Digital Footprints and Policing: The Socioeconomic Impacts of Mobile Payment Reform on China's Sex Industry

Ting Chen Zhaoguang Wang Yifan Zhang *

August 31, 2025

Abstract

This paper examines China's 2016 real-name reform—which mandated ID verification for mobile payments and granted police access to transaction records—on law enforcement. Using a continuous difference-in-differences design exploiting prereform mobile-payment adoption across prefectures, we find the reform increased sex-trade arrests, especially those involving mobile payments. Triple-difference estimates show stronger effects in regions with higher male-to-female ratios. While enforcement efficiency improved, the socioeconomic consequences were mixed: Sexual violence rose, sexually transmitted infections fell, divorces linked to infidelity increased, and domestic violence incidents became more frequent. Our findings show how technology-enabled enforcement reshapes illegal markets while generating unintended social costs.

Keywords: Law enforcement; Mobile payment; Sex industry; Sexual violence

JEL codes: I18, J40, K42

*Ting Chen: Department of Economics, Hong Kong Baptist University, tingchen@hkbu.edu.hk; Zhaoguang Wang: Department of Economics, The Chinese University of Hong Kong, zgwang@link.cuhk.edu.hk; Yifan Zhang: Department of Economics, The Chinese University of Hong Kong, yifan.zhang@cuhk.edu.hk.

1 Introduction

The sex industry—one of the oldest in human history—has long been the subject of contentious policy debates. While prior research has examined the effects of formal legal reforms (Cunningham and Shah, 2021), far less attention has been paid to how improvements in enforcement capacity affect the industry. In many countries, even where the sex trade is criminalized, authorities struggle to enforce laws effectively. Recent technological advances—such as DNA databases for crime-solving and IT systems to optimize patrol strategies—have substantially increased the cost-effectiveness and reach of law enforcement (Doleac, 2017; Mastrobuoni, 2020). This paper studies a new channel: how access to mobile payment transaction data enhances police enforcement within the sex industry and can have unintended consequences for sexual violence, public health, marriage stability, and domestic violence.

This paper examines a particularly powerful enforcement channel: digital payment surveillance. Mobile payment data are uniquely effective for policing the sex industry because sexual transactions share three structural features that make them especially susceptible to digital detection. First, commercial sex work almost always involves monetary exchange, unlike many illegal activities that can be carried out through barter or informal networks. Second, the industry's shift toward online platforms and private venues has increased the reliance on mobile payments for discretion and convenience. Third, these transactions often exhibit distinctive, verifiable patterns—such as recurring payments to the same individuals or transfers at specific times and locations—that are difficult to disguise and readily distinguishable from legitimate commerce when analyzed systematically.

Enhanced enforcement can produce ambiguous social consequences. On the one hand, restricting access to commercial sex may increase sexual violence if commercial and noncommercial encounters are substitutes (Bisschop et al., 2017; Cunningham and Shah, 2018). On the other hand, targeted crackdowns can affect public health by changing higher-risk services and sexually transmitted infections (STIs) (Gertler and Shah, 2011; Cameron et al., 2021). The effects on marriage stability and domestic violence are also uncertain: Eliminating commercial-sex-based infidelity could strengthen marriages, but replacing it with longer-term affairs may be more damaging to marital stability. This setting offers us a rare opportunity to measure these mechanisms within the same market and use detailed data to disentangle their relative contributions.

China provides an ideal setting for this analysis. First, although prostitution has been illegal since the establishment of the People's Republic, the country is home to one of the world's largest sex industries, with UNAIDS estimating that 4.5 million sex workers were active in the country in 2006.[1] Second, China is the global leader in mobile payment adoption, home to 911 million of the world's 1.75 billion mobile payment accounts in 2022 (PCAC, 2023; Raithatha and Storchi, 2024), which facilitates a highly digitalized illegal sex trade. Most importantly, in July 2016, with the initial aim of improving financial security and ensuring payment transparency, China passed a *real-name* reform for mobile payments, requiring ID verification and explicitly granting law enforcement access to digital transaction data for criminal investigations. This policy change significantly reduced the cost of detecting and verifying illegal sex transactions, leading to a substantial and exogenous increase in enforcement capacity for crimes conducted through digital transactions. This shift provides a natural experiment framework for identifying causal effects. Our empirical strategy exploits cross-sectional variation in the prereform intensity of mobile payment adoption as a proxy for treatment exposure, combined with the temporal discontinuity introduced by the 2016 reform. This approach is credible because mobile payment adoption was driven primarily by commercial and technological factors—such as urban development, internet infrastructure, and consumer preferences—rather than sex industry enforcement priorities. To measure adoption intensity, we use the Digital Financial Inclusion Index of China (DFIIC), developed through collaboration between Peking University and Ant Group on the basis of billions of payment records from Alipay. We use this index as an intent-to-treat measure in a continuous difference-in-differences (DID) framework to identify the reform's effects. To address potential endogeneity concerns related to adoption intensity, we augment the analysis with a triple difference (DDD) design that leverages variation in the sex ratio, a proxy for regional differences in demand for commercial sex.

Our analysis relies on a comprehensive dataset of Chinese law enforcement activities constructed from two complementary sources. We systematically collect administrative penalty records from *Pkulaw.com* to create the *China Administrative*

[1]Report by aidsdatahub.org, "Sex Work HIV China" https://www.aidsdatahub.org/sites/default/files/resource/sex-work-hiv-china.pdf.

Penalty (CAP) database, capturing sanctions for acts below the threshold of criminality that police impose and publicly disclose. For criminal violations, we gather court verdicts from *China Judgements Online (CJO)*. Together, these sources provide broad coverage of sex industry enforcement activities—our primary outcome of interest. We apply identical data collection methods for sexual violence, domestic violence, and human trafficking to test for unintended consequences and for theft and robbery in placebo tests. Our dataset spans 2014–2019, encompassing crucial periods both before and after the 2016 reform implementation, and covers all prefecture-level jurisdictions in China.

Our empirical analysis begins by testing whether digital payment surveillance strengthened enforcement against the sex industry. We document that the real-name reform substantially increased sex industry arrests in high-exposure prefectures. Direct evidence shows that the proportion of mobile-payment-based arrests increased substantially in these regions, confirming that digital payment surveillance reduced tracking and verification costs. The DDD strategy yields similar results, with higher ratios of men to women amplifying the reform's impact. Placebo tests using theft and robbery arrests show no significant effects, ruling out changes in overall police capacity as an alternative explanation. The results are robust to our (i) using alternative measures of mobile payment adoption, including binary treatment and different DFIIC metrics, (ii) accounting for WeChat Pay adoption, (iii) excluding key cities such as Dongguan, Hangzhou, and Shenzhen, (iv) controlling for temporal trends in reporting, and (v) using the CAP and CJO samples separately. Taken together, these findings show that mobile payment technology substantially improved enforcement capacity in China's sex industry.

We find further evidence of improvements to enforcement efficiency. The reform increased arrests in high-exposure prefectures for cases transacted at private venues (e.g., residential buildings, as opposed to public venues such as massage parlors and nightclubs)—a type of case previously difficult for police to detect and prosecute. Mobile payment records also enabled more accurate estimates of transaction values, leading to steeper fines and longer detention periods for offenders in affected regions. Market indicators confirm industry contraction, with sex service prices rising postreform as supply decreased amid sustained demand.

Next, we document significant but mixed unintended social consequences. The prefectures more affected by the reform experienced substantial increases in sexual violence against women, including both sexual harassment and rape. The DDD estimation shows that such effects were amplified in prefectures with higher male-to-female sex ratios. We attribute these patterns to unmet demand for commercial sex as the industry contracted, which may have led to substitution toward noncommercial encounters. We find, in support of this interpretation, that the rise in violence is concentrated among women outside the sex industry, with no significant change among sex workers themselves. This finding helps rule out the alternative explanation that sex workers became more vulnerable to violence after being pushed further underground.

We next observe, in contrast to this worsening of sexual violence, improvements in public health outcomes, with significant reductions in HIV and syphilis incidence, likely reflecting more targeted enforcement against higher-risk industry segments. Finally, we document increases in domestic violence incidents and rates of divorce attributed to marital infidelity but no significant changes in human trafficking patterns in high-exposure regions.

This paper contributes mainly to several interconnected strands of literature. First, we advance research on sex industry regulation by introducing the novel perspective of technology-enabled enforcement. While existing work examines formal legal changes such as criminalization, decriminalization, and licensing, our study demonstrates how technological improvements in enforcement capacity can generate effects comparable to formal legal reforms (see Cunningham and Shah, 2021, for a comprehensive summary). Similarly to authors who examine the impact of sex industry (de)criminalization (Bisschop et al., 2017; Cunningham and Shah, 2018; Gao and Petrova, 2022) on outcomes including sexual violence, we confirm that enhanced enforcement leads to increased sexual violence against women, including sexual harassment and rape. Conversely, Cameron et al. (2021) find that criminalization increases STIs among female sex workers because of decreased access to and use of condoms. However, our findings align more closely with those of Gertler and Shah (2011), as we confirm that improved enforcement targeting higher-risk services reduces HIV and syphilis incidence. An area that the literature has examined less is the impact of sex industry regulation on marriage stability. We find that the divorce rate due to marital infidelity increased because digital payment records exposed previously hidden commercial sex purchases and that domestic violence increased. These findings point to a novel channel through which regulation of the sex industry generates negative social impacts.

