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Compliance with Commuter Tax Allowances and the Role of Evasion Spillovers

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Working Paper No. 1411

September 2014

Supported by the Austrian Science Funds

FWF

The Austrian Center for Labor Economics and the Analysis of the Welfare State

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Abstract

We provide first field evidence on evasion spillovers as an important determinant of the individual compliance decision. Exploiting discontinuities in a self-reported commuter tax allowance, we observe a substantial share of taxpayers misreporting their claims. Using exogenous variation in job changes we find that individual evasion decisions are influenced by the compliance behavior of other co-workers, with job changers from low- to high-cheating companies starting to evade much more after they move. In contrast, movers from high- to low-cheating companies do not alter their reporting. The most likely explanation is information transmission, including increased knowledge about the possibilities for non-compliance.

Keywords: Tax Evasion, Self-Reporting, Spillover Effects, Information Frictions
JEL codes: H24, H26, D83

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Parts of the paper were written during a research visit of Jörg Paetzold at the UC Berkeley Center of Equitable Growth. We are grateful for the center's hospitality, and are especially to Emmanuel Saez for research guidance and invaluable advice. We would like to thank James Alm, David Card, Martin Halla, Peter Huber, Nirupama Rao, Andreas Peichl, Daniel Reck, Johannes Rincke, Rupert Sausgruber, Joel Slemrod, Christian Traxler, and numerous seminar participants at Berkeley, CEBID 2014, CESifo Munich, Mannheim, NOeG 2014, Oxford University Centre of Business Taxation Annual Symposium 2014, SEEK 2014, and Vienna for helpful comments and discussions. Special thanks to Attila Lindner, who provided inestimable support at different stages of the project. Finally, we are indebted to Karel Mauric (Post AG) for providing data on Austrian zip-codes, and Matthias Stöckl for data assistance. Financial support from the "The Austrian Center for Labor Economics and the Analysis of the Welfare State" (project number S106-G16) and the University of Salzburg is gratefully acknowledged.

1 Introduction

It is well documented that, given low audit rates and modest penalties, tax evasion in advanced economies should be much higher than empirically observed (see Andreoni, Erard and Feinstein 1998, Slemrod and Yitzhaki 2002 and Slemrod 2007, for comprehensive surveys). Recent research concludes that not only psychological, moral or cultural aspects are responsible for preventing people from cheating but also the lack of opportunity to do so. Notably Kleven et al. (2011), relying on a large scale audit experiment in Denmark, show that third-party reporting effectively inhibits people's possibility to cheat on their taxes while self-reported income is prone to be evaded (see Slemrod, Blumenthal and Christian 2001, for related evidence).

While it is striking how self-reported items are subject to tax evasion, it is interesting to note that there are still many taxpayers not availing this easy opportunity for non-compliance (e.g., Kleven et al. 2011 report non-compliance rates of purely self-reported income of around 40 percent). This paper adds one explanation to this observation showing that the individual evasion decision is crucially influenced by the compliance behavior of other taxpayers in one's vicinity. In particular, our results suggest the existence of evasion spillovers among taxpayers, with individuals becoming more likely to start cheating when being exposed to a more non-compliant environment. While there exist empirical documentations of indirect deterrent effects of increased enforcement on the compliance of non-audited taxpayers (see Rincke and Traxler 2011, Pomeranz 2013), evidence of complementary evasion spillovers among taxpayers regarding easy opportunities to cheat is extremely scarce. Only Fortin, Lacroix and Villeval (2007) and Alm, Jackson and McKee (2009) touch on this issue, presenting lab evidence that taxpayers' reporting is sensitive to the evasion decision taken by other taxpayers. Our paper is the first to provide field evidence that evasion spillovers are indeed an important determinant in the individual compliance decision. Further, our results suggest that the evasion spillover we observe has more to do with an information environment in which taxpayers learn about the easy opportunity to cheat than it does with peer or social norm effects, which might also explain such spillover patterns.

To begin with, we present compelling evidence to the finding that self-reported tax items are prone to be evaded, examining the role deductions play for wage earners to underreport their taxable income. We demonstrate how employees, faced with a tax system heavily based on third-party reporting and employers' withholding, use minor features of self-reporting in the tax code to lower their taxable income. In particular, we focus on the degree of tax evasion via a commuter tax allowance in Austria, compensating employees for their travel-to-work expenses and representing the biggest standard deduction available for Austrian wage earners.[1] This commuter allowance is designed as a step function of the distance between residence and the workplace, creating sharp discontinuities at each bracket threshold. According to the Austrian tax code, employees report their eligibility for a certain distance bracket to the employer who, as the third-party, has to validate

[1]Similar compensations are also applied in other countries. They are either included in general work-related deductions (e.g., France or Italy) or designed as a single allowance for commuters (e.g., Germany, Netherlands, Denmark or Austria). The US does not offer a specific deduction of commuter expenses via its income tax code, but employers are allowed to grant transit tax free benefits to their employees up to a certain amount.

these claims and adjusts taxable income before withholding. In practice, however, employers do not sufficiently double-check these claims, turning the allowance into a (quasi-)self-reported item. Since tax authorities do not systematically check whether the self-reported information is accurate, the scheme offers employees an easy opportunity to overreport their travel distance to work and hence, to receive a tax allowance higher than they are actually entitled to.

To reveal tax evasion regarding commuter allowances, we employ a dataset consisting of merged tax and matched employer-employee panel information from Austria, including earnings information over the whole population of Austrian (private-sector) employees between 1995 and 2005 (about 3 million taxpayers, of which around 725,000 are commuters). Our database includes information on the allowance amount each taxpayer received as a commuter. It also contains the employer's location and the commuter residence, which allows us to calculate the actual driving distance between both locations and the corresponding hypothetical commuter allowance, enabling us to compare this figure with the one the taxpayer has received. By doing this, we are in the unique position to track the compliance behavior of each Austrian commuter over a time period of up to ten years.

Our results show that tax evasion via self-reported commuter allowances is substantial. We find that around 30 percent of all allowance claims are overstated, and, consistent with deliberate tax evasion, we observe sharp reactions of taxpayers who reside close to the thresholds where the allowance discontinuously jumps to a higher amount. This high rate of evasion for a widely used form of tax deduction – more than 30 percent of all employees with income tax liabilities request this deduction – makes the commuter allowance an ideal laboratory to understand the determinants of tax evasion. First, we examine the impact of socio-demographic variables on individual compliance. In line with previous studies (e.g., Kleven et al. 2011), their impact on the evasion decision is rather limited. In contrast, variables that display the proximity of the taxpayer to a certain bracket threshold and, therefore, capture the opportunity and incentive to overreport, have strong effects on the compliance decision. Further, we take advantage of our data situation which allows us to observe not only the compliance decision of a single taxpayer but also the compliance behavior of his colleagues at the workplace. Since the allowance claim is filed via the payslip at the firm level, the critical environment to study potential evasion spillovers is the workplace. Indeed, we find that the individual evasion behavior strongly correlates with the evasion behavior of other co-workers within the same firm.

To uncover the causal effect of the work environment on individual cheating, our empirical strategy rests on a sub-sample of job changers moving between employers that differ in the share of workers overstating their commuter allowance. Hence, our identification strategy exploits variation in job changes to reveal spillover effects from the new work environment on the individual compliance decision.[2] These spillover effects might be explained by social or moral norms within a workplace, by peer effects or by information about the easy opportunity to cheat. To distinguish between

[2]Since taxpayers can actually circumvent the employer by filing eligibility for a commuter allowance through the tax return at the end of the year, a sorting of taxpayers to certain companies is very unlikely. Hence, we treat the decision to start a new job as exogenous in regard to the compliance decision. In Section 4.1 we address this identifying assumption in detail.

these channels, we derive testable predictions about the behavior of job changers when moving to companies that differ in their shares of cheating co-workers (in the spirit of Chetty, Friedman and Saez 2013). More precisely, if tax evasion is due to information we would expect that individuals who change to a firm with a higher share of cheating co-workers should learn from their colleagues that non-compliance regarding the commuter allowance is easily done without detection and thus, should start overreporting much more. On the contrary, individuals moving to a firm with a lower cheating share should not change their cheating behavior and continue overreporting, since they know that misreporting is almost without consequences.

Turning toward the results obtained from our sample of job changers, we first find a significant impact of a taxpayer's work environment on the individual compliance decision. Second, we observe *asymmetric* effects of increases versus decreases in co-worker cheating shares when individuals move between companies. Specifically, job changers who move from a low-cheating to a high-cheating firm start overreporting much more after they move. In contrast, those who change from a high-cheating to a low-cheating firm experience no change in cheating behavior. We interpret this result as consistent with a class of explanations based on information, learning and memory, indicating that differences in knowledge about the possibilities for non-compliance are important when taxpayers make their reporting decisions. In fact, substantial peer or firm-specific effects on the reporting behavior would not translate into such an asymmetric impact of an individual's previous co-worker cheating share on current behavior. Instead, one would rather expect changes in overreporting to move alongside with changes in co-worker cheating shares. Having said this, we cannot fully rule out the existence of asymmetric persistence of norms, which suggests that taxpayers might tend to update their norms in one direction only. However, we present suggestive evidence against this notion of asymmetric persistence of norms, and conclude that information about low-hanging evasion fruits seems to be important to explain the pattern of tax evasion we observe.

Since our population tax data entails location information on the zip-code level, we complement our findings with case study evidence using exact residence and workplace addresses (including house numbers) of almost 40,000 employees of a large Austrian retailer. Using this data, we find virtually the same evasion pattern as for the whole population of Austrian taxpayers. Further, an analysis of the reporting behavior of new recruits to the retailer chain confirms our evidence of positive spillovers from the work environment on the individual compliance decision. In sum, our findings based on the retailer data support the argument that taxpayers take advantage of the self-reporting nature of the commuter allowance, and that the individual reporting decision is influenced by the compliance behavior of other co-workers.

Our paper relates to a larger body of empirical research studying the effect of information on economic decision making (see, e.g., Duflo et al. 2006, Kopczuk 2007, Chetty and Saez 2013). Specifically, our study corroborates survey evidence suggesting that taxpayers have imperfect knowledge of deterrence parameters, with less informed individuals tending to overestimate the actual risk of being audited (Scholz and Pinney 1995, Chetty 2009). Further, our study carries important implications for the design of optimal enforcement policies. The results suggest that information regarding deterrence parameters diffuses slowly over time, and governments as well as researchers should be aware of this when evaluating the impact of a new tax policy or reform. For instance, when choosing their optimal audit strategies, tax authorities may want to take potential spillover effects among taxpayers into account. Finally, we think that our research design looking at asymmetric effects on the behavior of subgroups of taxpayers (in our case job changers) can contribute to a wider body of the compliance literature. It points to a fruitful way to infer individual decision making in regard to tax compliance, even when official audit data is not available.

The paper proceeds as follows. In the next section, we present details on the Austrian commuter tax allowance and the dataset used to detect tax evasion. Section 3 illustrates graphically how driving distances to work are systematically misreported when taxpayers claim their commuter allowance. We also show tentative regression results to examine whether the individual compliance decision is driven by personal characteristics as well as the cheating behavior at the workplace. In Section 4, we test for the causal effect of the work environment on the evasion decision using a sub-sample of individuals moving between employers. Section 5 presents additional case study evidence stemming from the retailer data. Finally, Section 6 concludes. Additional details and results are provided in the Appendix.

2 Institutional Background and Data

2.1 Commuter Allowance in the Austrian Income Tax System

In Austria, wage earners are not required to file tax returns since employers are legally obliged to do exact and cumulative withholding via the employees' payslip. On the payslip, a taxpayer can claim standard deductions and allowances, reducing tax liability and hence, withholding (Figure A1 displays such a payslip). The commuter tax allowance is the biggest of these allowances, enabling employees to reduce taxable income by as much as EUR 3,672 per year (for 2012).[3] It is designed to encourage workers to take up jobs even when the work place is distant from their homes, and to compensate them for their traffic expenses. The allowance comes as a step function of commuting distance and offers higher rates if public transport is not available or unreasonably long. More precisely, the deductible amount increases with brackets of 2–20 km, 20–40 km, 40–60 km and more than 60 km of commuting (see Table 1). For each of these brackets (except for the first bracket of 2–20 km), there exists a minor scheme when public transport is available, and a major scheme if not. According to the tax code, employees have to report the shortest commuting distance by means of public transportation (for the minor scheme) or by using a private vehicle (for the major scheme). It should be noted that the distance brackets were unchanged since their introduction in 1988, creating constant and exogenous discontinuities taxpayers can respond to. Finally, the deductible amounts get inflation adjusted over time by legislative act.

[3]Given a progressive income tax schedule with a top tax rate of 50 percent (for incomes above EUR 60,000), the maximum amount of tax reduction is equal to EUR 1,836.

