The Effects of Education on Corruption: Evidence from Vietnam's University Expansion*

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Abstract

Education and corruption are negatively correlated at the cross-national level, but little is known about the causal relationship between the two. We combine data on Vietnam's expansion of universities with detailed survey data on experiences of corruption from over 170,000 respondents in 320 districts across 12 years. Using an age cohort difference-in-differences approach, we show that cohorts exposed to the university expansion are 64.8% more likely to have a university degree. However, this increase neither translates into a lower propensity to pay bribes nor an increased propensity to denounce corrupt officials. Instead, we find that education increases the propensity to pay bribes at the individual level. The mechanism for this increase that is most consistent with our data is that education raises household income and higher income leads to more bribe payments.

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1 Introduction

Corruption is a major global problem, but corruption is very unevenly distributed (Andvig and Moene, 1990; Svensson, 2005). Corruption is ubiquitous in some countries, while requests for bribes and other forms of petty corruption are almost completely rooted out in other settings. Typically, the development of the economy and state capacity of nations goes hand in hand with decreases in corruption. However, we know very little about the causal mechanisms of this development process.

One potential mechanism for why corruption decreases at higher levels of economic development is education, which typically increases substantially as part of the overall development process. Education may reduce corruption, as more educated individuals know their rights and can refuse to make illicit payments when government officials demand them. Furthermore, a more educated population may have the capacity to report corrupt officials, making it more difficult for corrupt officials to extract informal payments (Botero et al., 2013). Education may also increase the ability to comply with regulations and administrative processes, avoiding the need to pay bribes to bypass regulation. Finally, education may instill social norms that paying bribes is morally wrong.

There is also a strong negative cross-national correlation between education and corruption as shown in Appendix Figure A.1. It is therefore natural to hypothesize that expanding education could have the additional benefit of reducing corruption. In his influential review paper, Svensson (2005) cites Lipset (1960) and Glaeser et al. (2004), arguing that in their view "education and human capital is needed for courts and other formal institutions to operate efficiently, and government abuses are more likely to go unnoticed and unchallenged when the electorate is not literate."

We test the hypothesis that expanding education reduces corruption by using the

expansion of universities in Vietnam. Among low- and middle-income countries, Vietnam is considered highly successful in terms of educating its population (Asadullah et al. (2020) and Dang et al. (2023)). From 2006 to 2013, the government expanded university education to a larger share of the population by building 122 new universities. For the population of college-age or younger citizens, living in a district where a university opened, this expansion led to a 7.3% percentage points (64.8%) increase in the share of the population attending university. We use this rapid increase to test if higher levels of education reduces corruption.

To measure corruption, we employ the survey data used to create the Viet Nam Provincial Governance and Public Administration Performance Index (PAPI). The PAPI survey data contains detailed responses about corruption experiences among a representative sample of individuals from both rural and urban areas. We compile and harmonize 12 years of survey data from more than 170,000 individuals, providing precise and representative estimates of corruption measured in a consistent way over time for most of our outcomes. In particular, the PAPI data allows us to measure corruption in three ways: self-reported experiences of any type of corruption, perceptions of both petty and grand corruption, and bribe payments when interacting with government officials to apply for certificates and permits as well as to access public services. We then linked these data with data on university opening dates and locations compiled by Vu (2023).

We use an age cohort difference-in-differences approach to estimate the effect of university openings on corruption similar to the framework used in Duflo (2001). We compare districts where a university opened with districts that did not have a university open, and we compare age cohorts that were of college-age or younger when the university opened, with age cohorts that were too old to have had their education directly affected by the university opening. Using this approach we show that

increasing the share of a cohort with a university degree does not reduce the propensity to pay bribes. Instead, we show that both experiences with and perceptions of corruption *increase* with being exposed to a university opening. This suggests that at the individual-level, more education leads to being more exposed to corruption.

Using a standard difference-in-differences analysis to measure the effect on local communities as a whole—not just those directly affected by the increased access to education—we find no effect of educational expansion on citizen engagement in bribes. Overall, our evidence does not support education as an important channel for reducing corruption.

We also directly test the hypothesis that more educated individuals are more willing to report official misconduct to the authorities and that these frequent complaints reduce corruption (Lipset, 1960; Botero et al., 2013). We measure the effect of exposure to university openings on the probability of reporting a government official demanding a bribe, both conditional on having experienced such a demand and unconditionally, but we find no evidence that a university education increases the probability that individuals report corrupt government officials.

To better understand the *increase* in experiences of bribery among cohorts exposed to the university expansion, we investigate several potential mechanisms. We find no evidence for the hypothesis that more educated individuals are more comfortable reporting instances of corruption in the survey. Furthermore, we do not find any evidence that the results are driven by more educated government officials asking for more bribes or that migration to the districts where universities opened is affecting our results. The mechanisms most consistent with our results are instead one where individuals with higher incomes pay more bribes, either because they are more willing to pay or because they are targeted by government officials. We show that education increases household income and that households with higher incomes are more likely

to pay bribes. In particular, we find that being exposed to a university opening increases the propensity to pay middlemen when interacting with government officials. Although this result is tentative, this suggests that a higher opportunity cost of time may drive at least some of the increases in corruption.

Our paper contributes to three strands of literature. First, there is extensive literature studying corruption in educational institutions as well as the effects corruption has on learning (e.g. Reinikka and Svensson, 2005, 2011; Ferraz et al., 2012), but only limited evidence on how education affects corruption. Early empirical work by Glaeser and Saks (2006) uses historical congregationalism in US states as an instrument for the level of schooling today and finds that more educated states have less corruption as measured by the number of government officials convicted. Apart from that study, to the best of our knowledge, this is the first paper studying the causal effect of increased education on corruption. Contrary to the suggestive evidence from this literature, our causal evidence shows that a massive and generally successful expansion of tertiary education did not decrease corruption in this context. Our findings are consistent with the descriptive findings of Mocan (2008), who show that education is positively associated with being asked for a bribe at the individual level in developing countries.

Second, we add to the extensive body of research on the determinants of corruption, a significant subject in Economics, Sociology, and Political Science (e.g. Treisman, 2000, 2007; Fisman and Golden, 2017). By examining how bribes increase among cohorts exposed to university expansion in the short and long run, we complement applied literature that often examines shorter-term changes in corruption in response to specific policy interventions (e.g. Olken, 2007; Banerjee et al., 2020; Mattsson, 2023b). This is important as we only expect corruption to slowly reach a new equilibrium, as multiple actors adjust their behavior to the new dynamics and

incentives (Olken and Pande, 2012). Moreover, we are measuring not just one intervention for one government function, we are measuring a society-wide education intervention on society-wide corruption, where bribes are paid to bureaucrats and middlemen working for a wide variety of government institutions.

Lastly, we contribute to a broader literature on the roles of education in long-run economic development. At the fundamental level, access to education drives the accumulation of human capital, directly contributing to production (Lucas Jr, 1988), innovation and ideas (Romer, 1990), and thus economic growth (Hanushek and Woessmann, 2015; Hendricks and Schoellman, 2023).[1] More recent literature focuses on the interaction between human capital, economic institutions, and long-term economic development (Acemoglu, 2005). The pool of highly educated workers can shape the environment and incentives for innovative activities which subsequently affects long-term growth (Acemoglu et al., 2003, 2006; Vandenbussche et al., 2006; Carneiro et al., 2023). The supply of educated workers can also alter the organization of firms (Blundell et al., 2022; Engbom et al., 2024) and political participation (Campante and Chor, 2012). Corruption is often treated as a separate institutional aspect of economic development (e.g., Fisman and Svensson, 2007; Aghion et al., 2016; Ang, 2020). Our paper studies the relationship between education and this important institutional driver of economic growth.

The rest of the paper is structured as follows. Section 2 describes the context and the data. Section 3 describes our empirical strategy and Section 4 presents our results. Section 6 concludes.

[1] See Valero (2021) for an extensive review of this literature.

2 Context and Data

2.1 Corruption in Vietnam

Vietnam has a substantial corruption problem and is ranked at the 46th percentile for Control of Corruption of the 2022 World Bank's Worldwide Governance Indicators, meaning that it is only better than about half of the 214 countries on the list.[2] This creates a challenging business environment. Bribery incidence (percent of firms experiencing at least one bribe payment request) is 31.1% in Vietnam, relative to an average of 17.6% in East Asia and Pacific, and an average of 13.3% of all economies, according to the World Bank Enterprise Survey.

To combat corruption, the General Secretary of the Communist Party of Vietnam initiated the "Blazing Furnace" anti-corruption campaign in 2016. While earlier efforts to limit corruption have fizzled out without much effect (Malesky et al., 2019), the campaign started in 2016 brought more meaningful changes. Since 2016, over 200,000 party members, including two Presidents, the Speaker of the National Assembly, and several Politburo members, have been disciplined (Giang, 2024).[3] Surveys of firms and citizens also indicate that petty corruption has decreased. For example, in 2016, 66% of firms reported that informal charges are common, while the same number in 2023 was only 33% (Malesky et al., 2024). Vietnamese citizens also report lower levels of bribery and are more likely to believe the government is willing to combat corrupt activities (Centre for Community Support and Development Studies, Centre for Research and Training of the Viet Nam Fatherland Front (VFF-CRT), Real-Time Analytics (RTA), United Nations Development Programme (UNDP), 2023). It is possible that the scale and intensity of this anti-corruption

[2] https://www.worldbank.org/en/publication/worldwide-governance-indicators/interactive-data-access

[3] For more details on the anti-corruption campaign see Appendix A.1.

campaign moderated the impact of education on bribery, as it created more avenues for contesting and appealing corrupt activity. We explore this possibility in more detail in Section 5.3.

2.2 Vietnam's University Expansion

Against the backdrop of rising demands for skilled labor in the early 2000s (McGuinness et al., 2021), the government issued Decree 121/2007 in 2006, which served both as a policy road-map for a national expansion of higher education and a green light for local governments and private enterprises to apply to open new universities. The decree laid out the government's strategy of rapidly scaling up higher education and established an overall expansion plan for the 2006 to 2020 period.

The expansion resulted in the establishment of 122 new universities across the country from 2006 to 2013.[4] As new universities were opened, the number of university and college teachers also rose substantially, from 48.6 thousand in 2005 to 91.4 thousand in 2014 (Vu and Nguyen, 2018). The number of tertiary-level students increased rapidly from 1.4 million to 2.1 million between 2005 and 2014, representing an increase in gross enrollment rate from 16.1% to 30.4%.

Prior to the expansion in 2006, universities were highly concentrated; only 62 out of 713 districts had a university. During the expansion, new universities were opened in 79 districts, 48 of which had not had a university (see Appendix Figure A.2). We use the establishment of these universities to estimate the effect of a new university being established on both education and corruption.

[4]This includes existing universities opening campuses in a new province.

2.3 Data on Education and Corruption

To measure education and corruption we use the Vietnamese Provincial Governance Administration and Public Administration Performance Index (PAPI) annual survey from 2011 to 2022. Our sample consists of more than 170,000 surveys from repeated cross-sections of randomly selected individuals living across 320 districts.[5] For more details on the PAPI survey see Appendix A.2.

Education is measured with an variable indicating if the respondent has at least some university education. We do not treat vocational training programs or courses at colleges as having had a university education.

Corruption is notoriously challenging to measure (Olken and Pande, 2012), but the PAPI survey employs several methods to create a comprehensive set of corruption measures. In particular, we use three different types of corruption measures from the survey. First, we use self-reported experiences of corruption. We create two indicator variables "directly affected" and "indirectly affected" based on whether the respondent states that they themselves were affected by bribe-taking by a government official in the past year, or if a family member was affected.[6] We use these as our main measures of corruption as they are they are the most comprehensive measures of corruption experiences across all interactions with government officials.

Second, we use corruption perceptions. In particular, the PAPI survey includes questions about perceptions of the need to pay bribes to get land use rights, government jobs, public medical services, and more attention paid to your child in school.

[5] Districts are selected using probability proportion to size sampling.

[6] The question asked is "Have you or anyone in your household been affected by an act of bribe-taking by a government official in the past year? Consider all types of bribe-giving ranging from giving a small bribe to a traffic official to giving a commission in order to get a government contract." The respondents could answer using the options: "not affected," "personally affected," or "other family members affected." The variable "indirectly affected" indicates whether either of the two answers is yes.

The survey also includes questions about perceptions of government officials diverting government funds for their personal benefit and getting "kickbacks" for the approval of construction permits. While corruption perceptions have been shown to be predictive of actual corruption, it is also known to be a noisy proxy (Olken, 2009). A benefit of perception measures is that respondents who may not be comfortable discussing their own experiences with corruption may be more willing to describe their general perceptions.

Finally, we use self-reported experiences of corruption during six specific types of government interactions. Four of these interactions are government procedures: applying for certifications, construction permits, or land use rights, as well as "other administrative procedures" such as applying for state subsidies. The other two interactions are when accessing public services, in particular public primary schools and public hospitals. Using self-reported experiences of corruption during these six interactions with government officials has the benefit of allowing us to separate out corruption experiences conditional on having an interaction with the government from corruption experiences for all respondents regardless of their interactions with the government. Furthermore, for three of the interactions, list experiments were used to measure the frequency of corruption experiences. The list experiments are designed to elicit information about corruption experiences without the respondent having to admit to paying a bribe and may therefore overcome the problem of social desirability bias or non-response due to fears of government reprisals (Malesky et al., 2015; Agerberg, 2022).

As each corruption measure has its own benefits and drawbacks, we present results for each of them separately. For more details on the construction of the outcome variables, including the phrasing of the questions, see Appendix A.5.

3 Empirical Strategy

We take advantage of the variation in the opening dates and locations of universities to identify the effects of a university being established on individuals.[7] At the district level, we compare districts that established universities for the first time (treatment districts) and those that never established a university before (control districts). At the individual level, we use the PAPI data to create five-year age bins for different birth cohorts.

Using the two-way fixed effects model, we let g denote the year that a given district has its first university, t the survey year, and c the age cohort relative to the first university opening year, where c is the five-year age cohort described earlier. It is a function of the first university opening year g and the individual's age a at survey year t.