Second, we contribute to research on technology's transformation of law enforcement by examining digital payment surveillance—an increasingly important but understudied enforcement tool. Previous studies have documented how DNA databases and information systems improve police productivity (Mastrobuoni, 2020; Doleac, 2017; Anker et al., 2021). Our work extends this literature by analyzing a technology that fundamentally alters the cost structure of detecting crimes and is particularly effective in the sex industry. More broadly, our paper is also related to the literature studying the effect of information technology and AI on governance. For example, Beraja et al. (2023) reveal how autocratic governments leverage AI to suppress unrest and maintain political control, while Axbard and Deng (2024) demonstrate how air pollution monitors improved enforcement and reduced pollution. Our paper highlights both the intended enforcement benefits and the unintended social costs of such technological capabilities.

Third, we contribute to the digital economics literature (Goldfarb and Tucker, 2019), particularly in the context of government use of digital platform data for governance. Digital technologies reduce transaction costs, influencing price discrimination, enabling personalized advertising, and shaping reputation systems (Ba and Pavlou, 2002; Athey and Gans, 2010; Fudenberg and Villas-Boas, 2012; Suri, 2017). Recent work shows that mobile payments also lower tracking and verification costs (Goldfarb and Tucker, 2019), which makes them powerful tools for policing digitally mediated crimes. Our findings underscore the political economy implications of mobile payment technology—they enhance enforcement capacity in illegal markets while reinforcing state control—and contribute to debates on digital privacy by showing how the real-name reform improved enforcement effectiveness but imposed substantial social costs.

Finally, our paper relates to a small but growing literature on the dynamics of the sex industry. For example, Cunningham and Kendall (2011a,b) provide groundbreaking evidence on how the internet transformed sex markets. We contribute by showing how mobile payment technology has reshaped both the organization of the sex industry and the enforcement capacities targeting it. In the Chinese context, we offer new evidence on policing in the sex industry, complementing He and Peng (2022) on police complicity in organized prostitution. In contrast to these authors, we focus on reforms that increase enforcement efficiency.

The remainder of this paper proceeds as follows. Section 2 introduces China's digitalized sex industry and the real-name reform for mobile payments. Section 3 describes our data sources and empirical strategies. Section 4 presents the main findings on the reform's impacts on enforcement against the sex industry. Section 5 explores indirect effects on socioeconomic outcomes, including sexual violence, public health, and family stability. Section 6 concludes with policy implications and directions for future research.

2 Background

China provides a unique laboratory for us to examine how digital payment surveillance affects illegal markets. The country simultaneously maintains a strict prohibition on commercial sex work while hosting one of the world's largest sex industries and leads the world in mobile payment adoption. This paradox—strict formal prohibition alongside widespread illegal activity and ubiquitous digital payments—in combination with the 2016 real-name reform, which fundamentally altered surveillance capabilities, creates ideal conditions for identifying the causal effects of new affordances in enforcement technology on the dynamics of illegal markets.

2.1 China's Sex Industry in the Digital Age

Commercial sex work has been illegal in China since 1949. According to the *Criminal Law* and *Public Security Administration Punishments Law*, individuals involved in the sex industry face penalties ranging from fines and detention to imprisonment (see Appendix Table A.1 for detailed legal provisions). Despite this prohibition, the sex industry reemerged during the economic reform period of the 1980s and has grown substantially since.

UNAIDS estimates that by 2006, China was home to approximately 4.5 million sex workers, making it one of the world's largest sex markets. The industry operates across diverse venues—from high-end hotels, nightclubs, and karaoke bars to massage parlors and residential apartments—with transaction prices ranging from under 100 yuan to several thousand yuan. This heterogeneity in venues and pricing reflects both market segmentation and varying enforcement risks across different transaction types.

Since 2011, China's rapid adoption of mobile payment technology has fundamentally transformed this landscape.[2] By 2022, China's 911 million mobile payment users—representing 64.5% of its total population—held 6.4 billion accounts, averaging three transactions daily with a mean transaction value of 322 yuan.[3] The dominance of two platforms—Alipay (whose market share declined from 74.9% in 2015 to 54% in 2022) and WeChat Pay (whose share rose from 25.1% to 42%)[4]—created a duopoly that processed 982.79 billion transactions worth 317.33 trillion yuan in 2022 alone.

This digital payment infrastructure proved particularly attractive for sex industry transactions for several reasons. First, mobile payments eliminated the risks associated with carrying large amounts of cash, protecting both sex workers from robbery and clients from theft. Second, the convenience of instant transfers facilitated deposit systems, whereby clients pay partially in advance to secure appointments—a practice that became standard for higher-end services. Third, mobile payments enabled discreet transactions that appeared legitimate, as small transfers between individuals aroused little suspicion. By 2015, industry observers noted that mobile payments had become the dominant transaction method, even in lower-tier cities.[5]

Before the 2016 reform, law enforcement faced significant obstacles in policing the digitalized sex industry. While police could observe suspicious activity in public venues, tracking financial flows required formal requests to payment companies. These requests—which required approvals at multiple levels, from local police stations through municipal and sometimes provincial authorities—created substantial administrative burden and delays. Payment companies, citing user privacy, often required court orders to produce detailed transaction records.

As a result, enforcement remained largely reactive and campaign based. The 2014 crackdown on Dongguan City—known as China's "sin city"—exemplified this approach: This massive, coordinated operation temporarily disrupted the local industry but failed to create lasting change. Police could raid visible venues but struggled to detect transactions in private apartments or hotels. Even when

[2] Alipay, the first and largest mobile payment app, was launched in 2004 as part of Alibaba Group's Taobao.com platform but received its payments business license from the People's Bank of China only in 2011. Two years later, WeChat Pay, developed by Tencent, entered the market as Alipay's major competitor.

[3] All statistics in this paragraph are sourced from the China Payment Industry Report 2023 (PCAC).

[4] The remaining 4% share is held by the state-owned UnionPay, according to a report by EnterpriseAppsToday at https://www.enterpriseappstoday.com/stats/alipay-statistics.html.

[5] See supra note 2.

authorities identified suspected sex workers or clients, proving monetary exchange without payment records remained challenging, often resulting in cases being dismissed or only minimal penalties being imposed.

2.2 Real-Name Reform for Mobile Payment

On July 1, 2016, the People's Bank of China implemented the *Administrative Measures for the Online Payment Business of Non-Banking Payment Institutions*. While officially aimed at "preventing payment risks and protecting the parties' lawful rights and interests," the reform had profound implications for law enforcement. The regulation mandated that all users of nonbanking payment platforms verify their identity through a tiered system, providing national ID numbers for basic accounts, adding bank account verification for higher limits, and submitting additional documentation for the highest transaction tiers.

Critically, the reform's real-name verification requirements created a system in which all transactions could be traced back to verified identities. Under the regulation, payment companies were required to authenticate customer identity information through at least three external channels, particularly for class II accounts—used for everyday deposits, small-value spending, fee payments, and investment or wealth management transactions—and class III accounts—designed for high-frequency, small-value transactions. To meet these requirements, payment companies were encouraged to collaborate with police departments to enhance database integration. This shift transformed payment platforms from relatively private systems into infrastructures accessible to law enforcement agencies.

The reform fundamentally changed the economics of sex industry enforcement through three mechanisms: First, it eliminated anonymity. Every transaction became traceable to verified identities on both sides. Sex workers could no longer use aliases or fake accounts, and clients left permanent digital footprints with each payment. This visibility extended beyond direct participants—police could identify entire networks by analyzing payment patterns, discovering previously hidden organizers and facilitators.

Second, the reform created admissible evidence. Digital payment records provided prosecutors with timestamped, verified proof of monetary exchange—the key element often missing in sex crime cases. In contrast to cash transactions, which leave no trace, or witness testimony, which can be recanted, payment records offer incontrovertible evidence. Courts began accepting these records as primary evidence, which dramatically increased conviction rates.

Third, the reform reduced monitoring costs. Inquiries that had previously required weeks of paperwork and approvals became routine database queries. The local police gained direct access to the payment verification systems, enabling real-time investigation. Authorities developed algorithms to flag suspicious payment patterns—recurring transfers between unrelated individuals, payments at unusual hours, or transactions near known venues. This proactive capability transformed enforcement from reactive raids into systematic surveillance.

This setting enables us to test fundamental questions about the regulation of illegal markets. Economic theory predicts that reducing monitoring costs should increase detection probability, effectively raising the price of illicit goods and reducing the quantity traded. In a simple supply–demand framework, enhanced enforcement shifts the supply curve leftward as providers exit the market or demand risk premiums, while also shifting demand leftward as buyers factor in higher expected penalties.

However, the welfare implications depend critically on substitution patterns. If commercial sex and noncommercial sexual encounters are substitutes, restricting the former may increase sexual violence—a negative externality that could outweigh any benefits from reduced prostitution. Conversely, if they are complements, or if enforcement curtails the most harmful segments of the industry (such as trafficking or underage prostitution), the welfare effects may be positive. Our empirical analysis addresses these theoretical ambiguities.

3 Data and Empirical Strategy

In Section 3.1, we first construct a comprehensive dataset on police enforcement by merging two data sources. This dataset provides detailed information on arrests for illegal activities, including offenses related to the sex industry, sexual violence, domestic violence, human trafficking, theft, and robbery. We then present our measures of mobile payment adoption, with a particular focus on the DFIIC, which serves as an indicator of regional exposure to the real-name reform. Furthermore, we complement our dataset with additional data sources to construct the main sample and provide descriptive results. In Section 3.2, we outline our empirical strategies, including DID and DDD.

3.1 Data

3.1.1 Police Enforcement

Our dataset on police enforcement comes from two complementary administrative sources that capture different levels of legal violations in China.

