719	9097	7562	6018
h=6	-0.00134	-0.00309	0.00413	0.00956**	0.00193
	(0.00286)	(0.00284)	(0.00446)	(0.00421)	(0.00472)
N	12467	11662	10904	9142	7264
Mean at c-1	0.00384	0.00616	0.00761	0.00263	0.00489
Mean at [c-6,c-1]	0.00489	0.00353	0.00324	0.00591	0.00746
Mean at [c-12,c-1]	0.00559	0.00422	0.00390	0.00754	0.00989
B: With Covariates					
h=2	-0.00110	-0.00401	0.00155	0.01299*	0.00317
	(0.00436)	(0.00350)	(0.00501)	(0.00746)	(0.00909)
N	4102	3830	3607	2998	2333
h=3	-0.00238	-0.00558	0.00149	0.01222*	-0.00230
	(0.00449)	(0.00389)	(0.00545)	(0.00652)	(0.00816)
N	6154	5748	5401	4484	3499
h=4	-0.00238	-0.00427	0.00344	0.01085*	-0.00486
	(0.00392)	(0.00301)	(0.00482)	(0.00567)	(0.00729)
N	8201	7668	7187	5978	4661
h=5	-0.00135	-0.00361	0.00558	0.01086**	-0.00272
	(0.00336)	(0.00260)	(0.00435)	(0.00513)	(0.00694)
N	10245	9587	8972	7462	5886
h=6	-0.00005	-0.00352	0.00560	0.00912**	-0.00158
	(0.00299)	(0.00245)	(0.00402)	(0.00464)	(0.00683)
N	12292	11503	10754	8946	7112
Mean at c-1	0.00389	0.00520	0.00662	0.00266	0.00497
Mean at [c-6,c-1]	0.00496	0.00324	0.00310	0.00565	0.00732
Mean at [c-12,c-1]	0.00568	0.00395	0.00387	0.00749	0.00992

Notes: RD estimation using local linear regression on both sides of the threshold, c, with rectangular kernels and varying bandwidths. The dependent variable is an indicator for whether employee i exits the company in month t. RD coefficients are reported in rows labeled "h=...", and their standard errors are reported in the rows below. N is the number of observations. Mean at (c-1) reports the mean exit rates in months immediately preceding the threshold, i.e. month (c-1). Coefficients on covariates in Panel B are reported in the Appendix. Standard errors clustered at the individual level are in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 6: RD regression on exit rates, by level

	(1) c=12	(2) c=24	(3) c=36	(4) c=48	(5) c=60
A: Plan 1					
level 1-4	0.00298	0.00587**	-0.00260	-0.00050	-0.00993
	(0.00332)	(0.00299)	(0.00388)	(0.00629)	(0.00736)
N	3438	3507	3534	3450	3280
Mean at c-1	0.00000	0.00000	0.00171	0.00521	0.00730
Mean at [c-6,c-1]	0.00031	0.00171	0.00142	0.00546	0.00391
Mean at [c-12,c-1]	0.00127	0.00128	0.00243	0.00498	0.00446
level 5-6	0.01301**	0.00013	0.01845**	-0.01311*	0.00286
	(0.00586)	(0.00709)	(0.00642)	(0.00731)	(0.00630)
N	3230	3226	3191	3070	2932
Mean at c-1	0.00000	0.00561	0.00000	0.00778	0.00407
Mean at [c-6,c-1]	0.00096	0.00249	0.00156	0.00549	0.00336
Mean at [c-12,c-1]	0.00244	0.00341	0.00264	0.00655	0.00333
level 7-10	0.00960	-0.01489	0.01345*	0.00576	0.00549
	(0.00741)	(0.01134)	(0.00774)	(0.00736)	(0.00485)
N	1060	1052	1045	1011	926
Mean at c-1	0.00000	0.00575	0.00000	0.00000	0.00000
Mean at [c-6,c-1]	0.00194	0.00191	0.00286	0.00196	0.00213
Mean at [c-12,c-1]	0.00149	0.00237	0.00332	0.00340	0.00674
B: Plan 2					
level 1-4	0.00572	0.00280	-0.01277	0.02066**	-0.02669
	(0.00630)	(0.00226)	(0.01686)	(0.00960)	(0.02293)
N	1375	1341	1244	824	522
Mean at c-1	0.00000	0.00000	0.01905	0.00000	0.01075
Mean at [c-6,c-1]	0.00360	0.00368	0.00629	0.00695	0.00960
Mean at [c-12,c-1]	0.00425	0.00220	0.00656	0.00724	0.01337
level 5-6	-0.00701	-0.00804	0.00573	0.01738*	0.00234
	(0.00659)	(0.00522)	(0.00577)	(0.00975)	(0.01111)
N	3652	3381	3183	2785	2279
Mean at c-1	0.00657	0.00530	0.00187	0.00214	0.00517
Mean at [c-6,c-1]	0.00619	0.00290	0.00217	0.00630	0.00737
Mean at [c-12,c-1]	0.00729	0.00456	0.00306	0.00771	0.00857
level 7-10	-0.01489	-0.01243	-0.00680	-0.01150	0.00140
	(0.01134)	(0.01036)	(0.01360)	(0.01059)	(0.01204)
N	1052	1026	974	875	698
Mean at c-1	0.00575	0.01163	0.00613	0.00680	0.00000
Mean at [c-6,c-1]	0.00191	0.00379	0.00203	0.00226	0.00526
Mean at [c-12,c-1]	0.00237	0.00418	0.00302	0.00709	0.01117

