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Taking the high road? Compliance with commuter tax allowances and the role of evasion spillovers☆

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ABSTRACT

This paper provides evidence of evasion in the context of a widely used commuter tax allowance, and explores evasion spillovers as a determinant of the individual compliance decision. For this purpose, we exploit discontinuities in the commuter allowance scheme and employ a research design resting on a large panel of individual tax returns. We find that around 30% of all allowance claims are overstated and, consistent with deliberate tax evasion, we observe sharp reactions of taxpayers to thresholds where the allowance discretely jumps to a higher amount. Further, we use variation in job changes to uncover spillover effects from the work environment on the individual compliance decision. These effects appear to be asymmetric: Job changers moving to companies with a higher fraction of cheaters increase their cheating. In contrast, movers to companies with a lower fraction of cheaters tend not to alter their reporting behavior. We provide suggestive evidence that the spillover has more to do with an information environment, but can ultimately not reject other behavioral explanations such as asymmetric persistence of norms.

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, e.g., Slemrod, 2007 for an overview). 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.

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 self-reported income of around 40%). This paper aims to add one explanation to this observation showing that the individual evasion decision is influenced by the compliance behavior at one's work environment. In particular, our results suggest the existence of evasion spillovers, 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 (e.g., Pomeranz, 2015; Rincke and Traxler, 2011), evidence of complementary evasion spillovers regarding opportunities to cheat is limited to either lab experiments (e.g., Fortin et al., 2007) or some aggregated social externalities (Galbiati and Zanella, 2012). Our paper is the first to

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provide field evidence that evasion spillovers might indeed influence the individual compliance decision.

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. 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. 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 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 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 each taxpayer received as a commuter. It also contains the employer's location and the commuter residence on zip-code level, which allows us to approximate the real driving distance between both locations and the hypothetical commuter allowance the taxpayer should have received. A comparison between the hypothetical allowance and the claimed allowance reveals who misreports the commuter allowance. 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.[1]

Our results show that tax evasion via self-reported commuter allowances is substantial. We find that around 30% 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% 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 that allows us to observe not only the compliance decision of a single taxpayer but also the compliance behavior of his colleagues at 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 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] Studying the switching behavior of job movers also accounts for recent research suggesting that optimization frictions such as inattention can lead to sluggish behavioral adjustments (see, e.g., Chetty, 2012). Since moving to a new employer forces taxpayers to reconsider their compliance decision when reporting their new allowance eligibility, it presents a valuable situation to study potential behavioral responses to a new environment.

Turning toward the results obtained from our sample of job movers, 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 to a firm with a higher fraction of cheaters start overreporting much more after they move. In contrast, those who move to firms with a lower fraction of cheaters tend not to change their reporting behavior. This asymmetry in the effect of the job change rejects explanations based on sole firm-level mechanical effects, such as some firms thoroughly screening the commuter allowance claims of their employees while other firm do not. In fact, substantial 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 expect changes in overreporting to move alongside with changes in co-worker cheating shares.

The existence of such asymmetric effects of job moves is consistent with models based on the asymmetric persistence of norms as well as with models based on information, memory and learning. The question which of the these models are more important in our context is difficult to be settled here. To make progress, we study take-up rates of two other (quasi-) self-reported deduction items employees can file at the firm level. Our results show that job changers who move to high-cheating firms (in terms of the commuter allowance) do not show a higher propensity to start filing for the two other items. In fact, it seems that the impact of a high-cheating work environment is contained to the very item of the commuter tax allowance. While this suggests that a broader corruption of norms may not sufficiently explain the evasion spillover we observe with respect to the commuter allowance, we interpret this result as informative but not conclusive.

Our study adds to the fairly slim literature on tax evasion via personal allowances and provides implications for the design of optimal tax collecting policies. To our knowledge, our paper is the first to provide evidence of evasion in the context of commuter tax allowances – a deduction item available in many countries.[3] Furthermore, our findings corroborate research showing that the responsiveness of taxpayers regarding itemized deductions is sensitive to the design and enforcement of the respective policy item (e.g., Fack and Landais, 2016). Related to this, the case of the Austrian commuter allowance nicely demonstrates the deficiencies of a poorly designed tax (allowance) scheme comprising sharp discontinuities in the deductible amount and an eligibility criteria that is very difficult to verify for the government. Finally, we think that our research design looking at asymmetric effects on the behavior of subgroups of taxpayers (in our case job changers) can also contribute methodologically to a wider body of the compliance literature. It

[1] We complement our findings using exact residence and workplace addresses of about 3500 commuters of a large Austrian retailer. Using this data, we can confirm our findings from the population tax data (see Appendices A.4 and A.5).

[2] Since taxpayers can actually circumvent the employer via filing for the 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.

[3] Compensation for travel expenses are sometimes included in general work-related deductions (e.g., France or Italy), designed as a single allowance for commuters (e.g., Germany, Netherlands, Denmark) or come in the form of tax-free benefits paid by the employer (e.g. in the U.S.).

Table 1
Commuter allowances in the Austrian tax code (EUR in 2012).

	Public transport	
Allowance bracket	Available (minor scheme)	Not available (major scheme)
2 –20 km	–	372
20–40 km	696	1476
40–60 km	1356	2568
More than 60 km	2016	3672

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.

relates to an emerging strand of research that examines ‘traces of evasion’ (Slemrod and Weber, 2012) by presenting 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 Section 2, we present details on the Austrian commuter tax allowance and the dataset used to detect tax evasion. Section 3 illustrates how driving distances to work are systematically misreported when taxpayers claim their commuter allowance. We also show tentative regression results to examine the individual compliance decisions. In Section 4, we test for the causal effect of the work environment on the evasion decision using a subsample of individuals moving between employers. Finally, Section 5 concludes.

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. The commuter tax allowance is the biggest of these allowances, enabling employees to reduce taxable income by as much as EUR 3672 per year (for 2012).[4] It is designed to encourage workers to take up jobs even when the workplace 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 in place, and a major scheme if not. Hence, employees who commute less than 20 km to their workplace are only eligible for the allowance when public transport is not available.[5]

To receive the commuter allowance via the payslip, employees report their eligibility for one of the four brackets 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.[6] 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 much 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, keeping the risk of detection low. 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 misreporting by commuting taxpayers, we calculate driving distances between the zip-codes of an employee and her firm by employing 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. Austria comprises 2208 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 use this proxy of the true commuting distance to determine the corresponding (hypothetical) commuter allowance bracket the taxpayer would have been entitled to. We can then detect misreporting by comparing this hypothetical allowance bracket with the one the taxpayer has actually claimed.[7] We classify taxpayers as overreporters (i.e. cheaters) when they received a higher allowance than what they would have been entitled to, and as underreporters when they claimed too little of what they actually should have received.

[4] Given a progressive income tax schedule with a top tax rate of 50% (for incomes above EUR 60,000), the maximum amount of tax reduction is equal to EUR 1836.

[5] It should be noted that the commuter allowance and its distance brackets were unchanged since the introduction in 1988, creating constant and exogenous discontinuities taxpayers can respond to.

[6] Besides lack of incentive and lack of deterrence for employers to double-check the claims of their employees, it should be emphasized that during the period of our study, online route planners such as Google Maps were still at their infancy and not very common. This made it rather complicated and time-consuming for most employers to validate the actual driving distances of the employees.

[7] Notice that taxpayers only report eligibility to one of the four allowance brackets on the payslip but not exact distances.