For our baseline specification, we estimate the following two-way fixed effects (TWFE) model for the individual-level event study graphs:

$$y_{i,d,c} = \sum_{c} \delta_c * (T_{i,d} * Cohort_{i,d,c}) + \gamma_d + \eta_a + \theta_t + \epsilon_{i,d,c} \tag{1}$$

where $T_{i,d}$ is an indicator variable for whether individual i surveyed in district d that has a university recently opened and $Cohort_{i,d,c}$ are age cohort dummies whether individual i is in cohort c relative to the university opening year, while γ_d, η_a, and θ_t are district, age, and survey year fixed effects. δ_c is the coefficient of interest that captures the differential impact of university expansion on different age cohorts, relative to individuals from districts that never have a university. Standard errors are clustered at the district level. In some specifications, we further include demographic controls such as gender and ethnicity. The full set of controls additionally add a set

[7] The empirical strategy is similar to that of Vu (2023).

of indicators variables for being a migrant, a party member, or a government official. Since some of these controls could be affected by education, we do not include them in our main specification. Nonetheless, our results are robust to including these these different versions of controls.

In our main figures, we use the age group of 23-27 as the reference group for individual-level analysis, we compare this cohort with birth cohorts that were 22 years old or younger when the new university was built in their district and, thus, would benefit from having access to the new university (i.e., the exposed cohorts). We then compare cohorts that were above 27 when the university opened to establish parallel trends before the opening of the university.

We chose the age cohort of 23-27 as the reference group as they are the first cohort where it is unlikely that the university opening directly affected their education. While students are typically 18 when they start university, those in the 18-22 age bin at the opening of the new university might have been partially exposed if they repeated a class at earlier ages or transferred to a university after attending a college or a university in a different district.

Although our data is at the individual level, some questions are about outcomes at the family or household level, such as household income. For these outcomes we use the age group of 28-32 as the reference group. This is because individuals surveyed from the cohort that was 23-27 years old when the university opened might have younger spouses or siblings in the family, for whom the new university had a direct effect on their education. Choosing an older age group of 28-32 reduces such spillover concerns.

For the baseline specification, we use a two-way fixed effects model. However, as a robustness check, we outline our stacked difference-in-differences model in Appendix Section B to account for the staggered university openings across districts to

circumvent the problems of negative weights as mentioned in the recent literature on difference-in-differences (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Wooldridge, 2021; Gardner, 2022; Dube et al., 2023; Borusyak et al., 2024; Wing et al., 2024)

4 Results

4.1 Effect of University Openings on Education

In Figure 1, we present cohort-level difference-in-differences estimates by using the specification in Equation 1. We estimate the effect of a university opening on the share of the population with a university education. The impact on university education is large and positive. When comparing cohort differences in education level across districts that established universities and those that never had a university, we observe in Table 1 that the share of the younger cohorts with some university education increases by 7.3 percentage points or 64.8% for those aged 22 and below when the university opened, relative to the mean of districts that never had a university.[8]

This result relies on an identifying assumption that in the absence of the university expansion, cohort differences would trend similarly across the treated districts that have a university opened, and control districts that never have a university opened. In other words, there are no omitted time-varying and district-specific effects that are correlated with the university expansion and at the same time only affect the cohorts aged 22 and below. Although we cannot test this assumption directly, we show that for the older cohorts who were 23 and above when the university opened, there are no differential pre-tends.

[8]This is consistent with the results of Vu (2023) who used data from Vietnam's Labor Force Survey to estimate that the university openings increased college completion by 57%.

4.2 Effect of University Openings on Corruption

4.2.1 Effect on Self-reported Experiences of Corruption

Figure 2 presents our main result for the effect of a university opening on the incidence of corruption. We apply the same empirical framework from Equation 1 and report the effects on two corruption measures, an indicator of being personally affected by an act of bribe-taking (shown in red with triangle markers) and an indicator that also includes a family member having been affected by an act of bribe-taking (shown in blue with square markers). Table 2 shows corruption incidences increased significantly by 55.1% for younger cohorts aged below 23 when the university opened, relative to the mean of districts that never had a university opened. The sharp increase in corruption also only occurs in the younger age cohorts exposed to university expansion and there are no differential pre-trends among respondents who were too old to be exposed to the university opening.

One potential confounding factor is that survey respondents with more education may be more comfortable talking about corruption. If this was the case, we should see a decrease among survey respondents refusing to answer the question, or answering by saying "I don't know." Appendix Figure A.3 estimates the effect on respondents not answering the question in these ways. We do not find any evidence that being exposed to the university opening affected the share of such answers.

4.2.2 Effect on Perceptions of Corruption

We now turn to the effect of being exposed to a university opening on perceptions of corruption. Figure 3 and Table 3 show that new university openings increased perceptions of corruption across a wide range of government activities. Respondents were asked to what extent they agreed with different statements about corruption and

we present the results as standard deviations in the responses. Respondents exposed to the university opening are more likely to agree with the statement that people have to bribe to obtain a land title, construction permit, or to have better care for their children in primary schools.[9] They are also more likely to agree with the statement that "officials divert funds for their personal benefit." While the effects on agreement with the statements that one has to bribe to get "a government job" or "medical treatment in the district's hospitals" are not statistically significant, they are positive and consistent with the other results. We create an overall index that captures all these components. We detail the construction of the perception measures in Section A.5. Overall, exposure to a university increases this corrupt perception index by 0.05 standard deviations. We also test the robustness of this index in Appendix Table A.1 with additional controls and the results are very similar.

4.2.3 Effect on Corruption in Specific Interactions with the Government

Finally, in Figure 4 we show the effect on corruption during specific interactions with the government, both conditionally on having had these interactions and for all respondents. We construct two summary measures, one for making informal payments while applying for public certifications, construction permits, land use rights, and other administrative procedures such as state subsidies. The other measure is based on making informal payments while accessing public services at hospitals and primary schools. We explain in detail how the measures are constructed in Section A.5.

In panels (a) and (b) of Figure 4, we combine both paying the middleman and bribes as informal charges and pool them across different government procedures. Panel (a) shows the results conditional on having had the interaction, while panel (b)

[9]Educational bribes are usually paid to teachers in the form of payments for extra tutoring outside of class - with an implicit agreement that this will improve test performance.

shows the results for everyone in the population. We do not find evidence in support of changes in corruption as a result of the exposure of a university opening. In panels (c) and (d) of Figure 4, we estimate the effect on corruption in the two government services, conditionally and unconditionally on having obtained the government service. We do not observe any effect of education in individuals' bribing behaviors when obtaining these services. We also investigated each government interaction separately and empirical evidence does not support any changes in bribing behavior for any individual government service. The point estimates of the effects of university expansion on these measures of corruption are reported in Appendix Table A.2. All point estimates are positive but close to zero and not statistically significant.

To reduce concerns of under-reporting of corruption, the PAPI survey uses a list experiment when asking if a bribe was paid during an interaction with the government, this is in addition to the direct question used for the analysis in Figure 4. In the list experiment, the survey respondents are given a list of actions they may have taken during the interaction with the government. One action is corruption-related while the other actions are not. Respondents are randomized into being shown a list of all actions or a list with the corruption-related action removed. By comparing responses from the group shown the corruption action and the group not shown the corruption action allows us to estimate how many people took the action on average, although we cannot measure which specific individual took the action. In Appendix Table A.4, we estimate the share of people having taken the corruption action.[10] We find no evidence of a reduction in informal payments when using the responses from the list experiment. Two out of the three estimates are positive but they are not precise and not statistically significant.

[10]For more information on the estimation procedure, see Tsai (2019).

4.3 Robustness Tests

We show that our main result is robust to a variety of specifications. First, our stacked difference-in-differences model detailed in Appendix Section B accounts for the staggered university openings across districts. We create sub-datasets with only districts in G_g (treatment group with university opening year g) and the never-treated districts (control group), similar to Cengiz et al. (2019), which enables a clean comparison between the treatment group G and a never-treated comparison group in each year. Appendix Figure A.4 and Appendix Figure A.5 show that results from the stacked regressions are similar to our baseline results.

Second, we test for robustness of our main results to potential violations of the parallel trends assumption. Conventionally, the existence of pre-trends (or the lack thereof) is often used to infer the validity of parallel trends assumption, which cannot be tested directly. When we employ (Rambachan and Roth, 2023)'s smoothness restriction test, which accounts for the exact linear extrapolation of pre-trends to the post periods, we find that our main results for education and corruption are robust to such violations of the parallel trends assumption. Furthermore, we employ (Rambachan and Roth, 2023)'s partial identification approach, which estimates the confidence interval for the treatment effect when the violation of the parallel trends in the post-treatment period (which is unobserved) is less than or equal to a fraction ($\bar{M}$) of the largest deviation from the parallel trend in the pre-treatment period. For example, the confidence interval for $\bar{M} = 1$ includes the true treatment effect at least 95% of the time when the violation of parallel trend in post-treatment is

at most equal to the largest deviation in the pre-treatment period.[11] We report the confidence intervals in Appendix Figure A.6. We use the stacked regression described in Equation 3 to test for the effect in the first post-period in panels (a), (c), and (e), and the average causal effects in panels (b), (d), and (f). The test of the average causal effect across all seven post-periods in panel (b) is more stringent than the default test for the first period because the treatment and control groups have more time to diverge.[12] Our estimates for the effect of the university expansion on education are robust to differential trends that are equal to the largest differential pre-treatment trends ($\bar{M} = 1$) in the first post-treatment period (panel (a)) as well as in all post-treatment periods (panel (b)). Although the confidence intervals for our corruption measures exclude 0 only with a less stringent restriction of allowing parallel trend deviations in the first post-treatment period (panels (c) and (e)).

Lastly, we assess the robustness of our estimates to different sizes of the age bin. Appendix Figure A.7 additionally present the corruption exposure results using age bins of three and age bins of two. The main patterns are very similar.

5 Discussion and Potential Mechanisms

Overall, our results show a dramatic increase in university education as a consequence of opening new universities. However, this increase in education did not translate

[11] Formally, denote $\gamma_{t=\{pre,post\}}$ difference in trends between the control and treatment groups (i.e., pre-trends and post-trends). The parallel trends assumption is $\gamma_{post} = 0$, which is unobserved, and γ_{pre} can be estimated in an event study regression. Rambachan and Roth (2023)'s approach yields a confidence interval of treatment effect when post-trends are only as large as $\bar{M}$ of the largest pre-trends period:

$$|\gamma_{t+1} - \gamma_t| \leq \bar{M} \cdot \max_{1 \leq k \leq t} |\gamma_{k+1} - \gamma_k|$$

where k and t denote the pre and post periods. The robust confidence intervals have accounted for the fact that there is estimation error both in the treatment effects estimates and estimates of the pre-trends.

[12] The identified set for the second period will be twice as large as for the first period, three times as large in the third period, and so on (Rambachan and Roth, 2023).

into a decrease in corruption. Instead, we observe substantial increases in both self-reported incidences of corruption and perceptions of corruption. We now turn to potential explanations for the lack of a decrease in corruption and mechanisms for the increase in corruption.

These findings point to two potentially countervailing mechanisms connecting education to corruption. The first is the *knowledge effect*, which has received the most attention in the literature (Lipset, 1960; Svensson, 2005; Glaeser and Saks, 2006). According to this logic, education equips citizens with greater knowledge and capabilities to avoid corruption. Furthermore, it is more risky for government officials to ask for bribes from educated individuals as they have better means to report the government officials to the authorities. For this reason, educated actors should also be less likely to be targeted by officials for bribe requests (Fried et al., 2010).

However, our findings above also suggest an *income effect* that may push against and even overwhelm the knowledge effect (Mocan, 2008). Higher incomes, generated by increased education, generate both greater exposure to situations in which bribes may be requested and it may also raise the relative value of paying these bribes. Those with higher incomes are more likely to engage in activities, such as buying land, expanding a residence, or obtaining a driver's license, that expose them to more bribe requests from regulators than those with lower incomes. Higher incomes may also raise the relative value of an individual's time, as time away from the job is more costly. Burdensome administrative procedures and long waiting periods therefore appear more consequential and wealthier individuals may be more willing to pay bribes to avoid these costs. If this higher willingness-to-pay is observable for those requesting bribes, higher incomes may also increase bribe requests as part of systematic price (or bribe) discrimination (Olken and Barron, 2009; Mattsson, 2023a).

The direction of the effect of education on bribery therefore depends on the set-

ting. The knowledge effect is likely to be dominant where accountability channels are strong but education is required to access these channels. For example, if there is effective anti-corruption legislation and institutions where citizens can report corrupt officials.[13]

On the other hand, the income effect is likely to dominate in settings without effective accountability channels or where such channels are thought to be illegitimate and distrusted. Knowledge about denunciation and complaint mechanisms is of little value when citizens believe that the government has limited political will to punish corrupt actors (Brinkerhoff, 2000; Ankamah and Manzoor E Khoda, 2018)

In this section, we attempt to demonstrate the role of these two mechanisms. First, we test whether the knowledge effect exists in our setting by establishing whether university education increases knowledge about the policy prescriptions in the Anticorruption Law. Second, we test if more education also increases the likelihood of individuals denouncing corrupt officials. Third, we trace the income effect by establishing that university opening of universities had a positive effect on income and that higher-income individuals were more likely to bribe and, to save time, hire middlemen to facilitate their bribe payments. Fourth, we take advantage of the 2016 Blazing Furnace anti-corruption campaign to see if the highly publicized punishments of corrupt officials and promotion of mechanisms for disputing bribes, along with e-governance channels for avoiding bureaucratic discretion altered the relative importance of the knowledge and income effects in Vietnam. We finish the section by ruling out several reasonable alternative mechanisms.

[13]The institutions could include an ombudsman and complaint mechanisms for bureaucratic corruption, courts to sue corrupt officials, and government inspectorates who can investigate and punish corrupt acts. In these cases, educated citizens will have greater knowledge that the institutions exist and a greater capacity to avail themselves of them (Grossman and Slough, 2022).

5.1 The Knowledge Effect

We will now test the hypothesis that a more educated population would make more complaints against corrupt officials and thereby hold them accountable (Svensson, 2005; Botero et al., 2013). Appendix Table A.5 shows suggestive evidence that education raises knowledge about Vietnam's anti-corruption legislation, although the levels of knowledge is generally low and the estimated effects are imprecise. Panel (a) of Figure 5 shows the effect on the probability of denouncing a government official asking for a bribe, among respondents that were asked for a bribe. Panel (b) shows the same analysis using the whole sample. Panel (b) hence takes into account both the effect on the probability of having been asked for a bribe and the effect on the probability of denouncing the government official after having been asked to pay a bribe. Nevertheless, for both measures, we don't find any effects of the university openings on the reporting of government officials for corrupt behaviors. This is evidence against the hypothesis that education makes people more likely to report corrupt officials and it is consistent with our overall result that education does not reduce corruption.