The first source, the CAP database, records sub-criminal-level violations handled directly by police and other government authorities. Compiled by `www.pkulaw.com`, this database includes over 31 million penalty records from central and local governments. These administrative penalties apply to illegal activities below the threshold of criminality, with authorities required to disclose the records on official websites. The second source, CJO, documents criminal cases that proceed to trial. Established by the Supreme People's Court in 2013 to promote judicial transparency, this platform contains more than 140 million court verdicts that all Chinese courts are mandated to publish. Liu et al. (2022) provide a detailed overview of this database.

From these combined sources, we extract enforcement records for the period 2014–2019 using systematic keyword searches and AI-assisted validation (detailed in Appendix A.1). Our extraction yields 225,439 records of sex-industry-related arrests (our primary outcome of interest), 58,276 sexual violence records, 121,174 domestic violence records, and 2,630 human trafficking records. For our placebo tests, we also collect 1,115,679 theft and 88,445 robbery records. Each record contains standardized information including the arrest date, location, involved parties, case facts, punishments, and legal basis, which we use to construct our enforcement variables.

Although this dataset represents the most comprehensive collection of enforcement records available for China, we acknowledge potential limitations from incomplete or selective disclosure. We address these concerns through several strategies: including province$\times$year fixed effects to account for regional reporting differences, conducting robustness checks with coverage trend controls, and analyzing the CAP and CJO data separately.

3.1.2 Mobile Payment Adoption

To measure regional exposure to the real-name reform, we use the DFIIC, developed through collaboration between Peking University and Ant Group. Based on billions of Alipay transaction records, this index provides prefecture-level measures of mobile payment adoption for 2011–2018.

The DFIIC comprises three dimensions, coverage breadth, usage depth, and digitalization level, containing 33 underlying variables (detailed in Appendix Table A.3). For our primary analysis, we focus on the coverage breadth dimension measured in 2015 (prereform), which best captures the extensive margin of mobile payment adoption relevant for enforcement. This dimension combines three indicators: the number of Alipay accounts per 10,000 people, the proportion of accounts linked to bankcards, and the average number of bankcards per account. We use this prereform measure as our intent-to-treat variable, with higher values indicating greater exposure to the reform's enforcement effects. We also use the comprehensive measure in robustness checks to verify that our results are not sensitive to the specific adoption metric employed.

3.1.3 Complementary Datasets

We supplement our core enforcement data with additional variables for identification and outcome analysis.

Variables for Identification Strategy. To address concerns that mobile payment adoption may correlate with prefecture characteristics, we employ the ratio of men to women as an additional source of variation in our DDD design. Following Edlund et al. (2013) and Cameron et al. (2019), who document that male-biased sex ratios increase demand for commercial sex, we calculate the ratio of men to women aged 30–60 using data from the 2015 population census. Prefectures above the median ratio are classified as "high-male-sex-ratio" regions. This variable provides exogenous variation in the demand for commercial sex that is orthogonal to economic development, as confirmed by the balance tests in Table A.4. We also control for standard economic indicators—population, GDP per capita, fiscal revenue, and fiscal expenditure—from the China City Statistical Yearbook.

Additional Outcome Variables. We examine broader societal impacts through two additional data sources. First, to assess public health consequences, we obtain province-level STI incidence rates (HIV, gonorrhea, and syphilis) from the Center for Disease Control.[6] Second, to measure impacts on family stability, we identify divorce

[6] Prefecture-level STI data are unavailable, so we are limited to a province-level analysis for the health outcomes.

cases involving marital infidelity from the CJO database by searching for keywords related to infidelity in divorce verdicts. By examining these supplementary outcomes, we can trace both the public health and social consequences of enhanced enforcement of the sex industry.

3.1.4 Descriptive Results

Our main sample comprises 1,710 prefecture–year observations covering 285 prefectures from 2014 to 2019. We exclude China's four province-level municipalities (Beijing, Shanghai, Tianjin, and Chongqing) because they lack comparable administrative structures and the variation across them is absorbed by our province×year fixed effects. Furthermore, approximately 40 prefectures, primarily in western and central China, are excluded because we are missing data on key variables. The sample period begins in 2014, when both the CAP and the CJO databases achieved comprehensive coverage, and ends in 2019 to avoid confounding effects from the COVID-19 pandemic.

Table 1 provides summary statistics. Panel A shows substantial variation in our main outcome variables. The average prefecture records 3.22 sex business arrests per 100,000 people annually, although this masks considerable heterogeneity (SD = 14.56). Among arrests with identifiable payment methods, 18.95% involve mobile payment records, a share that increases dramatically during our sample period. Enforcement patterns also vary: 21.52% of the arrests occur in private locations, average fines reach 18,646 yuan, and average detention spans 534 days.[7] Sexual violence arrests average 0.77 per 100,000, with sexual harassment (0.23) and rape (0.48) showing distinct patterns.

Panel B presents summary statistics for the DFIIC measures. All indices are normalized to 100 in the 2011 base year, so the values represent percentage growth relative to this baseline. Our primary exposure measure, the coverage breadth index, is averaged at 165.54 across prefectures with substantial variation (SD = 25.45). The highest values correspond to Shenzhen (242.9) and Hangzhou (238.4)—which is unsurprising since these cities host the headquarters of WeChat Pay and Alipay, respectively. This variation, driven primarily by commercial and technological factors rather than law enforcement priorities, provides the identification for our empirical

[7] Both average fines and detention are high because our records include organizers of the sex business.

strategy. The total DFIIC, which combines all three dimensions, averages 172.3 and follows a similar geographic pattern.

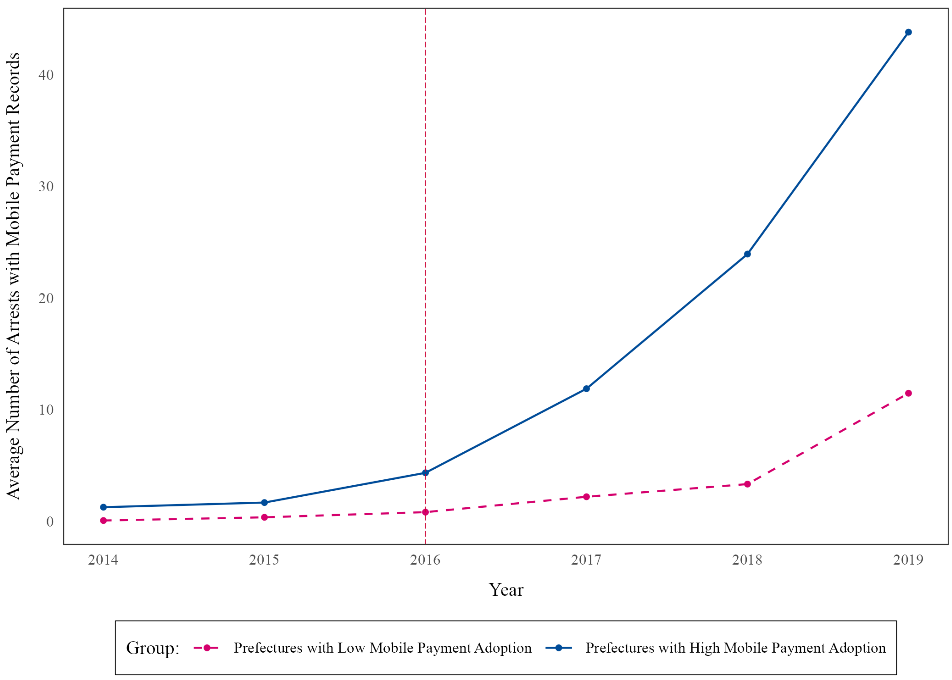
Figure 1 presents motivating evidence for our analysis. Panel A illustrates a sharp divergence in enforcement patterns following the 2016 reform: Prefectures with above-median mobile payment adoption experienced substantial increases in arrests involving mobile payment records, whereas for low-adoption prefectures, we observe minimal change. This pattern is striking—the average prefecture's share of arrests with mobile payment records rose from 2.7% in 2014 to 50.1% in 2019 (Appendix Figure A.4), with the increase concentrated in high-adoption regions. Panel B of Figure 1 documents a strong positive correlation between sex business arrests and sexual violence arrests, suggesting potential substitution effects that we explore in Section 5.

The spatial distribution of our key variables provides additional context. Figure 2 depicts the spatial pattern of the sex business arrests (Panel A) and sexual violence arrests (Panel B) across prefectures, revealing geographical clustering. Notably, Guangdong and Zhejiang provinces—China's economic powerhouses—show the highest arrest rates for both categories. These provinces also lead in mobile payment adoption, as shown in Appendix Figure A.5, consistent with our hypothesis that digital payment infrastructure affects enforcement capacity. However, this correlation alone cannot establish a causal relationship, as these regions differ in multiple dimensions. Our identification strategy, detailed in Section 3.2, addresses endogeneity concerns through temporal variation and multiple sources of cross-sectional heterogeneity.