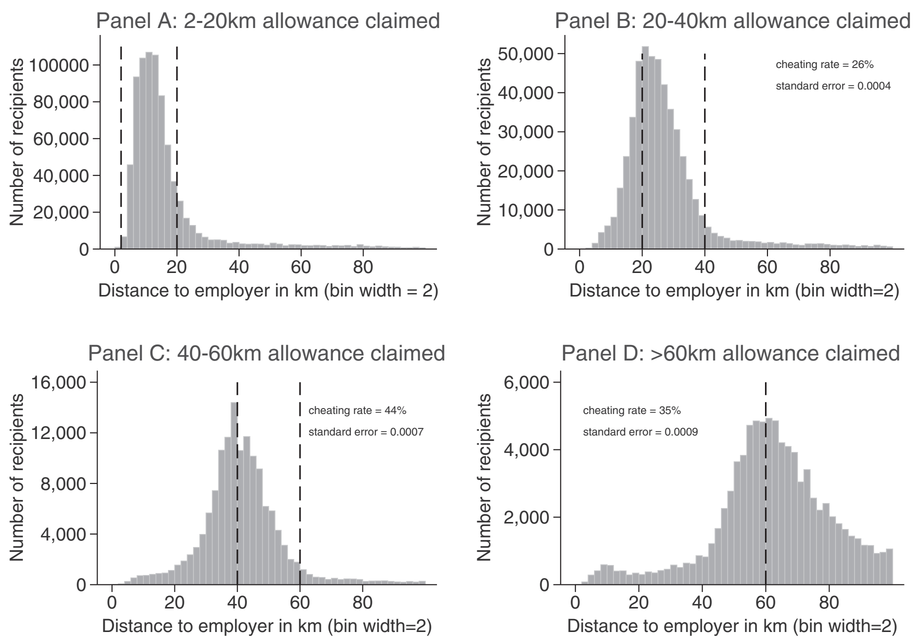

Fig. 1. Allowance claimed and actual distance to employer (by bracket). *Notes*: The figure displays the histogram of actual distance to employer (by 2 km bins) by allowance brackets. The histograms include allowance recipients for all years 1995–2005. Distance to employer is the driving route as calculated 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.

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% 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% 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% 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 workplace for these users of public transport, we exclude them from the subsequent analysis and only focus on recipients of the major scheme. This leaves us with a sample of around 1.9 million observations (or about 500,000 taxpayers).

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 between 1995 and 2005. Fig. 1 reports the histograms for actual distance-to-work (by 2 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 workplace when they filed their claim correctly.

Two features are worth noting in Fig. 1. First, panels B to D show that there are many commuters clustering well to the left of each lower bound.[8] These commuters reside closer to their workplace than what they claim and thus, overreport the allowance bracket. Second, for panels A to C we find some recipients commuting longer to work than what they actually claim. Such underreporting could be either due to people not paying close attention to their eligibility based on the driving distance, or due to some noise in our distance measure between the zip-codes of an employee's residency and the employer's location. Although the asymmetric distribution of commuters around the lower versus the upper bound of each bracket suggests that employees tend to overreport their commuting distance, Fig. 1 does not reveal whether individuals actually target their distance reporting to 'game' the specific thresholds created by the tax law. To separate between noise in our distance measure and the systematic overreporting by commuters we now want to look at discontinuities in misreporting around the bracket thresholds. For this purpose, we pool data across all brackets and display the fraction of misreporters (defined as commuters who either over- or underreport their driving distance) by bins of distance to their employers. In the case that individuals actually target their reporting to take advantage of the bracket structure of the commuter allowance, we should observe a discontinuity in the fraction of misreporters at the thresholds: On the one side of the threshold it will be cheaters and noise, while on the other side of the threshold it will be just noise (plus some commuters who do not pay close attention to the bracket structure of the allowance). Panel A of Fig. 2 displays the fraction of misreporters by 2 km wide bins of commuting distance. The dashed lines indicate the thresholds where the allowance discretely increases to a higher amount.

8 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.

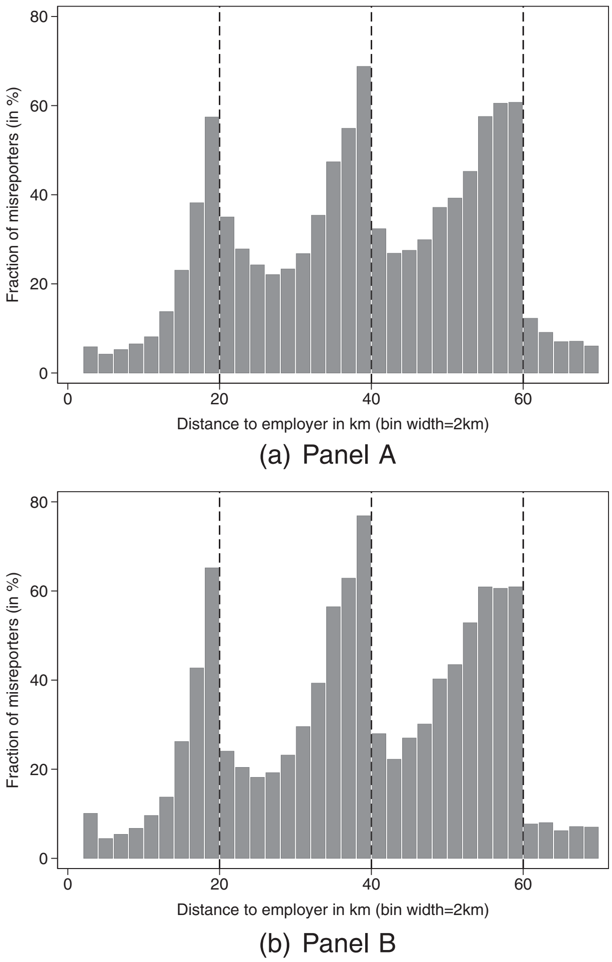

Fig. 2. Distance to bracket and misreporting. *Notes*: The figure displays the reporting behavior of commuters by their distance to the workplace (bin width = 2 km). Panel A displays the whole sample, while panel B excludes commuters who reside or work in zip-code areas larger than the average surface area of Austrian zip-codes (<40 km^2 as the cut-off point). The bars show the fraction of misreporters 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 histogram includes allowance recipients for all years between 1995 and 2005.

We observe a sharp reaction of taxpayers to these thresholds. The closer commuters live to a respective bracket the more prone they are to misreport the allowance claim. Overall, more than 60% of the individuals closest to the brackets misreport the allowance bracket they are eligible for. At each threshold, the fraction of misreporting falls discontinuously, indicating that taxpayers are aware of the allowance scheme's structure and its incentive to overreport at the margin. Note that misreporting becomes fairly small beyond the 60 km threshold. Every misreporter beyond this point must be an underreporter since cheating is by the structure of the allowance scheme no more possible when residing more than 60 km away from the workplace (correspondingly, every misreporter below the 20 km threshold is a cheater since underreporting is not possible when residing less than 20 km away from the workplace). In sum, the pattern we observe shows the importance of proximity to the next higher bracket for the reporting decision, which is consistent with recent evidence suggesting behavioral responses ('bunching') at salient discontinuities of a tax code (see, e.g., Saez, 2010).

We are aware that the approximation of true commuting distances using zip-code centroids produces some noise when inferring the hypothetical allowance bracket and classifying commuters as

Table 3
Share of misreporting by bracket and zip-code size.

Allowance bracket	Observations	Underreporter in %	Overreporter in %
A. All zip-codes			
2–20 km	1,073,045	10.9	–
20–40 km	568,621	4.9	26.1
40–60 km	160,646	5.8	43.7
More than 60 km	115,378	–	35.1
B. Excluding zip-codes above average surface area (>40 km^2)			
2 –20 km	441,532	6.4	–
20–40 km	206,952	4.2	30.7
40–60 km	59,870	5.8	48.1
More than 60 km	43,560	–	38.8

Notes: Number of observations is 1,917,690 for the full sample (see Table 2).

misreporters.[9] However, the pattern of asymmetric misreporting around the bracket thresholds is difficult to reconcile with sole measurement error in our distance measure. Building on the plausible assumption that noise in our distance measure increases with the size of the zip-code area, panel B of Fig. 2 displays only commuters who reside and work in zip-codes with a surface area below average of 40 km^2 as the cut-off point. Using this smaller sample of commuters (around 40% of the overall population) we find the same asymmetric pattern of misreporting, with even sharper discontinuities this time. Furthermore, we complement our findings with case study evidence using exact residence and workplace addresses of a large Austrian retailer, which reveals a very similar pattern of misreporting. Finally, we propose a way to use the retailer data to correct Fig. 2 of the noise introduced by the zip-code centroids (see Appendices A.4 and A.5).