5.2 The Income Effect

In Figure 6, we demonstrate a significant 14.4% increase in households' monthly income for cohorts exposed to the university opening. Additionally, in Figure A.8, we find a positive association between household income and corruption exposure. Although the relationship between income and bribery should not be interpreted as causal, this suggests that more educated families may bribe more as they become richer.

One way in which higher incomes may lead to bribery is by increasing the number

of interactions the individual has with potentially corrupt government officials. For example, individuals may be more likely to apply for construction permits as income increases. In Appendix Figure A.9 and Appendix Table A.7, we analyze the effect of education on the number of government procedures (certifications, construction permits, land use rights, and other administrative procedures) and services (public healthcare and primary school) the individual applied for or used. For this group of government interactions, we rule out that individuals have more interactions with the government officials following the university expansion. Instead we see a decrease in the use of public services, which is not surprising given that the increase in income is likely to have caused a switch from public to private providers of these services. While the survey data does not cover all of the potential interactions with government officials, it does cover some of the most common types of interactions where individuals often pay bribes. However, it is possible that education, and subsequently income, increased interactions with government officials that are not covered by the survey, and that an increase in these interactions is a contributing factor to the increase in experienced and perceived corruption.

Another reason why individuals with a higher income may engage in more corruption is that they face higher opportunity costs for their time. They may therefore be more willing to pay to avoid the administrative hassle. We test this hypothesis by focusing on payments to "middlemen" who help out with applications for government procedures. While paying a middleman does not necessarily involve paying a bribe, in practice it is common for the middleman to pay the government official. Appendix Figure A.10 and Appendix Table A.9 present suggestive evidence that university education leads to an increase in payments to middlemen. This is consistent with some of the increase in corruption being driven by increases in the opportunity cost of time for those with a university education.

In Appendix Figure A.11, we use a standard difference-in-differences specification, utilizing variations created by districts with university expansion that happened during the survey waves, and we do not find an increase in corruption exposure for the entire district population. These results should be interpreted cautiously as we lose much of our variation in exposure to the university opening when we are not using the differences between cohorts. However, these results are consistent with a targeting of high education (and therefore high-income) individuals, rather than an increase in corruption across all of society when a new university is established.

5.3 Did the Anti-corruption Campaign Increase the Importance of the Knowledge Effect?

In Appendix Table A.10, we analyze the differences in the effects of education before and after the start of the Anti-Corruption Campaign described in Section 2.1 and Appendix A.1. In Column (1), we show that the first-stage effect of university expansion on corruption is very similar before or after the anti-corruption campaign. In Column (2), we show that the effect of education on knowledge about the Law on Corruption Prevention was larger after the start of the anti-corruption campaign. The difference in effects is not statistically significant, but it is intuitive that more educated individuals gather more knowledge about the government's anti-corruption efforts once they believe that these efforts are sincere. In Column (3), we show that the effect of education on actually denouncing corrupt behaviors is more positive after the anti-corruption campaign started. In columns (4) and (5) we show that the effects of education on corruption are marginally smaller after the start of the corrup-

tion campaign.[14] Taken together these results tentatively indicate that the relative magnitudes of the knowledge effect and income effect depend on institutions and the trust the population has in the government's commitment to combating corruption. When the institutions are more committed to combating corruption, the knowledge effect starts to counteract the income effect, as educated individuals are more likely to denounce corrupt behaviors. However, since the differences in effects on corruption experiences are not statistically significant, they should interpreted very cautiously.

5.4 Alternative Mechanisms

We also test for two other potential mechanisms. First, it is possible that new universities led to better-educated government officials and that the officials with higher education are more corrupt. Appendix Figure A.12 restricts the PAPI sample to those who work as government officials and measures the effect of exposure to a university opening on the education level of government officials. We do not find any evidence of an increase in education levels among government officials and thus it is unlikely that the education levels of government officials are driving the effect on corruption. Second, we also rule out the potential mechanisms that migration into districts with a university is driving our effects. Appendix Figure A.13 shows the effect of exposure to a university opening on the share of migrants in our sample. We do not find any

[14] Appendix Table A.11 shows the results before and after the start of the anti-corruption campaign for our other corruption outcomes. We see that the positive effect on corruption perceptions is substantially larger after the start of the corruption campaign, which could be explained by respondents with higher education being more aware of the corruption cases revealed in the campaign. For corruption during specific interactions with government officials, we do not find any evidence of a difference in the effect before and after the start of the anti-corruption but the estimates are relatively imprecise.

evidence that exposure to a university opening is associated with being a migrant.[15]

6 Conclusion

In this paper, we explore the impact of increased university education on corruption, leveraging Vietnam's university expansion, detailed survey data and a cohort difference-in-differences approach. Despite a significant increase in university attendance among the affected cohorts, our findings indicate that this educational uplift does not lead to a reduction in corruption. In fact, the propensity to pay bribes increased among individuals exposed to the university expansion.

Our analysis revealed that the higher likelihood of bribery among the educated cohorts is associated with higher household incomes, which correlate with a greater propensity to pay bribes. This suggests that while education raises income levels, it also inadvertently increases opportunities and capacities for corrupt transactions, contradicting the assumption that education inherently reduces corruption.

Our paper has several limitations. First, we are mostly limited to studying the effects of increasing the education levels of individual cohorts on the corruption levels that these cohorts are exposed to. It is possible these these effects are different from the effects of education on corruption at a societal level where the whole population is more educated. We expand the analysis to measure district-level effects and these results are also consistent with there being no negative effect of education on corruption, but as most of our variation comes from the differences between cohorts these results are less precisely measured. Furthermore, we investigate some of the pro-

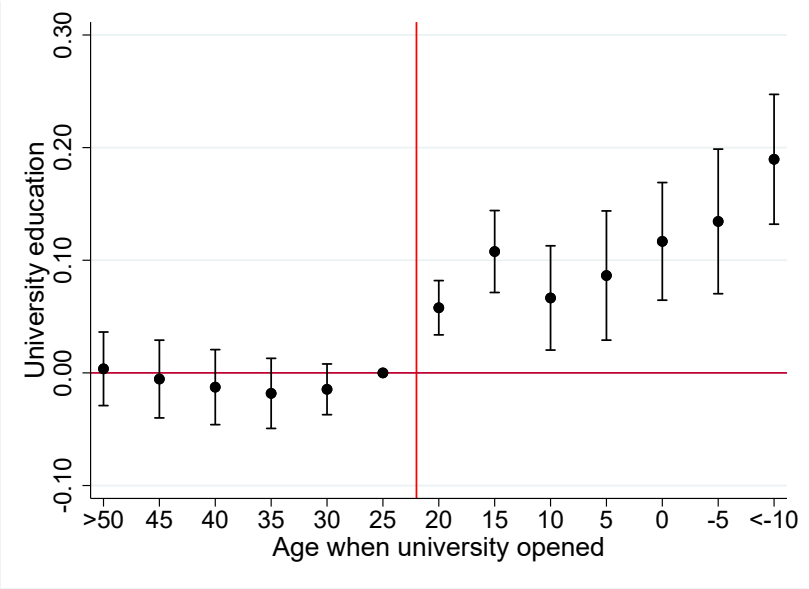
[15] We do not claim there is no effect of university openings on migration because the way that the PAPI survey respondents are sampled is not suitable for such an analysis. The PAPI sample is mostly drawn from the population of permanent residents. However, even though we are unable to measure the effect on migration overall, we can still rule out the potential mechanism of migration driving the effects we see in our PAPI sample.

posed mechanisms for why education may reduce corruption at a societal level, such as increasing the rate at which citizens report corrupt government officials. Second, our measures of corruption are self-reported and there is always a risk that they are biased due to social desirability or fear of being held accountable for giving bribes. It is reassuring that results are consistent with using a list experiment to reduce such biased, but the list experiment questions are only available for a small share of our outcome variables. Third, we can only directly measure small-scale corruption between citizens and government officials. For larger-scale corruption taking place at higher levels of government, we only have citizens perceptions. It is possible that education has a different effect on actual larger scale corruption activities than it has on petty corruption and that this effect is not captured by citizens perceptions.

Finally, our setup allows us to examine both the short-run and long-run effects of university expansion on the bribing behavior of the younger cohorts. However, as is indicated in our discussion section, it is possible that these results are contingent on the specific institutional context in which the increase in education happened. Specifically, we show suggestive evidence that as the institutional context changed to one where the government took a stronger anti-corruption position, the knowledge effect causes those with more education to be more likely to report corrupt government officials. While our findings cast doubt that education is a straightforward tool for combating corruption, they also point at a complex relationship between education, income, and corrupt behavior.

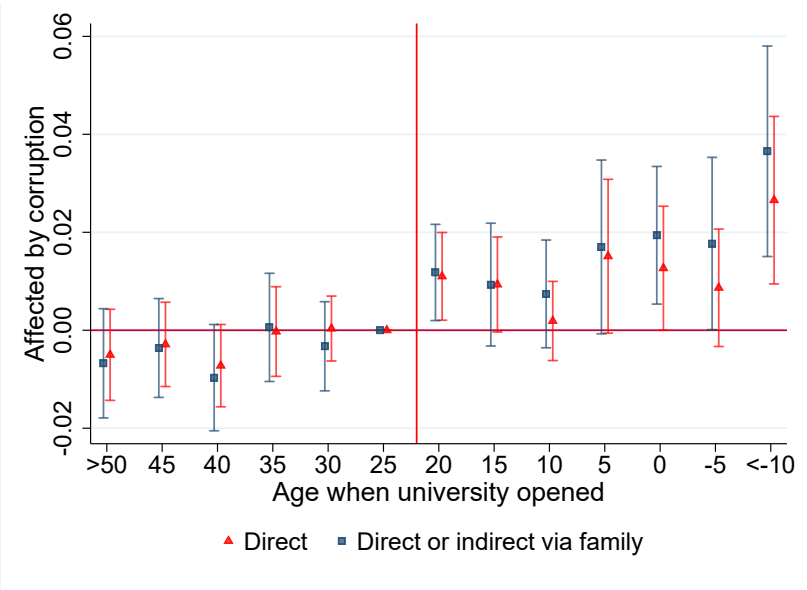
Figures

Figure 1: Effect of university expansion on university education



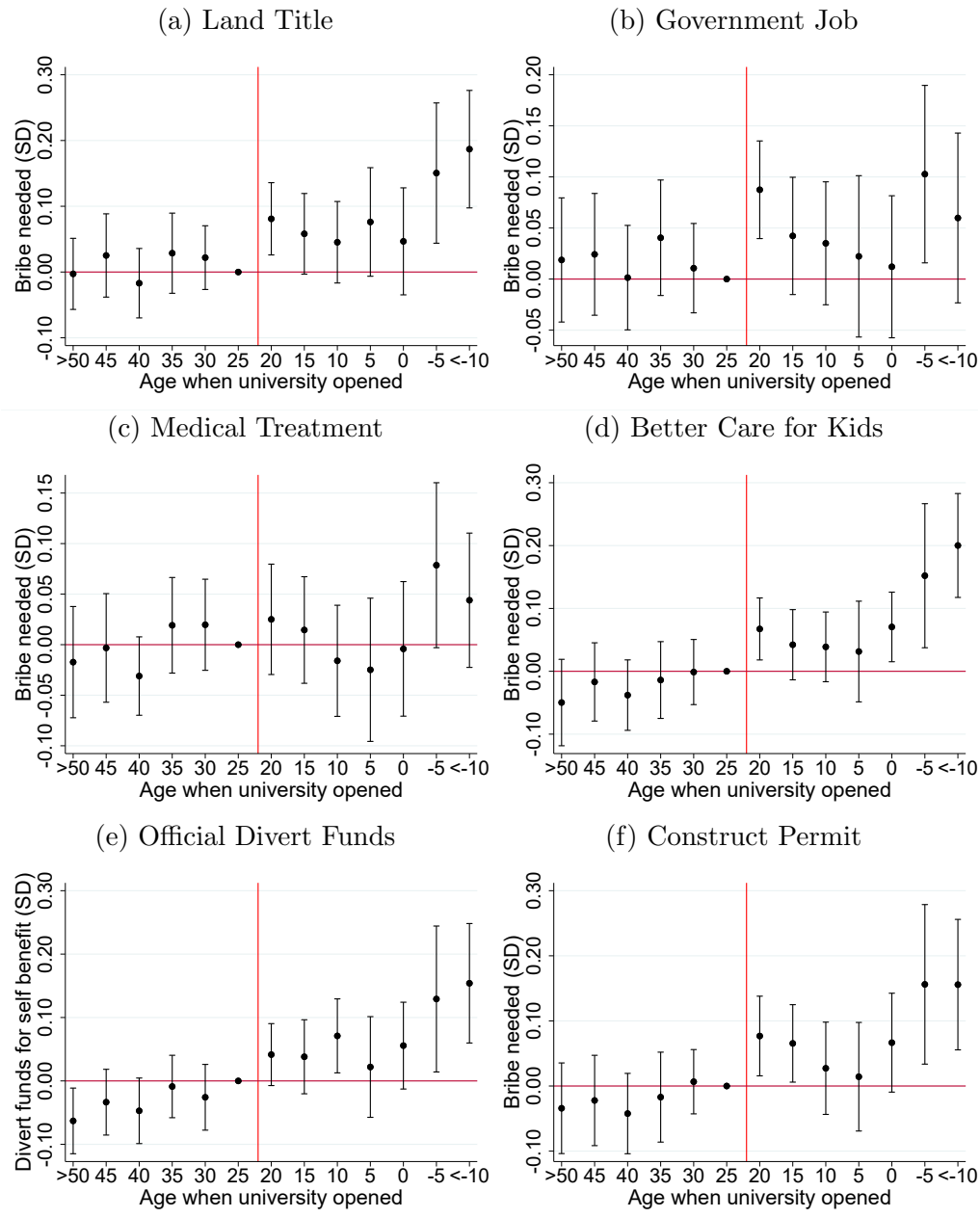
Note: Figure 1 shows the effect of cohorts being exposed to a university opening on the share having a university education. We use equation 1 to obtain the estimates for each cohort. The outcome variable is an indicator of being university-educated. The mean of the outcome variable in districts that never had a university is 0.112. The number of observations is 171,240. 95% confidence intervals are constructed using standard errors clustered at the district level. The difference-in-differences estimate for all cohorts is reported in Table 1. The stacked difference-in-differences estimates are reported in Appendix Figure A.4. See discussion in Section 4.1.