Notes: RD estimation using local linear regression on both sides of the threshold, c, with rectangular kernels and varying bandwidths. The dependent variable is an indicator for whether employee i exits the company in month t. RD coefficients are reported in rows labeled "h=...", and their standard errors are reported in the rows below. N is the number of observations. Mean at (c-1) reports the mean exit rates in months immediately preceding the threshold, i.e. month (c-1). Coefficients on covariates in Panel B are reported in the Appendix. Standard errors clustered at the individual level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 7: Inferring fractions of the delayed and of the retained

Unit:%	(1) Level 1-4	(2) Level 5-6	(3) Level 7-10
Predicted one-year exit rates without options (E)	53.82	32.73	68.53
Observed one-year exit rates of option owners (e)	3.93	4.07	2.45
Total retention effects (r=E-e)	49.89	28.67	66.08
Retention effects on the delayed ($r_d = 1-(1-(\pi_C-\pi_A))^{12}$)	2.83	13.23	10.93
Retention effects on the retained ($r_r = r- r_d$)	47.06	15.43	55.15
Fraction of the delayed (r_d/r)	5.67	46.16	16.54
Fraction of the retained (r_r/r)	94.33	53.84	83.46

Notes: Predicted one-year exit rates without options are counterfactual exit rates should the option owner have not received options. They are calculated using observable option owner characteristics and the elasticities of turnover to these characteristics estimated in the probit regression in Table A4. Observed one-year exit rates are actual exit rates one year after the grant date. Total retention effect is defined as the difference between counterfactual and actual exit rates. Retention effects on the delayed is the annualized retention effect within one year after the grant date as captured by the baseline RD regression under bandwidth choice $h = 3$. Retention effects on the retained are inferred from the total retention effects and the retention effects on the delayed as the residual effects.

Table 8: Cost-benefit analysis of Ctrip options

Unit:1000 yuan	(1) Level 1-4	(2) Level 5-6	(3) Level 7-10
Predicted wages without options (W)	82.31	129.66	256.52
Observed wages of option owners (w)	65.39	110.48	239.46
Saved wgaes (s=W-w)	16.92	19.18	17.06
Fair value of options (C)	373.79	888.84	3590.75
Fair value of options vested after 1 year (c=C×(1-e)/3)	119.70	284.23	1167.58
Retention benefit after 1 year (b=r×W)	41.06	37.17	169.51
Break-even turnover cost ((c-s)/b)	2.50	7.13	6.79

Notes: All monetary variables are in thousand 2014 yuan. Predicated wage without options are counterfactual wages estimated using wages in past years and the annual wage growth rate. Observed wages are actual annual wages of option owners. Saved wages is the estimated wage cost that Ctrip has saved on option owners. Fair value of options are average Black-Scholes values (BSV) of the options in the grant year as reported in the Ctrip annual reports. Fair value of options are expected value of options that have vested and not reverted to Ctrip one year after granting. The retention benefit after one year is defined as the expected value of employees (both the retained and the delayed), where the value of an individual employee is measured by her annual wage should she have no options. The break-even turnover cost is the cost, measured in multiples of the annual wage, W, of individual turnover such that the total benefit of options (the sum of saved wages and retention benefits) just offsets the total cost of options (BSV). For example, a break-even turnover cost of 2 means turnovers need to be as costly as twice the quitting employee's annual wage in order for the benefit of options to justify the cost of granting options.