Table 3 summarizes the share of over- and underreporters by each allowance bracket. Panel A of the table displays misreporting using all zip-codes. The cheating shares for the single brackets add up to 26%, 44% and 35%, respectively. Underreporting shares vary by bracket between 5 and 11%. When excluding commuters who reside or work in zip-codes with a surface area above average (40 km^2 as the cut-off point), the difference between under- and overreporting becomes even larger (panel B of Table 3). In the subsequent analysis, we account for this pattern (i) by controlling for the size of the zip-code area, and (ii) by reporting estimation results for the full sample and for commuters who live and work in zip-code areas below average size.

In sum, Fig. 2 and Table 3 indicate that tax evasion via self-reported commuter allowances is substantial, and, consistent with deliberate tax evasion, we observe misreporting to be much more widespread on the 'tax-favourable' side of each bracket threshold. Focusing on a sample of taxpayers from smaller zip-code areas where noise in our distance measure is arguably smaller, we continue to find sharp differences between over- and underreporting. While it is striking how overreporting increases when the distance to the next bracket threshold 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. In the following, we make use of the richness of our data to address this variation in cheating. 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 at the workplace. The latter turns out to be highly correlated with

[9] An additional source of noise could be that we only have information about driving distances between zip-codes at one point in time (2010), and Austria's road network might has improved between 1995 and 2010.

Table 4
Estimation results (linear probability model).

Dependent variable: Indicator variable with entry one if a taxpayer overreports distance to work	Model A	Model B	Model C	
			Model C.1	Model C.2
Age	−0.002***	−0.002***	−0.001***	−0.001***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Gender (1: Female)	−0.006*	−0.009***	−0.011***	−0.010***
	(0.003)	(0.003)	(0.002)	(0.002)
Education (1: Tertiary education)	0.040***	0.045***	0.042***	0.052***
	(0.012)	(0.011)	(0.009)	(0.014)
Employee status (1: White-collar worker)	0.007	0.017***	0.017***	0.015***
	(0.005)	(0.004)	(0.002)	(0.003)
Nationality (1: Non-native)	−0.001	0.001	0.0004	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Gross income (log)	0.044***	0.037***	0.026***	0.026***
	(0.005)	(0.005)	(0.004)	(0.005)
Marginal income tax rate	0.065**	0.052**	0.037**	0.044*
	(0.026)	(0.023)	(0.018)	(0.023)
Distance to bracket <2 km		0.518***	0.499***	0.525***
		(0.008)	(0.007)	(0.010)
Distance to bracket ≥2 and <5 km		0.319***	0.307***	0.328***
		(0.006)	(0.006)	(0.007)
Distance to bracket ≥5 and <10 km		0.120***	0.113***	0.118***
		(0.003)	(0.003)	(0.004)
Zip-code size (combined)		−0.0002***	−0.0002***	−0.0003***
		(0.0000)	(0.00002)	(0.0001)
Firmsize			−0.001	−0.004***
			(0.001)	(0.002)
Co-worker cheater share			0.350***	0.352***
			(0.008)	(0.011)
R^2	0.021	0.216	0.246	0.261
Fixed effects (*p*-value)				
Regions [8]	0.000	0.000	0.000	0.000
Industries [16]	0.000	0.000	0.000	0.000
Years [10]	0.000	0.000	0.001	0.035

Notes: 1,534,774 observations in Models A, B and C.1 (covering all zip-codes). 947,381 observations in Model C.2 (covering only zip-codes with a surface area below the average zip-code size, i.e., 40 km^2). Base category for distance-to-bracket dummies: Distance of more than 10 km. Firmsize: 1 for less than or equal 10 employees, 2 for 10 to 50 employees, 3 for more than or equal 50 employees. Constant not reported. Firm clustered standard errors are in parentheses, degrees of freedom are in square brackets.

* Indicates significance at 10%-level.

** Indicates significance at 5%-level.

*** Indicates significance at 1%-level.

the individual compliance 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 non-compliance: tentative regression analysis

To investigate whether a taxpayer's compliance decision can be explained by individual and other characteristics, we estimate a linear probability model, where the conditional probability to overreport is explained by $p_{it} \equiv P(c_{it} = 1|\mathbf{x}_{it}) = \mathbf{x}_{it}'\beta$. c_{it} is an indicator variable with entry one if taxpayer i is overreporting at year t, and zero else. Depending on the covariates $\mathbf{x}_{it}$ included to estimate p_{it}, we distinguish between three alternative specifications: Model A incorporates personal characteristics (i.e., age, gender, educational status, nationality and employee status). Further, we add the (log of) taxpayers' gross income and the corresponding marginal income tax rate. In Model B, we account for our descriptive Fig. 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 addition, we control for the size of the zip-code area. In Model C, we add firm-specific information on firmsize. Further, we account for the share of cheating co-workers at the workplace. In particular, the co-worker cheating share w of an individual i at year t is calculated as

$$w_{it} = \frac{1}{N_{kt}} \sum_{j \neq i} p_{jkt} \tag{1}$$

where p_{jkt} is the cheating decision of the co-worker j at firm k at year t. N_{kt} denotes the number of co-workers at firm k in t. Hence, when calculating the co-worker cheating share of an individual i at time t, we omit the cheating decision of the individual being studied (i.e., using the 'leave-out cheater share').[10] In all models, we include fixed effects for regions (the nine provinces of Austria), industries and years. Model C is estimated for (i) the full sample including all zip-codes (Model C.1), and (ii) a subsample excluding commuters

[10] 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 4. 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 et al., 2013).

who live or work in zip-codes with a surface area above the average zip-code size, i.e., 40 km^2 (Model C.2).

Table 4 summarizes our estimation results. Generally, and in line with Kleven et al. (2011), we find that most individual-specific variables are either insignificant or almost negligible in magnitude. Income and the marginal income tax rate enter significantly positive and seem more important than the other individual-specific characteristics. In Model B, we can see that the distance-to-bracket dummies exhibit substantial and significantly positive coefficients that increase with the proximity to the next allowance bracket, as expected. Firmsize seems to have a negative (Model C.2) but not a strong impact on a taxpayer's cheating behavior. Perhaps most importantly, Model C shows that a firm's share of cheating co-workers exhibits a significant and positive effect on the individual compliance decision.

However, the significant impact of cheating co-workers on the individual reporting behavior does not allow for a causal interpretation of the relationship between the work environment and the individual evasion decision. To establish a causal link between the work environment and individual responses to marginal incentives, we follow Chetty et al. (2013) and employ a subsample of job changers to track individuals as they move between employers with different shares of cheating co-workers.[11] 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 enables 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. Focusing on job moves eliminates the 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 effect from the work environment.[12]

The second advantage of the job mover sample relates to recent research on optimization frictions and the sluggish adjustment of economic decisions (e.g. Chetty, 2012). Since moving to a new employer necessarily results in a new payslip, a job move forces employees to reconsider their compliance decision. In fact, we observe only few commuters deviating from the allowance claim of the previous year when not experiencing a change of the employer or a change in residence. Although this inertia in claiming the allowance might be seen as an argument against the existence of potential spillovers from the work environment, such effects seem rather plausible when considering the institutional setting of the commuter tax allowance. For instance, it might prove difficult for permanent workers to ask the employer for a higher allowance bracket without reporting any change in the residence address and hence, without reporting any change in the commuting distance to work. From the employers' perspective, these commuters will easily be perceived as cheaters trying to claim a higher allowance than actually entitled to. Probably even more important, any upgrade in the allowance bracket from one fiscal year to another without reporting a change in the residence or employer address might cause suspicion in the case of a company audit. In contrast, individuals who change jobs do not face those restrictions since a change in the employer can more easily be used to claim a change in the commuting distance, offering job movers an opportunity to deviate from their previous reporting decision.