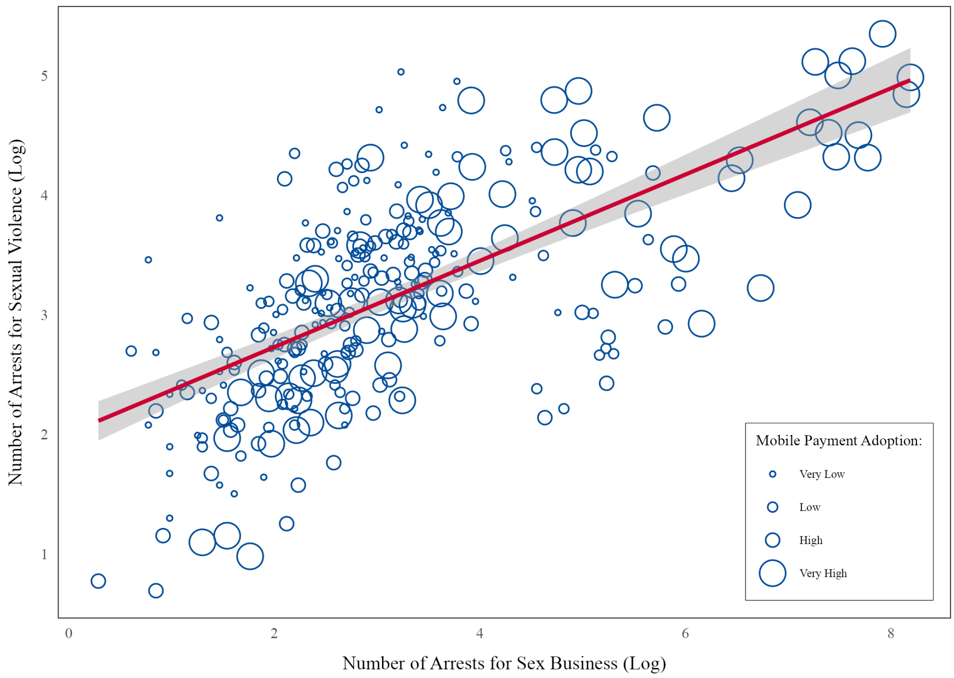
Table 1: Summary Statistics

	Mean	S.D.	Obs
Panel A. Dependent Variables			
Sex business arrests per 100,000	3.22	14.56	1,710
Share of arrests with mobile payment records (%)	18.95	27.37	1,497
Share of arrests with private crime scenes (%)	21.52	22.80	1,544
Average amount of fines (yuan)	18,646	71,795	1,541
Average days of detention	534	377	1,542
Average price of sex services	433	464	1,239
Sexual violence arrests per 100,000	0.77	0.93	1,710
Sexual harassment arrests per 100,000	0.23	0.53	1,710
Rape arrests per 100,000	0.48	0.51	1,710
Divorce cases due to marital infidelity per 100,000	6.33	8.02	1,710
Domestic violence cases per 100,000	1.56	2.27	1,710
Human trafficking arrests per 100,000	0.05	0.12	1,710
Theft arrests per 100,000	13.09	12.41	1,710
Robbery arrests per 100,000	1.10	1.54	1,710
Incidence of HIV per 100,000	6.86	6.03	155
Incidence of gonorrhea per 100,000	8.15	7.28	186
Incidence of syphilis per 100,000	37.36	18.37	186
Panel B. Independent Variables			
Total DFIIC	172.34	18.15	1,710
Coverage breadth index	165.54	25.45	1,710
Usage depth index	145.48	21.76	1,710
Digitalization level index	243.59	13.97	1,710
Male sex ratio	1.04	0.06	1,710

Notes: This table reports summary statistics for key variables. Panel A provides summary statistics for the dependent variables, including prefecture-level police/court enforcement records and province-level incidence of sexually transmitted infections. Panel B presents summary statistics for the independent variables, including various measurements of mobile payment adoption and other key prefecture-level characteristics.



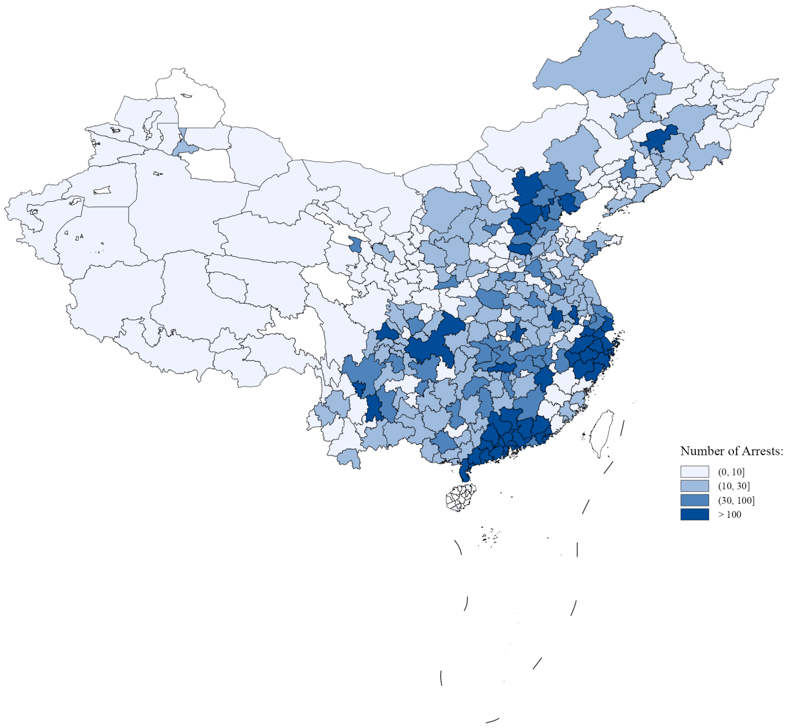
Panel A: Trends in Sex Business Arrests with Mobile Payment Records



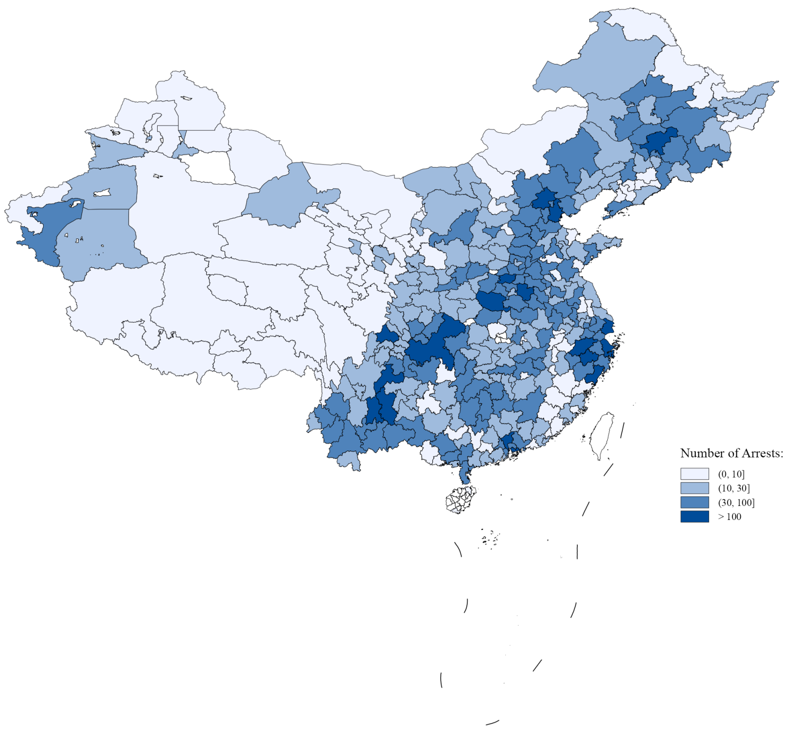
Panel B: Correlation Between Sex Business Arrests and Sexual Violence Arrests

Figure 1: Descriptive Patterns for Main Variables

Notes: The data on the sex industry and sexual violence are sourced from the police enforcement dataset, which integrates the CAP and CJO datasets. The measurements of mobile payment adoption are derived from the DFIIC.



Panel A: Average Number of Sex Business Arrests per Year



Panel B: Average Number of Sexual Violence Arrests per Year

Figure 2: Spatial Distribution of the Sex Business and Sexual Violence Across Prefectures

Notes: The data on the sex industry and sexual violence are sourced from the police enforcement dataset, which integrates the CAP and CJO datasets.

3.2 Empirical Strategy

3.2.1 DID

We employ a continuous DID design to identify the causal effect of the real-name reform on law enforcement outcomes. This approach leverages both temporal variation from the reform's implementation in July 2016 and cross-sectional variation in prefectures' exposure to the reform, measured by their prereform intensity of mobile payment adoption. Our main specification is the following:

$$Y_{ct} = \beta \cdot MPA_c \cdot Post_t + X_{ct} \cdot \gamma + \theta_c + \lambda_t + \epsilon_{ct}, \tag{1}$$

where c indexes prefectures and t indexes years. The outcome variable Y_{ct} represents various enforcement measures, including the number of arrests per 100,000 people for different illegal activities and the share of arrests with mobile payment records, and other societal consequences measures. The treatment intensity MPA_c is the prefecture's mobile payment adoption rate in 2015, measured by the coverage breadth dimension of the DFIIC. This prereform measure captures differential exposure to the policy change while avoiding mechanical endogeneity. The indicator $Post_t$ equals one for years after 2016.

We incorporate prefecture fixed effects θ_c to control for time-invariant factors specific to each prefecture and province×year fixed effects λ_t to account for time-varying confounders at the province level, such as regional reporting differences in criminal cases and provincial policy changes. We also control for prefecture×year economic characteristics X_{ct}, including GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita, all in logged term. Standard errors are clustered at the prefecture level.

The coefficient β identifies the differential effect of the reform across prefectures with varying intensities of mobile payment adoption. A causal interpretation requires that the trends of prefectures with different adoption levels would have developed in parallel absent the reform. We test this assumption using an event study specification:

$$Y_{ct} = \sum_{t=2014}^{2019} \beta^t \cdot MPA_c \cdot I_t + X_{ct} \cdot \gamma + \theta_c + \lambda_t + \epsilon_{ct}. \tag{2}$$

We normalize $\beta^{2016} = 0$, making 2016 the reference year. The coefficients $\beta^t t < 2016$ test for differential pretrends, while $\beta^t t > 2016$ trace out the dynamic treatment effects. Finding insignificant prereform coefficients and significant postreform effects would validate our identification strategy.