Table 1: Commuter allowances in the Austrian tax code (EUR per year)

	Public transport	
Allowance bracket	Available (minor scheme)	Not available (major scheme)
2–20 km	–	372
20–40 km	696	1,476
40–60 km	1,356	2,568
More than 60 km	2,016	3,672

To receive the commuter allowance via the payslip, employees report their distance to work and the availability of public transport to their employer who, according to the tax code, should validate their claims before applying certain allowances to the tax withholding. In practice, however, it turns out to be a (quasi-)self-reported feature, with employers generally not meeting their responsibility to double-check the allowances claimed.[4] This offers employees the opportunity to overstate their commuting distance and hence, receive higher tax allowances than they are entitled to. Furthermore, the probability of detection is rather low. There are no official audit rates available for Austria, but given that employees are not required to file tax returns at the end of the year, false reporting on the payslip can only be detected when the employer is audited (in case of detection, the fine is typically levied on the employee). However, tax authorities usually do not focus on employees' deductions when conducting firm inspections. Moreover, they do not rely on computer-assisted software to calculate a taxpayer's driving distance to the workplace, lowering the risk of detection even further. In sum, this lenient enforcement offers commuters an opportunity to cheat easily on their allowances.

2.2 Dataset on Commuter Allowances

The data used to analyze tax evasion via the commuter allowance is the result of a unique merge of two comprehensive administrative datasets. The first one stems from the Austrian Ministry of Finance and covers earnings information of the whole population of Austrian taxpayers, broken down by tax code line items (as recorded on the payslip). The second source of data is the Austrian Social Security Database (ASSD), a linked worker-firm dataset that comprises the universe of private sector employment in Austria (Zweimüller et al. 2009). Using the ASSD, we are able to link taxpayers to their workplace and extract additional characteristics as well as information regarding the location of the employer. This results in a dataset that allows us to observe the reporting decision of every single taxpayer as well as of all of his colleagues at the workplace, embedded in a panel structure over time. In order to identify overreporting by commuting taxpayers, we calculate driving distances between the zip-codes of an employee and the corresponding firm by employing

[4]To understand why employers do not sufficiently double-check the claims of their employees we want to highlight that during most of the years of our study, online route planners such as Google Maps were not available for Austria. This made it a very costly and time-consuming affair for employers to validate the actual driving distance of their employees.

a geographic information system (GIS) as commonly used in various navigation devices. It returns the shortest driving route between the centroids of each pair of Austrian zip-codes, saved in a distance matrix.[5] Austria comprises 2,208 zip-code areas, with a median surface area of 27 km^2 and a median circumradius of around 3 km (for comparison, the average surface area for a U.S. zip-code is around 300 km^2).

We start with a dataset of almost 14.4 million observations, including about 3 million taxpayers who filed a payslip at least once between 1995 and 2005. As shown in Table 2, around 725,000 of them are recipients of the commuter tax allowance, leading to a commuter sample of about 2.7 million observations. Around 10 percent of commuting taxpayers receive the maximum allowance for a driving distance above 60 km, about 40 (15) percent request the allowance for the second (third) bracket, i.e., within a distance of 20–40 km (40–60 km). Around 35 percent obtain the smallest allowance, available for a distance between 2 and 20 km.

Table 2 indicates that the major allowance scheme is claimed by about 70 percent of all commuters. Regarding the recipients of the minor scheme, i.e., individuals who make use of public transportation, we have to emphasize that the actual driving routes usually do not correspond to the shortest driving distances computed from GIS (public means of transportation, for example, often make detours when going from location A to B). Since we are not able to measure precisely the commuting distance between the residence and work place in these cases, we exclude them from the subsequent analysis and only focus on recipients of the major scheme. This leaves us with a sample of around 2 million observations (or about 500,000 taxpayers).

Table 2: Sample composition

	Observations	**Taxpayers**	%
Total	2,714,354	723,509	
By allowance bracket			
2–20 km	1,073,045	253,260	35.0
20–40 km	1,050,936	288,574	39.9
40–60 km	348,896	107,176	14.8
More than 60 km	241,477	74,499	10.3
By allowance scheme			
Major	1,917,690	506,622	70.0
Minor	796,664	216,887	30.0

Notes: Overall, the dataset includes 14,357,039 observations from 2,952,984 individuals over the years 1995 to 2005.

One important concern is that there is no clear provision in the ASSD whether the employer iden-

[5] We complement our results for the population tax data with case study evidence using exact residence and workplace addresses of almost 5,000 commuters of a large Austrian retailer. Using this data, we virtually find the same evasion patterns as for the whole population of Austrian commuters presented below. See Section 5 for an analysis of the retailer data.

tifier is used for a firm or for smaller single establishments of a big firm. If a certain enterprise with multi-establishments consists of only one entry in the ASSD, our GIS-matrix that, in essence, calculates the road distance between a firm's location and the residence of the employee might then be correct only for some workers. Fink et al. (2010) examined this concern in more detail, concluding that multi-establishment firms are not an important component in the Austrian firm demographic. They compared the number of employer identifiers and their distribution in different size classes as retrieved from the ASSD with official numbers from the Austrian statistical office, showing substantial similarity of the descriptive statistics from both datasets. With the number of employer identifiers being slightly higher in the ASSD than the number of firms in the official statistic, it suggests that the ASSD tracks single employers more accurately (and hence, more locally by providing the zip-code of every identifier). More importantly, they show that the structure of Austrian firms is characterized by small and medium sized businesses. For instance, 67% of all firms in 2005 consisted of only 1 to 4 employees (and 87% of all firms with less than 10 employees), reassuring us that multi-establishment firms only play a negligible role in the Austrian market (Fink et al. 2010: 4). In an effort to limit any other source of measurement error, we remove companies listed only with P.O. boxes from our sample, since these firms are sometimes difficult to match to a certain zip-code area and potentially consist of multiple establishments. Taking all these precautions into account, we confidently conclude that our dataset traces the actual workplace of the taxpayers very precisely.

3 The Anatomy of Tax Evasion

3.1 Reporting Behavior of Commuters

Let us start with an analysis of all recipients of the major allowance scheme for the whole time period, pooling data between 1995 and 2005. Figure 1 reports the histograms for actual distance-to-work (by 1 km bins) of commuters, broken down by the respective allowance bracket they claimed. Each graph also depicts the upper and lower bound of the respective allowance bracket in dashed lines along the x-axis. The area between the two dashed lines represents the distance that commuters should actually travel to their work place when they filed their claim correctly.

Two features are worth noting in Figure 1. First, panels B to D show that there are many commuters clustering well to the left of each lower bound.[6] These commuters reside closer to their work place than they claim on their payslips. Thus, they take advantage of the (quasi-)self-reporting feature and deduct higher allowances than they are actually entitled to. Overall, the cheating shares for panel B to D add up to 26 percent, 44 percent and 35 percent, respectively. Second, for the panels A to C, the number of recipients declines dramatically as we move closer to

[6]Note that the first commuter bracket of 2–20 km does not display clustering to the left of the lower bound since the qualifying commuting distance of 2 km may lie within one zip-code area, confronting us with the problem of fuzziness when measuring the driving distance between firm location and the residence of the commuter. Given the fact that we cannot be sure if in such a case a commuter cheats or reports honestly, we do not draw any conclusions from panel A in this context. In the following, we leave out these taxpayers from the empirical analysis, obtaining an overall sample size of around 1.7 million observations (or about 440,000 commuters).

the upper bound of each bracket, with only a small fraction of all recipients actually commuting longer to work than they claim (see Appendix A.2 providing additional information on those underreporters). This indicates the salience of the single brackets, with most commuters switching to the more favorable higher bracket when their driving distance hits the upper bound. Specifically, the asymmetric distribution of commuters around the lower versus the upper bound of each bracket reassures us that we are able to measure the travel-to-work distance quite precisely. If measurement of driving distances would be poor, we would expect that the measured distance of commuting would disperse boundlessly on both sides of the respective bracket. After all, the sheer graphical inspection of Figure 1 clearly suggests that employees systematically overreport their commuting distance and thus, the tax allowance they claim.

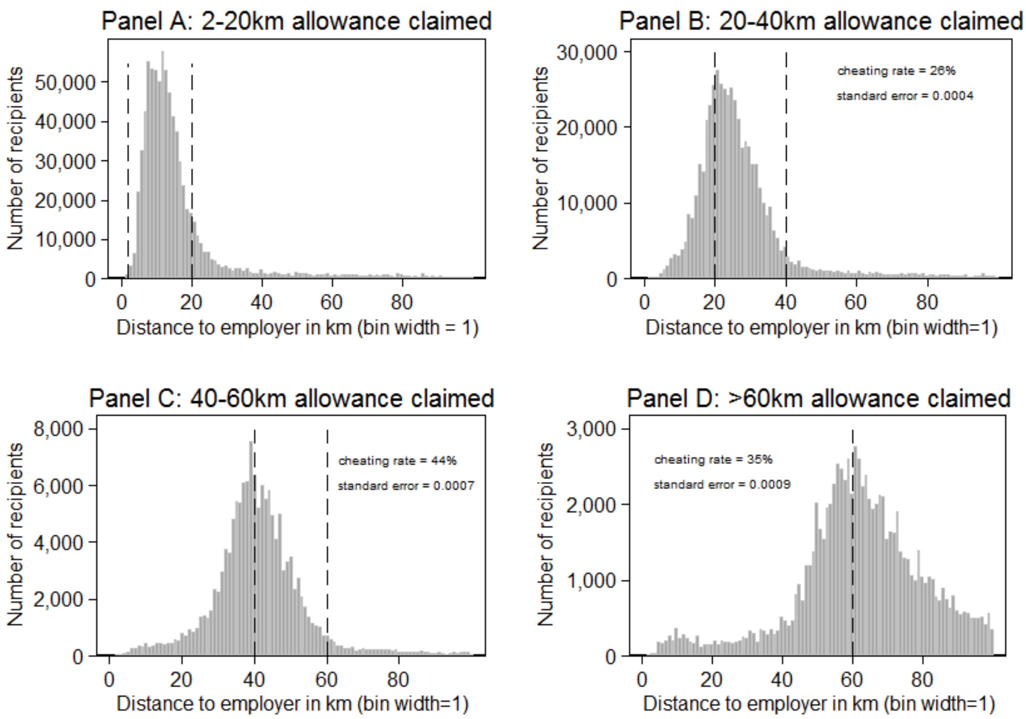


Figure 1: Allowance claimed and actual distance to employer (by bracket)

Notes: The figure displays the histogram of actual distance to employer (by 1 km bins) for commuters by allowance brackets of 2–20 km (panel A), 20–40 km (panel B), 40–60 km (panel C), and more than 60 km (panel D). The histograms include allowance recipients for all years 1995–2005 (around 1.7 million observations or about 440,000 commuters). Distance to employer is the driving route as calculated by GIS between the zip-code of an employee's residency and the employer's location. Commuters residing between the two dashed lines (indicating the respective allowance bracket) filed their claims correctly, whereas recipients left of each lower bound overreported their travel distance. Note that individuals to the right of each upper bound are measured as underreporters mostly due to single-entry enterprises (see Appendix A.2 for further explanations).

Figure 2 presents additional evidence about the salience of the single brackets. This time we pool data across all brackets and display the fraction of cheaters by bins of distance to their employers (bin width=1.25 km). Again, the dashed lines indicate the bounds where the allowance discontinuously increases to a higher amount. We observe a sharp reaction of taxpayers to these thresholds. The closer commuters live to a respective bracket the more prone they are to overreport their distance (and hence the allowance claimed). In the regression analysis below, we provide a statistical test for this observation. Overall, up to 60 percent of the individuals closest to the brackets misreport their actual driving distance to the working place. In contrast, commuters who reside just to the right side of a respective threshold report their commuting distance very accurately (here, only 5 percent of taxpayers misreport their driving distance). These reporting responses indicate that employees are aware of the allowance scheme' structure and shows the importance of proximity to the next higher bracket for the evasion decision. This pattern is also in accordance with recent evidence suggesting strong behavioral responses ('bunching') at salient discontinuities of the tax code (see, e.g., Saez 2010, Chetty et al. 2011, Kleven and Waseem 2013, and Kreiner, Leth-Petersen and Skov 2014). Further, the solid line in Figure 2 displays the total number of cheaters in each bin (right scale of the figure). Similar to the fraction of cheaters, we observe that the number of cheaters peaks at the threshold of each bracket before dropping afterwards. Notice that the number of cheaters is lower in higher distance bins, since the total number of commuters per bin declines as distance to the workplace increases.

While it is striking how overreporting increases when the distance to the next bracket bound declines, it is also interesting to notice that a substantial number of employees still report their commuter allowance honestly, even when they reside very close to the next higher bracket. Below, we use a sample of job movers to address this variation in overreporting, looking at the importance of evasion spillovers among co-workers in this regard. Overall, Figure 2 clearly shows that tax evasion via self-reported commuter allowances is substantial not only in regard to the relative but also in the absolute numbers of taxpayers.

In the remaining sections, we take a more systematic look on the reporting behavior of Austrian commuters. In Section 3.2, we rely on some tentative regression results to show how the individual compliance decision is influenced by personal and firm-specific characteristics, as well as by a worker's environment regarding the cheating behavior of colleagues at the workplace. The latter turns out to be decisive in explaining non-compliant behavior. Section 4 explores this issue further by focusing on a sub-sample of job movers and applying an event study approach.