While in other situations job moves may not present a useful source for identification due to selection issues, we argue that in our context job changes can be seen as exogenous regarding the compliance decision in two important aspects: First, it seems reasonable to assume that in the decision of what employer to choose, other aspects than the possibility to cheat on allowances dominate. Second, employees willing to evade can easily bypass companies that potentially inhibit them to cheat by filing and claiming the commuter allowance through the tax return at the end of the year. Hence, there is no need for job movers to sort themselves into certain companies that allow them to cheat. Unfortunately, we do not have information regarding commuters who file for the allowance via the tax return, who comprise only around 20% of all commuter allowance recipients (see Statistik Austria, 2009). However, we can check whether previous cheaters systematically discontinue payslip filing when moving to a *low* evasion environment in order to circumvent the employer. Fig. A.2 in the Appendix plots termination of payslip filing against co-worker cheating shares, where the slope has, if anything, the opposite sign.

Finally, we want to derive testable predictions about the behavior of job changers in order to explore the potential nature of the evasion spillover. In the spirit of Chetty et al. (2013), we hypothesise that models based on information, memory and learning predict an *asymmetric* effect of the job change on compliance behavior: Individuals who change to a firm with a higher cheating share should learn from their new work environment that evasion via commuter allowances is easily done. In this case, we expect taxpayers to start overreporting the commuter allowance. The second is *memory*: Individuals who move to a firm with a lower fraction of cheaters should not change their cheating behavior and continue overreporting, since they know that misreporting is almost without consequences. Furthermore, testing for asymmetry of effects allows us to separate behavioral-based explanations from explanations based on sole firm-level mechanical effects, such as some firms or (HR departments) thoroughly screening the commuter allowance claims of their employees while other firms do not. In fact, substantial 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. For instance, 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. Finally, models based on the notion of asymmetric persistence of norms would predict asymmetric effects of job moves similar to information-based explanations. We are not in a position to rule out those models here definitely, but we test for the presence of asymmetric persistence of norms by studying evasion spillovers of two other (quasi-)self-reported items in the Austrian tax code (see Appendix A.3).

In order to isolate the effect of a change of the co-worker cheating share on the compliance behavior of the new recruit, we aim to keep other factors influencing the reporting decision as constant as possible over the course of the job change. Thus, we focus on individuals who switch jobs within the same firm zip-code area, eliminating effects stemming from a change in commuting distance and ensuring

[11] Unlike Chetty et al. (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.

[12] It should be noted that our tax data is only available at an annual basis and, therefore, we do not observe the exact day when employees start filing the allowance at the new firm. Hence, we have to be agnostic about the question whether filing as well as the potential spillover occurs when the new recruit arrives at the new job or after some weeks. This means that we include HR staff when speaking about co-workers: For instance, it may be that some of the spillover stems from HR asking the right questions, etc. In this respect, it is important to note that the commuter allowance can be filed retroactively during a fiscal year. This guarantees that there is no need for job changers to file an allowance in the very beginning of the new job.

Table 5
Summary statistics of job mover sample vs. full sample.

	Job mover sample	Full sample
A. Individual characteristics		
Age	39.38	37.61
Female (%)	28.73	28.98
Tertiary education (%)	2.21	2.08
White-collar worker (%)	54.40	49.18
Non-native worker (%)	14.13	15.57
Gross income (Tsd. EUR)	33.03	30.60
Distance (km)	39.22	41.87
Distance to bracket (km)	11.31	11.21
Observations	*18,772*	*1,534,902*
B. Firm characteristics		
Age	36.87	36.35
Female (%)	17.45	18.45
Tertiary education (%)	0.45	0.49
White-collar worker (%)	44.34	38.96
Non-native worker (%)	4.68	7.01
Gross income level (Tsd. EUR)	29.52	26.06
Distance (km)	34.94	33.42
Distance to bracket (km)	12.89	13.32
Firm size >10 employees (%)	78.06	65.75
Observations	*4098*	*148,887*

that the route stays the same (i.e., the route GIS calculates between two zip-code centroids). Further, we exclude individuals who move residence and employer at the same time to wipe out effects caused by a change in residence on the reporting behavior. Hence, we aim to make sure that job movers face equivalent incentives to overreport before *and* after their job change.[13] This gives us a sample of about 18,700 job changers.

Table 5 provides descriptive statistics of both the job mover sample and the full sample (as used for the regressions of Table 4) for comparison. Panel A indicates that job changers have, on average, slightly higher gross wages, consistent with studies on wage premiums of (volunteer) job moves (Topel and Ward, 1992). Individuals in our job changer sample also tend to be more white-collar compared to the full sample, but are very similar in terms of other socio-demographic characteristics such as age, sex, nationality or education. Most importantly, the commuting distance as well as the proximity to the next higher allowance bracket is equivalent for the sample of job movers and the full sample.

In panel B of Table 5, we show descriptive statistics of the new firm job changers move to. We find slight differences in income levels between the firms in our job changer sample compared to the overall sample. Firms in our job changer sample also tend to be slightly bigger, more white-collar, and native worker dominated. The mean commuting distance of employees from both samples is equivalent and they also share the same compositions of their workforce regarding other socio-demographic characteristics, such as age, education or sex ratio. In sum, firms and individuals in our job mover sample are very similar in their characteristics when compared to those from the full sample.

4.2. Event study on reporting behavior of job movers

To test for the presence and potential 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. For both the old and the new employer, we observe the share of cheating co-workers as defined in Eq. (1) in the years before and after the job change. From this, we divide our sample into three terciles of cheating shares in the year before an individual changes the job and track their reporting behavior as they move to different terciles of co-worker cheating shares. We focus on employees in the middle tercile of cheating co-workers prior to the job change and divide them into three groups: job changers to the lowest, to the middle, and to the highest co-worker cheating tercile.

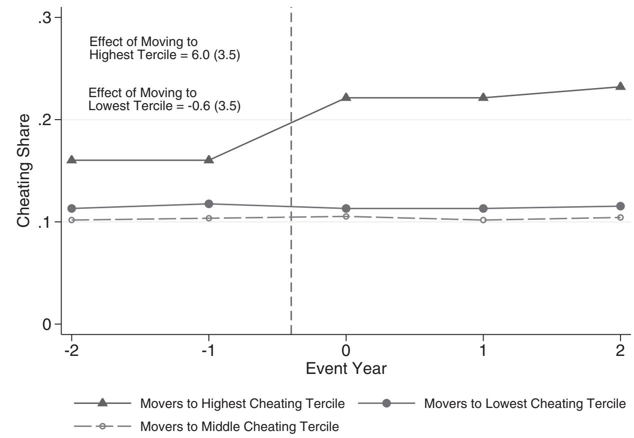

Fig. 3. Impact of changing to firms with lower versus higher cheating shares. *Notes*: The figure is based on the sample of job changers. Event time is defined as the calendar year minus the first year after the job change, so year 0 is the first year in which the individual claims the commuter allowance at the new workplace. 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 three terciles of co-worker cheating shares prior to the job change. From this, we plot an event study of individuals who move from the 2nd tercile to the 1st, 2nd, and 3rd tercile 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 3rd or 1st tercile with changes for those moving to the 2nd tercile.