Figure 2: Effect of university expansion on whether respondent was affected by bribe-taking



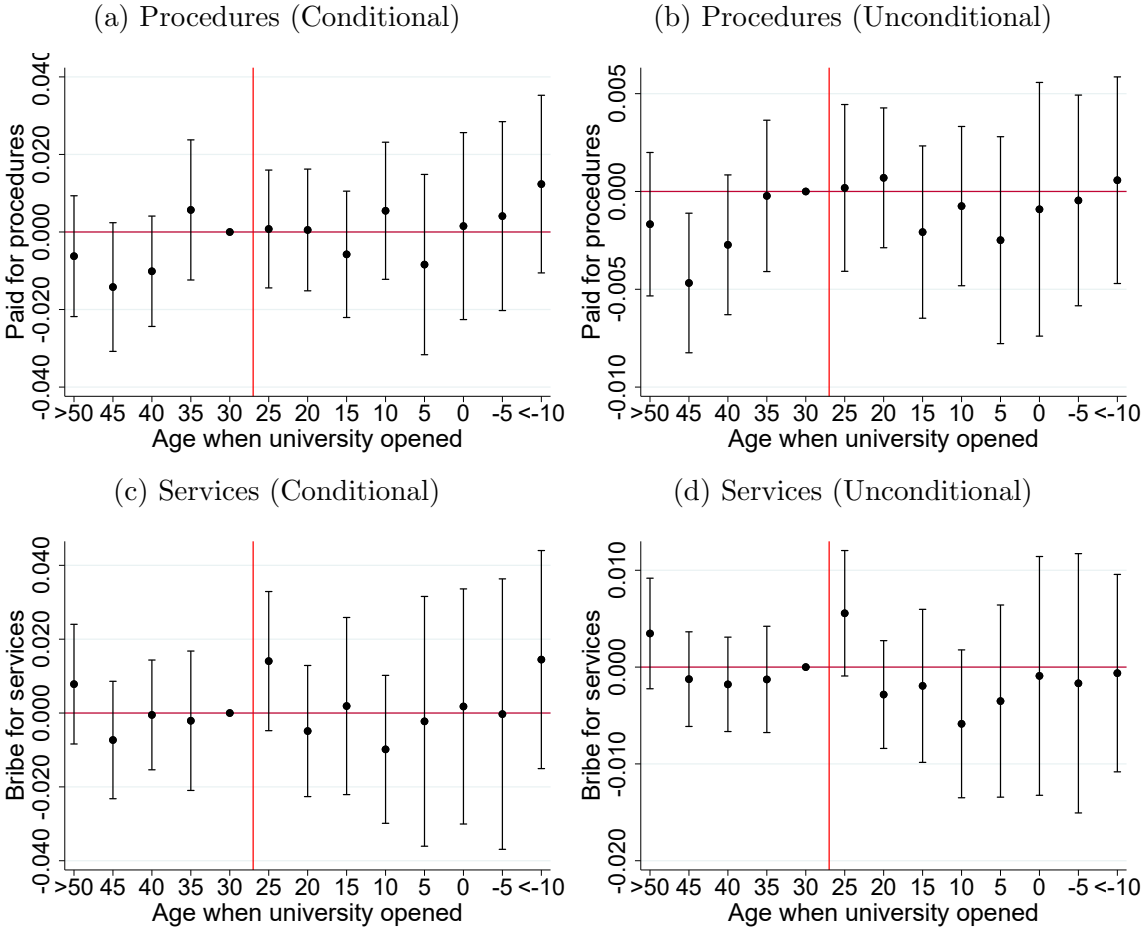
Note: Figure 2 shows the effect of cohorts being exposed to a university opening on the share having experienced an act of corruption in the previous year. We use equation 1 to obtain the estimates for each cohort. The outcome variable "Direct" is an indicator of being personally affected by an act of bribe-taking (N=169,095) and "Indirect" is an indicator of being personally affected or having a family member affected (N=169,095). The mean of the outcome variables in districts that never had a university is 0.015 and 0.027 for direct and indirect experiences, respectively. The survey question is "Have you or anyone in your household been affected by an act of bribe-taking by a government official in the past year? Consider all types of bribe-giving ranging from giving a small bribe to a traffic official to giving a commission in order to get a government contract." 95% confidence intervals are constructed using standard errors clustered at the district level. The difference-in-differences point estimate for all cohorts is reported in Table 2. The stacked difference-in-differences estimates are reported in Appendix Figure A.5. See discussion in Section 4.2.

Figure 3: Effect of university expansion on perceptions of corruption



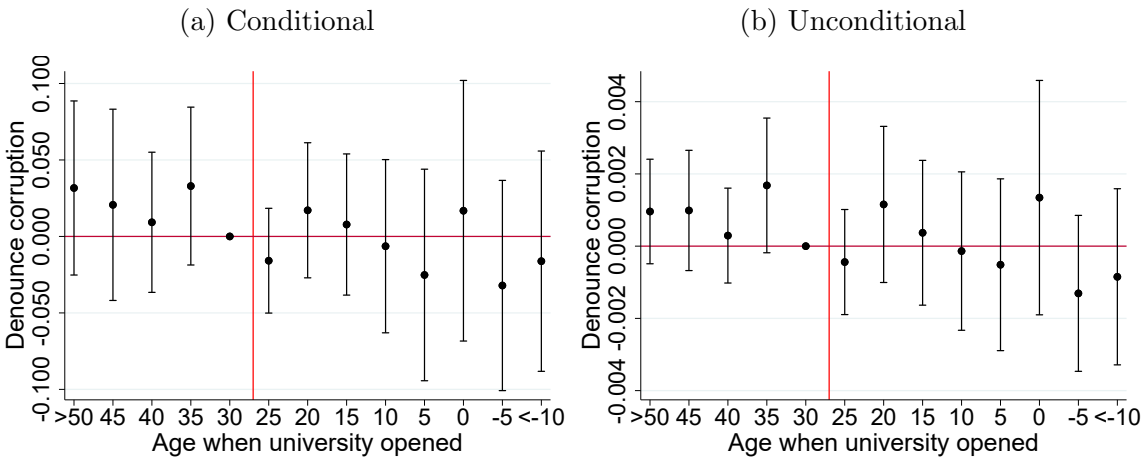
Note: Figure 3 shows the effect of cohorts being exposed to a university opening on the perception of corruption. We use the specification in Equation 1 to obtain estimates for each cohort. The outcome variables are standardized measures of agreement with different statements related to corruption perceptions. 95% confidence intervals are constructed using standard errors clustered at the district level. The difference-in-differences estimates are reported in Table 3. See discussion in Section 4.2.

Figure 4: Effect of university expansion on making informal payments



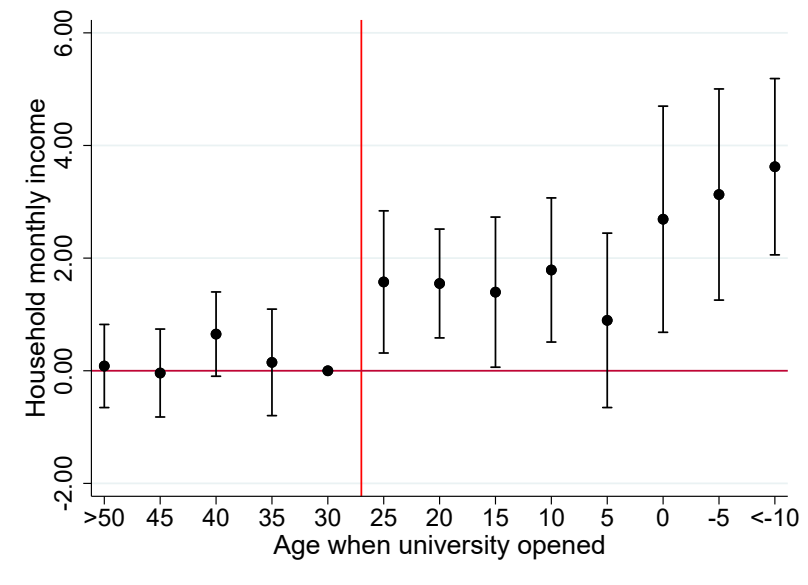
Note: Figure 4 shows the effect of cohorts being exposed to a university opening on making informal payments. We use equation 1 to obtain estimates for each cohort. The unit of observation is at the respondent level. The outcome variable is an indicator variable if an individual or an individual's family paid a bribe or a middleman as part of an interaction with the government. The government procedures in Panel (a) and (b) are applications for a certification, construction permit, land use rights, or other administrative procedures. The government services in panels (c) and (d) are public primary schools or public hospitals. Panel (a) and (c) are conditional on having had the interaction (N=100,464 and N=85,169, respectively). Panels (b) and (d) use all observations (N=171,240 and N=171,240, respectively). The means of the outcome variable in districts that never have a university are 0.105, 0.023, 0.092, and 0.028 respectively for panels (a), (b), (c), and (d). 95% confidence intervals are constructed using standard errors clustered at the district level. The difference-in-differences estimates are reported in Appendix Table A.2. See discussion in Section 4.2.

Figure 5: Effect of university expansion on denouncing bribe-taking acts



Note: Figure 5 shows the effect of cohorts being exposed to a university opening on denouncing bribe-taking acts. We use equation 1 to obtain the event study estimates. The outcome variable "denounce corruption" is an indicator for attempting to denounce the act of corruption. Panel (a) performs the analysis on a subset of observations (N=5,092) conditional on being affected by bribe-taking acts while Panel (b) is the unconditional on whether being affected or not (N=171,240). The mean of the indicator in the panel for districts that never have a university is 0.070 in panel (a) and 0.002 in panel (b). The PAPI survey question is "Did you or your family member attempt to denounce this act of corruption?" 95% confidence intervals are constructed using standard errors clustered at the district level. The combined difference-in-differences point estimates for the younger cohorts are reported in Appendix Table A.6. See discussion in Section 5.

Figure 6: Effect of university expansion on household income



Note: Figure 6 shows the effect of cohorts being exposed to a university opening on household income. We use equation 1 to obtain the event study estimates. The outcome variable "Household monthly income" is in million VND. The number of observations is 44,549. The mean of the household income for districts that never have a university is 7.535 million VND. 95% confidence intervals are constructed using standard errors clustered at the district level. The combined difference-in-differences point estimates for the younger cohorts are reported in Appendix Table A.8. See discussion in Section 5.

Tables

Table 1: The effect of university expansion on receiving some university education

	University Education		
	(1)	(2)	(3)
University Expansion	0.073***	0.070***	0.079***
	(0.011)	(0.011)	(0.012)
Observations	171,240	171,240	171,240
Cluster No.	320	320	320
Adjusted R2	0.109	0.116	0.314
Mean of Never Treated	0.112	0.112	0.112
District FE	✓	✓	✓
Year FE	✓	✓	✓
Age FE	✓	✓	✓
Demographic Controls		✓	✓
Full Controls			✓

Note: Table 1 shows that the university expansion increases the share of individuals with some university education by 7.3 percentage points. This is a 65.2% increase, relative to the mean of districts that never have a university opened. We combine the age cohort dummies in Equation 1 into an independent variable "University Expansion" which equals 1 if the survey participant aged below 25 at the time when the university opened and 0 otherwise. The outcome variable is the same as that in Figure 1. Demographic controls include gender and ethnicity and full controls additionally include an indicator for party membership, an indicator for working for the government, and an indicator for being a migrant. Standard errors are clustered at the district level. See discussion in Section 4.1.

Table 2: The effect of university expansion on corruption exposure

	Direct			Indirect		
	(1)	(2)	(3)	(4)	(5)	(6)
University	0.0084**	0.0081**	0.0081**	0.0120***	0.0117***	0.0117***
Expansion	(0.0037)	(0.0037)	(0.0037)	(0.0043)	(0.0044)	(0.0044)
Observations	169,095	169,095	169,095	169,095	169,095	169,095
Cluster No.	320	320	320	320	320	320
Adjusted R2	0.015	0.017	0.017	0.019	0.020	0.020
Never T. Mean	0.015	0.015	0.015	0.027	0.027	0.027
District FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓
Demo. Controls		✓	✓		✓	✓
Full Controls			✓			✓

Note: Table 2 shows that the university expansion increases the probability of direct corruption exposure by 0.8 percentage points, a 55.1% increase, relative to the mean of districts that never have a university opened. It also increases the probability of indirect corruption exposure via family members by 1.2 percentage points, a 44.5% increase, relative to the mean of districts that never have a university opened. We combine the age cohort dummies in Equation 1 into an independent variable "University Expansion" which equals 1 if the survey participant aged below 25 at the time when the university opened and 0 otherwise. The outcome variables are the same as those in Figure 2. Demographic controls include gender and ethnicity and full controls additionally include an indicator for party membership, an indicator for working for the government, and an indicator for being a migrant. Standard errors are clustered at the district level. See discussion in Section 4.2.

Table 3: The effect of university expansion on corruption perception

	(1) Land	(2) Govt Job	(3) Medical	(4) Kids	(5) Funds	(6) Permit	(7) Index
University Expansion	0.048**	0.033	0.013	0.062**	0.069***	0.072***	0.049**
	(0.018)	(0.018)	(0.019)	(0.023)	(0.017)	(0.021)	(0.019)
Observations	140,234	138,437	149,996	149,627	135,884	138,622	163,800
Cluster No.	320	320	320	320	320	320	320
Adjusted R2	0.037	0.114	0.075	0.041	0.018	0.051	0.061
Mean of Never Treated	-0.006	-0.032	-0.009	-0.046	0.009	-0.053	-0.026
District FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓	✓

Note: Table 3 shows that the university expansion increases the perception of several corrupt behaviors, such as "people have to bribe to obtain a land title", or "construction permit", or "have their children better attended", or "officials divert funds for their personal benefit". However, they are not more likely to believe that "people have to bribe for a government job" or "receive medical treatment in the district's hospitals". Respondents choose an option among "agree", "somewhat agree", and "disagree". As a result, the outcome measures are all standardized. The overall index is a standardized weighted average of these standardized components, which also increases as a result of university expansion. These outcome variables are the same as those in Figure 3. Standard errors are clustered at the district level. We test the robustness of the corruption perception index in Appendix Table A.1 with additional controls. See discussion in Section 4.2.