Table 9: Comparison of characteristics between the early quitters and the delayed

	Difference=(early quitters-delayed)	
	Plan 1	Plan 2
Years of schooling	-1.031**	-0.015
	[-2.10]	[-0.03]
Age	0.871	2.679
	[0.55]	[1.41]
Years of work experience	1.472	2.694
	[0.78]	[1.63]
Years of experience in Ctrip	0.038	-0.238
	[0.04]	[-0.26]
Level	0.268	-0.085
	[0.35]	[-0.13]
Monthly wage(1,000 yuan)	2.520	15.644*
	[1.11]	[1.78]
Option values(1,000 yuan)		
Level 1-4	-2924.951	202.340*
	[.]	[2.36]
Level 5-6	1896.625	4083.471
	[0.63]	[1.64]
Level 7-10	-48895.883	-6088.662
	[-0.87]	[-0.54]
Observations	51	37

Notes: The table shows t-test results between the early quitters (those quitting one month before a vesting day) and the delayed (those quitting one month following a vesting day). The rows with variable names report the mean of early quitters minus the mean of the delayed for Plan 1 and Plan 2 owners, respectively. t statistics are in brackets. The t statistic of Plan 1's option values is unavailable for Level 1-4 because there is only one early quitter. * $p<0.1$, ** $p<0.05$.

Appendix A Proof of Equation (9)

To Prove Equation (9), we first make the following assumptions.

Assumption 1. *Quitting time absent of options is uniformly distributed, hence*

$$\Pr(Z_0^0 = 0, Z_1^0 = 1) = \Pr(Z_0^0 = 1, Z_1^0 = 1)$$

i.e. workers are equally likely to quit before and after the vesting point.

The immediate implication of this assumption is that among quitters, the fractions quitting when $D = 0$ and when $D = 1$ are the same:

$$\Pr(Z_0^0 = 0, Z_1^0 = 1, Z_0^1 = 0, Z_1^1 = 1) = \Pr(Z_0^0 = 1, Z_1^0 = 1, Z_0^1 = 1, Z_1^1 = 1) = \pi_q/2$$

Assumption 2. *All else equal, a worker always weakly prefers the same job with options than without, i.e.*

$$Z_D^0 \geq Z_D^1, \ \forall D = 0, 1$$

Given these two innocuous assumptions, we decompose the fraction of always-takers as follows:

$$\begin{aligned}
\pi_A &= \Pr(Z_0^1 = 1) \\
&= \Pr(Z_0^0 = 1, Z_0^1 = 1) + \Pr(Z_0^0 = 0, Z_0^1 = 1) \\
&= \Pr(Z_0^0 = 1, Z_0^1 = 1) \\
&= \Pr(Z_0^0 = 1, Z_1^0 = 1, Z_0^1 = 1, Z_1^1 = 1) \\
&= \pi_q/2
\end{aligned}$$

The first equality is by our definition; the second is by basic properties of probability; the third is by Assumption 2; the fourth is again by definition; and the last is by Assumption 1.

Similarly, the fraction of never-takers and compliers can be written as:

$$\begin{aligned}
\pi_N &= \Pr(Z_0^1 = 0, Z_1^1 = 0) \\
&= \Pr(Z_0^1 = 0, Z_1^1 = 0, Z_D^0 = 1, \exists D) + \Pr(Z_0^1 = 0, Z_1^1 = 0, Z_D^0 = 0, \forall D) \\
&= \pi_r + \pi_l \\
\pi_C &= \Pr(Z_0^1 = 0, Z_1^1 = 1) \\
&= \Pr(Z_0^1 = 0, Z_1^1 = 1, Z_0^0 = 0, Z_1^0 = 1) + \Pr(Z_0^1 = 0, Z_1^1 = 1, Z_0^0 = 1, Z_1^0 = 1) \\
&= \pi_q/2 + \pi_d
\end{aligned}$$

Rearrange to get Equation (9),

$$\pi_d = \pi_C - \pi_A, \quad \pi_q = 2\pi_A, \quad \pi_l + \pi_r = \pi_N$$

Appendix B Supplemental Tables

Table A1: RD regression on exit rates, Plan 1 (coefficients on covariates)