Fig. 3 plots the cheating behavior of the job changers around the year of the job move. We can see that job changers moving to the 3rd cheating tercile exert a change in their reporting, whereas the cheating behavior of individuals moving to the 2nd or 1st tercile is unaffected by the job move.[14] To test for the magnitude and significance of this effect we apply a difference-in-difference approach, regressing the binary cheater variable on a dummy variable for moving to the highest tercile, one for the event year, and an interaction term between these two dummy variables. We limit the regression to the middle and the highest tercile and to event years −1 and 0, so that the interaction term captures the effect of changing to a firm in the 3rd tercile relative to one in the 2nd tercile. The same setup is used to estimate the effect of changing to the lowest tercile, again using the middle tercile as the control group. The results show a significant increase of 6 percentage points in cheating for job changers moving to the highest tercile of cheating co-workers. In contrast, moving to the lowest tercile results in a very small and insignificant change of cheating of around 0.6 percentage points.

Fig. 4 aims to test for effect of increases versus decreases in co-worker cheating shares more directly. This time, we calculate

[13] Using only job moves within the same company zip-code also controls for potential differences in the strictness of monitoring by tax authorities which we cannot observe. Since tax enforcement is organized on district level and, therefore, does not change within a given zip-code area, using variation within zip-codes rules out regional differences in monitoring as the sole explanation for changes in compliance.

[14] Among the three groups displayed in Fig. 3, we only find (weakly) significant differences in the initial cheating shares between the top- and the middle-tercile groups. With regard to the pre-event slopes, we do not observe any significant differences among all three tercile groups. Similar patterns also has been found in related contributions (see, for example, Figures 4, 5 and the corresponding footnote 41 in Chetty et al. (2012).

the change in co-worker cheating shares between the old and new employer. We construct a variable capturing the change in overreporting from the year before the job change (event year −1) to the year after the job change (event year 0). Fig. 4 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. In the presence of asymmetric spillover effects, there should be a 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. Graphical inspection of Fig. 4 reveals a clear kink at around 0, and the hypothesis that the two slopes are equal is rejected with a p-value smaller than 0.01.

Table 6 presents fully-controlled regressions equivalent to the specification in Fig. 4, including the full set of covariates. Specifically, we estimate the regression

$$\Delta p_i = \alpha + \beta_1 \theta_i + \beta_2 \Delta w_i + \beta_3 \theta_i \Delta w_i + \delta \Delta X_i + \varepsilon_i \tag{2}$$

where Δp_i denotes the change in the binary cheater variable, θ_i denotes an indicator for moving to a firm with a higher cheating share than the previous firm, Δw_i denotes the change in the co-worker cheating share induced by the move, and ΔX_i controls for changes in covariates. The coefficient of interest, β_3, measures a possible kink at the reference point of 0. We use only data from the year before and the year after the firm switch and cluster standard errors by the new firm. Model A presents the baseline results without any controls. The reported estimates show that there is a significant kink in the relationship between the change in individual cheating and the change in the co-worker cheating share at zero. Furthermore, the slope to the right of the kink is significant and positive: A 10 percentage point increase in the share of cheating co-workers increases, on average, the probability of overreporting by about 1 percentage point. On the contrary, a 10 percentage point reduction in the share of cheating co-workers leads to a statistically insignificant change in cheating of 0.02 percentage points. Model B extents the baseline estimation by including the full set of covariates as used in Table 4.

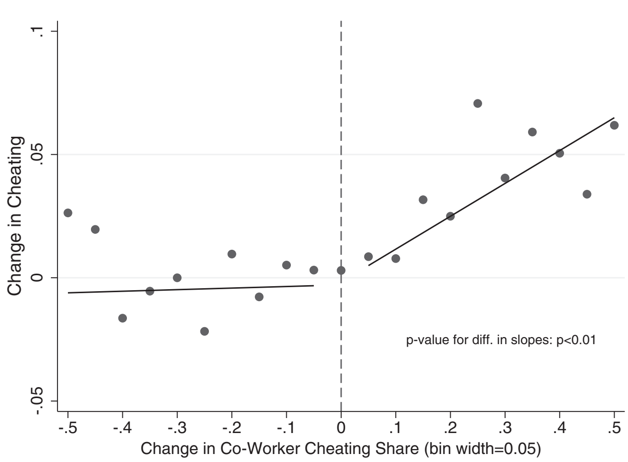

Fig. 4. Asymmetric effects of changes in co-worker cheating shares. *Notes*: The figure plots changes in cheating behavior from the year before the job change (event year −1) to the year after the job change (event year 0) versus the change in co-worker cheating shares across the old and new employer. 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). The solid lines represent best-fit linear regressions estimated on the microdata separately for observations above and below 0.

Table 6
Estimation results (job mover sample).

Dependent variable: Change in cheating decision.			
	Model A	Model B	
		Model B.1	Model B.2
Change in co-worker cheater share	0.018	0.018	0.027
	(0.018)	(0.018)	(0.030)
Positive change in co-worker cheater share (dummy)	−0.001	−0.001	0.002
	(0.005)	(0.003)	(0.004)
Co-worker cheater share × positive change	0.082**	0.081**	0.070**
	(0.032)	(0.032)	(0.030)
Covariates included (Model C of Table 4)	*No*	*Yes*	*Yes*
R^2	0.01	0.02	0.02
Observations	18,712	18,712	6939

Notes: Models B.1 and B.2 include the full set of covariates as used in Model C of Table 4. In Model B.2, we exclude commuters who reside or work in zip-codes with a surface area above average (40 km^2 as the cut-off point). Firm level clustered standard errors are in parentheses.
* Indicates significance at 10%-level.
** Indicates significance at 5%-level.
*** Indicates significance at 1%-level.

The coefficient of the interaction term remains stable and significant. Most of the covariates are either insignificant or only small in magnitude. Focusing on commuters who live and work in zip-code areas with a surface area below the average zip-code size (Model B.2) leaves our empirical results unaffected.

In sum, using event studies of job movers we show evidence indicating the existence of spillover effects from the work environment on the individual compliance decision. Importantly, these effects appear to work asymmetrically — workers increase cheating when moving to firms with a higher share of cheaters, but not the reverse. This asymmetry in the effect of the job change suggests a behavioral mechanism and rejects explanations based on sole firm-level mechanical effects (such as some HR managers or firms thoroughly screen commuter allowance claims while others do not). In fact, substantial firm-specific effects on the reporting behavior would not predict such an asymmetric impact of an individual's previous co-worker cheating share on current behavior.

Finally, Appendix A.3 aims to shed some light on the potential behavioral mechanism underlying the observed asymmetric effect of the job change. As discussed above, asymmetric effects of job moves might be explained by models based on the asymmetric persistence of norms as well as by models based on information, memory and learning. While the results presented in the Appendix suggest that a broader corruption of norms does not sufficiently explain the evasion spillover we observe, we interpret the findings regarding behavioral mechanisms as informative but not conclusive.

5. Conclusion

This paper provides first evidence of evasion responses of taxpayer to commuter tax allowances. 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 and an overall evasion rate of around 30%. In line with previous research, the impact of most socio-demographic variables on evasion is small in comparison to variables that capture the opportunity and incentive to misreport (e.g. Kleven et al., 2011). Further, we make use of rich administrative data to examine the presence of evasion spillovers on the individual compliance decision. 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. Interestingly, we find asymmetric effects of increases versus decreases in co-worker cheating shares when individuals move between companies. This result suggests the existence of evasion spillovers, with individuals becoming more likely to start cheating when being exposed to a more non-compliant environment.

Our study carries important implications for the design of optimal tax collecting strategies. First, our study suggests that third-party reporting is not always a panacea against tax evasion. In the spirit of the third-party model proposed by Kleven et al. (2016), our results underpin that employers need the means as well as the incentive to correctly record tax-relevant items of their workers to be a reliable partner to the government. Specifically, Kleven et al. (2016) suggest that when it comes to salaries and wages, companies have sufficient incentive to keep accurate business records in order to enhance productivity. They conclude that it is the existence of such business records evidence that makes third-party tax enforcement so successful. In contrast, companies lack such an added value of exact recording on productivity in the realm of the commuter allowance, further aggravated by the poor design of the allowance scheme and lenient enforcement of the authorities. This combination of lack of enforcement and lack of incentive leads to a dysfunctional system of third-party reporting. Furthermore, our example of the Austrian commuter allowance demonstrates the deficiencies of a poorly designed tax (allowance) scheme which combines sharp discontinuities in eligibility rules that are very difficult to monitor for the government. 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 not available.