3.2 Explaining Misreporting in Commuter Allowances: Tentative Regression Analysis

To investigate whether a taxpayer's compliance decision can be explained by individual and other characteristics, we estimate a simple binary choice model, where the conditional probability to overreport is explained by $p_{it} \equiv P(c_{it} = 1|\mathbf{x}_{it}) = F(\mathbf{x}'_{it}\beta)$. c_{it} is an indicator variable with entry one if taxpayer i is non-compliant at year t, and zero else. $F(\cdot)$ represents the cumulative distribution function, which is assumed to be standard normal (i.e., we estimate probit models).

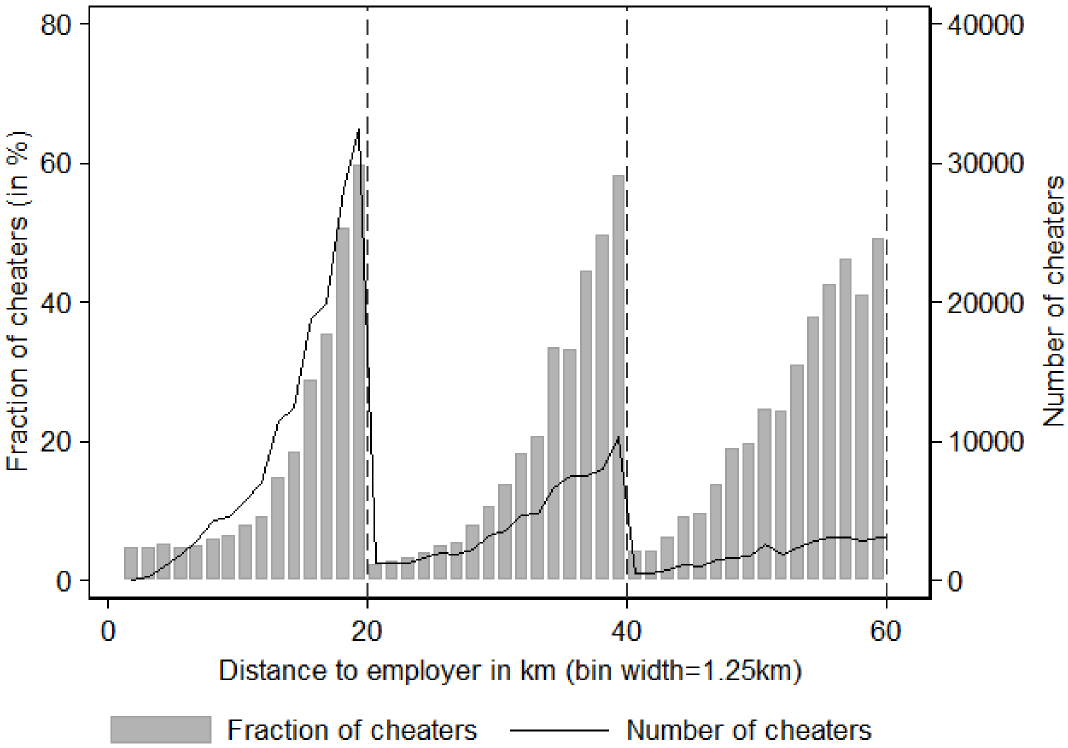


Figure 2: Distance to bracket and cheating behavior

Notes: The figure displays the reporting behavior of commuters by their distance to the workplace (bin width=1.25 km). The bars show the fraction of cheaters for each bin. The dashed lines represent the thresholds where the allowance discontinuously increases to a higher amount (at 20, 40, and 60 km, respectively). The solid line indicates the total number of cheaters per bin. The histogram includes allowance recipients for all years 1995-2005.

Depending on the covariates $\mathbf{x}_{it}$ included to estimate p_{it}, we distinguish between three alternative specifications: Model A is the most parsimonious, incorporating only personal characteristics such as age, gender, educational status (dummy variable for a tertiary degree), nationality (non-native worker or not) and employee status (white- or blue-collar worker). Further, we add the (log of) taxpayers' gross income. In Model B, we account for our descriptive Figures 1 and 2 and include four dummy variables for the proximity to the next higher allowance bracket: For (actual driving) distances below 2 km to the next bracket, for distances between 2 and 5 km, for distances between 5 and 10 km and for distances above 10 km (the last forms the reference group). We would expect decreasing parameter estimates for these variables as taxpayers with larger driving distances to the next allowance bracket may think that misreporting gets detected more easily, hence providing less incentives to evade. In Model C, we add firm-specific information on firm size to account for the above mentioned fact that the Austrian firm population is dominated by small- and medium-sized firms. In particular, we use a dummy variable taking entry one for firms with more than 10 employees, and zero else.[7] Further, we include a variable for working in the 'informal

[7]Incorporating dummy variables for more firmsize-classes does not change our findings regarding the impact of firmsize on tax evasion. The same holds when using a continuous firmsize measure (the log of employees) rather

sector' defined as agriculture, forestry, construction, and hospitality industries (this classification is meant to capture industries that are generally prone to informal activities). Finally, we account for the share of cheating co-workers at the workplace. In particular, we use the 'leave-out' cheater share, i.e., excluding worker i from both the nominator and the denominator when calculating the co-worker cheater share of a firm.[8] In all models, we include fixed effects for regions (the nine provinces of Austria) and years, and estimate pooled probit models.

Table 3 summarizes our estimation results. Throughout, we report average marginal effects that are computed as the average impact of partial or discrete changes over all observations (standard errors are adjusted accordingly). Generally, and in line with Kleven et al. (2011), we find that most individual-specific variables are either insignificant or almost negligible in magnitude throughout. For instance, the (significantly) negative impact of age indicates that, on average, the probability of being non-compliant decreases by only 2 percentage points if an individual grows 10 years older. Similar magnitude of effects can be observed for gender, employee status and nationality. Education and income enter significantly positive and seem more important than the other individual-specific characteristics in explaining cheater behavior.

In Model B, we observe a considerable improvement of fit (the R^2 increases from 2 percent to 23 percent), is due to the inclusion of our distance-to-bracket dummies. They exhibit substantial and significantly positive coefficients that increase with the proximity to the next allowance bracket, as expected. For instance, living close (within 2 km) to a bracket increases the probability of overreporting by about 56 percentage points as compared to living at a greater distance from an allowance threshold.

Finally, we include firm-specific variables in Model C. First, we can see a significantly positive impact of firm size, indicating that a taxpayer's probability of evading taxes is higher in larger than in smaller firms. However, this difference in the evasion probability amounts to only a small value of 0.3 percentage points. Second, working in the informal sector is positively and significantly associated with evasion, but it raises the probability to overreport only by a very small margin of 0.7 percentage points. Perhaps more importantly, Model C shows that a firm's share of co-workers being non-compliant exhibits a significantly positive sign and is also large in absolute terms. Accordingly, an increase of the cheater share by 10 percentage points translates into an increase of an individual's probability of being non-compliant by about 2.7 percentage points, which is a large impact compared to most of the other explanatory variables in Model C. This suggests that an individual's compliance decision is influenced by the corresponding cheating environment at the workplace.

than a binary one.

[8]Calculating these co-worker cheating shares produce, by construction, lower cheating shares for cheaters than for non-cheaters. Hence, the inclusion of firm fixed effects would induce a downward bias to the co-worker cheating share. However, using *firm cheater shares* instead of *co-worker cheating shares* (i.e., not applying the 'leave-out' procedure to calculate these shares) and employing firm fixed effects, we receive similar results as presented in Table 3. Yet, we prefer using the 'leave-out' co-worker cheating shares since they avoid the mechanical correlation between a cheating employee and the firm's cheating share (see Card, Heining and Kline 2012).

Table 3: Estimation results (average marginal effects)

Dependent variable: Indicator variable with entry one if a taxpayer overreports distance to work

	Model A	**Model B**	**Model C**
Age	-0.002^{***}	-0.002^{***}	-0.002^{***}
	(0.000)	(0.000)	(0.000)
Gender (1: Female)	-0.008^{***}	-0.013^{***}	-0.013^{***}
	(0.001)	(0.001)	(0.001)
Education (1: Tertiary education)	0.039^{***}	0.043^{***}	0.041^{***}
	(0.002)	(0.002)	(0.002)
Employee status (1: White-collar worker)	0.007^{***}	0.021^{***}	0.019^{***}
	(0.001)	(0.001)	(0.001)
Nationality (1: Non-native)	-0.001	0.001^{*}	0.001
	(0.001)	(0.001)	(0.001)
Gross income (log)	0.042^{***}	0.033^{***}	0.025^{***}
	(0.001)	(0.001)	(0.001)
Distance to bracket < 2 km		0.560^{***}	0.534^{***}
		(0.001)	(0.001)
Distance to bracket ≥ 2 and < 5 km		0.366^{***}	0.348^{***}
		(0.001)	(0.001)
Distance to bracket ≥ 5 and < 10 km		0.149^{***}	0.142^{***}
		(0.001)	(0.001)
Firm size ($>$ 10 employees)			0.003^{***}
			(0.001)
Informal sector			0.007^{***}
			(0.001)
Co-worker cheater share			0.274^{***}
			(0.001)
Pseudo-R^2	0.020	0.226	0.267
Fixed effects (p-value)			
Regions [8]	0.000	0.000	0.000
Years [10]	0.000	0.000	0.000

Notes: 1,534,902 observations in all models. Base category for distance-to-bracket dummies: distance of more than 10 km. Constants are not reported. Clustered standard errors are in parentheses, degrees of freedom are in square brackets. *, ** and *** indicate significance at 10-, 5- and 1-percent levels, respectively.

However, the significant impact of cheating co-workers on the individual reporting behavior does not necessarily establish a causal effect of the work environment on the evasion decision. For instance, our regression results might also reflect mirroring or sorting effects of both cheaters and co-workers. This would lead to biased inferences when regressing the individual decision to overreport on the cheating share of co-workers. Further, a statistically significant correlation might also be induced by firm specific differences in allowing employees to overreport. To deal with these issues of establishing a causal link between the work environment and individual responses to marginal incentives, we follow Chetty, Friedman and Saez (2013) and employ a subsample of job changers tracking individuals as they move between employers with different shares of cheating co-workers.[9] Focusing on changes in reporting when taxpayers switch the employer, we make sure that a job changer had no influence on the cheating decision of his co-workers, which, in turn, allows a causal interpretation of the observed relationship between the individual compliance decision and a firm's cheater share.

4 Identifying and Explaining Evasion Spillovers

4.1 A Sample of Job Movers

Starting from the full dataset on commuter allowances, we construct a sub-sample consisting of taxpayers who move between employers, resulting in a dataset of job changers. The sample of job changers is advantageous in at least two regards. First, focusing only on job movers eliminates the above mentioned possibility that causality runs in both directions, i.e., the individual compliance decision is affected by co-workers' cheating and vice versa; hence, it allows identification of any causal effects of the work environment. Second, recent research suggests that a lack of knowledge about the tax code as well as optimization frictions can lead to sluggish adjustment of economic decisions (see, e.g., Chetty 2012). Since moving to a new employer necessarily results in a new payslip, it forces employees to reconsider their allowance claim decision while exposing the individual to a new work environment, presenting a valuable moment for a researcher to examine changes in behavior.

Thus, our identification strategy rests on exogenous variation in job changes in regard to the compliance decision to reveal spillover effects from the new work environment on the individual evasion decision. Job changes can be seen as exogenous in regard to the compliance decision in three aspects: First, it is reasonable to assume that in the decision of what employer to choose, other aspects than the possibility to cheat on allowances dominate. Second, it is quite difficult for job seekers to evaluate potential evasion opportunities at prospective employers *before* starting work. Third, and perhaps most importantly, employees willing to cheat can easily bypass companies that potentially inhibit them to do so by filing and claiming a commuter allowance through the tax return at the end of the year. Specifically, the Austrian tax code does not require employees to file a tax return but offers wage earners the possibility to claim standard allowances via their payslips at work (Figure A1 displays such a payslip). Filing allowances via the payslip reduces the monthly tax withholding done by the employer and hence, increases the disposable income of the taxpayer. However, wage earners who do not apply allowances via the payslip can also claim eligibility by voluntarily filing a tax return at the end of the year. This 'outside'-option for overreporting via the tax return makes the sorting of taxpayers to certain companies very unlikely.

To examine the scale of this 'outside'-option, we use official data provided by Statistik Austria (the Austrian statistical office), comparing the number of total recipients of the commuter allowance

[9] Unlike Chetty, Friedman and Saez (2013), the critical environment in regard to the commuter allowance is not the neighborhood of an individual but his co-workers, since the allowance claim is stated via the payslip filed at the workplace.

(the sum of all claims via a payslip or tax return) to the number of filed claims via payslip only. Unfortunately, information to make this distinction is only available from 2008 onwards. In 2008, almost 80% of all commuter allowance recipients (808,944 out of 1,051,900 total recipients) did state their claim via the payslip at the workplace (Statistik Austria 2009). Note that since 2003-2004, filing a tax return has become greatly facilitated by introducing electronic (online) filing. Hence, in all likelihood, the share of taxpayers claiming allowances via the payslip has been even higher during the years of our study. Finally, we check whether previous cheaters systematically discontinue payslip filing when moving to a low evasion environment in order to circumvent the employer, but we do not find any evidence in this direction (see Figure 3). In sum, this suggests that there is little necessity for willing cheaters to bypass their companies, emphasizing the exogenous character of job changes in regard to the compliance decision.