	(1) c=12	(2) c=24	(3) c=36	(4) c=48	(5) c=60
Value of other options	0.00000	0.00000	0.00001***	0.00014***	-0.00002
	(0.00004)	(0.00004)	(-1.86000)	(0.00003)	(0.00002)
Value of restricted stocks	0.00028	0.00028	0.00039	-0.0002201	0.00066
	(0.00086)	(0.00086)	(0.00082)	(0.00070)	(0.00067)
Months before other options vest	-0.00015	-0.00015	-0.00002	-0.00011	0.00047
	(0.00026)	(0.00026)	(0.00031)	(0.00035)	(0.00037)
Months before restricted stocks vest	-0.00015	-0.00015	-0.00022	0.00035	-0.00025
	(0.00033)	(0.00033)	(0.00026)	(0.00039)	(0.00026)
1{t is in first half of the quarter}	0.00013	0.00013	-0.00224	-0.00420**	-0.00090
	(0.00179)	(0.00179)	(0.00186)	(0.00166)	(0.00235)
1{promoted in month (t-1)}	-0.00119	-0.00119	-0.00090	0.00139	-0.00226
	(0.00179)	(0.00179)	(0.00175)	(0.00185)	(0.00322)
Age	-0.06948	-0.06948	0.03679	0.00547	0.03989
	(0.05105)	(0.05105)	(0.03176)	(0.02161)	(0.04133)
Years of experience in Ctrip	0.07077	0.07077	-0.03958	-0.00621	-0.04084
	(0.04962)	(0.04962)	(0.03064)	(0.02169)	(0.04114)
1{in product department}	0.00514**	0.00514**	-0.00151	0.00049	-0.01137
	(0.00214)	(0.00214)	(0.00582)	(0.00426)	(0.00994)
1{in service department}	0.00050	0.00050	0.00164	0.01443**	0.67261***
	(0.00362)	(0.00362)	(0.00586)	(0.00561)	(0.02179)
1{in IT department}	-0.00207	-0.00207	-0.00999**	-0.00778	-0.00550
	(0.00191)	(0.00191)	(0.00404)	(0.00484)	(0.00492)
1{Level 3-4}	-0.00468*	-0.00468*	-0.00546	0.02699	-0.00519
	(0.00248)	(0.00248)	(0.00561)	(0.03080)	(0.00378)
1{level 5-6}	-0.00615	-0.00615	-0.01473**	0.02348	-0.00265
	(0.00423)	(0.00423)	(0.00712)	(0.03063)	(0.01054)
1{level 7-10}	-0.00792	-0.00792	-0.01655	0.01486	-0.00683
	(0.00608)	(0.00608)	(0.01063)	(0.03125)	(0.01155)

Notes: Value of other options is (fair value per share)×(shares granted)×(exchange rate), and annual base wage is (monthly base wage*12). Value of restricted stocks are defined as(closing stock price on the grant date)×(shares granted)×(exchange rate). Calendar year dummies are included but not reported. Other details are the same as those in Table 4.

Table A2: RD regression on exit rates, Plan 2 (coefficients on covariates)

	(1) c=12	(2) c=24	(3) c=36	(4) c=48	(5) c=60
Value of other options	-0.00001	-0.00006	-0.00010	-0.00004*	-0.00007
	(0.00001)	(0.00004)	(0.00007)	(0.00002)	(0.00017)
Value of restricted stocks	0.00001	-0.00051	0.00366	-0.00020	-0.00001
	(0.00055)	(0.00037)	(0.00230)	(0.00091)	(0.00181)
Months before other options vest	-0.00018	-0.00034*	0.00013	-0.00068	-0.00003
	(0.00017)	(0.00019)	(0.00072)	(0.00085)	(0.00092)
Months before restricted stocks vest	-0.00003	-0.00002	0.00078*	0.00114	0.00071
	(0.00026)	(0.00014)	(0.00043)	(0.00106)	(0.00105)
1{t is in first half of the quarter}	-0.00121	-0.00255	-0.00473*	-0.00898*	-0.00462
	(0.00193)	(0.00241)	(0.00251)	(0.00467)	(0.00572)
1{promoted in month (t-1)}	0.00044	0.00154	-0.00227	0.00534	0.00647
	(0.00246)	(0.00154)	(0.00668)	(0.00394)	(0.00830)
Age	-0.00314	0.00936	0.03329	0.03588	-0.03001
	(0.01956)	(0.03079)	(0.05386)	(0.02968)	(0.05858)
Years of experience in Ctrip	0.00825	-0.00746	-0.02268	-0.03000	0.03185
	(0.02080)	(0.02866)	(0.05153)	(0.02763)	(0.05779)
1{in product department}	0.00262	0.00245	0.01979**	0.04601**	0.02310
	(0.00283)	(0.00161)	(0.00970)	(0.02326)	(0.01968)
1{in service department}	-0.00399	0.00799*	0.02961**	-0.00109	-0.16964
	(0.19129)	(0.00446)	(0.01144)	(0.00826)	(0.23072)
1{in IT department}	1.06058***	-0.00006	-0.01452**	0.00756	0.00367
	(0.07704)	(0.00117)	(0.00710)	(0.02085)	(0.02239)
1{Level 3-4}	-0.00472	-0.00555**	-0.01300**	-0.01715***	-0.01620**
	(0.00430)	(0.00281)	(0.00603)	(0.00528)	(0.00712)
1{level 5-6}	-0.00821	-0.00733	-0.07135*	-0.10934**	-0.09614*
	(0.00595)	(0.00466)	(0.04284)	(0.04872)	(0.05116)
1{level 7-10}	-0.01267*	-0.01358**	-0.07670*	-0.12155**	-0.03695
	(0.00740)	(0.00671)	(0.04495)	(0.04856)	(0.09198)