Appendix

A.1. Split misreporting into over- and underreporting

In this section we present another way of displaying cheating in the tax data. Specifically, we decompose the overall misreporter share of Fig. 2 into a share of overreporters and a share of underreporters. Panel A of Fig. A.1 provides this decomposition for the full sample, panel B for small zip-code areas only (using the average surface area of Austrian zip-codes as a cut-off point). The bars in both panels display the fraction of misreporters by commuting distance. Each bar is broken down between misreporters who overreport (dark area) and misreporters who underreport (light area). For instance, for the distance bin 38–40 km we find about 57% of all commuters overreporting and about 12% underreporting (panel A), leading to an overall misreporting share of about 69%.

As can be seen from Fig. A.1, there is some underreporting taking place, especially right above the bracket thresholds.[15] Nevertheless, it remains evident from Fig. A.1 that overreporting is much more prevalent in the data than underreporting, with sharper discontinuities when focusing on small zip-codes only. In sum, displaying misreporter-shares as in Fig. 2 might still be the clearest way to present evidence of cheating in the raw tax data: In a situation in which taxpayers report their bracket eligibility without any cheating, the noise in our distance measure should produce smooth increases and decreases in misreporting around each threshold and no discontinuity. The fact that we observe a discontinuity in misreporting strongly suggests that taxpayers do target the bracket cutoffs with their reporting to take advantage of the step function of the commuter allowance.

A.2. Termination of payslip filing vs. cheating share at new firm

Fig. A.2 shows the termination of payslip filing after job change vs. co-worker cheating share at new firm.

A.3. Evasion spillovers and potential behavioral mechanisms: asymmetric persistence of norms vs. information

This section aims to explore potential behavioral mechanisms underlying the observed asymmetric effect of the job change. As mentioned in the main text, asymmetric effects of job moves might be explained by models based on the asymmetric persistence of norms as well as by models based on information, memory and learning. To make progress, we propose a testable prediction about the compliance behavior of job changers in case that information trump norms: When information about the easy opportunity to cheat via the commuter allowance is the driver of the evasion spillover, we expect it to be contained to the very item of the commuter allowance. In contrast, if getting to know 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 evaded. Hence, we would expect employees moving to high-cheating companies in terms of the commuter allowance to start searching also for other low-hanging evasion fruits (since they learn that tax evasion is an acceptable habit). 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.[16] 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 know whether employees cheat on these two tax items, since we do not observe their family situation or their partner's income. However, if the transmission of norms operates in an asymmetric way, we still should 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, a 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.

Fig. A.3 is equivalent to Fig. 4, using the same job mover sample.[17] Panel A plots changes in take-up rates for the single-parent tax credit

[15] The reason why we also observe some underreporting right below the bracket thresholds has to do with the administrative nature of our data. The ASSD provides no clear provision whether the employer identifier is used for a firm or for the single establishments of a (bigger) firm. If only the headquarter of a multi-establishment company is recorded in the ASSD, our estimate for the commuting distance between the firm's location and the residence of its employees is upward biased for some workers, inflating the share of underreporters. Restricting our analysis to employer identifier with less than 10 employees (which are most likely single-establishments), underreporting at the top-end of the bracket cutoffs falls by around 50%. Furthermore, using this restriction does not change our main findings, especially the ones in Table 6 (results available upon request).

[16] 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.

[17] Both panels of Fig. A.3 are somewhat noisy due to less observations, caused by missing information for the respective tax item on some payslips.

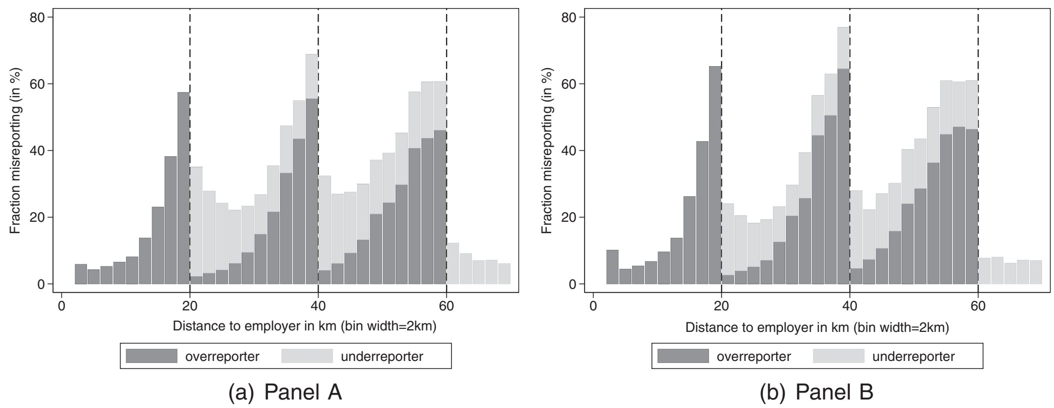

Fig. A.1. Distance to bracket and over- vs. underreporting (tax data). *Notes*: The bars in this figure display the fraction of misreporters by commuting distance (bin width = 2 km). Each bar is broken down between misreporters who overreport (dark area) and misreporters who underreport (light area). For example, the total fraction of misreporters in the 38–40 km bin is 69% (panel A). Out of this 69%, 57% are overreporters while the remaining 12% are underreporters. Panel A depicts the reporting behavior of the entire population in the tax data. Panel B excludes taxpayers who reside or work in zip-code areas larger than the average surface area of Austrian zip-codes (<40 km^2 as the cut-off point). The dashed lines represent the thresholds where the allowance discontinuously increases to a higher amount (at 20, 40, and 60 km, respectively). The histogram comprises all years from 1995 to 2005.

against changes in co-worker cheating shares (in regard to the commuter allowance). Panel B displays changes in take-up rates for the single-earner tax credit. In both panels, we test for the presence of a kink at the reference point of 0 equivalent to Model A of Table 6. For the single-parent tax credit, the null hypothesis of equal slopes cannot be rejected. For the single-parent tax credit, we observe a kink to be significant at the 10% level. However, the direction of the trend break at 0 goes in the opposite direction than a (positive) spillover would predict, with movers to high-cheating companies being less likely to start claiming the single-parent tax credit. Furthermore, for both panels none of the slopes to the left and right are significantly different from zero. In sum, we do not find a significant effect of co-workers evading the commuter allowance on the take-up rates of other tax credits. While this evidence suggests that a broader corruption of norms might not explain the evasion spillover observed with the commuter tax allowance, this finding cannot be seen as conclusive here. For instance, it might be the case that taxpayers view cheating on commuter distances to be normatively quite different from cheating on family statuses. Furthermore, it could be that the existence of the single-earner (or single-parent) tax credit had simply never occurred to our sample of job movers, an argument in line with the importance of tax salience. However, the evidence presented here works against the proposition that the evasion spillover with respect to the commuter allowance is driven by certain firms allowing their employees to cheat, since it is not clear why firms with such a policy should not also encourage their workers to take-up other deductions.