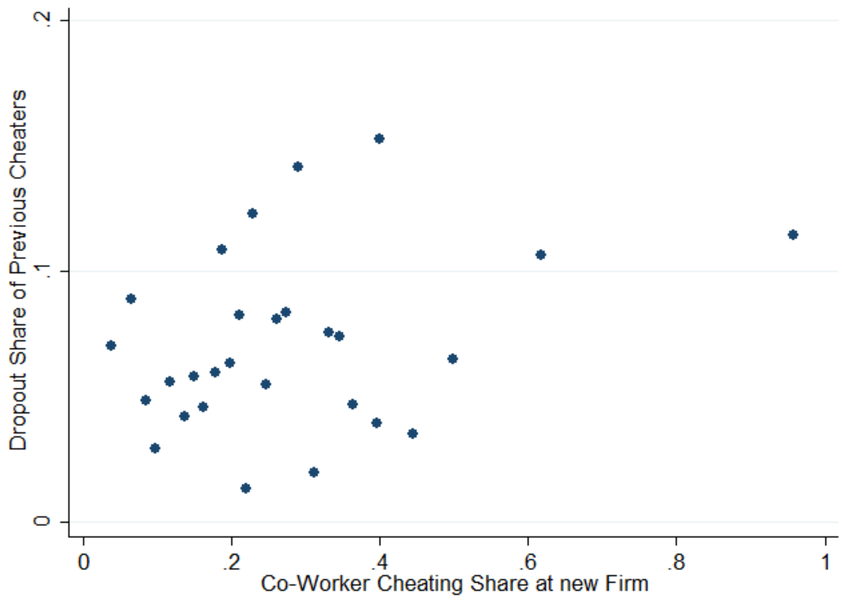


Figure 3: Dropouts from payslip filing after job change vs. co-worker cheating share at new firm

Notes: The figure plots the relationship between the share of job changers (focusing on previous cheaters only) that stop payslip filing at the new workplace versus the co-worker cheating share of the new firm. To construct the figure, we split the observations of job changers who overstate their distance prior to the move into 25 equal-sized bins based on the co-worker cheating share of the new firm. We then plot the mean share of job changers who discontinue payslip filing after the move versus the mean share of cheating co-workers in each bin. We restrict our sample to job changers within the same firm zip-code to take out any potential effects from a change in zip-code area or commuting distance on the filing decision. Thus, we keep the eligibility of job changers for the commuter allowance constant over the course of the job move. The graphical inspection of the figure reveals no systematic relationship between job changers who stop payslip filing and the level of co-worker cheating shares. In particular, it shows that previous cheaters do not systematically take up the 'outside'-option of filing via the tax return when moving to a low evasion environment.

To ensure that job changers face similar incentives to overreport, and given the fact that proximity to the next higher bracket has a strong effect on the evasion decision (see Table 3), we restrict our sample to those individuals with comparable distances to the next higher allowance bracket after the job change.[10] Further, we exclude individuals who change residence and employer at the same time to eliminate any effects stemming from the change in location or neighborhood on the reporting behavior. This gives us a sample of about 14,000 job changers.

Since we are not only interested in uncovering the causal effect of the work environment on individual cheating but also in the nature of the evasion spillover, we derive testable predictions about the behavior of job changers. Drawing on Chetty, Friedman and Saez (2013), the hypothesis that the variation in cheating depends on differences in knowledge predicts an *asymmetric* impact on the behavior of job changers that is fueled by two channels. The first is learning: Individuals who change to a firm with a higher cheating share should learn from their co-workers that non-compliance regarding commuter allowances is costless, in the sense that misreporting is practically undetected. In this case, we expect taxpayers to begin overreporting the commuter allowance. The second is memory: Individuals who move to a firm with a lower share of cheating co-workers should not change their cheating behavior and continue overreporting, since they know that misreporting is almost without consequences. Put differently, we expect information frictions to produce an *asymmetry* in the effects of positive versus negative changes in co-worker cheating shares on individual reporting behavior. This allows us to separate information from other plausible explanations for the variation in overreporting. For instance, if non-compliant behavior is refused by colleagues at the workplace, the individual taxpayer may feel forced to behave in the same way and comply with the tax code.[11] However, such peer or firm-specific effects on the reporting behavior would not directly predict that an individual's previous work environment should have an asymmetric impact on current behavior. Instead, one would expect changes in overreporting to move alongside with changes in co-worker cheating shares. For example, if firms with a low cheating share would effectively inhibit their employees from overstating the allowance, we would expect job changers moving from a high-cheating to a low-cheating firm to alter their reporting decision and start complying with the tax code.[12]

4.2 Event Study on Reporting Behavior of Job Movers

To test for the presence and asymmetry of the evasion spillover, we first construct an event study of cheating for job changers around the year in which they change the employer. We define the year of the move (year 0) as the first (full) calendar year in which an individual claims the commuter allowance at the new workplace. Using the first (full) calendar year after the job change accounts for the fact that job changers might not immediately change their reporting behavior, but rather

[10] The proximity used as the cut-off point is a quarter of the next bracket threshold, i.e., lower than 5km to the first, lower than 10km to the second, and lower than 15km to the third bracket, respectively. This restriction accounts for the fact that especially for commuters who live reasonably close to the bracket thresholds, the allowance can be seen as truly self-reported. In an attempt to fully eliminate any distance effect on the reporting behavior we validated our results using only job moves within the very same firm zip-code. Qualitatively, our results do not change when using this sample (see Appendix A.3).

[11] See Ichino and Maggi (2000), Mas and Moretti (2009) or Cornelissen, Dustmann and Schönberg (2013) for related studies in the context of shirking, work efforts and productivity at the workplace.

[12] One may be concerned that norms about tax compliance could have asymmetric persistence as well, implying that taxpayers tend to update their norms in one direction only. Appendix A.4 presents suggestive evidence against this notion of asymmetric persistence of norms.

within the first year. For both the old and the new employer, we observe the share of cheating co-workers in the years before and after the job change (omitting the cheating decision of the job changer when calculating the shares, i.e., using the 'leave-out' cheating share). From this, we divide our sample into five quantiles of cheating shares in the year before an individual changes the job and track their reporting behavior as they move to different quantiles of co-worker cheating shares. We focus on employees in the middle quantile of cheating co-workers prior to the job change and divide them into three groups: job changers to the lowest, the middle, and the highest co-worker cheating quantile.

Table 4 provides some descriptive statistics for the sample of job changers, focusing on movers to the lowest versus the highest co-worker cheating quantile. Panel A indicates that job changers moving to low-cheating firms have, on average, slightly lower gross wages, but the relative raise in income associated with the job change is very similar for both groups. Job changers to high-cheating firms tend to be male and white-collar compared to those moving to low-cheating firms, but are the same in terms of other socio-demographic characteristics such as age, education, or nationality. Most importantly, the overall commuting distance as well as the proximity to the next higher allowance bracket are equivalent for movers to low- versus high-cheating firms, giving both groups the same incentive to overreport.

Table 4: Characteristics of job-movers and firms in job mover sample

Variable	**Low cheating** (1)	**High cheating** (2)	**Difference** (2)–(1)	**p-value**
A. Characteristics of job movers				
Age	38.32	38.06	−0.25	0.433
Female (%)	28.23	25.02	−3.21	0.045
Tertiary education (%)	1.71	2.45	0.74	0.143
White-collar worker (%)	53.57	58.72	5.15	0.004
Non-native worker (%)	13.70	12.39	−1.31	0.284
Gross income before job move (Tsd. EUR)	28.46	32.22	3.76	0.000
Gross income after job move (Tsd. EUR)	30.57	35.16	4.59	0.000
Distance (km)	31.87	32.86	0.99	0.050
Distance to bracket (km)	4.85	4.99	0.14	0.282
B. Firm characteristics				
Age	36.61	36.36	−0.26	0.189
Female (%)	16.74	13.63	−3.12	0.015
Tertiary education (%)	0.53	0.63	0.10	0.665
White-collar worker (%)	37.75	41.22	3.46	0.049
Non-native worker (%)	4.14	3.59	−0.55	0.271
Gross income level (Tsd. EUR)	27.55	30.73	3.18	0.000
Gross income level of previous firm (Tsd. EUR)	25.83	27.40	1.57	0.000
Distance (km)	23.66	21.81	−1.85	0.015
Firm size > 10 employees (%)	69.50	81.34	11.84	0.000
Informal sector (%)	10.22	8.38	−1.84	0.144

In panel B of Table 4, we show descriptive statistics of the new firm job changers move to. We find slight differences in income levels between low- and high-cheating firms, but the relative increase in income levels is about the same for movers to low- versus high-cheating firms. High-cheating firms tend to be slightly bigger, white-collar, and more male dominated. The mean commuting distance of employees from both groups of firms are roughly the same and they display also very similar compositions of their workforce regarding other socio-demographic characteristics, such as age or the level of education.

Figure 4 plots the cheating behavior of job changers to the lowest, the middle, and the highest co-worker cheating quantiles around the year of the job move. We can see that job changers moving to the 5th cheating quantile exert sharp reactions in their reporting behavior, whereas the cheating behavior of individuals moving to the 3rd or 1st quantile is almost unaffected by the change of the employer. To test for the magnitude and significance of this effect we apply a difference-in-difference approach (see Wooldridge 2010 for details), regressing the binary cheater variable on a dummy variable for moving to the highest quantile, one for the event year, and an interaction term between these two dummy variables. We limit the regression to the middle and the highest quantile and to event years -1 and 0, so that the interaction term captures the effect of changing to a firm in the 5th quantile relative to one in the 3rd quantile. The same setup is used to estimate the effect of changing to the lowest quantile, again using the middle quantile as the control group. The results show a sharp and significant increase of 19.4 percentage points in cheating for job changers moving to the highest quantile of cheating co-workers. In contrast, moving to the lowest quantile results in only a small and insignificant decrease of cheating (the effect being about 4.3 percentage points).

To test for the information channel more directly and to further separate the impact of learning and memory, we test for asymmetric effects of increases versus decreases in co-worker cheating shares when individuals change jobs. This time, we calculate the change in co-worker cheating shares between the old and new employer. In other words, we define the change in knowledge about the possibility to cheat as the difference between the share of cheating co-workers where the job changer worked before the move and the cheating share of the firm the job changer is exposed to after the move. Further, we observe the reporting decision individuals take prior to and after the job change. We construct a variable capturing the changes in overreporting from the year before the job change (event year -1) to the year after the job change (event year 0).

Figure 5 plots these changes in cheating versus the change in co-worker cheating shares that a job changer experiences through the move. We bin the x-axis variable (i.e., the change in co-worker cheating share) into intervals and plot the change in mean cheating (the y-axis) within each bin. If the variation in cheating is due to information, there should be a clear kink in this relationship around 0: Increases in co-worker cheating shares should turn an increasing number of job changers into cheaters, whereas decreases in the cheating share should leave the reporting behavior unaffected. We test for the presence of such a kink by fitting separate linear control functions to the points on the left and right of the vertical line, weighted by the number of observations in each bin (see Card and Lee 2007). The hypothesis that the two slopes are equal is rejected with a p-value smaller than 0.001. Moreover, the slope to the right of the kink is significant

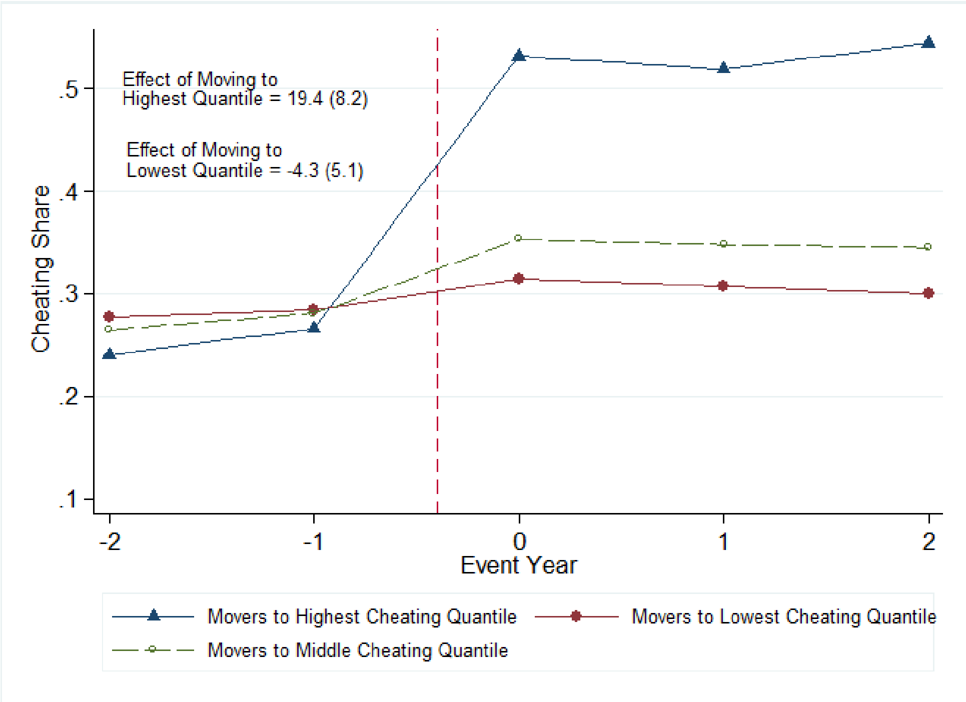


Figure 4: Impact of changing to firms with lower versus higher cheating shares

Notes: The figure is based on the sample of job changers. Individuals who change the employer more than once are excluded from the event study. Event time is defined as the calendar year minus the first year after the job change, so year 0 is the first year in which the individual claims the commuter allowance at the new workplace. To ensure that we have at least two years of data before and after the move, we restrict the sample to individuals who change employer in some year between 1997 and 2003. For both the old and the new employer, we calculate the share of cheating co-workers in the year before the job change occurs. We then divide the sample into five quantiles of co-worker cheating shares prior to the job change. From this, we plot an event study of individuals who move from the 3rd quantile to the 1st, 3rd, and 5th quantiles of co-worker cheating shares. The coefficients and standard errors are computed by difference-in-difference estimations comparing changes from year -1 to 0 for job changers to the 5th or 1st quantile with changes for those moving to the 3rd quantile.

and positive: A 10 percentage point increase in the share of cheating co-workers increases, on average, the probability of overreporting by 5.3 percentage points. On the contrary, a 10 percentage point reduction in the share of cheating co-workers leads to a statistically insignificant change in the mean of cheating of 0.3 percentage points.