As a robustness check, we also estimate a standard binary DID specification, defining high-adoption prefectures as those whose adoption rate in 2015 was above the median.

3.2.2 DDD

Although our DID design includes rich fixed effects and controls, mobile payment adoption may correlate with unobserved time-varying prefecture characteristics that affect enforcement trends. To address this concern, we implement a DDD strategy that adds variation in local demand for commercial sex, proxied by the male-to-female sex ratio.

The intuition is straightforward: The reform's impact on sex industry enforcement should have been stronger in regions with both high mobile payment adoption (supply of enforcement technology) and a high male sex ratio (demand for commercial sex). Previous literature shows that a higher male sex ratio is linked to an increased incidence of crime (Edlund et al., 2013; Cameron et al., 2019), but the sex ratio is relatively independent of economic performance (Cameron et al., 2019). We also confirm this point with the results of the balance tests, as shown in Table A.4. Although mobile payment adoption correlates with economic indicators (column (1)), the sex ratio is largely orthogonal to these variables (column (2)), and their interaction is uncorrelated with economic characteristics (column (3)).

This gives us confidence to further use a DDD design for identification. Specifically, we estimate the following equation:

$$\begin{aligned} Y_{ct} = \beta_1 \cdot MPA_c \cdot SexRatio_c \cdot Post_t + \beta_2 \cdot MPA_c \cdot Post_t + \beta_3 \cdot SexRatio_c \cdot Post_t \\ + X_{ct} \cdot \gamma + \theta_c + \lambda_t + \epsilon_{ct}, \end{aligned} \tag{3}$$

where $SexRatio_c$ is an indicator for prefectures with above-median ratios of men to women (ages 30–60) in 2015. The coefficient β_1 captures the additional effect in high-sex-ratio regions, β_2 identifies the effect in low-sex-ratio regions, and $\beta_1 + \beta_2$ gives the total effect in high-sex-ratio prefectures.

This approach offers two advantages. First, it provides a more credible control group by comparing prefectures with similar mobile payment adoption but different sex ratios. Second, it tests a specific mechanism: If our results reflect improved enforcement rather than spurious correlations, the effects should be amplified where demand for commercial sex is higher.

We also estimate a dynamic DDD specification:

$$Y_{ct} = \sum_{t=2014}^{2019} [\beta_1^t \cdot MPA_c \cdot SexRatio_c \cdot I_t + \beta_2^t \cdot MPA_c \cdot I_t + \beta_3^t \cdot SexRatio_c \cdot I_t] \\ + X_{ct} \cdot \gamma + \theta_c + \lambda_t + \epsilon_{ct}. \quad (4)$$

This allows us to test for differential pretrends across the three dimensions of variation and trace out dynamic heterogeneous treatment effects.

4 Impacts of the Real-Name Reform on the Sex Industry

This section examines the impacts of the real-name reform on police enforcement in China's sex industry. We first establish that the reform significantly increased arrests through improvements to digital surveillance capabilities in Section 4.1. We then validate these findings through extensive robustness checks in Section 4.2 and document broader impacts on enforcement efficiency and market dynamics in Section 4.3.

4.1 Impacts on Police Enforcement in the Sex Industry

4.1.1 DID Results

Table 2 presents our baseline DID estimates of the reform's impact on sex industry enforcement. All regressions include prefecture and province×year fixed effects and cluster the standard errors at the prefecture level. The results reveal substantial effects across multiple dimensions of police activity in the sex industry. Our primary outcome—sex business arrests per 100,000 people—increases significantly in prefectures with higher mobile payment adoption. Column (1) shows that a one-standard-deviation increase in mobile payment adoption (25.45 points) corresponds to 5.09 additional arrests per 100,000 people postreform. This effect, which remains robust to our inclusion of controls at 4.86 additional arrests in column (2), represents a 466% increase relative to the prereform mean of 1.04 arrests. This magnitude suggests that—far from marginally improving existing practices—digital payment surveillance has fundamentally altered enforcement capacity. Appendix

Table C.1 confirms similar effects across all participant categories: organizers, sex workers, and clients.

The mechanism driving these results becomes clear in columns (3)–(4). High-adoption prefectures experienced significant increases in the share of arrests involving mobile payment records—direct evidence that police leveraged digital transaction data for enforcement.

The event study in Figure 3 reinforces our causal interpretation: The parallel pretrends validate our identification assumption, while the persisten ce and growth of the postreform effects rule out the possibility that they were the result of temporary enforcement campaigns.

Columns (5)–(6) present crucial placebo tests using theft and robbery arrests. The null effects demonstrate that our results reflect improved capacity for detecting digitally mediated transactions, not general improvements in police resources or efficiency. This specificity strengthens our interpretation that mobile payment data uniquely facilitate sex industry enforcement.

A potential concern is that sex industry participants might substitute cash for mobile payments in response to enhanced surveillance. However, multiple lines of evidence suggest that, in practice, the possibilities for such substitution are limited. First, the structure of China's modern sex industry makes avoiding digital payments increasingly difficult. Most transactions originate through online platforms where initial contact, negotiation, and scheduling occur digitally. These platforms typically require advance deposits via mobile payment to confirm appointments—a practice that creates traceable records before any physical meeting. Even when clients intend to pay cash for the actual service, these booking deposits leave digital footprints.

Recent investigative reporting confirms the pervasiveness of digital payments in the industry. According to Lianhe Zaobao's 2024 investigation, sex workers acknowledge that mobile payments have become virtually unavoidable despite awareness of surveillance risks. The report documents how the industry has adopted coded language, referring to services as "classes," providers as "teachers," and clients as "students," but these linguistic disguises cannot mask the underlying digital transaction patterns that authorities can detect through big data analysis.[8]

[8]See Lianhe Zaobao's report, "China's Sex Industry Uses 'Over-the-Wall Ladder' but Can't Escape the Eye of Big Data." The title's metaphor of an "over-the-wall ladder" refers to attempts to circumvent surveillance that ultimately prove futile against modern data analytics. https://www.zaobao.com/news/china/story20240721-3918655.

Second, our data provide direct evidence against large-scale payment substitution. The share of arrests involving mobile payment records increased substantially throughout our study period, rising from 2.7% in 2014 to 50.1% in 2019. If widespread cash substitution occurred, we would observe this share declining or stabilizing postreform. Instead, the continued increase suggests that mobile payments remain integral to sex industry transactions despite the heightened enforcement risks.

Third, while some experienced participants may successfully shift to cash-only operations, this represents a marginal response that would attenuate rather than invalidate our estimates. Such substitution is most feasible for established client–provider relationships with repeat transactions. However, the industry's reliance on new client acquisition through digital platforms limits the scope for complete cash conversion. Moreover, any substitution that does occur strengthens our interpretation: Our estimates represent lower bounds of the reform's true enforcement effects, as we capture only the transactions that remained digitally visible.

Last, the concern may arise that the control group may not be directly comparable to the treatment group, as mobile payment adoption could correlate with other prefecture characteristics. While we include prefecture and province×year fixed effects in our analysis and the event study graph reveals no pretrend, we further address this issue in a DDD design below.

4.1.2 DDD Results

Table 3 reports our DDD estimates, which address potential concerns about the comparability of high- and low-adoption prefectures. By leveraging variation in the male sex ratio, a plausibly exogenous proxy for demand, we identify effects within more comparable groups.

The results strongly reinforce our main findings. The triple interaction coefficient in column (1) shows that prefectures with both high mobile payment adoption and high male sex ratios experienced 6.21 additional arrests per 100,000 people, beyond the baseline effect in low-sex-ratio regions. This heterogeneity aligns with economic theory: Enforcement technology should matter more where demand for commercial sex is higher. The amplified effect on the share of mobile payment arrests (columns (3)–(4)) further confirms that digital surveillance drives our results.

The event study in Figure 4 reveals no differential pretrends across the DDD dimensions, validating our identification strategy. The postreform divergence emerges precisely in 2017, consistent with implementation lags as police developed capabilities to access and analyze payment data. The continued null effects in placebo tests (columns (5)–(6)) rule out spurious correlations with general law enforcement trends.

4.2 Robustness Checks

We subject our findings to extensive robustness tests, summarized here. Full results appear in Appendix B.

Alternative Mobile Payment Adoption Measures: Our results are robust to different measures for mobile payment adoption. Using a binary treatment variable (above/below median adoption) yields qualitatively similar results with intuitive magnitudes: High-adoption prefectures experienced 3.6 additional arrests and a 5-percentage-point increase in mobile payment arrests (Appendix Tables B.1 for the DID estimation and B.2 for the DDD estimation). The comprehensive DFIIC, which incorporates usage depth and digitalization, produces slightly larger effects (Appendix Tables B.5 and B.6), suggesting that our baseline estimates are relatively conservative.

Accounting for Platform Heterogeneity: While our primary measure captures Alipay adoption, WeChat Pay represented approximately 25% of the market in 2015 and 42% in 2022. By omitting the effect on WeChat Pay users, we may introduce selection bias from platform-specific variation. Since we do not have access to WeChat Pay transaction records, we use the 2016 WeChat adoption rates—defined as the ratio of WeChat-installed phones per capita—as a proxy. In this robustness check, we construct a new measure by combining the two adoption rates from Alipay and WeChat Pay. Specifically, we calculate the z-score for each adoption rate measure and then aggregate them by taking a weighted average of the Alipay measure (74.9%) and the WeChat Pay measure (25.1%) by their market share in 2015.[9] Tables B.9 and B.10 show that our main findings remain consistent when we use the new weighted index.