Notes: Value of other options is (fair value per share)×(shares granted)×(exchange rate), and annual base wage is (monthly base wage*12). Value of restricted stocks are defined as(closing stock price on the grant date)×(shares granted)×(exchange rate). Calendar year dummies are included but not reported. Other details are the same as those in Table 5.

Table A3: RD regression on exit rates, polynomial fit

	(1) c=12	(2) c=24	(3) c=36	(4) c=48	(5) c=60
A: Plan 1					
h=2	0.00642***	0.00392	0.00500***	0.00010	0.00278
	(0.00227)	(0.00325)	(0.00167)	(0.00217)	(0.00271)
N	5152	5195	5178	5020	4764
h=3	0.00759**	0.00308	0.00726**	-0.00049	0.00201
	(0.00237)	(0.00286)	(0.00227)	(0.00254)	(0.00283)
N	7728	7785	7770	7531	7138
h=4	0.00772***	0.00224	0.00797***	-0.00090	0.00136
	(0.00247)	(0.00296)	(0.00255)	(0.00296)	(0.00319)
N	10301	10373	10358	10040	9508
h=5	0.00681**	0.00128	0.00879**	-0.00498	0.00151
	(0.00285)	(0.00373)	(0.00345)	(0.00482)	(0.00497)
N	12777	12959	12942	12529	11879
h=6	-0.00684	0.00700	0.01112	-0.05875*	0.01823
	(0.01328)	(0.01349)	(0.01793)	(0.03332)	(0.02659)
N	15252	15549	15503	15018	14244
B: Plan 1					
h=2	0.00300	-0.00087	0.00777*	0.00841	0.00211
	(0.00328)	(0.00186)	(0.00420)	(0.00631)	(0.00815)
N	4102	3830	3607	2998	2333
h=3	0.00210	-0.00236	0.00763*	0.00680	-0.00286
	(0.00330)	(0.00208)	(0.00438)	(0.00567)	(0.00721)
N	6154	5748	5401	4484	3499
h=4	0.00226	-0.00353	0.00703	0.00843	-0.00230
	(0.00345)	(0.00251)	(0.00462)	(0.00544)	(0.00700)
N	8201	7668	7187	5978	4661
h=5	0.00513	-0.00642	0.00542	0.01230*	0.00333
	(0.00533)	(0.00438)	(0.00576)	(0.00705)	(0.00862)
N	10245	9587	8972	7462	5886
h=6	0.02547	-0.02873	0.02098	0.02915	0.04028
	(0.03289)	(0.02393)	(0.01907)	(0.04052)	(0.04334)
N	12292	11503	10754	8946	7112

Notes: RD estimation using local linear regression on both sides of the threshold, c, with rectangular kernels and varying bandwidths. The dependent variable is an indicator for whether employee i exits the company in month t. RD coefficients are reported in rows labeled "h=...", and their standard errors are reported in the rows below. N is the number of observations. Mean at (c-1) reports the mean exit rates in months immediately preceding the threshold, i.e. month (c-1). The same set of covariates as in Panel B of Table 4 and 5 are included but not reported. Standard errors clustered at the individual level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A4: Elasticity of turnover to wage, probit estimates

	1{quitting in one year}
log(wage)	-1.546***
	(0.041)
Age	-0.176***
	(0.016)
Age^2	0.002***
	(0.000)
Years of experience in Ctrip	-0.225***
	(0.007)
Years of schooling	-0.012
	(0.008)
Male	0.202***
	(0.027)
Job category FE	Yes
level FE	Yes
N	32826
Pseudo R^2	0.529

Notes: Each observation is an individual-year. We restrict the sample to employees who were nominated to receive option awards but did not win in the end. The dependent variable is an indicators for quitting from Ctrip within one year. Wage is the annual wage imputed from average monthly wages. It differs from the actual annual wage only for those not in Ctrip for the entire year (because of either entry or exit). A full set of job categories and group of job level dummies are also included. Heteroskedasticity robust standard errors are in parentheses. ** $p<0.05$, *** $p<0.01$.