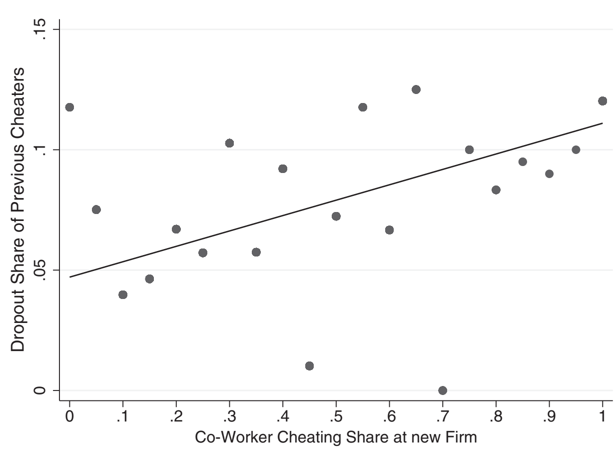

Fig. A.2. Termination of payslip filing after job change vs. co-worker cheating share at new firm. *Notes*: The figure plots the relationship between the share of job changers (focusing on previous cheaters only) that stop payslip filing at the new workplace vs. the co-worker cheating share of the new firm. To construct the figure, we group job changers who overstate their distance prior to the move into 0.05 percentage point-wide bins of the co-worker cheating share at the new firm. We then plot the mean share of job changers who stop 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.

A.4. Using exact addresses from the retailer data to correct misreporter-shares of tax data

We present evidence against the concern that the compliance pattern we observe is driven by some noise in our distance measure. For this purpose, we rely on an additional dataset from a large Austrian retailer chain (in total 40,000 employees) which provides us with exact residence and workplace addresses of almost 5000 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 the employees received via the payslip in 2012. Similar to our population of tax data from the Ministry of Finance, we find that the majority of commuters (3575) receive one of the major allowances, claiming that public transport is not available. Again, we focus on recipients of the major allowance.

First, we apply our distance measure based on zip-code level information (i.e., centroid-based measure) as used for the tax data to define misreporting.[18] Hence, Fig. A.4 is constructed in exactly the same way as Fig. 2. Fig. A.4 shows a very similar pattern of misreporting as found in the tax data, with a discontinuity at each bracket threshold. Further, we plot the number of observations per bin on the second y-axis. It is evident from the graph that we are left with

[18] Note that misreporting is again defined as the fraction of commuters who either over- or underreport the allowance bracket they qualify for.

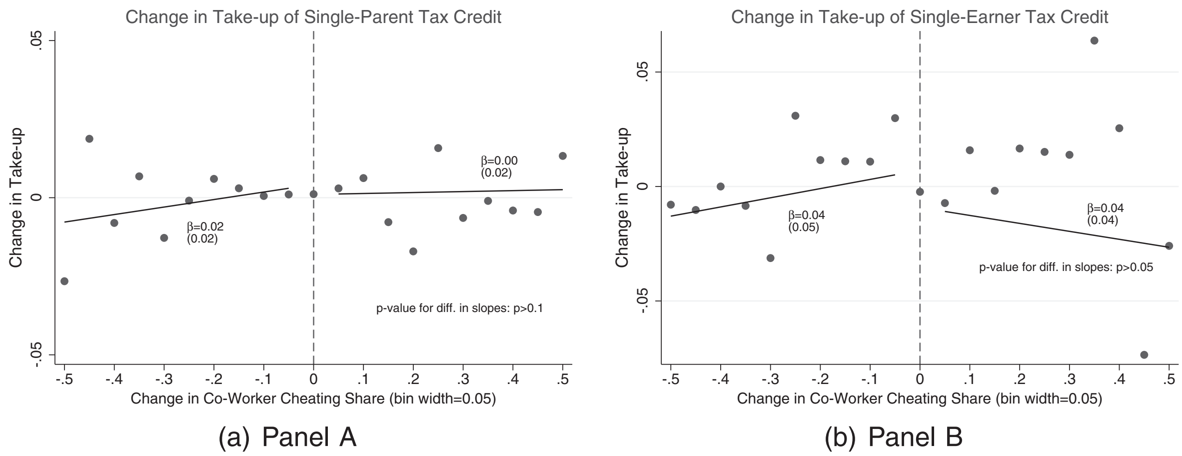

Fig. A.3. Impact of changing to a firm with lower versus higher (commuter) cheating share on other self-reported tax items. *Notes*: Panel A (B) plots changes in take-up rates of the single-parent (single-earner) tax credit from the year before the job change to the year after the job change (event year 0) versus the change in co-workers cheating on the commuter tax allowance. To construct the 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). The solid lines represent best-fit linear regressions estimated on the microdata separately for observations above and below 0.

only a few observations for higher-distance bins, which sometimes results in a complete lack of misreporting in some of these bins. We will come back to the issue below.

Second, we redo Fig. A.4 using driving distances based on the exact door-to-door distance between the employees' residence and the workplace address to define misreporting. Fig. A.5 displays extremely clear discontinuities, echoing the pattern of sharper discontinuities when using only smaller zip-code areas as documented by panel B of Fig. 2. The reason why the discontinuity becomes more pronounced using exact addresses is that the centroid-based distance measure allocates some commuters to the wrong distance-bin, which clouds the actual pattern of misreporting. Hence, the distribution of the tax data displayed in Fig. 2 might be seen as a lower bound of the actual evasion response.

Next, we want to explore the possibility to exploit the retailer data to correct the distribution of the tax data (as displayed in Fig. 2) of the noise introduced by the use of zip-code level information (i.e., centroid-based measure). In other words, we aim to correct Fig. 2 from the main text of the noise generated by the zip-code level information using the retailer data. This exercise is made difficult by the low number of observations in our retailer data. Having about 2200 × 2200 zip-code pairs but only 3575 observations in the retailer data renders a correction on zip-code level impossible.

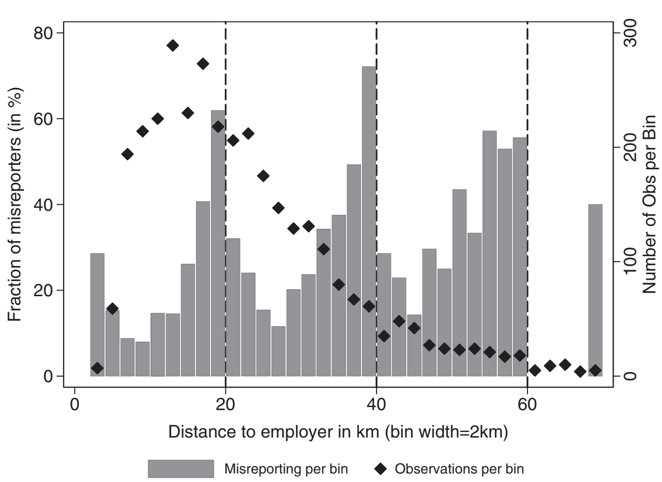

Fig. A.4. Distance to bracket and misreporting (retailer data and centroid-based distance measure). *Notes*: The figure displays the reporting behavior of commuters by distance to the workplace using the retailer data (bin width = 2 km). Distance is computed based on the zip-code level information (i.e. centroid-based measure as used for the tax data). The bars show the fraction of misreporters 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 histogram includes allowance recipients working for the retailer in the year of 2012.

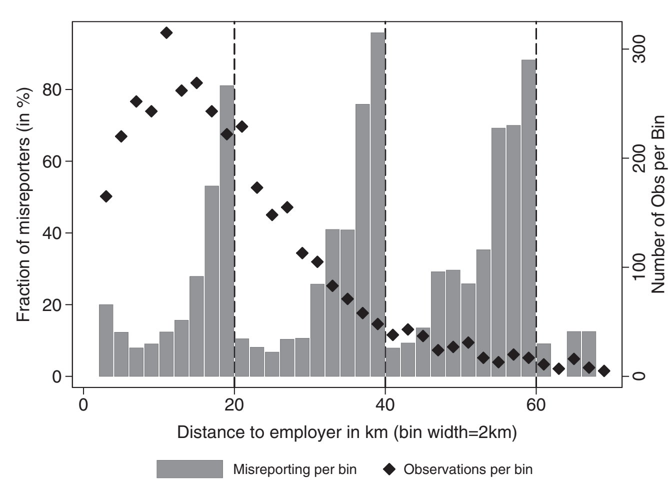

Fig. A.5. Distance to bracket and misreporting (retailer data and exact door-to-door measure). *Notes*: The figure displays the reporting behavior of commuters by distance to the workplace using the retailer data (bin width = 2 km). Distance is computed based on the exact door-to-door distance between the employees' residence and the workplace address. The bars show the fraction of misreporters 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 histogram includes allowance recipients working for the retailer in the year of 2012.