The kink at zero constitutes non-parametric evidence of asymmetric responses to changes in co-worker cheating shares, indicating that at least parts of the evasion spillover as well as the variation in overreporting are due to differences in knowledge about the possibilities for non-compliance. In sum, the observed pattern displayed in Figures 4 and 5 suggests an increase of cheating over time, consistent with the spread of information as individuals move between companies. In fact, we find an increase of overreporting from 24.8 percent (1995) to 35.9 percent (2005) for the sample of job changers, echoing the effect of memory and learning.

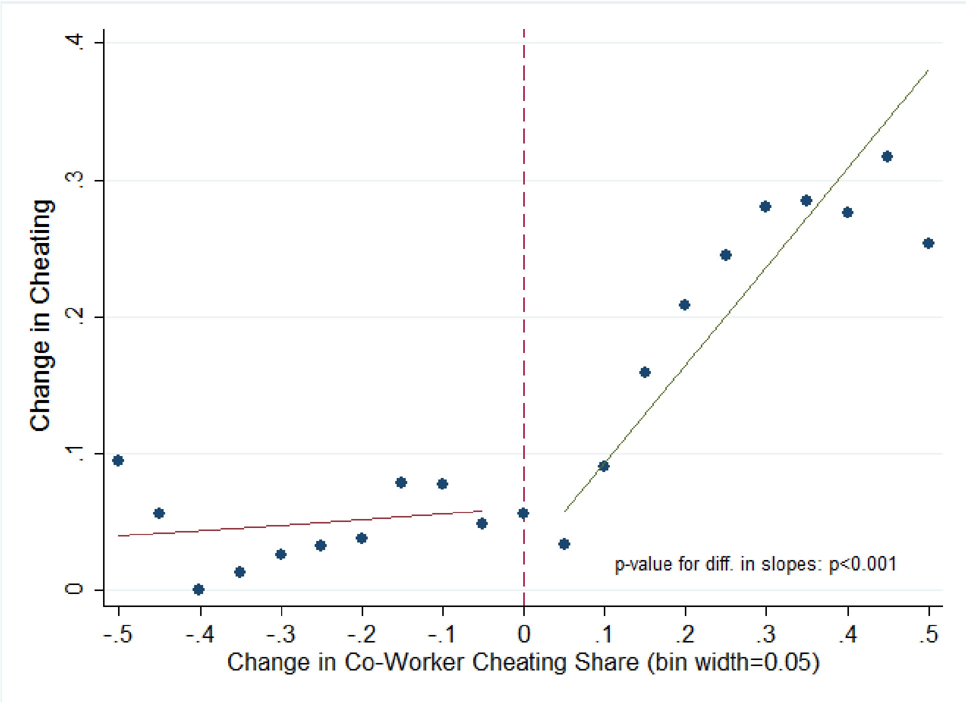


Figure 5: Asymmetric effects of changes in co-worker cheating shares

Notes: The figure plots changes in cheating behavior from the year before the job change (event year -1) to the year after the job change (event year 0) versus the change in co-worker cheating shares across the old and new employer. We group individuals into 0.05 percentage point-wide bins on changes in co-worker cheating shares (the x-axis) and then plot the change in mean cheating within each bin (the y-axis). We fit separate linear control functions to the points on the left and right of the vertical line, weighted by the number of observations in each bin.

4.3 The Tax Value of the Job Change

One might ask which tax value did job changers realize after moving to a new evasion environment and after potentially gaining new information about the possibility to overreport. To estimate the effect of the new evasion environment on the size of the allowance claim, we first observe changes in the allowance benefit of job changers from the year before the move to the year after the move. We then construct two variables capturing changes in co-worker cheating shares that an individual experiences when she moves to a new employer: A first variable records exact positive changes (increases) in the co-worker cheating share and takes entry zero in all other cases, whereas a second variable records exact negative changes (decreases) in the co-worker cheating share, and entry zero otherwise. Hence, the two variables capture the magnitude of the change in co-worker cheating shares, separated by increases versus decreases of the share. Similar to the regression kink design used in Figure 5, we expect increases in co-worker cheating shares to raise the allowance benefit while reductions should leave the allowance amount unaffected. We test for such asymmetric responses by regressing the change in the allowance benefit on both variables capturing increases and decreases of the share, respectively.

Table 5 displays results from OLS regressions estimated on the microdata, using the sample of job changers. For our baseline specification, a 10 percentage point increase in the co-worker cheating share raises the allowance benefit significantly by EUR 94.4. In contrast, we estimate that a 10 percentage point reduction in co-worker cheating share results in a statistically insignificant change in the allowance benefit of EUR 16.4. The results are also robust to the inclusion of control variables capturing changes in income, firm size, or the distance to the next higher allowance bracket related to the job change. In sum, we again find asymmetric effects of changes in co-worker cheating shares when employees move across companies, indicating that differences in knowledge about the possibilities for non-compliance are important when taxpayers take their reporting decisions.

Table 5: Change in allowance benefit after job change

Dependent variable: Change in allowance amount (in EUR)

	Without controls	Controls included
Decrease of co-worker cheater share	163.38	119.55
	(104.42)	(124.25)
Increase of co-worker cheater share	943.93	1063.33
	(194.10)	(215.83)
R^2	0.01	0.02
Observations	14,002	14,002

Notes: Results are estimated based on the sample of job changers, using only the year before and after the change to another firm. The dependent variable is the change in the commuter allowance amount that occurred with the job change. Coefficients are from OLS regressions estimated on the microdata. Standard errors are in parentheses. Column 2 adds controls for changes in income, firm size, and distance to the next higher bracket related to the job change.

4.4 Commuters Changing Residence but not the Firm

Next, we want to focus on taxpayers who only change their residence but not their employer. Similar to a job change, moving residence location constitutes another valuable moment for taxpayers to reconsider their compliance decision since they have to report their new address and commuting distance to the employer. Thus, focusing on employees changing their residence accounts for optimization frictions such as inattention, as it requires taxpayers to restate their compliance decision when reporting their new allowance eligibility. Indeed, almost every alteration in already existing commuter allowance claims of taxpayers we observe in our dataset occured with either a change of the employer or a change in residence location. In the case of taxpayers changing only their residence but not the employer, we expect differences in the change of the compliance decision between movers working in high- versus low-cheating companies. Hence, different levels of cheating co-workers regarding the commuter allowance should influence the compliance decision of those colleagues changing their residence. We study this effect of differences in co-workers' cheating on the reporting decision by tracking the compliance decision of movers around the year in which they change their residence (while keeping the firm and commuting distance constant). We expect movers in high-cheating companies to overreport much more after the move, while employees of low-cheating companies should not change their behavior.

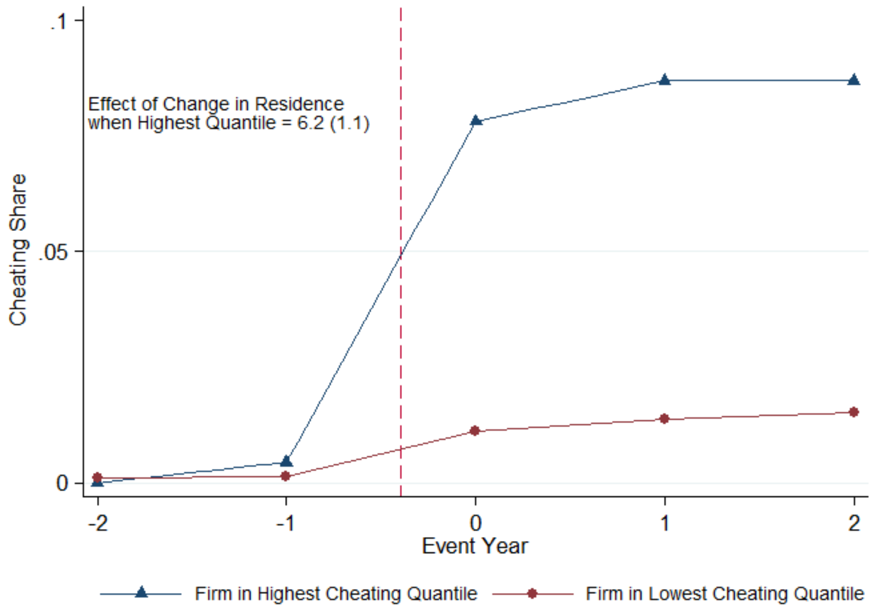


Figure 6: Effect of changing residence on reporting behavior when working in high cheating vs. low cheating firms

Notes: The figure is based on a sample of individuals moving residence but not the employer and who do not overreport prior to the move. Commuting distance is held constant before and after the move to eliminate any effects from a change in distance on the reporting behavior. Event time is defined as the calendar year minus the first year after the move, so year 0 is the first year in which the individual lives at the new residence location. To construct the figure, we first calculate the share of cheating co-workers of each mover two years before the change in residence and divide our sample into five quantiles of shares of cheating co-workers. From this, we plot an event study of individuals who change residence and work in a firm within the 1st and 5th quantiles of cheating co-workers, respectively. The coefficient and standard error is computed by difference-in-difference estimation comparing changes from year -1 to 0 for movers working in the 5th quantile with changes for those working in the 1st quantile.

Figure 6 plots an event study of cheating for movers around the year in which they change residence location, focusing on those being non-cheaters prior to the move. To construct this figure, we define the year of the move as the first year a payslip was filed with a new zip-code address. Event time is computed as the calendar year minus the year of the move, so event year 0 is the first year the individual lives in the new zip-code. We observe the share of cheating co-workers two years before the move and divide our sample into five quantiles of shares of cheating co-workers. We then focus on two groups of movers, those changing residence and working in companies within the highest co-worker cheating quantile versus commuters working in companies within the lowest co-worker cheating quantile. Figure 6 illustrates the cheating behavior of these two groups around the event year. It can be seen that movers working in the 5th cheating quantile exert a sharp increase in their cheating behavior, whereas the reporting behavior of movers in the 1st quantile is almost unaffected. Again, we employ a difference-in-difference estimate to test for the magnitude and significance of this effect: The binary cheater variable is regressed on a dummy variable for working at a firm in the highest quantile, one for the event year, and an interaction term between these two dummy variables. We limit the regression to event years -1 and 0, so that the interaction term captures the effect of moving when working in the 5th quantile relative to one in the 1st quantile. The results show a sharp and significant increase of 6.2 percentage points in cheating for movers in the highest quantile, whereas for movers in the lowest quantile cheating rises only very slightly (but still statistically significant) by about 0.9 percentage points.[13]

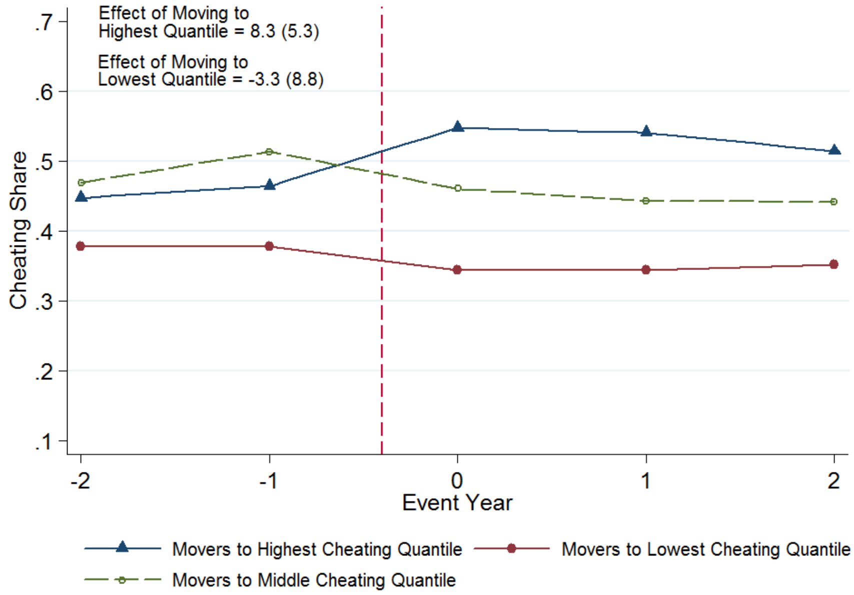
Although this empirical test does not allow us to separate the nature of the spillover into an information and peer effect, it suggests that movers make use of the opportunity to reconsider their compliance decision as they change residence, with commuters working in high-cheating companies being much more responsive than those working in low-cheating companies. In other words, focusing on taxpayers who are moving residence does not allow exploitation of any asymmetries of changes in co-worker cheating shares nor the disentanglement of the role of information from other explanations of the spillover effect. However, it again shows how sensitive taxpayers react to their work environment.