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Appendix

A Data Appendix

A.1 Additional details on the Blazing Furnace Anti-Corruption Campaign

An important change in Vietnam's commitment to anti-corruption occurred in the middle of our study period in 2016 (Hung, 2018). Throughout the country's reform era, multiple legislative efforts have been made to limit corruption, but most of these efforts fizzled or were limited in scope (Malesky et al., 2019). This changed in 2016 after several high-level corruption scandals that implicated critically important state-owned enterprises (SOEs) and several high-ranking officials. Immediately after the Party Congress in 2016, General Secretary Nguyen Phu Trong initiated an effort to root out corruption at its source. Trong coined the term "blazing furnace" at a meeting of the Central Steering Committee for Anti-Corruption in July 2017, arguing that corruption was constraining growth and promising to hold local and central officials accountable. Consequently, since 2016, over 200,000 party members, including two Presidents, the Speaker of the National Assembly, eight Politburo members, 39 Central Committee members, and 52 military and police generals, have been disciplined (Giang, 2024). Two other components of the blazing furnace efforts have been less attention-grabbing but are also consequential. First, Trong led a rewriting of VCP statutes to reduce malfeasance by limiting the permissible types of financial and business transactions. Second, the government pushed forward a series of e-governance reforms that have moved several public procedures and services online to streamline them and reduce opportunities for discretion by bureaucrats. The effort was intended

to combat corruption by reducing bottlenecks in official transactions and discretion by bureaucratic gatekeepers but has had limited take-up thus far (Malesky and Bui, 2024).

Surveys of firms and citizens indicate that the anti-corruption mobilization has been successful at reducing petty corruption. Today, 43% of businesses report that informal charges are common compared to 66% in 2016, and only 3.8% of businesses make bribe payments greater than 10% of annual revenue, compared to 9.1% in 2016.[16] Vietnamese citizens also report lower levels of bribery to local government in accessing public services and state employment and are more likely to believe the government is willing to combat corrupt activities.[17]

It is possible that the scale and intensity of the anticorruption campaign may have moderated the impact of education on bribery. After 2016, citizens had more avenues for contesting and appealing corrupt activity and more avenues for avoiding bribery in daily transactions. If education increases awareness of these possibilities, we might expect a stronger negative relationship after 2016. We explore this possibility in more detail below.

A.2 The PAPI Dataset

To measure individual-level experiences with corruption we employ the Vietnamese Provincial Governance Administration and Public Administration Performance Index (PAPI) annual survey from 2011 to 2022. The PAPI survey is administered by the United Nations Development Program (UNDP) in collaboration with the Centre for

[16]Edmund Malesky, Pham Ngoc Thach, Truong Duc Trong, Phan Tuan Ngoc, Quynh Nguyen, *The Vietnam Provincial Competitiveness Index: Measuring Economic Governance for Private Sector Development, 2022 Final Report* (Hanoi, Vietnam: Chamber of Commerce and Industry and United States Agency for International Development, 2023): 71

[17]CECODES, VVF, and UNDP "The 2022 Viet Nam Governance and Public Administration Performance Index (PAPI 2022): Measuring Citizens' Experiences." CECODES. Technical report (2023): 10

Community Support and Development Studies (CECODES) and Vietnam Fatherland Front (VFF). Since its initiation in 2009, it has grown to be Vietnam's largest public opinion survey with between 12,000 and 18,000 annual respondents.[18] In this project, we use a dataset of repeated cross-sections of individuals from within selected Vietnamese communes. While the communes are consistent over time between 2011 and 2022, the sample of individuals is refreshed every year.

The PAPI sampling design is ideal for our project because it includes a large set of representative samples of 320 districts,[19] including all 63 provincial capitals, that were surveyed every year between 2011 and 2022 and can be matched directly to the locations of university openings.[20] In 2011,[21] the PAPI research deployed a multi-level sampling approach to ensure representative samples of Vietnamese citizens over eighteen years old at the national level and for all 63 Vietnamese provinces and national-level cities. Units below the provincial administration level (district, commune, and village) were selected using probability proportion to size (PPS) sampling. Within each province, the capital district was always selected as a certainty unit, however, other districts were selected in a lottery weighted by the size of the district at the time of the 2010 Vietnamese Population Census. In provinces with over two million people, six districts were selected using PPS, while for provinces with under two million people, three districts were chosen. This same PPS approach is used to select capital and representative communes and villages within each district.

Once villages are selected, the PAPI research team travels to each village to ask for the most recent village household residency list, which includes all adults (over

[18] Additional information on the methodology of the PAPI survey, dataset, and its usage can be found here: https://papi.org.vn/eng/

[19] Out of a total of 713 districts

[20] The PAPI survey was re-calibrated to match the 2019 population census and sampled a new set of communes, disrupting 50% of the village panels

[21] In 2009 and 2010, PAPI only surveyed 3 and 30 provinces respectively, as it was refining its methodological approach

eighteen) in each village household. At this point, a simple random sample is used to select 20 households in 61 provinces and 40 households in the two metropolises of Hanoi and Ho Chi Minh City, which have populations of over 80 million people. The nearest birthday method (i.e. selection of the respondent with the birthday closest to the data of the survey) is used to provide an additional household-level randomization to ensure age and gender representativeness in the survey. The average response rate in our sample is 83% due to the highly reliable contact information provided by the village residence list.

Once an individual is selected, they are sent an invitation to travel to the village cultural center for an interview on a selected date.[22] Interviews within each village are staggered and take place in secluded locations within the cultural center to provide confidentiality, and a separate "expert survey" was designed to interview curious local government officials at a separate location away from the center to avoid creating anxiety among respondents about answering sensitive governance questions. Enumerator-answered control questions are available in the survey to track respondents' level of comprehension, anxiety, and any salient disruptions.

CECODES deploys a survey research team of several hundred interviewers, who are selected from the province to ensure cultural, ethnic, and accent matches with the respondents. However, no interviewers are ever deployed in their home village. These survey teams are selected and trained by a group of survey managers from all 63 provinces, who travel to Hanoi each summer for a one-week training program on the sampling strategy, survey design, and questionnaire with research managers. At that time, complex and sensitive questions are explained and pre-scripted nudges are provided to help respondents aid respondents who struggle to comprehend questions.

[22]Culture centers were deemed more appropriate than within-home interviews because some households in mountainous or delta areas could be extremely difficult to reach and because poorer respondents were reluctant to allow enumerators into their homes.

The PAPI survey cover six governance modules including detailed batteries on corruption and quality of education. It also includes two modules on basic demographics (age, ethnicity, gender, party membership) and income and household assets. The survey, which since 2012 has been administered on a handheld tablet computer is about 54 pages long and, depending on the filter pattern, takes between 45 and 75 minutes to complete.

A.3 Data on University Openings

We manually collected data on the opening dates of all universities in Vietnam established before 2023. The data comes from a variety of official government documents and is an updated version of the university openings data used by Vu (2023).

A.4 Linking the PAPI Survey Data and the University Openings

The university openings data and the PAPI survey data are linked through administrative geocodes provided by the Vietnam General Statistics Office (GSO).

As administrative units in Vietnam change throughout the years, we have used the 2019 GSO codes for each district throughout the project to maintain consistency in identifying the locations of entities (universities and individuals).

A.5 Construction of Outcome variables

In this section, we explain how key outcome variables are defined. To measure the level of education, we created an indicator variable for some university education based on a survey question in PAPI, "What is your highest level of education?". If the survey participant responds with "some university education", "university education

completed", or "post-graduate degree", the indicator variable is one, otherwise, it is 0.

For the main corruption measures, we created two indicator variables "directly affected" and "indirectly affected" based on a survey question "Have you or anyone in your household been affected by an act of bribe-taking by a government official in the past year? Consider all types of bribe-giving ranging from giving a small bribe to a traffic official to giving a commission in order to get a government contract." If the response is "not affected," the indicator variables are zero. If "personally affected" is selected, the variable "directly affected" equals 1. If "personally affected" or "other family members affected," the variable "indirectly affected" equals 1.

We also leverage detailed questions in the PAPI survey to construct measures of corruption perceptions. Respondents choose "agree", "somewhat agree", or "disagree" for statements such as "people have to pay bribes in order to obtain a land title." Besides land titles, respondents are also asked about perceptions of the need to pay bribes when obtaining a government job, receiving medical treatment at the district's hospital, getting public primary school teachers to care more for one's child, and when obtaining a construction permit, as well as to what extent government officials are diverting public funds for personal benefits. These measures are standardized and used in Figure 3 and Table 3. To create a summary index using these six perception measures, we follow Anderson (2008) to construct a weighted index using the inverted covariance matrix of all perception measures.

To test the hypothesis that more educated cohorts may denounce government officials since they have the capacity and understanding, we use this survey question in the PAPI data, "Did you or your family member attempt to denounce this act of corruption?" We create an indicator variable that is equal to 1 if the answer is yes. Panel (a) of Figure 5 only looks at the sub-sample of respondents who reported they

were personally affected or their family members were affected by the act of bribe-taking, while panel (b) imputes the indicator variable to be 0 for everyone else who is unaffected, and hence look at the entire population in the districts, unconditional on being affected or not.

To examine the effect of university expansion on household income, we use the PAPI data's survey question: "What is your household monthly income on average?" We take the mid-value of the income brackets selected by the survey participants. The outcome variable is denominated in a million Vietnamese dong (VND).

We also examine detailed interactions with government procedures and government-provided services to investigate the potential mechanism that more educated individuals may be more likely to have a certain interaction with government bureaucrats where bribes are common. We leverage questions in the PAPI data that ask specifically about the interaction with the government. For example, the PAPI survey asks the respondent to indicate "yes" or "not" to the statements "I/my family don't have to pay bribes to obtain the service" and "Did you hire a middleman/facilitator in order to obtain this service for you?" when prompted with the most recent experience with the public certification service. The survey asks about four specific government procedures: applying for a public certification, applying for a construction permit, applying for land use rights certificate, and other administrative procedures such as obtaining state subsidies. The PAPI data does not ask about if a middleman was used for government services of public hospitals and public primary schools. We then create two indicators which equal to 1 if the survey participant paid extra informal charges for any of the 4 government procedures or bribe for any of the 2 government services. If not, the indicators will be zero. In panels (a) and (c) of Figure 4, we focus on the sub-sample of the population that used any of the related government procedures or services. In panels (b) and (d) we impute the indicator variables to be

0 for everyone else who did not have any interaction with the government, and hence we look at the entire population in the districts, taking into account composition changes in the usage.

B Robustness Using Stacked Regression

As discussed in Section 3, our baseline two-way fixed effects model might suffer from negative weight issues given the variations in university opening dates across different districts. We present a stacked difference-in-differences model similar to that in Cengiz et al. (2019) and Vu (2023) to show that our finding is robust after addressing this issue.

The stacked design creates sub-datasets with only districts in G_g (treatment group with university opening year g) and the never-treated districts (control group):

$$y^G_{i,d,c} = \sum_c \delta^G_c * (T^G_{i,d} * Cohort^G_{i,d,c}) + \gamma_d + \eta_a + \theta_t + \epsilon_{i,d,c} \tag{2}$$

where $y^G_{i,d,c}$ denotes the outcome of individual i in district d of cohort c; $T^G_{i,d}$ indicates whether district d had the first university during the expansion; $Cohort^G_{i,d,c}$ indicates the age cohort of individual i. District and age fixed effects are γ_d and η_a for this specific sub-dataset with university opening year g. $\delta^{G,t}$ captures the treatment effect of one specific sub-dataset. One can then aggregate δ^G across all G to obtain the weighted average effect for different groups of districts.

Therefore, alternatively, we can combine all sub-datasets s and estimate the following model:

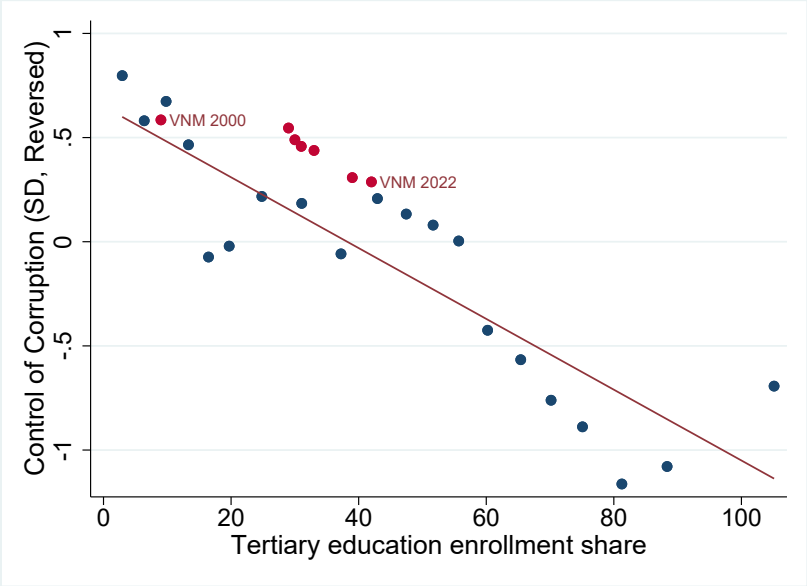
$$y_{i,d,c,s} = \sum_c \delta_c * (T_{i,d,s} * Exposed_{i,d,c,s}) + \gamma_{d,s} + \eta_{a,s} + \theta_{t,s} + \epsilon_{i,d,c,s} \tag{3}$$

where the fixed effects further interact with the sub-dataset label. This approach proposed by Cengiz et al. (2019) enables a clean comparison between treatment group G with a never-treated control group in each given year, thus circumventing the problem of negative weights mentioned in the literature.

Using this approach, we report the impacts of university expansion on the share of the population with some university education in Appendix Figure A.4 and on the incidences of corruption exposure in Appendix Figure A.5 and they are very similar to results from the baseline model.