[9] The results remain robust to alternative normalization methods and weighting schemes.

Table 2: Real-Name Reform and Police Enforcement in the Sex Industry – DID

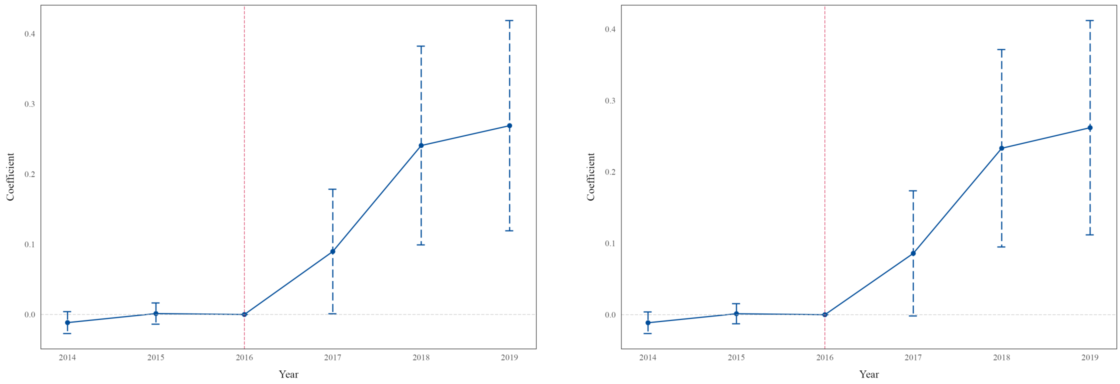
	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption × Post	0.200***	0.191***	0.088**	0.083**	0.028	-0.005
	(0.060)	(0.060)	(0.041)	(0.042)	(0.021)	(0.003)
Observations	1,710	1,710	1,086	1,086	1,710	1,710
R-squared	0.772	0.774	0.717	0.717	0.924	0.922
Preshock Mean Y	1.043	1.043	4.160	4.160	12.294	1.205
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table 3: Real-Name Reform and Police Enforcement in the Sex Industry – DDD

	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.244***	0.248***	0.179**	0.173**	0.041	-0.003
× Post × Male Sex Ratio	(0.070)	(0.071)	(0.080)	(0.080)	(0.031)	(0.004)
Observations	1,710	1,710	1,086	1,086	1,710	1,710
R-squared	0.792	0.793	0.719	0.720	0.925	0.922
Preshock Mean Y	1.043	1.043	4.160	4.160	12.294	1.205
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

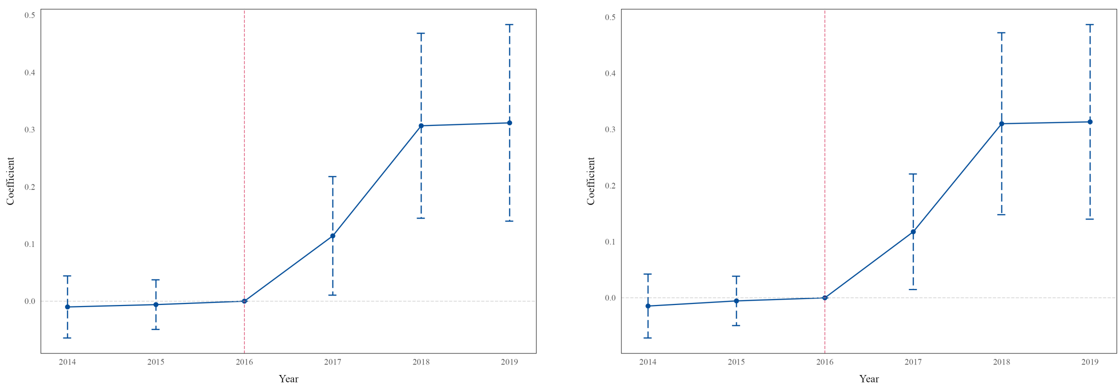
Notes: This table reports the DDD results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.



Panel A: Baseline

Panel B: Plus Controls

Figure 3: Event Studies for Number of Sex Business Arrests – DID



Panel A: Baseline

Panel B: Plus Controls

Figure 4: Event Studies for Number of Sex Business Arrests – DDD

Excluding Outliers: Excluding potential outliers—Dongguan ("Sin City"), Hangzhou (Alipay headquarters), and Shenzhen (WeChat Pay headquarters)—produces slightly attenuated but statistically robust effects (Appendix Tables B.13 and B.14). This confirms that our findings reflect systematic patterns rather than idiosyncratic experiences of particular cities.

Data Coverage on the Arrested Cases: We use two approaches to address potential biases from incompleteness in the administrative records on the arrested cases. First, controlling for prefecture-specific reporting trends leaves our estimates virtually unchanged (Appendix Tables B.17 and B.18). Second, analyzing the CAP and CJO datasets separately yields patterns (Tables B.21 and B.22 using CAP data and Tables B.25 and B.26 using CJO data) consistent with the CAP's focus on mild sex crime and the CJO's emphasis on more serious crimes.

4.3 Further Impacts on the Sex Industry

4.3.1 Enhanced Enforcement Efficiency

Next, we investigate whether the real-name reform increased police enforcement efficiency, given that mobile payment technology significantly reduces tracking and verification costs while providing stronger evidence. Table 4 documents how mobile payment surveillance improved enforcement efficiency in multiple dimensions.

First, the reform enabled the detection of previously hidden transactions. Columns (1)–(2) show that high-adoption prefectures experienced significant increases in arrests at private locations—residential buildings and hotels rather than traditional venues such as massage parlors. With a one-standard-deviation increase in adoption, the private arrest share rises by 4.5 percentage points (21% of the baseline mean). This shift represents a fundamental change in enforcement capacity: Before the creation of digital surveillance, police relied primarily on street patrols and raids of known establishments.

Second, digital evidence strengthened legal cases. Mobile payment records provide timestamped, verified documentation of illegal transactions, enabling severer penalties. Columns (3)–(4) reveal that fines increased by 2,200 yuan per standard deviation of adoption (a 20% increase), while columns (5)–(6) show that detention periods extended by 41 days (a 7.5% increase). Notably, Appendix Table C.2 shows no changes at the extensive margin of punishment types, indicating that the enhancements to enforcement operated through intensity rather than expansion in the scope of penalties. These findings suggest that the real-name reform improved enforcement efficiency in the sex industry by reducing the costs associated with tracking and identifying offenders and providing stronger evidence through mobile payment records.

Table 4: Real-Name Reform and Efficiency of Police Enforcement in the Sex Industry

	Share of Private Crime Scenes (%)		Amount of Fine (yuan)		Days of Detention	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.169***	0.176***	88.626*	86.870*	1.618***	1.610***
× Post	(0.049)	(0.048)	(45.329)	(47.002)	(0.573)	(0.579)
Observations	1,200	1,200	1,092	1,092	1,206	1,206
R-squared	0.402	0.404	0.489	0.491	0.577	0.578
Preshock Mean Y	21.519	21.519	10844	10844	550	550
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on the efficiency of police enforcement in the sex industry. Columns (1)–(2) focus on the effect on the share of arrests with private crime scenes. Columns (3)–(4) examine the effect on the amount of fines (yuan). Columns (5)–(6) explore the effect on the days of detention. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

4.3.2 Market Contraction and Price Effects

Here, we examine whether the real-name reform led to market contraction and price changes in the sex industry.

Table 5 provides evidence of a contraction in the sex industry following the reform. Using transaction prices extracted from arrest records, we find that as mobile payment adoption rises by one standard deviation, sex service prices rise by 32.3 yuan (10.4% of the prereform mean).

This price increase reflects classic supply-side contraction: Enhanced enforcement raised participation risks for sex workers and organizers, reducing market supply while demand remained relatively stable. The magnitude suggests substantial market disruption—consistent with the large increase in arrests—rather than marginal adjustments.

While the price data from arrest records may not perfectly represent the whole market, the systematic increase across high-adoption prefectures provides compelling evidence of reform-induced scarcity. Combined with our arrest evidence, these results paint a consistent picture: By raising detection risks and enforcement costs, digital surveillance led China's sex industry to significantly contract.

Table 5: Real-Name Reform and Prices of Sex Services

	Prices of Sex Services	
	(1)	(2)
Mobile Payment Adoption	1.169*	1.268**
× Post	(0.639)	(0.634)
Observations	1,149	1,149
R-squared	0.458	0.459
Preshock Mean Y	311	311
Control Vars	No	Yes
Prefecture FE	Yes	Yes
Prov × Year FE	Yes	Yes

Notes: This table reports the DID results of the real-name reform on the prices of sex services. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

5 Unintended Consequences: Socioeconomic Impacts of Enhanced Enforcement

The contraction of China's sex industry through digital surveillance generated far-reaching social consequences. While policymakers focused on reducing illegal commercial sex, the reform may have triggered complex substitution patterns and spillover effects across multiple domains. The literature has established a link between regulation of the sex industry and various social outcomes (Cunningham and Shah, 2021). This section documents these unintended consequences, beginning in Section 5.1 with the most concerning finding—increased sexual violence—before examining the impacts on public health in Section 5.2 and family stability and domestic violence in Section 5.3.

5.1 Impacts on Sexual Violence

The relationship between commercial sex availability and sexual violence remains central to policy debates. Theoretical predictions are ambiguous: Prohibition could reduce violence by shrinking an industry associated with exploitation (Farley, 2004, 2005) or increase it if commercial and noncommercial sexual encounters are substitutes. Recent empirical evidence predominantly supports the substitution hypothesis, finding that restrictions in the sex industry increase sexual violence (Cunningham and Shah, 2018; Gao and Petrova, 2022; Ciacci, 2024). Our setting provides unique evidence on this mechanism from the world's largest market.