4.5 Remote Firm Location and the Reporting Behavior of Job Movers

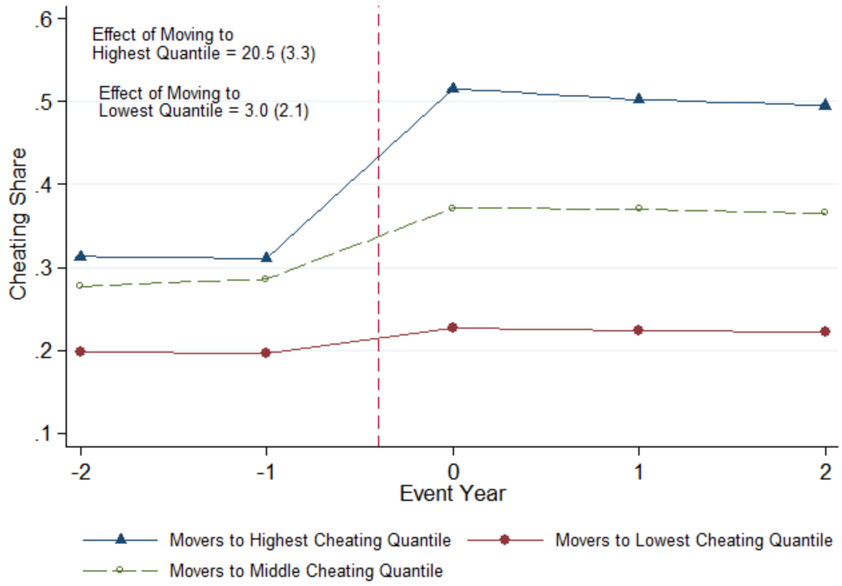
Although our descriptive statistics as presented in Table 4 show no significant differences between the distance to the next higher bracket for job movers to high- versus low-cheating firms, one might be concerned that high-cheating firms are actually located in more isolated places *within* the single zip-code areas, resulting in a misclassification of its commuting employees as cheaters when they actually report honestly. Since we do not have the exact street address of the firms in our population tax data, we cannot test for this effect directly. However, if the location of a firm within a zip-code dominates the reporting decision of its commuters decisively, we should observe some change in the reporting behavior of job changers when they leave a high-cheating (and potentially remote) firm and move to a low-cheating (and maybe more centrally located) firm. In Figure 7, we test this by replicating the event study as presented in Figure 4, this time focusing on job changers who come from the highest versus the lowest quantile of cheating co-workers prior to the move. Again, we split our sample of job changers into five quantiles of co-worker cheating shares in the year before an individual changes the job and track their reporting behavior as they move to different quantiles of co-worker cheating shares.

First, we want to focus on employees in the highest quantile of cheating co-workers prior to the job change and divide them into three groups: job changers to the lowest, the middle, and the highest co-worker cheating quantiles. Panel A of Figure 7 plots the cheating behavior of these job changers around the event year. We can see that this time, job movers do not differ in the

[13]Notice that the magnitude of the effect of a change in residence is somewhat weak compared to those focusing on job changers presented in the previous sections. This stems from the fact that the sample of job changers is composed of taxpayers with a commuting distance rather close to the next higher bracket *after* the job change and hence, they are much more responsive in their evasion decision. In contrast, for the sample of taxpayers who change residence, we fix the commuting distance in order to eliminate any potential effects from a change in distance on the reporting decision (similar to A.3 in the Appendix).



(a) Panel A



(b) Panel B

Figure 7: Event study of job changers coming from the highest vs. lowest quantile of cheating co-workers

Notes: This figure replicates Figure 4, restricting the sample to job changers who come from the highest vs. the lowest quantile of cheating co-workers prior to the move. Panel A displays an event study of cheating for employees in the highest quantile of cheating co-workers prior to the job change. Panel B focuses on individuals who depart from the lowest quantile of cheating co-workers. From this, we plot two event studies of individuals who move to the 1st, 3rd and 5th quantile of co-worker cheating shares. The coefficients and standard errors are computed by comparing within-group changes from year -1 to 0. See the notes to Figure 4 for more details on the construction of both panels.

change of their reporting behavior, irrespective of the quantile of cheating co-workers to which they move. For none of the three groups we observe significant differences in reporting before and after the job move. This result is consistent with our expectations that decreases in co-worker cheating shares should not affect the individual reporting behavior. More importantly, this result works against the concern that it is the true location of a firm within a zip-code area that actually turns its commuters into cheaters. If the degree of cheating commuters of a firm would be the sole result of its actual distance to the center of a zip-code as we measure it, job changers coming from the highest quantile of cheating co-workers and moving to a firm within the lowest quantile of cheating co-workers should change their reporting at least in some ways. Since we do not observe any significant changes for these job movers but find strong effects for job changers moving to a higher cheating environment, we conclude that it is not the location of a firm that dominates the cheating behavior of its commuting employees.

Second, we focus on job changers departing from the lowest quantile of cheating co-workers. Again, we divide them into three groups: job changers who either stay in the lowest, move to the middle, or change to the highest co-worker cheating quantile. We observe sharp reactions of job changers moving to a firm with a higher share of cheating co-workers, whereas those who stay in the lowest quantile do not show a significant change in reporting behavior. Note that individuals moving to the highest quantile exhibit more cheating even prior to the move since we do allow for differences in total commuting distance before the job change occurs, which influences prior reporting. Nevertheless, the overall pattern we observe here with sharp reactions of individuals moving to higher-cheating companies versus no change in reporting when job changers depart from the highest quantile works in favor of our hypotheses. The fact that we find asymmetric effects between increases versus decreases of co-worker cheating shares suggests that it is not the location of a firm that dominates the cheating behavior of its commuting employees. Finally, this confirms that our results are robust to changes of the co-worker cheating quantile job changers depart from when they move, and mirror our findings from the regression kink analysis as presented in Figure 5.[14]

The next section presents further evidence against the notion that the compliance pattern we observe is driven by the employer's location, i.e., his remoteness within a given zip-code. For this purpose, we rely on an additional dataset that allows us to calculate the exact driving distance (door-to-door) between the location of the employer and the commuter's residence.

[14] In addition, we perform an analysis removing commuters with a distance of less than three kilometers to the next higher allowance bracket from the sample. In this way, we exclude commuters where imprecise measurement due to remote firm location could result in a classification as cheaters when they actually report honestly. It turns out that the evasion pattern in this case is very similar to the one of our original event study presented in Figures 4 and 5, backing our conclusion that the spillovers we observe are not the sole product of firm-specific characteristics like the actual location of a firm within a zip-code area. For the sake of brevity, we do not display the results of this exercise here, but they are available from the authors upon request.

5 A Case Study on Exact Residence and Workplace Addresses of a Large Austrian Retailer Chain

A large Austrian retailer chain (in total 40,000 employees) provided us with exact residence and workplace addresses (including house numbers) of almost 5,000 of its commuting employees, working at one of 546 stores scattered all across Austria. In addition, we obtained information from the retailer regarding the commuter allowance that the employees received via the payslip. Similar to our whole population from the Ministry of Finance, we find that the majority of commuters (3,857) receive one of the major allowances, claiming that public transport is not available. Again, we focus on those recipients of a major allowance and use a GIS (geographic information system) to calculate the real driving distance between both street addresses to compare it with the allowance the employees actually received. The emerging picture is striking and confirms our results from the population tax data: Cheating on the commuter allowance is pervasive, with more than 39 percent of all commuters overreporting their travel distance to work.

To begin with, Figure 8 replicates Figure 2 from the main section, showing that employees react sharply to the thresholds where the allowance discontinuously increases to a higher amount. Again, we find that the closer commuters live to a respective bracket, the more prone they are to overreport their distance and, hence, the allowance they claim. In fact, our retailer data and distance measure based on exact street addresses indicates even stronger reactions to the bracket thresholds, with sometimes up to 90 percent of the commuters overreporting their travel distance when residing exactly at the cut-off point. In sum, the results confirm our findings that cheating on commuter allowances is substantial and clearly suggest that employees systematically overreport their commuting distances.

Furthermore, we received information from the retailer about the reporting behavior of new recruits at the single stores in the year of the analysis. We will use the compliance decision of these new recruits to test for the presence of positive spillovers from the existing co-worker cheating share at the single stores on the reporting behavior of the new entrant. Again, the identifying assumption is that the job move of the new recruit is exogenous in regard to the evasion opportunity, as we discussed in Section 4.1. In total, our sample comprises 362 new (commuting) recruits starting to work at one of 139 different stores of the retailer in 2012. Hence, around 10 percent of the 3,857 employees who receive a major commuter allowance in 2012 are new entrants to the retailer chain. We estimate a simple binary choice model regressing an indicator variable of the new recruit's compliance decision on the co-worker cheating share of the workplace at the time of the job move, while controlling for the commuting distance as well as the size of the store (unfortunately, we did not obtain any further information on personal characteristics, such as age or gender). In Model A, we include the co-worker cheater share of the workplace at the time of the job move. Model B mirrors the setup of our event studies and uses dummy variables for recruits entering the lowest, middle, or highest quantiles of co-worker cheating shares of the workplace at the time of the job move (with the middle quantile forming the reference group). Since we do have information about the exact residence and workplace addresses of the new recruits, we insert the precise distance to

Figure 8: Distance to bracket and cheating behavior (retailer data)

Notes: The figure is based on exact residence and workplace addresses of 3,857 commuters working at one of the 546 stores of a large Austrian retailer. We display the reporting behavior of commuters by bins of distance to their workplace (bin width=1.25 km). The bars show the fraction of cheaters for each bin. The dashed lines indicate the thresholds where the allowance discontinuously increases to a higher amount (at 20, 40, and 60 km, respectively). The solid line indicates the total number of cheaters per bin. The histogram includes recipients of the major allowance for the year 2012.

the next higher allowance bracket as a continuous predictor for the easy opportunity to overreport. Further, we include a dummy variable taking entry one for stores with more than 10 employees, and zero else.

Table 6 displays our estimation results. First of all, we again find a strong effect for the proximity to the next higher allowance bracket on the compliance decision. Specifically, a one kilometer increase of the distance to the next higher allowance bracket results in a decrease of the probability to overreport by about 2.5 percentage points. More importantly and consistent with our evidence form the population tax data, we find a positive and significant effect of the co-worker cheating share of a store on the individual compliance decision in both models. In Model A, an increase of the cheater share by 10 percentage points translates into an increase of an individual's probability of being non-compliant by about 2.7 percentage points. Using dummy variables in Model B, we find a significant increase of 10.4 percentage points in cheating for new recruits starting to work at a store within the highest quantile of cheating co-workers. In contrast, moving to the lowest quantile has only a small and insignificant effect on the probability to overreport. In both models, we do not see a significant effect of the size of the store on cheating.

Table 6: Case study evidence (average marginal effects)

Dependent variable: Indicator variable with entry one if new recruit is cheating

	Model A	**Model B**
Distance to bracket	-0.025^{***}	-0.026^{***}
	(0.003)	(0.003)
Store size (> 10 employees)	-0.018	0.017
	(0.036)	(0.046)
Co-worker cheater share	0.267^{***}	
	(0.100)	
High-cheating store$^{a)}$		0.104^{*}
		(0.062)
Low-cheating store$^{a)}$		0.007
		(0.056)
Pseudo-R^2	0.217	0.208
Observations	362	362

Notes: $^{a)}$ Indicator variable with entry one if new recruit starts at a workplace in the highest (lowest) quantile of cheating co-workers, and zero else. Constants are not reported. Clustered standard errors are in parentheses. *, ** and *** indicate significance at 10-, 5- and 1-percent levels, respectively.

In sum, the analysis based on the retailer data seems to confirm our evidence of positive spillovers from the work environment on the individual compliance decision. Having precise information regarding the exact residence and workplace addresses of the commuters, the results here work against the proposition that it is the actual location of a firm within a zip-code area that dominates the pattern of cheating we observe. Rather, our findings suggest that taxpayers' reporting is sensitive to the corresponding cheating environment at a given workplace.