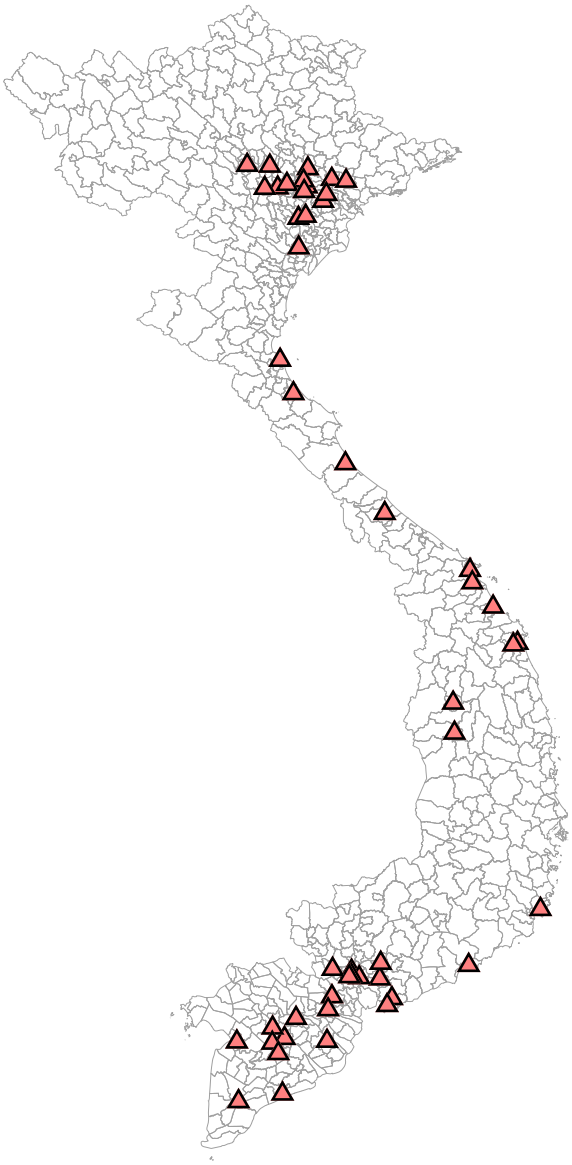
C Appendix Figures

Figure A.1: Cross-national correlation between corruption and higher education



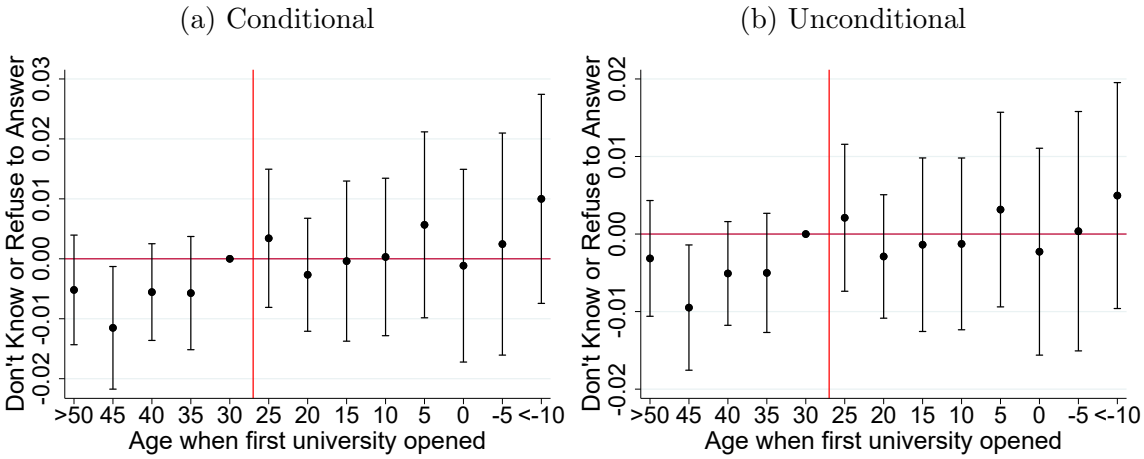
Note: This figure is a binned scatter plot based on two World Bank data series of tertiary education enrollment (% gross) and "Control of Corruption" for 180 countries from 2000 to 2022. The gross enrollment ratio is the ratio of total enrollment, regardless of age, to the population of the age group that officially corresponds to the level of education. The control of corruption variable captures perceptions of the extent to which public power is exercised for private gain, including both petty and grand forms of corruption, as well as "capture" of the state by elites and private interests, in units of standard deviations. The correlation is -0.502. The slope is -0.017, meaning that an increase of one percentage point in the share of tertiary enrollment is associated with a 0.017 standard deviation decrease in the corruption measure. Observations for Vietnam are highlighted in red. See discussion in Section 1.

Figure A.2: Locations of New Universities Established During the Expansion



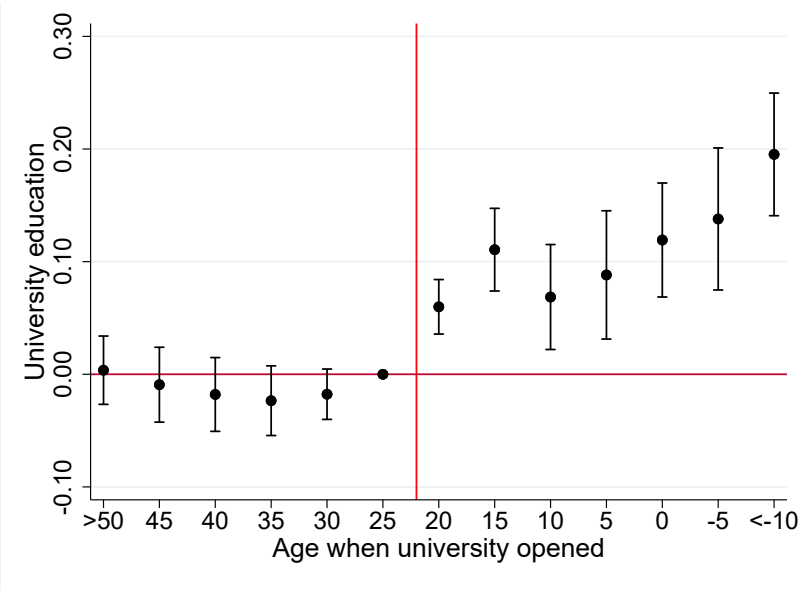
Note: This map displays districts that had a new university established during the higher education expansion. See discussion in Section 2.2.

Figure A.3: Effect of university expansion on answering bribe questions with don't know or refuse to answer



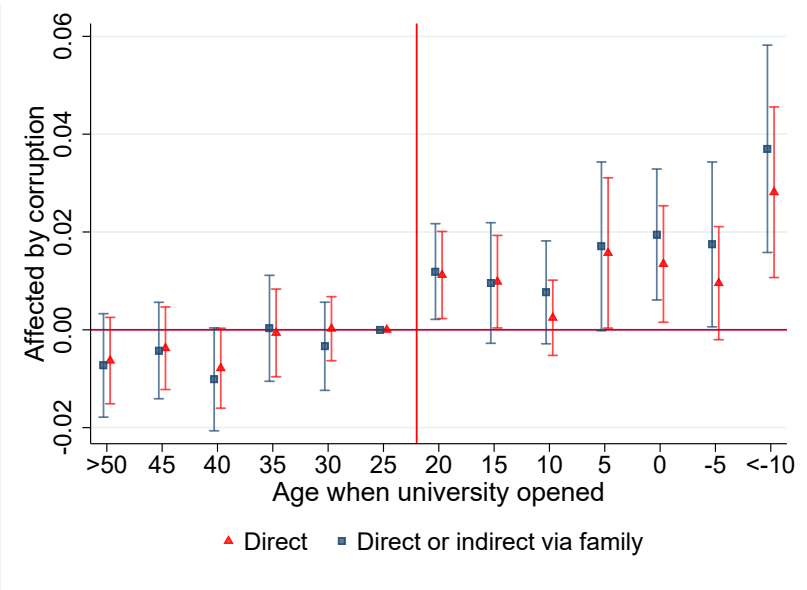
Note: We use equation 1 to obtain the event study estimates at the individual level. The outcome variable is an indicator variable if an individual responds to any of the bribing questions with "Don't know" or "Refuse to answer". A bribe action may occur when interacting with a government procedure or a government service. A government procedure could be an application for a certification, construction permit, land use rights, or other administrative procedures. A government service could be a treatment at the local hospital or having a child who attends the local public primary school. 95% confidence intervals are constructed with standard errors clustered at the district level. See discussion in Section 4.2.

Figure A.4: Stacked regression estimating the effect of university expansion on university education



Note: We use the stacked difference-in-differences equation 3 to obtain the event study estimates. The outcome variable is an indicator of being university-educated. The mean of the outcome variable in districts that never have a university is 0.112. The number of observations is 4,100,899. The question is, "What is your highest level of education?" 95% confidence intervals are constructed with standard errors clustered at the district level. The event study estimates from the baseline two-way fixed effects model are reported in Figure 1. See discussion in Appendix Section B.

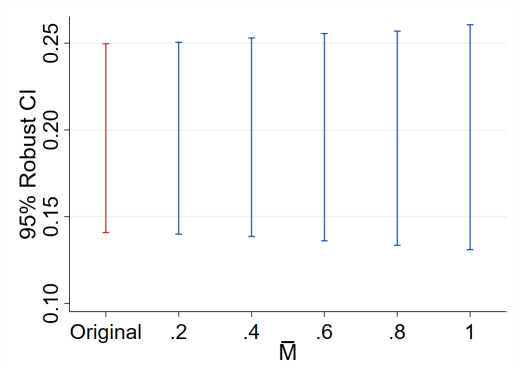
Figure A.5: Stacked regression estimating the effect of university expansion on whether respondent was affected by bribe-taking



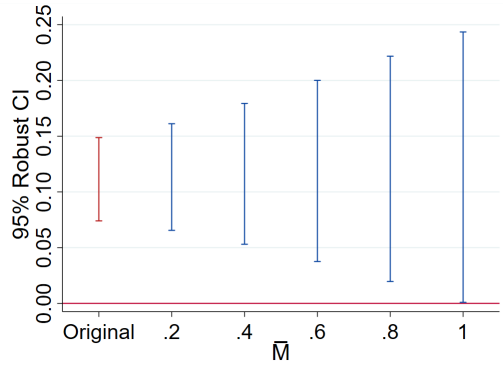
Note: We use the stacked difference-in-differences equation 3 to obtain the event study estimates. The outcome variable "Direct" is an indicator of being personally affected by an act of bribe-taking (N=4,046,498) and "Indirect" is an indicator of being personally affected or having other family members affected (N=4,046,498). The mean of the former in districts that never have a university is 0.015 and the mean of the latter is 0.027. The PAPI survey question is "Have you or anyone in your household been affected by an act of bribe-taking by a government official in the past year? Consider all types of bribe-giving ranging from giving a small bribe to a traffic official to giving a commission in order to get a government contract." 95% confidence intervals are constructed with standard errors clustered at the district level. The event study estimates from the baseline two-way fixed effects model are reported in Figure 2. See discussion in Appendix Section B.

Figure A.6: Sensitivity to violations to the parallel trends assumption

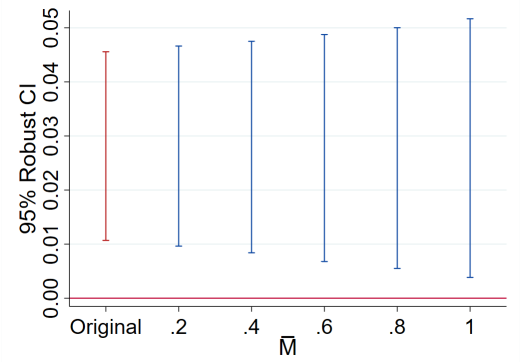
(a) University Education (First Period)



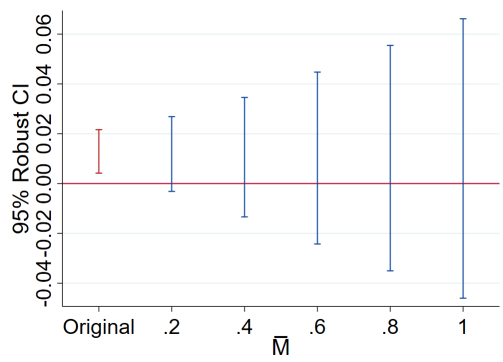
(b) University Education (All Periods)



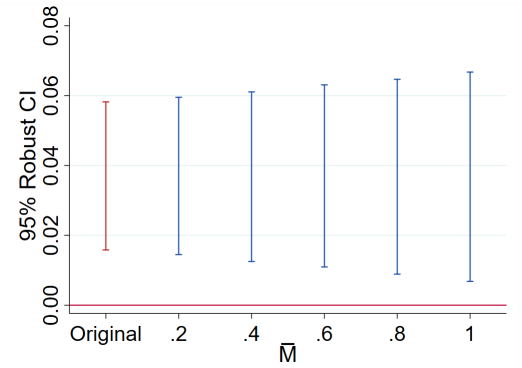
(c) Directly affected (First Period)



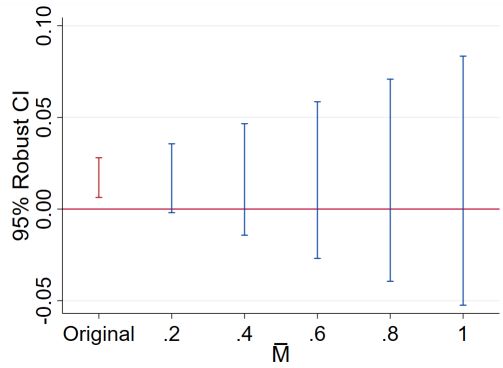
(d) Directly affected (All Periods)



(e) Directly/Indirectly affected (First Period)



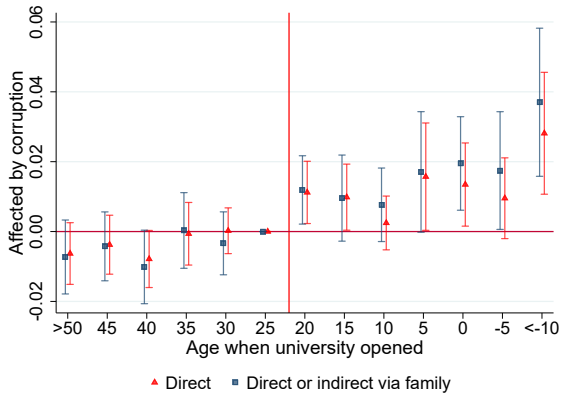
(f) Directly/Indirectly affected (All Periods)



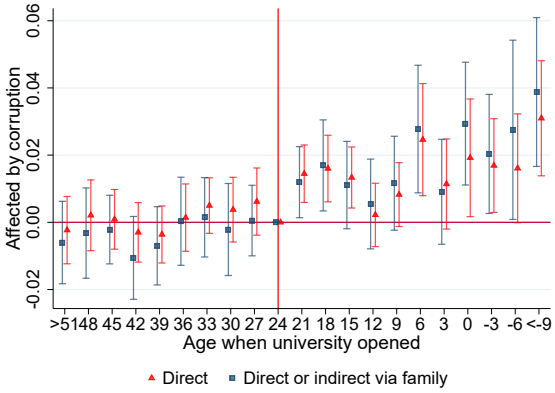
Note: This Figure shows confidence intervals robust to limited violations of the parallel trends assumption following Rambachan and Roth (2023). $\bar{M}$ indicates the extent of the post-treatment parallel trends assumption. $\bar{M} = 1$ allows for the post-treatment parallel trends assumption to be violated by to the same extent in each period as the largest violation in the pre-treatment period. Using equation 3, panels (a), (c), (e) test the effect in the first post-period. Panels (b), (d), (f) test the average effect for all post-periods. See discussion in Section 4.3.