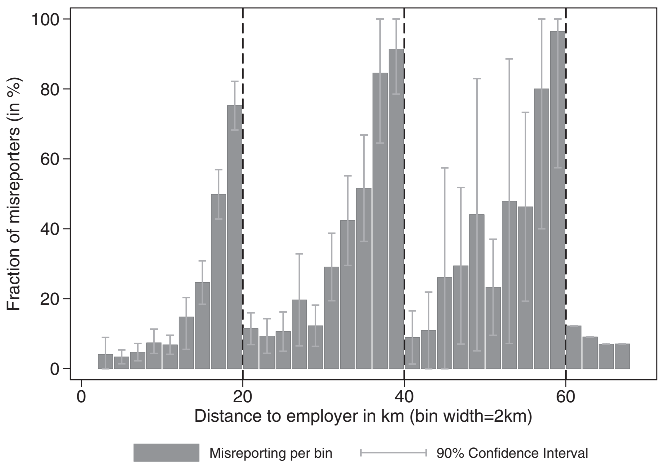

Fig. A.6. Distance to bracket and corrected misreporting (tax data corrected using retailer data). *Notes*: The figure displays corrected misreporter-shares per distance-bin of commuters from the tax data (uncorrected misreporter-shares are displayed in Fig. 2). Misreporter-shares have been corrected using the ratio of the misreporter-share based on the exact distance measure (= door-to-door) to the misreporter-share based on zip-code level information (= centroid-based measure) from the retailer data. The bars show the corrected fraction of misreporters for each bin. Further, we display corrected misreporter-shares using the upper and lower limit of the 90% confidence interval of the ratio. Dashed lines represent the thresholds where the allowance discontinuously increases to a higher amount (at 20, 40, and 60 km, respectively).

Hence, we aim to correct the share of misreporting per distance-bin in the tax data using the retailer information. Specifically, we calculate the ratio of the misreporter-share based on the exact (door-to-door) distance measure to the misreporter-share based on (centroid based) zip-code level information from the retailer data. We calculate this ratio for each distance-bin and then multiply it with the misreporting-share of the same distance-bin of the tax data. More formally, we first use the retailer data to calculate the ratio of the misreporter-share based on the exact distance measure to the misreporter-share based on zip-code level information, i.e.,

$$R_b = \frac{E_b}{Z_b},$$

where the superscript b denotes the distance-bin and E be the misreporter-share based on the exact (door-to-door) distance measure. Z stands for the misreporter-share based on (centroid based) zip-code level information. Notice that R_b is estimated solely from the retailer data. We then use R to correct the misreporter-shares of the tax data, i.e.,

$$C_b = R_b \cdot T_b,$$

where T denotes the uncorrected misreporter shares per distance-bin from the tax data displayed in panel A of Fig. 2. Thus, C_b gives back the corrected misreporter shares per distance-bin of the tax data using the noise estimates from the retailer data.

Fig. A.6 displays the corrected misreporter-shares per distance-bin from the tax data. The corrected misreporter-shares show clear discontinuities at each bracket threshold and are much sharper than the uncorrected shares displayed in Fig. 2. We also calculate 90% confidence intervals of the corrected misreporter-shares using bootstrapped standard errors. Note that for higher distance-bins the number of people in the retailer data becomes very small which results in rather large confidence intervals.[19]

A.5. Compliance behavior and the work environment in the retailer data

The dataset of the Austrian retailer allows to compare compliance effects from the work environment for two alternative distance measures: One where we use the door-to-door distance between the employees' residence and workplace address to determine eligibility (Model A), and one where we rely on zip-code level information (Model B). Hence, the difference between Model A and B reflects the noise generated by using zip-code level information to compute driving distances. In both cases, we estimate simple binary choice models similar to the ones in Table 4. Again, we regress an indicator variable informing about the individual compliance decision on the co-worker cheating share of the workplace, 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).

We use two samples to figure out the differential impact of our alternative distance measures: First, we extract information on all 3575 commuters from the retailer data. Second, we focus on the reporting behavior of *new recruits* at the single stores in the year of the analysis. Thus, we test for the presence of an evasion spillover from the existing co-worker cheating share at the single store on the compliance decision of the new recruit. In total, this sample comprises 362 new (commuting) recruits starting to work at one of 139 different stores of the retailer in 2012.

Table A.1 displays our estimation results. Since we have information about the exact residence and workplace addresses, panel A of the table employs a continuous distance measure (in km) to control for the effect of proximity to the next higher allowance bracket. Panel B runs the same regressions but uses distance-to-bracket dummies, analogous to our estimations based on the population tax data (as in Table 4). In both specifications, we again find a strong effect of the proximity to the next higher allowance bracket on the compliance decision. Taking the specification with the continuous distance measure as explanatory variable, we observe that a one kilometer increase of distance to the next higher allowance bracket results in a decrease of the probability to overreport by about 2.5 percentage points. The store size, captured by an indicator variable taking entry one for stores with more than 10 employees and zero else, turns out insignificant in nearly all specifications (with the exception of Model B in the full sample, where we observe a significant effect of around 3.1 percentage points for larger firms).

Most importantly, and consistent with our evidence from the population tax data, we find a positive and significant effect of a store's co-worker cheating share on the individual compliance decision. The parameter estimate is robust over all specifications and samples, lying around 0.25. 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 2.5 percentage points. Comparing estimates using exact residence and workplace addresses (i.e., house numbers) with those based on zip-code level information (centroid measure), the results turn out to be very similar. In sum, using information regarding the exact residence and workplace addresses of commuters we continue to find taxpayers' reporting to be sensitive to the corresponding cheating environment at a given workplace.

[19] Since we observe no misreporting for some of the distance-bins beyond 60 km, we cannot calculate confidence intervals for those bins.

Table A.1
Case study evidence (average marginal effects).

Dependent variable: Indicator variable with entry one if new recruit is cheating				
	All commuters		New recruits	
	Model A	Model B	Model A	Model B
A. Continuous distance measure				
Distance to bracket (continuous)	−0.027***	−0.026***	−0.024***	−0.024***
	(0.001)	(0.001)	(0.003)	(0.003)
Store size (>10 employees)	−0.017	0.031**	−0.023	−0.001
	(0.014)	(0.013)	(0.039)	(0.035)
Co-worker cheater share	**0.262***	**0.216***	**0.294**	**0.310***
	(0.044)	(0.044)	(0.125)	(0.102)
Pseudo-R^2	0.167	0.235	0.177	0.250
B. Discrete distance measures				
Distance to bracket <2 km	0.670***	0.588***	0.658***	0.674***
	(0.022)	(0.030)	(0.076)	(0.076)
Distance to bracket ≥2 and <5 km	0.393***	0.386***	0.183**	0.352***
	(0.029)	(0.029)	(0.089)	(0.087)
Distance to bracket ≥5 and <10 km	0.086***	0.166***	0.078	0.189***
	(0.019)	(0.018)	(0.053)	(0.054)
Store size (>10 employees)	−0.018	0.031**	−0.028	0.011
	(0.014)	(0.013)	(0.036)	(0.035)
Co-worker cheater share	**0.242***	**0.212***	**0.193***	**0.302***
	(0.042)	(0.044)	(0.105)	(0.091)
Pseudo-R^2	0.223	0.220	0.215	0.290
Observations	3575	3575	362	362

Notes: Model A refers to exact residence and workplace addresses (i.e., house numbers); Model B uses the zip-code level information (i.e., centroids). Constant not reported. Standard errors are clustered at store level.
* Indicates significance at 10%-level.
** Indicates significance at 5%-level.
*** Indicates significance at 1%-level.

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