6 Conclusion

This paper aims to contribute empirical evidence to the question of why people evade taxes. First, we detect tax evasion in regard to a commuter allowance by re-measuring the real driving distance between the location of a firm and a taxpayers' residence. We find that cheating is substantial with sharp reactions of taxpayers to thresholds where the allowance discontinuously increases, with an overall evasion rate of around 30 percent. Further, we make use of our rich administrative panel data set to identify the impact of evasion spillovers on the individual compliance decision. By focusing on individuals moving between companies with different levels of cheating co-workers, we uncover the effect of the work environment on the individual reporting behavior. To test for the nature of the evasion spillover, we generate testable predictions about the behavior of job changers. We find asymmetric effects of increases versus decreases in co-worker cheating shares when individuals move between companies, consistent with a class of explanations based on information, learning and memory. This result indicates that once individuals learn that overreporting goes undetected, they are more likely to start cheating. In contrast, being exposed to an environment of compliance does not change previous cheating behavior. In sum, the asymmetric pattern we observe suggests that information about the easy opportunity to cheat is important for the compliance decision taken by taxpayers.

Our study carries important implications for the design of optimal tax collecting strategies. First, it shows that third-party reporting is not necessarily a panacea against tax evasion. In the spirit of the third-party model proposed by Kleven, Kreiner and Saez (2009), our results underpin that employers need the means as well as the incentive to correctly record taxable items of their workers to be a reliable partner to the government. Specifically, Kleven, Kreiner and Saez (2009) suggest that when it comes to wages, companies have sufficient incentive to keep accurate business records in order to enhance productivity. In a nutshell, it is the existence of such business records evidence that makes third-party tax enforcement so successful. In contrast, companies lack such a value-added of exact recording in the realm of the commuter allowance, combined with a lack of enforcement by the tax authorities. This combination of lack of enforcement and incentives leads to a dysfunctional system of third-party reporting. Second, our results indicate that information regarding changes in tax policies diffuses only slowly over time until they fully materialize, and legislators as well as researchers have to be aware of this fact when evaluating the impact of a new legal act or reform. Finally, by exploiting behavioral asymmetries within a sub-sample of job movers, our paper represents also a methodological contribution to the compliance literature. It points to a promising way to study the determinants of the individual compliance decision, even when official (or randomized) audit data is absent.

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A Appendix

This appendix presents additional material on the Austrian commuter allowance, descriptive statistics, and robustness checks briefly described in the main sections of the paper.

A.1 Zip-codes

A potential concern might be that the centroid of a zip-code does not sufficiently represent the location of residential areas in the case of bigger zip-codes, distorting our distance measure, and hence, the number of commuters who overreport. To investigate this issue further, we present cheater shares by brackets for different size-classes of zip-code areas. The upper part of Table A.1 reports cheating shares using all zip-codes, as displayed in Figure 1. Then, we restrict our sample to zip-codes with a surface area below average (40km^2 as the cut-off point). Finally, we exclude cities with more than 100,000 inhabitants (see lower part of Table A1). As can be seen from the table, the share of filers overreporting their commuter allowance does not change considerably with these restrictions. In fact, the number of overreporters increases even further when excluding bigger zip-code areas. This let us conclude that our results based on observations from all zip-code areas represent rather conservative estimates.

A.2 Underreporters

Figure 1 displays a number of employees who, according to our distance measure, commute much longer to work than what they actually claim on their payslips. Almost 50 percent of these underreporters reside more than 100km away from their workplace as recorded by the ASSD. Exploring this issue further, panel A of Figure A2 plots the relationship between the share of underreporters of a firm and the mean of underreported distance. The emerging picture indicates that the mean underreported distance increases sharply at higher levels of underreporting in a firm. Panel B displays the number of underreporting individuals by the share of underreporters of a firm, indicating that the majority of underreporters stem from companies with an underreporting share of 75 percent or more of all commuting employees. In addition, the median size of companies with an underreporting share between 75 percent and 100 percent is 7 times greater than for our overall sample (not displayed). This allowed us to conclude that those companies, in all likelihood, consist of multiple branches but are recorded as single-entry enterprises in the ASSD (for instance, only the Viennese headquarter). Excluding companies with an underreporting share of more than 75 percent reduces overall underreporting by two thirds. In sum, the fact that the bulk of underreporting concentrated to a small fraction of companies (around 10 percent), it suggests that the ASSD records the majority of workplaces quite accurately.

A.3 Job Changers within the Same Firm Zip-code

As mentioned in footnote 10, we validated our results by using a sample of job changers who move within the very same firm zip-code. This procedure eliminates any effects stemming from a change in distance on the reporting behavior. Figure A3 replicates Figures 4 and 5 of the main section, using this time the sample of job changers with constant zip-codes (and hence, distances). Panel A displays the event study of cheating for job changers around the year in which they change the employer. Again, we find a sharp and significant increase of 10.4 percentage points in cheating for job changers moving to the highest quantile of cheating co-workers. In contrast, moving to the lowest quantile results in a very small and insignificant change of cheating of around 0.7 percentage points.

To test for asymmetric effects of increases versus decreases in co-worker cheating shares more directly, panel B plots the change in cheating versus the change in co-worker cheating shares that a job changer experiences through the move. In line with our previous results, we find non-parametric evidence of asymmetric responses to changes in co-worker cheating shares around 0: Increases in co-worker cheating shares turn an increasing number of job changers into cheaters, whereas decreases in the cheating share leave their reporting behavior unaffected. The hypothesis that the two slopes are equal is rejected with a p-value smaller than 0.05. In sum, we again find asymmetric effects of changes in co-worker cheating shares when employees move across companies, indicating that differences in knowledge about the possibilities for non-compliance are important when taxpayers take their reporting decisions. Notice that the results here are somewhat more attenuated than those presented in the main section. This stems from the fact that the sample of job changers used in the main section comprises taxpayers who reside rather close to the next higher bracket after the job change and hence, are much more responsive in their evasion decision when receiving new information. However, the overall pattern and results from the main section are confirmed.

A.4 Can Asymmetric Persistence of Norms Explain the Compliance Decision?

One may be concerned that norms about tax compliance could lead to asymmetric persistence as well: Once an individual observes someone else cheating taxes, it becomes an acceptable habit. In other words, employees might update their norms in one direction only, i.e., once they learn that tax evasion is tolerated, they do not change their norms when being exposed to a more honest environment. Hence, asymmetric persistence of norms may also present a potential explanation for the empirical pattern and spillovers we observe in the previous section. This question is difficult to be settled here, but we present suggestive evidence that is not very supportive of this hypothesis.

From the hypothesis that norms might trump information in regard to the compliance decision, we can derive a testable prediction about the behavior of job changers: When information is the main driver of tax evasion, we expect the non-compliance behavior to be contained to the very item of the commuter allowance, when employees move between companies differing in the share of workers overstating this specific allowance. In contrast, if getting wind of other taxpayers' misreporting on one tax item does corrupt someone's tax morale more generally, we would expect individuals to look for other items in the tax code that can easily be used for evasion. Hence, we would expect employees moving to high-cheating companies in terms of the commuter allowance to start searching for other low-hanging evasion fruits as well (since they learn that tax evasion is an acceptable habit) while changing to a low-cheating firm should leave their behavior unaffected. The Austrian tax code offers two more (quasi-)self-reported items that can also be claimed through the payslip at the workplace and that are similar to the commuter allowance in terms of enforcement. Both items are tax credits, one for single parents and one for single earners, respectively. Employees report eligibility and the social security number of the dependent to the employer, who then adjusts taxable income before withholding.[15] Again, the employer lacks the means to double-check eligibility of the stated claims and, hence, misreporting can only be detected in the case of a firm audit. Unfortunately, we cannot say for sure whether employees cheat on these two tax items, since we do not observe their family situations or their partner's income. However, according to the hypothesis that norms have asymmetric persistence, we should still observe significant differences in the take-up rates of these items between employees moving to a high-cheating versus a low-cheating firm: When moving to a high-cheating (commuter) firm, job changer should learn that evading taxes is an acceptable habit and start looking for other items to reduce taxable income. When moving to a low-cheating (commuter) firm, this change in the normative environment should not result in a behavioral change. In contrast, if information about the easy opportunity to evade taxes via the commuter allowance is the key driver in the evasion decision and not a more general erosion of norms, there should be no observable asymmetric pattern like this. To ensure that new co-workers do not pass on information regarding the opportunity to evade via the single-parent or single-earner tax credit, we exclude job changers moving to firms with recipients of these two items. This makes sure that new co-workers can pass on only two kinds of messages: The first one is informational, namely that evasion via the commuter allowance is easily done without detection. The second is normative, teaching the new recruit that tax evasion is acceptable in a broader sense.

Panel A of Figure A4 plots changes in take-up rates for the single-earner tax credit against changes in co-worker cheating shares (in regard to the commuter allowance) that a job changer experiences through the move (panel B for the single-parent tax credit). If cheating becomes an acceptable habit once people observe others cheating on tax items, there should be a kink in this relationship around 0. Increases in the share of co-workers cheating on the commuter allowance should raise take-up rates of the other two tax items as well, whereas a decrease in cheating co-workers should leave the number of claimants unaffected. Again, we test for the presence of such a kink by fitting separate linear control functions to the points on the left and right of the vertical line weighted by the number of observations in each bin. The hypothesis that the two slopes are equal cannot be rejected with p-values of 0.442 and 0.751, respectively. Furthermore, none of the slopes to the left

[15]Note that a valid social security number of a dependent does not immediately qualify for the tax credit. Certain criteria regarding the family- and living situation (e.g., whether the taxpayer lives separated from his partner) have to be met as well, which are fully self-reported by the taxpayer. Similar to the enforcement of the commuter allowance, no automated checking system matching addresses of both parents in order to detect misreporting is used by tax authorities.

and right are significantly different from zero. In sum, we do not find asymmetric effects of changes in the share of cheating co-workers on the take-up rates of the two tax credits, suggesting that asymmetric persistence of norms do not play a major role when employees take their reporting decisions in regard to the two tax credits. In contrast, the pattern of strong asymmetric effects for the commuter allowance as documented in the previous section indicate that information about the possibility for non-compliance is important when it comes to the evasion decision. Finally, if new recruits do not take-up the single-earner tax credit (or single-parent) because the existence of these two items had simply never occurred to them, then this is a round about way of affirming our hypothesis that knowledge and information about a program is a crucial ingredient of the compliance decision.

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Bezugs/pensionsauszahlende Stelle

Steuer-Nr.

Arbeitnehmerin/Arbeitnehmer:

Familienname

Vorname

Titel

Adresse

PLZ

Ort

Arbeitnehmerin/Arbeitnehmer:

Soziale Stellung

Vers.-Nr.

Geburtsdatum

weiblich

männlich

Vollzeit-beschäftigung

Teilzeit-beschäftigung

AVAB wurde berücksichtigt (J/N)

AEAB wurde berücksichtigt (J/N)

Wenn Kinderzuschläge berücksichtigt wurden (ab Zeitraum 2004): Anzahl der Kinder gemäß § 106 Abs. 1 EStG 1988

Wenn AVAB: Vers.-Nr. der (Ehe)-Partnerin/des (Ehe)Partners

Bruttobezüge gemäß § 25 (ohne § 26 und ohne Familienbeihilfe) 210

Steuerfreie Bezüge gemäß § 68 .... 215 –

Bezüge gemäß § 67 Abs. 1 und 2 (innerhalb des Jahressechstels), vor Abzug der Sozialversicherungs-beiträge (SV-Beiträge) .... 220 –

Insgesamt einbehaltene SV-Beiträge, Kammerumlage, Wohnbauförderung ....

Abzüglich einbehaltene SV-Beiträge:

für Bezüge gemäß Kennzahl 220 .... 225 – .... 230 –

für Bezüge gemäß § 67 Abs. 3 bis 8, soweit steuerfrei bzw. mit festem Steuersatz versteuert .... 226 –

Landarbeiterfreibetrag gemäß § 104 .... 240 –

Übrige Abzüge:

Auslandstätigkeit gemäß § 3 Abs. 1 Z 10 u. 11 ....

* Pendler-Pauschale gemäß § 16 Abs. 1 Z 6 ....

Einbehaltene freiwillige Beiträge gemäß § 16 Abs. 1 Z 3b ....

Steuerfreie bzw. mit festen Sätzen versteuerte Bezüge gemäß § 67 Abs. 3 bis 8, vor Abzug der SV-Beiträge ....

Sonstige steuerfreie Bezüge ....

Summe übrige Abzüge 243 –

Steuerpflichtige Bezüge 245 =

Insgesamt einbehaltene Lohnsteuer ....

Abzüglich Lohnsteuer mit festen Sätzen gemäß § 67 Abs. 3 bis 8 –

Anrechenbare Lohnsteuer 260 =

Nach dem Tarif versteuerte sonstige Bezüge (§ 67 Abs. 2, 6, 10)

Nicht steuerbare Bezüge (§ 26 Z 4) und steuerfreie Bezüge (§ 3 Abs. 1 Z 16 b)

Arbeitgeberbeiträge an ausländi-sche Pensionskassen (§ 26 Z 7) ......