Figure A.7: Different age bins for the effect of university expansion on affected by bribe-taking

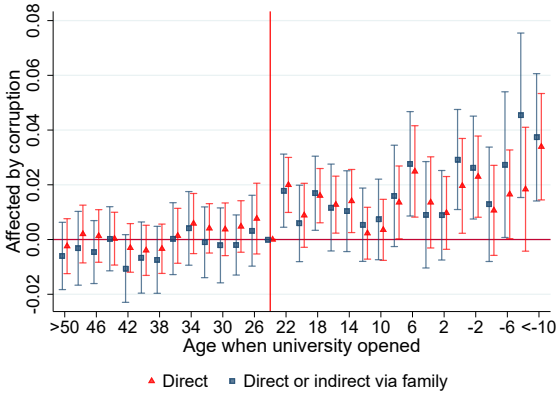
(a) Age bin of five



(b) Age bin of three

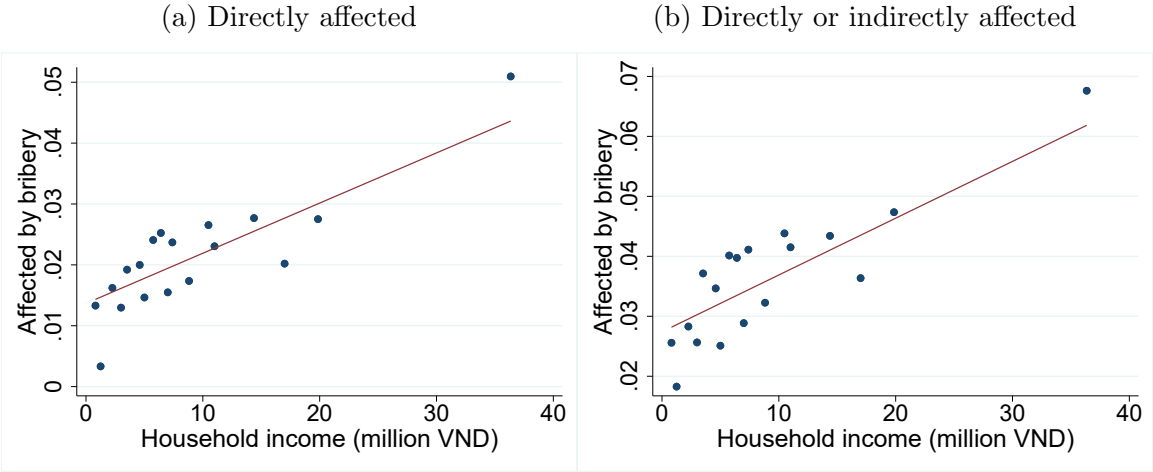


(c) Age bin of two



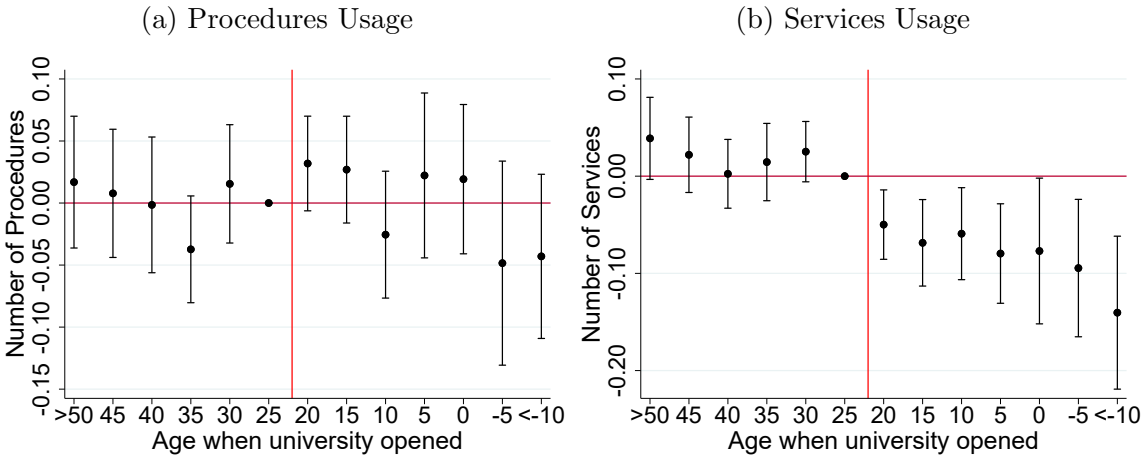
Note: We select different age bins to assess the robustness of our estimates for being affected by bribe-taking acts. In our main analysis, we use age bin of five and omit the age group of 23-27 as the reference group (see discussion in Section 3). In panel b, we use age bin of three and omit the age group of 23-25 as the reference group. In panel c, we use age bin of two and omit the age group of 24-25 as the reference group. See discussion in Section 4.3.

Figure A.8: Relationship between affected by bribe-taking and household income



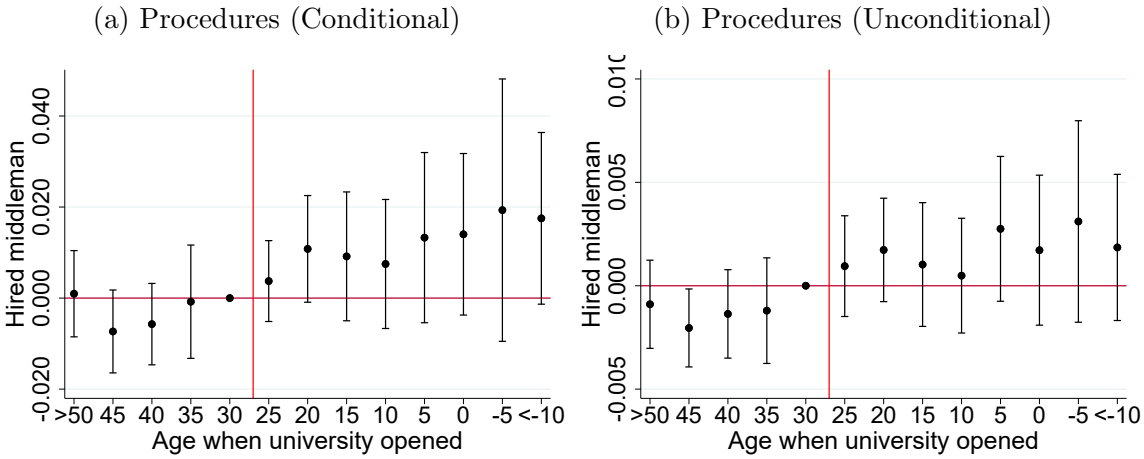
Note: The binscatter plots present the positive correlation between corruption exposure and household monthly income, which are created in the same manner as in Figure 2 and Figure 6 respectively. The OLS regression line was constructed using individual-level observations. See discussion in Section 5.

Figure A.9: Effect of university expansion on applying for government procedures and services



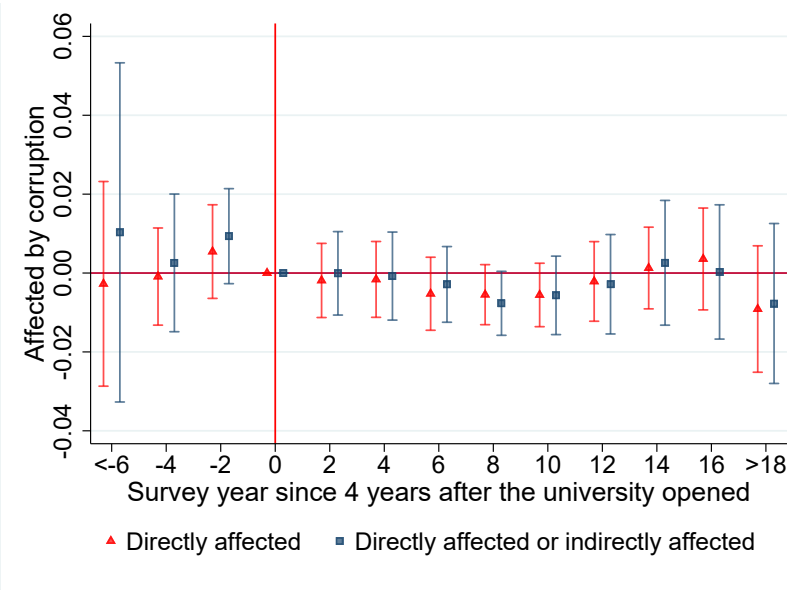
Note: We use equation 1 to obtain the event study estimates for the number of times applying for government procedures and services at the individual level. The outcome variables of panel a and panel b are the number of procedures and services used respectively. The procedures and services are the same as those in Figure 4. A government procedure could be an application for a certification, construction permit, land use rights, or other administrative procedures. The government services are public primary schools or public hospitals. 95% confidence intervals are constructed with standard errors clustered at the district level. The difference-in-differences point estimate for all cohorts is reported in Appendix Table A.7. See discussion in Section 5.

Figure A.10: Effect of university expansion on paying a middleman to facilitate government procedure



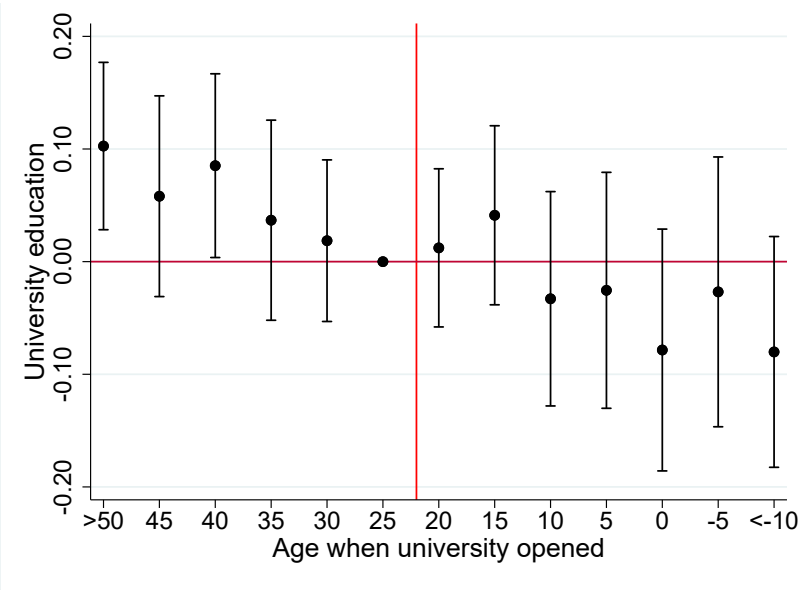
Note: We use equation 1 to obtain the event study estimates for paying a middleman to facilitate a government procedure at the individual level. The outcome variable is an indicator variable if an individual hired a middleman for a government procedure. A government procedure could be an application for a certification, construction permit, land use rights, or other administrative procedures. 95% confidence intervals are constructed with standard errors clustered at the district level. The question is, "Did you hire a middleman/facilitator in order to obtain this service for you?". The difference-in-differences point estimate for all cohorts is reported in Appendix Table A.9. See discussion in Section 5.

Figure A.11: Effect of university expansion on whether affected by bribe-taking at the district level



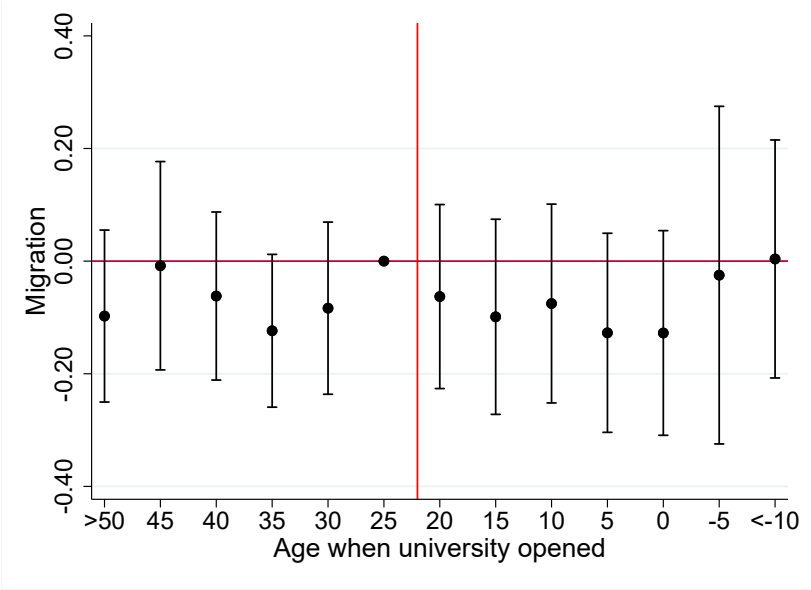
Note: Instead of using cohort difference-in-differences, Figure A.11 replaces cohort group dummies with survey year dummies in equation 1 and only keeps district and year fixed effects without age fixed effects. The observation numbers are 169,100 for those directly affected and 169,100 for those indirectly affected. 95% confidence intervals are constructed with standard errors clustered at the district level. See discussion in Section 5.

Figure A.12: Effect of university expansion on the education level of the government officials



Note: Figure A.12 shows the effect of a cohort being exposed to a university opening on the share having a university education for a subsample of government officials. We use equation 1 to obtain the estimates for each cohort. The outcome variable is an indicator of being university-educated. 95% confidence intervals are constructed with standard errors clustered at the district level. See discussion in Section 5.

Figure A.13: Effect of university expansion on sample composition of migrants



Note: Figure A.13 shows the effect of being exposed to a university opening on the share of the migrant population. We use equation 1 to obtain the estimates for each cohort. The outcome variable is an indicator of being a migrant. The outcome variable is equal to 1 if the permanent resident registration is not in the surveyed commune. The mean of the outcome variable in districts that never have a university is 0.117. 95% confidence intervals are constructed with standard errors clustered at the district level. See discussion in Section 5.