5.1.1 DID Results

Table 6 presents our DID estimates for the outcomes of sexual violence. The results reveal substantial increases across all categories of sexual violence in high-adoption prefectures after the reform.

Column (1) shows that a one-standard-deviation increase in mobile payment adoption corresponds to 0.31 additional sexual violence arrests per 100,000 people—an 89% increase relative to the prereform mean. This aggregate effect masks important heterogeneity by severity. Sexual harassment arrests (columns (3)–(4)) increase by 0.23 per 100,000, representing a striking 301% increase from the low baseline. Rape arrests (columns (5)–(6)) rise by 0.08 per 100,000, a 30% increase that, while smaller in relative terms, represents severe violent crimes.

The event study in Figure 5 validates our causal interpretation. The flat pretrends across prefectures with different adoption levels confirm the parallel trends assumption. The sharp post-2017 divergence coincides with the enforcement effects documented in Section 4, suggesting that sexual violence increased as a direct consequence of the reduced availability of commercial sex.

How do these magnitudes compare with existing evidence? Cunningham and Shah (2018) find that decriminalizing indoor prostitution in Rhode Island reduced rape offenses by 30%, while our results imply the inverse—improved enforcement increased rape by 30%. Ciacci (2024) documents 44–62% increases in rape following Sweden's criminalization of sex purchases, somewhat larger than our estimates. These comparisons suggest that enforcement intensity changes can generate effects comparable to those of formal legal reforms.

5.1.2 DDD Results

Table 7 reports DDD estimates that reinforce our findings. The coefficient on the triple interaction (column (1)) indicates that prefectures with high male sex ratios experienced an additional 0.33 sexual violence arrests per 100,000 people. This heterogeneity aligns with the substitution mechanism: Regions with greater demand for sex among men should experience greater increases in violence when commercial outlets are restricted.

The pattern holds across violence types, with high-sex-ratio regions showing amplified effects for both sexual harassment (column (3): additional 0.23 arrests) and rape (column (5): additional 0.10 arrests). The event study in Figure 6 shows no differential pretrends but significant postreform divergence, validating our identification strategy.

5.1.3 Mechanisms

What mechanisms drive the increase in sexual violence? Three explanations merit consideration:

First, the substitution hypothesis suggests that some men view commercial and noncommercial sex as substitutes (Posner, 1994). When enforcement raises the cost and risk of purchasing sex, marginal buyers may resort to coercion. This mechanism predicts violence against women generally, not specifically sex workers.

Table 6: Real-Name Reform and Sexual Violence – DID

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.012***	0.011***	0.009***	0.009***	0.003***	0.003***
× Post	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	1,710	1,710	1,710	1,710	1,710	1,710
R-squared	0.789	0.790	0.722	0.725	0.797	0.797
Preshock Mean Y	0.344	0.344	0.076	0.076	0.252	0.252
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov×Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table 7: Real-Name Reform and Sexual Violence – DDD

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.013***	0.013***	0.009***	0.009***	0.004***	0.004***
× Post × Male Sex Ratio	(0.004)	(0.004)	(0.003)	(0.003)	(0.001)	(0.001)
Observations	1,710	1,710	1,710	1,710	1,710	1,710
R-squared	0.800	0.801	0.738	0.740	0.800	0.801
Preshock Mean Y	0.344	0.344	0.076	0.076	0.252	0.252
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DDD results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Second, the increased vulnerability of sex workers could explain our results. As the industry moves further underground, sex workers might face greater violence from clients as a consequence of reductions in investments in security equipment, pressure to avoid reporting to law enforcement, or increases in opportunities for police corruption (Brents and Hausbeck, 2005; Church et al., 2001; Levitt and Venkatesh, 2007). This mechanism would be associated with concentrated violence against sex workers.

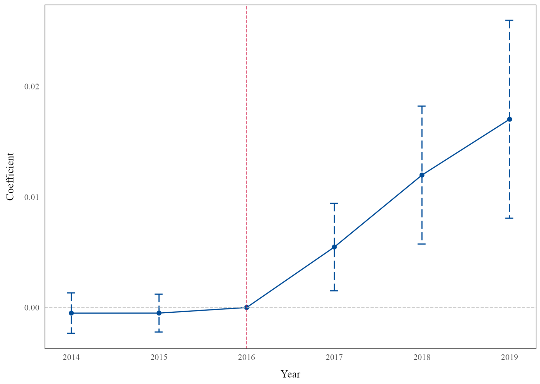
Third, the displacement of enforcement could mechanically increase observed violence if police shifted resources from investigating sexual crimes to sex industry enforcement or if the enhanced digital capabilities improved detection of all crimes (Draca et al., 2011; Adda et al., 2014).

Appendix Table C.3 provides decisive evidence on these mechanisms. When we separate sexual violence by victim type, we find significant increases only for common women (0.011 per 100,000), with no effect on sex workers themselves. This pattern rules out the second explanation. However, the third explanation is also less likely to hold, as the results of the placebo tests using theft and robbery cases (in columns (5) and (6) of Table 2) do not support the explanation of displacement of enforcement. Thus, substitution is the likeliest mechanism behind the increase in sexual violence following the strengthened enforcement of the sex industry. This is further supported by the DDD results, which introduce demand-side variation.

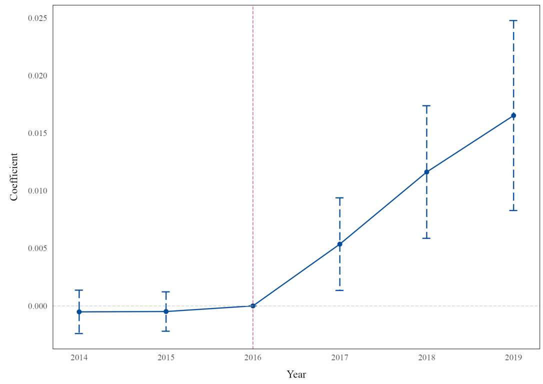
5.1.4 Robustness Checks

We subject our sexual violence findings to the same battery of robustness checks as our main results (detailed in Appendix B). The findings prove remarkably stable across specifications:

Alternative mobile payment adoption measures yield consistent results, with the regression using the binary treatment measure indicating 0.18 additional arrests in high-adoption prefectures (Tables B.3 and B.4) and that using the comprehensive DFIIC producing slightly larger effects (Tables B.7 and B.8). We address concerns about **platform heterogeneity** by incorporating WeChat adoption, which leaves the estimates unchanged (Tables B.11 and B.12). **Excluding outlier** cities attenuates the effects slightly, but they maintain statistical significance (Tables B.15 and B.16). **Data coverage** issues are mitigated through coverage trend controls and separate dataset analyses, all of which confirm the core findings (Tables B.23– B.28).