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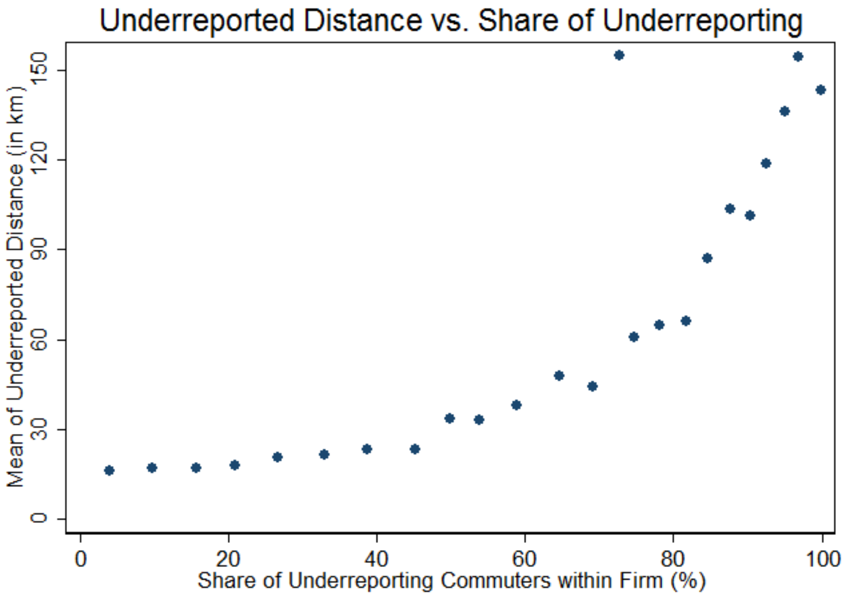
Figure A. 1: A blank payslip form

Notes: The figure displays a blank payslip form as provided by Austrian tax authorities. The field *Pendlerpauschale* (marked with*) refers to the commuter tax allowance. Employers fill in the allowance amount according to the commuting distance reported by their employees and adjust taxable income (field 245) before withholding (field 260).

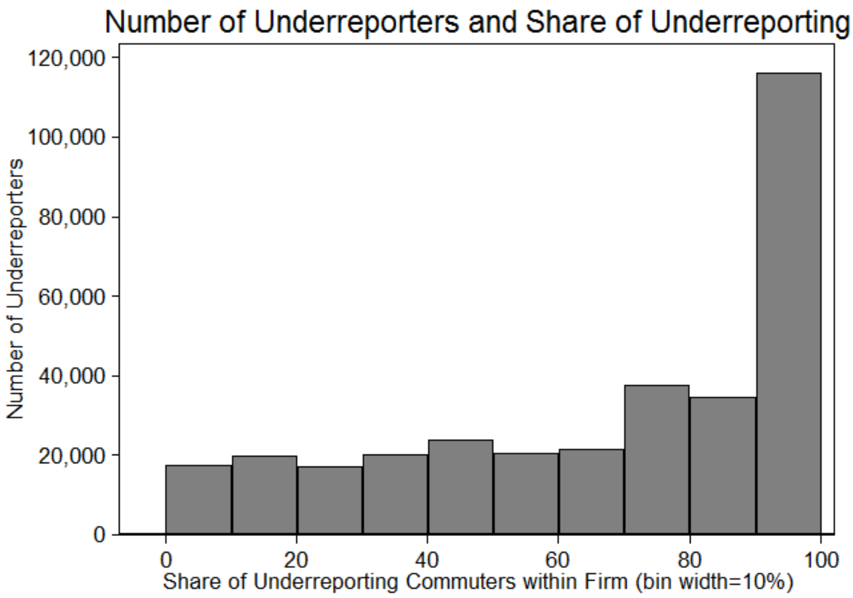
Table A. 1: Size of zip-code area and share of cheaters (by bracket)

	All zip-codes		
Allowance bracket	#Commuters	#Cheaters	Cheater share
20–40 km	568,621	148,756	26%
40–60 km	160,646	70,256	44%
More than 60 km	115,378	40,541	35%
	Only zip-codes < than average surface area		
20–40 km	206,952	63,582	30%
40–60 km	59,870	28,796	48%
More than 60 km	43,560	16,898	38%
	Only zip-codes with population < 100,000		
20–40 km	418,964	115,888	28%
40–60 km	96,023	43,804	46%
More than 60 km	46,648	18,880	40%

Notes: The table includes recipients of the major allowance by bracket and size of the zip-code area. Observations are pooled for all years 1995–2005. The middle block of the table uses the average surface area of Austrian zip-codes ($< 40\text{km}^2$) as a cut-off point. The lower block of the table excludes Vienna and four other state capitals with a population above 100,000.



(a) Panel A



(b) Panel B

Figure A. 2: Underreporting as a result of single-entry enterprises

Notes: Panel A plots the relationship between the share of underreporters (of a firm) and the mean of underreported distance. To construct panel A, we split the observations into 25 equal-sized bins based on the share of underreporters in the firm-by-year cell. We then plot the mean underreported distance versus the mean share of underreporters in each bin. Panel B displays the number of underreporting individuals by the share of underreporting commuters within firms. We bin companies by the share of underreporting commuters into intervals of 10 percentage points and plot the total number of underreporters within each bin.

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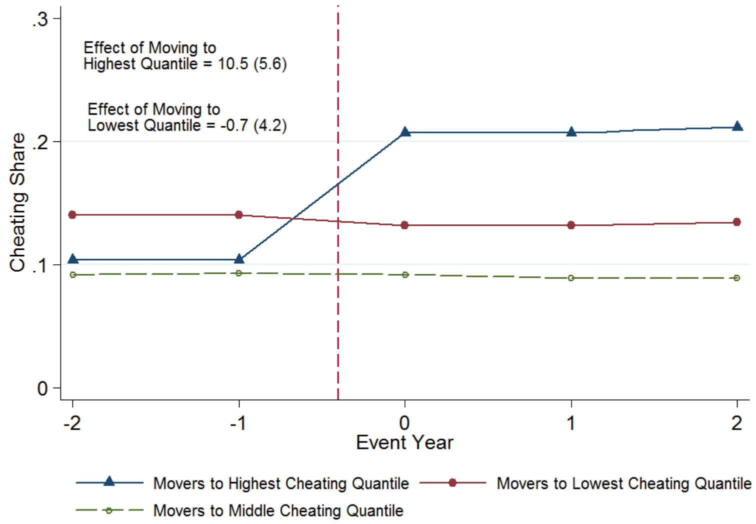
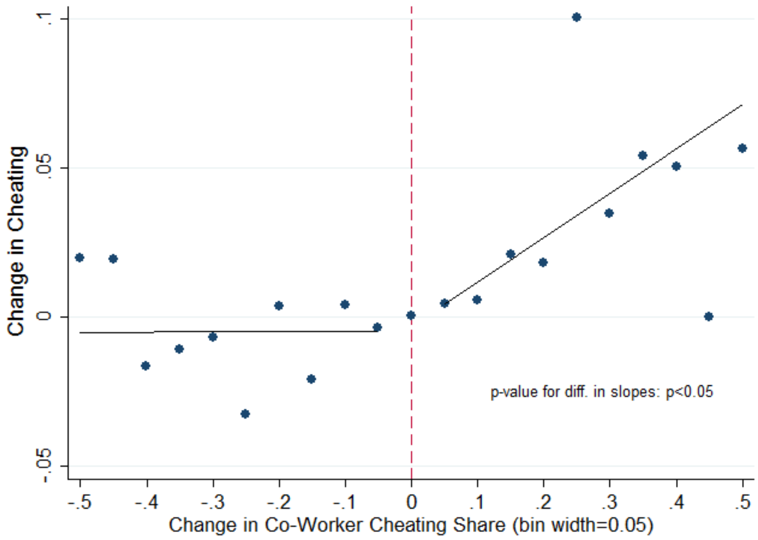


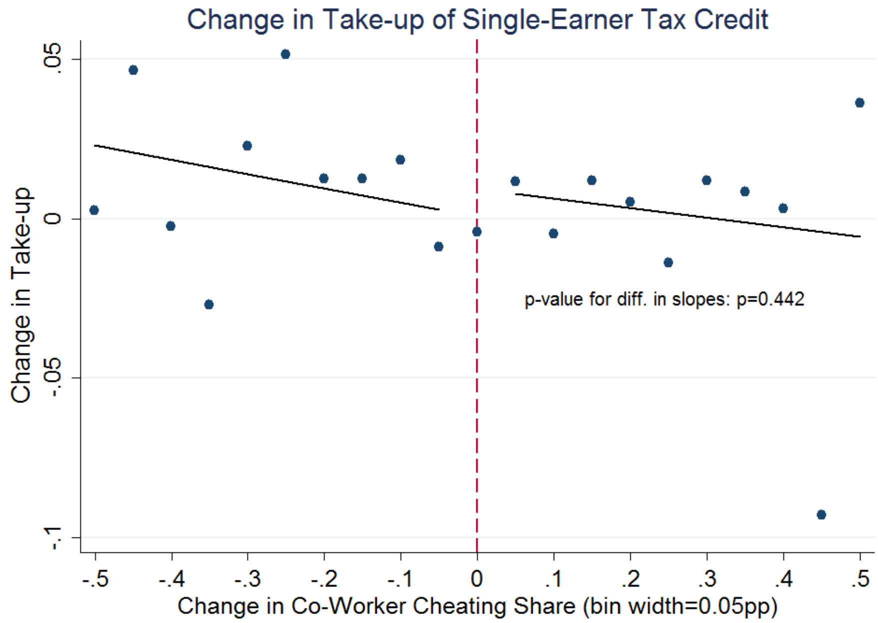
(a) Panel A



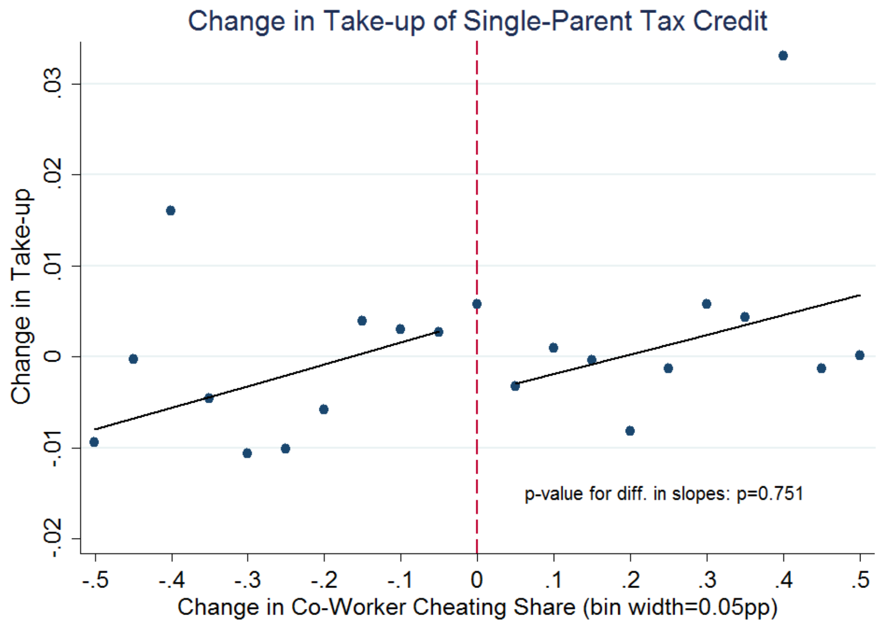
(b) Panel B

Figure A. 3: Impact of changing to firms with lower vs. higher cheating shares (constant zip-codes)

Notes: This figure replicates Figures 4 and 5 of the main section, using a sample of job changers with constant zip-codes (and hence, distances). Panel A displays an event study of cheating around the year of the job change. For both the old and the new employer, we calculate the share of cheating co-workers in the year before the job change occurs. We then divide the sample into five quantiles of co-worker cheating shares prior to the job change. From this, we plot an event study of individuals who move from the 3rd quantile to the 1st, 3rd, and 5th quantiles of co-worker cheating shares. The coefficients and standard errors are computed by difference-in-difference estimations comparing changes from year -1 to 0 for job changers to the 5th or 1st quantile with changes for those moving to the 3rd quantile. Panel B plots changes in cheating behavior from the year before the job change (event year -1) to the year after the job change (event year 0) versus the change in co-worker cheating shares across the old and new employer. We group individuals into 0.05 percentage point-wide bins on changes in co-worker cheating shares (the x-axis) and then plot the change in mean cheating within each bin (the y-axis). We fit separate linear control functions to the points on the left and right of the vertical line, weighted by the number of observations in each bin.



(a) Panel A



(b) Panel B

Figure A. 4: Impact of changing to firms with lower versus higher (commuter) cheating share on other self-reported tax items

Notes: Panel A plots changes in take-up rates of the single earner tax credit from the year before the job change (event year -1) to the year after the job change (event year 0) versus the change in co-worker cheating shares (regarding the commuter allowance) across the old and new employer (panel B in the case of the single-parent tax credit). To construct both panels, we group individuals into 0.05 percentage point-wide bins on changes in co-worker cheating shares (the x-axis) and then plot the change in mean take-up rates within each bin (the y-axis). We fit separate linear control functions to the points on the left and right of the vertical line, weighted by the number of observations in each bin.