D Appendix Tables

Table A.1: The effect of university expansion on corruption perception

	Perception Index		
	(1)	(2)	(3)
University Expansion	0.049***	0.051***	0.049**
	(0.019)	(0.019)	(0.019)
Observations	163,800	163,800	163,800
Cluster No.	320	320	320
Adjusted R2	0.061	0.062	0.063
Mean of Never Treated	-0.026	-0.026	-0.026
District FE	✓	✓	✓
Year FE	✓	✓	✓
Age FE	✓	✓	✓
Demographic Controls		✓	✓
Full Controls			✓

Note: Appendix Table A.1 shows that the university expansion significantly increases the perception of corruption among younger cohorts. We combine the age cohort dummies in Equation 1 into an independent variable "University Expansion" which equals 1 if the survey participant aged below 25 at the time when the university opened and 0 otherwise. The perception index is the same as that in Table 3, capturing individual corruption perception towards "land title", "government job", "medical treatment", "have children better attended", "public funds", and "construction permit". We detail its construction in Section A.5. Demographic controls include gender and ethnicity and full controls additionally include an indicator for party membership, an indicator for working for the government, and an indicator for being a migrant. Standard errors are clustered at the district level. See discussion in Section 4.2.

Table A.2: The effect of university expansion on paying informal changes

	Procedure			Service		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Conditional						
University	0.0041	0.0049	0.0043	0.0025	0.0030	0.0028
Expansion	(0.0050)	(0.0050)	(0.0049)	(0.0056)	(0.0057)	(0.0056)
Observations	100,464	100,464	100,464	85,169	85,169	85,169
Cluster No.	320	320	320	320	320	320
Adjusted R2	0.014	0.015	0.017	0.021	0.021	0.022
Never T. Mean	0.105	0.105	0.105	0.092	0.092	0.092
Panel B: Unconditional						
University	0.0014	0.0014	0.0014	0.0003	0.0004	0.0004
Expansion	(0.0013)	(0.0013)	(0.0013)	(0.0022)	(0.0022)	(0.0022)
Observations	171,240	171,240	171,240	171,240	171,240	171,240
Cluster No.	320	320	320	320	320	320
Adjusted R2	0.013	0.013	0.013	0.026	0.027	0.027
Never T. Mean	0.023	0.023	0.023	0.028	0.028	0.028
District FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓
Demo. Controls		✓	✓		✓	✓
Full Controls			✓			✓

Note: Appendix Table A.2 shows that the university expansion has no significant impact on younger cohorts' paying informal charges for government procedures or services if we condition on the age of the individuals. We combine the age cohort dummies in Equation 1 into an independent variable "University Expansion" which equals 1 if the survey participant aged below 30 at the time when the university opened and 0 otherwise. Panel (a) is conditional on using the government procedures or services and panel (b) does not condition on the usage. The outcome variables are the same as those in Figure 4. Demographic controls include gender and ethnicity and full controls additionally include an indicator for party membership, an indicator for working for the government, and an indicator for being a migrant. Standard errors are clustered at the district level. See discussion in Section 4.2.

Table A.3: The effect of university expansion on the bribery measured by list experiments

	(1) Land	(2) Medical	(3) Kids
University Expansion	0.020	0.069	-0.021
	(0.067)	(0.042)	(0.046)
Observations	12,712	55,007	27,481
Adjusted R2	0.038	0.063	0.101
Mean of Never treated	0.210	0.104	0.087
District FE	✓	✓	✓
Year FE	✓	✓	✓
Age FE	✓	✓	✓

Note: Appendix Table A.4 shows that the university expansion has no precise impact on bribe behaviors elicited through the list experiment. The list experiment elicits the number of the listed action items that a person did while interacting with government procedures or services. In one of the two list versions, there is an additional item related to "informal payment" which is randomly shown to half of the respondents. As the respondents only report the total number of items, they do not need to admit to paying a bribe. Although the list experiment has the benefit of reducing social desirability bias, the sample is restricted to individuals who had specific government interactions. Standard errors are clustered at the district level. See discussion in Section 4.2.

Table A.4: The effect of university expansion on the bribery measured by list experiments

	(1) Land	(2) Medical	(3) Kids
University Expansion	0.020	0.069	-0.021
	(0.067)	(0.042)	(0.046)
Observations	20,378	54,997	27,831
Adjusted R2	0.038	0.063	0.101
Mean of Never treated	0.210	0.104	0.087
District FE	✓	✓	✓
Year FE	✓	✓	✓
Age FE	✓	✓	✓

Note: Appendix Table A.4 shows that the university expansion has no precise impact on bribe behaviors elicited through the list experiment. The list experiment elicits the number of the listed action items that a person did while interacting with government procedures or services. In one of the two list versions, there is an additional item related to "informal payment" which is randomly shown to half of the respondents. As the respondents only report the total number of items, the respondents do not need to admit to paying a bribe. Although the list experiment has the benefit of reducing social desirability bias, the sample is restricted to individuals who had specific government interactions. Standard errors are clustered at the district level. See discussion in Section 4.2.

Table A.5: The effect of university expansion on knowledge about anti-corruption legislation

	Knowledge of Anti-corruption		
	(1)	(2)	(3)
University Expansion	0.012	0.006	0.013*
	(0.008)	(0.008)	(0.007)
Observations	170,670	170,670	170,670
Cluster No.	320	320	320
Adjusted R2	0.046	0.075	0.130
Mean of Never Treated	0.484	0.484	0.484
District FE	✓	✓	✓
Year FE	✓	✓	✓
Age FE	✓	✓	✓
Demographic Controls		✓	✓
Full Controls			✓

Note: Appendix Table A.5 shows that university expansion increases the knowledge of anti-corruption legislation. We combine the age cohort dummies in Equation 1 into an independent variable "University Expansion" which equals 1 if the survey participant aged below 25 at the time when the university opened and 0 otherwise. The outcome variable is an indicator that is equal to one if the survey respondent heard about the Law on Corruption Prevention – Anti Corruption and zero otherwise. Demographic controls include gender and ethnicity and full controls additionally include an indicator for party membership, an indicator for working for the government, and an indicator for being a migrant. Standard errors are clustered at the district level. See discussion in Section 5.

Table A.6: The effect of university expansion on denunciation

	Denounce		
	(1)	(2)	(3)
Panel A: Conditional			
University Expansion	-0.006	-0.006	-0.006
	(0.013)	(0.013)	(0.013)
Observations	5,092	5,092	5,092
Cluster No.	268	268	268
Adjusted R2	0.030	0.030	0.031
Mean of Never Treated	0.070	0.070	0.070
Panel B: Unconditional			
University Expansion	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)
Observations	171,240	171,240	171,240
Cluster No.	320	320	320
Adjusted R2	0.001	0.001	0.001
Mean of Never Treated	0.002	0.002	0.002
District FE	✓	✓	✓
Year FE	✓	✓	✓
Age FE	✓	✓	✓
Demographic Controls		✓	✓
Full Controls			✓

Note: Appendix Table A.6 shows that the university expansion has no significant impact on the denunciation actions of the exposed younger cohorts. We combine the age cohort dummies in Equation 1 into an independent variable "University Expansion" which equals 1 if the survey participant aged below 25 at the time when the university opened and 0 otherwise. Panel (a) is conditional on being affected directly or indirectly by the bribe-taking acts and panel (b) does not condition on being affected or not. The outcome variables are the same as those in Figure 5. Demographic controls include gender and ethnicity and full controls additionally include an indicator for party membership, an indicator for working for the government, and an indicator for being a migrant. Standard errors are clustered at the district level. See discussion in Section 5.

Table A.7: The effect of university expansion on the number of government procedures and services respondents applied to

	Number of Procedure			Number of Service		
	(1)	(2)	(3)	(4)	(5)	(6)
University Expansion	0.0214	0.0148	0.0220	-0.0572***	-0.0575***	-0.0554***
	(0.0149)	(0.0152)	(0.0152)	(0.0174)	(0.0173)	(0.0172)
Observations	171,240	171,240	171,240	171,240	171,240	171,240
Cluster No.	320	320	320	320	320	320
Adjusted R2	0.046	0.056	0.070	0.069	0.069	0.071
Never T. Mean	0.861	0.861	0.861	0.731	0.731	0.731
District FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓
Demo. Controls		✓	✓		✓	✓
Full Controls			✓			✓

Note: Appendix Table A.7 shows that the university expansion does not increase the usage of government procedures significantly and decreases the usage of public hospitals and primary schools. The outcome variables of columns 1-3 and columns 4-6 are the numbers of procedures and services used respectively. The procedures and services are the same as those in Figure 4. We combine the age cohort dummies in Equation 1 into an independent variable "University Expansion" which equals 1 if the survey participant aged below 25 at the time when the university opened and 0 otherwise. The outcome variables are the same as those in Appendix Figure A.9. Demographic controls include gender and ethnicity and full controls additionally include an indicator for party membership, an indicator for working for the government, and an indicator for being a migrant. Standard errors are clustered at the district level. See discussion in Section 5.

Table A.8: The effect of university expansion on household income

	Household Income		
	(1)	(2)	(3)
University Expansion	1.087***	1.010***	1.038***
	(0.273)	(0.270)	(0.272)
Observations	44,549	44,549	44,549
Cluster No.	208	208	208
Adjusted R2	0.168	0.177	0.200
Mean of Never Treated	7.535	7.535	7.535
District FE	✓	✓	✓
Year FE	✓	✓	✓
Age FE	✓	✓	✓
Demographic Controls		✓	✓
Full Controls			✓

Note: Appendix Table A.8 shows that the university expansion increases the average household monthly income by 1.09 million VND. This is a 14.4% increase, relative to the mean of districts that never had a university opened. We combine the age cohort dummies in Equation 1 into an independent variable "University Expansion" which equals 1 if the survey participant aged below 30 at the time when the university opened and 0 otherwise. The outcome variable is the same as that in Figure 6. Demographic controls include gender and ethnicity and full controls additionally include an indicator for party membership, an indicator for working for the government, and an indicator for being a migrant. Standard errors are clustered at the district level. See discussion in Section 5.

Table A.9: The effect of university expansion on hiring a middleman to facilitate a government procedure

	Payment to Middleman		
	(1)	(2)	(3)
Panel A: Conditional			
University Expansion	0.0094***	0.0096***	0.0094***
	(0.0036)	(0.0036)	(0.0036)
Observations	83,959	83,959	83,959
Cluster No.	209	209	209
Adjusted R2	0.009	0.009	0.009
Mean of Never Treated	0.033	0.033	0.033
Panel B: Unconditional			
University Expansion	0.0020**	0.0020**	0.0020**
	(0.0008)	(0.0008)	(0.0008)
Observations	171,240	171,240	171,240
Cluster No.	320	320	320
Adjusted R2	0.011	0.011	0.011
Mean of Never Treated	0.006	0.006	0.006
District FE	✓	✓	✓
Year FE	✓	✓	✓
Age FE	✓	✓	✓
Demographic Controls		✓	✓
Full Controls			✓

Note: Appendix Table A.9 Panel A shows that the university expansion increases the likelihood of hiring a middleman by 28.5% to facilitate a government procedure, conditional on having interacted with the government procedure. Panel B reports the unconditional version. The outcome variable is the same as that in Figure A.10. We combine the age cohort dummies in Equation 1 into an independent variable "University Expansion" which equals 1 if the survey participant aged below 30 at the time when the university opened and 0 otherwise. Standard errors are clustered at the district level. See discussion in Section 5.

Table A.10: The effect of university expansion before and after the anti-corruption campaign

	(1) Uni Edu	(2) Knowledge	(3) Denounce	(4) Direct	(5) Indirect
University	0.0627***	0.0026	-0.0344	0.0088*	0.0140**
Expansion	(0.0134)	(0.0107)	(0.0258)	(0.0046)	(0.0059)
University	0.0083	0.0111	0.0512*	-0.0010	-0.0040
Expansion × Post 2016	(0.0150)	(0.0115)	(0.0291)	(0.0050)	(0.0065)
Observations	171,159	170,593	5,078	169,028	169,028
Cluster No.	320	320	268	320	320
Adjusted R2	0.114	0.049	0.041	0.016	0.021
Mean of Never Treated	0.112	0.484	0.070	0.015	0.027
District FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓

Note: Appendix Table A.10 shows that the university expansion has similar effects on education, our main corruption measures, and the knowledge about the Law on Corruption Prevention before and after the anti-corruption campaign. After the anti-corruption campaign, there is suggestive evidence that the younger cohorts are more likely to denounce the corrupt behaviors. The outcome variables in columns 1 to 4 are the same as those in Figure 1, Figure 2, and Figure 5 panel (a). The outcome variable in column 5 is an indicator variable if the survey respondent has heard about the Law on Corruption Prevention. We combine the age cohort dummies in Equation 1 into an independent variable "University Expansion" which equals 1 if the survey participant aged below 25 at the time when the university opened and 0 otherwise. We further interact this indicator variable and fixed effects with the indicator variable which equals 1 if the study period is after the anti-corruption campaign. Standard errors are clustered at the district level. See discussion in Section 5.

Table A.11: The effect of university expansion before and after the anti-corruption campaign

		Conditional		Unconditional	
	Perception	Procedure	Service	Procedure	Service
	(1)	(2)	(3)	(4)	(5)
University	-0.0025	-0.0015	-0.0128	-0.0004	-0.0023
Expansion	(0.0251)	(0.0080)	(0.0137)	(0.0025)	(0.0029)
University	0.0801***	0.0022	0.0146	0.0012	0.0003
Expansion × Post 2016	(0.0284)	(0.0092)	(0.0149)	(0.0028)	(0.0038)
Observations	163,739	100,431	85,146	171,159	171,159
Cluster No.	320	320	320	320	320
Adjusted R2	0.066	0.018	0.024	0.015	0.028
Mean of Never Treated	-0.026	0.105	0.092	0.023	0.028
District FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓

Note: Appendix Table A.11 shows that the university expansion increases the perception of corruption significantly after the anti-corruption campaign. It also provides suggestive evidence that the university expansion increases informal payments after the anti-corruption campaign albeit none of the results on informal payments are significant. The outcome variable 'Perception' in column 1 is a summary index, the same as that in Appendix Table A.1 and the outcome variables from columns 2 to 5 are the same as those in Appendix Table A.2. We combine the age cohort dummies in Equation 1 into an independent variable "University Expansion" which equals 1 if the survey participant aged below 25 at the time when the university opened and 0 otherwise. We further interact this indicator variable and fixed effects with the indicator variable which equals 1 if the study period is after the anti-corruption campaign. Standard errors are clustered at the district level. See discussion in Section 5.