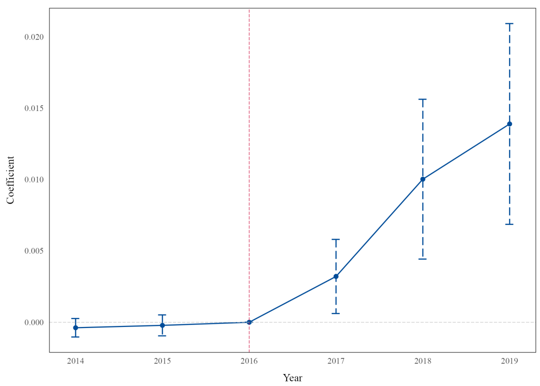


(a) Baseline

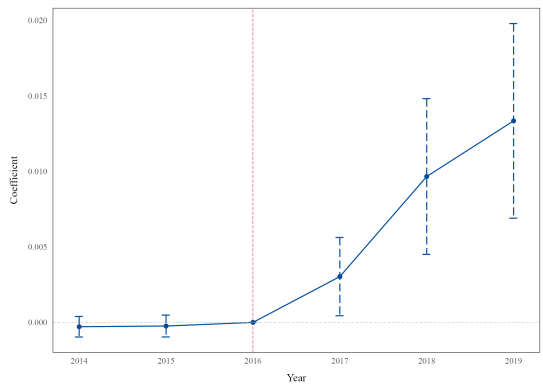


(b) Plus Controls

Panel A: Sexual Violence

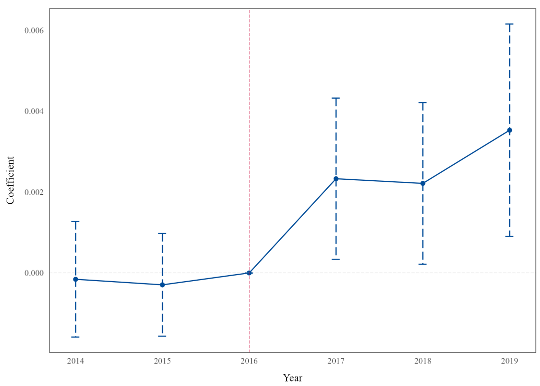


(a) Baseline

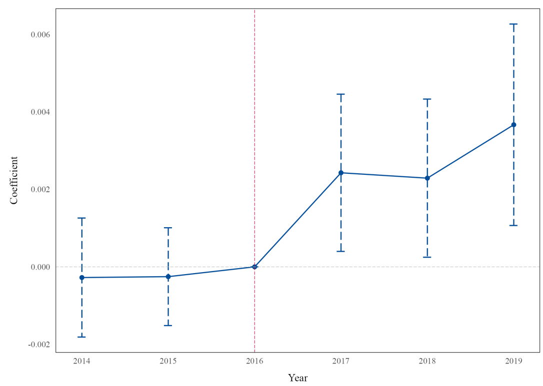


(b) Plus Controls

Panel B: Sexual Harassment



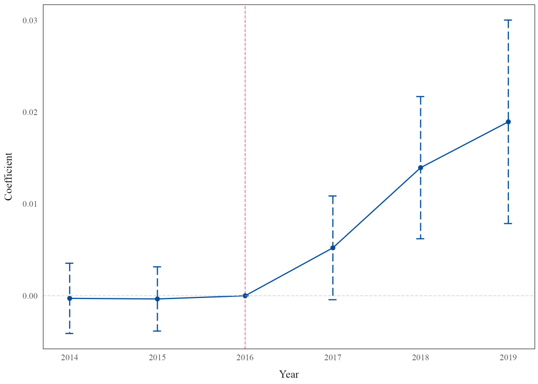
(a) Baseline



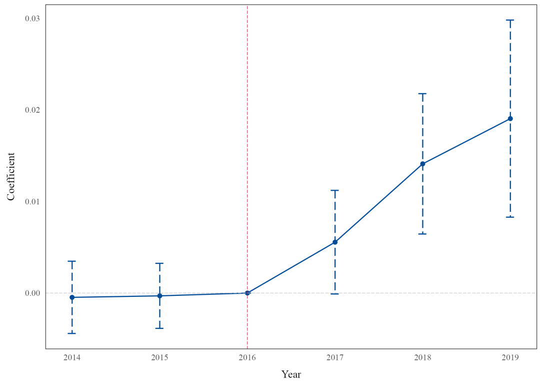
(b) Plus Controls

Panel C: Rape

Figure 5: Event Studies for Number of Arrests for Sexual Violence – DID

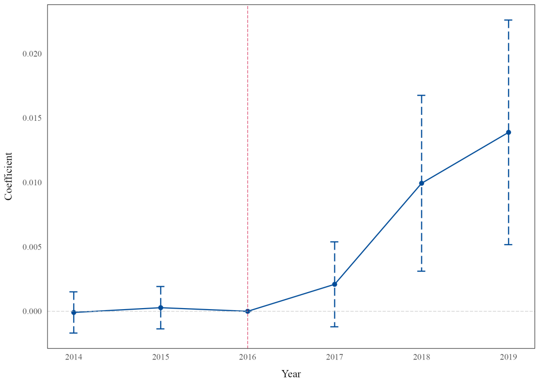


(a) Baseline

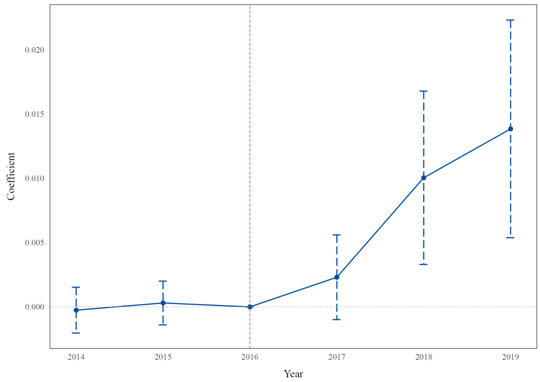


(b) Plus Controls

Panel A: Sexual Violence

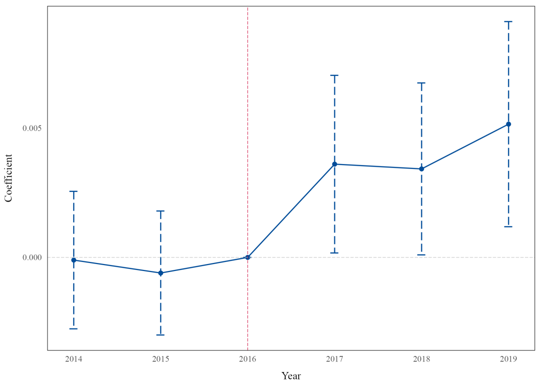


(a) Baseline

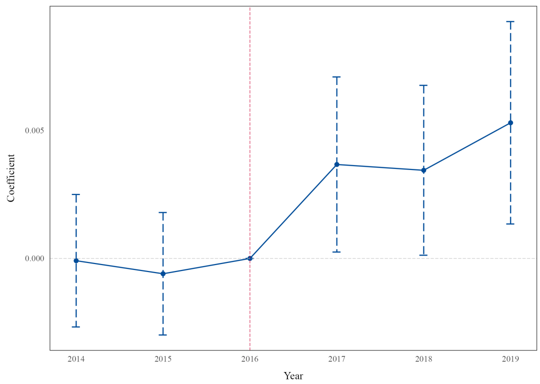


(b) Plus Controls

Panel B: Sexual Harassment



(a) Baseline



(b) Plus Controls

Panel C: Rape

Figure 6: Event Studies for Number of Arrests for Sexual Violence – DDD

5.2 Impacts on Public Health

Although the increase in sexual violence represents a severe negative consequence, the reform also generated public health benefits through reduced STI transmission. Table 8 presents evidence at the province level on STI incidence, as data at the prefecture level are unavailable.

We find significant reductions in the incidence of HIV and syphilis following the reform, while there is no statistically significant effect for gonorrhea. Two mechanisms could drive these health improvements. First, the overall contraction of the sex industry would mechanically reduce disease transmission opportunities. Second, the enhanced enforcement may have disproportionately affected higher-risk market segments. Digital surveillance particularly threatens providers who advertise online and maintain client databases—often higher-volume operators whose removal would substantially impact transmission networks.

Table 8: Real-Name Reform and Incidence of STIs

	HIV	Gonorrhea	Syphilis
	(1)	(2)	(3)
Mobile Payment Adoption	-0.016**	-0.010	-0.111***
× Post	(0.006)	(0.033)	(0.021)
Observations	155	186	186
R-squared	0.955	0.930	0.956
Preshock Mean Y	6.357	7.546	34.941
Control Vars	Yes	Yes	Yes
Province FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on the incidence of sexually transmitted infections per 100,000. Column (1) focuses on the effect on the incidence of HIV per 100,000. Columns (2)–(3) examine the effect on the incidence of gonorrhea and syphilis per 100,000, respectively. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the province level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

5.3 Impacts on Family Stability, Domestic Violence, and Human Trafficking

The reform's effects extended to household dynamics through two channels: exposure of infidelity and unmet demand for sex. Table 9 documents these impacts.

Columns (1)–(2) show that high-adoption prefectures experienced higher rates of divorce attributed to marital infidelity. This increase probably reflects two mechanisms. First, digital payment records exposed previously hidden commercial sex purchases, providing spouses with concrete evidence of infidelity. Second, some individuals may have substituted discreet commercial encounters for affairs that proved more damaging to marriages.

More concerningly, columns (3)–(4) document significant increases in domestic violence. The increase in domestic violence could reflect both direct effects (unmet demand for sex leading to spousal coercion) and indirect effects (increased marital conflict following revelations of infidelity). These results echo the finding of Berlin et al. (2019) that access to prostitution reduces domestic violence, although the sizes of the effects we identify are somewhat smaller.

Finally, columns (5)–(6) examine human trafficking, where the sign that theory predicts for the effects is ambiguous. Enhanced enforcement could reduce trafficking by shrinking the sex industry or increase it by driving the industry underground, where coercion is more prevalent. We find no significant effects, suggesting that these forces roughly offset each other. However, given the challenges associated with detecting and prosecuting trafficking cases, these null results should be interpreted with caution.

Table 9: Real-Name Reform and Other Socioeconomic Outcomes

	Number of Cases Per 100,000					
	Divorce – Marital Infidelity		Domestic Violence		Human Trafficking	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.034**	0.036**	0.012***	0.014***	0.0003	0.0004
× Post	(0.016)	(0.016)	(0.004)	(0.004)	(0.0002)	(0.0002)
Observations	1,710	1,710	1,710	1,710	1,710	1,710
R-squared	0.841	0.841	0.857	0.859	0.464	0.464
Preshock Mean Y	11.486	11.486	2.768	2.768	0.0475	0.0475
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on other socioeconomic outcomes. Columns (1)–(2) focus on the effect on the number of cases of divorce due to marital infidelity per 100,000. Columns (3)–(4) examine the effect on the number of cases for domestic violence per 100,000. Columns (5)–(6) show the effect on the number of arrests for human trafficking per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

6 Conclusion

Mobile payment technology has empowered police enforcement in China by significantly reducing tracking and verification costs. Using the real-name reform for mobile payment platforms as an exogenous shock, this paper examines the impacts of mobile payments data on police enforcement within China's digitalized sex industry and its unintended consequences.

This study compiles the most comprehensive dataset on police enforcement in China. Our analysis reveals that the real-name reform resulted in an increase in arrests related to the sex industry, a higher proportion of arrests involving mobile payment records, and improvements in police enforcement efficiency. In addition, we observe an increase in the price of sex services, reflecting the heightened costs and risks associated with entering the sex industry. As the contraction of the sex industry resulted in greater unmet demand for sexual services, the reform inadvertently contributed to an increase in sexual violence, including sexual harassment and rape. In addition, we find evidence suggesting a decrease in the incidence of STIs, including HIV and syphilis. We also find that the reform was associated with higher divorce rates attributed to marital infidelity and an increase in domestic violence, although it had no significant effects on human trafficking.

Our findings have critical policy implications. First, both the long-standing debate and the evidence we provide here of mixed unintended consequences underscore the complexities involved in regulating the sex industry. This study does not take a position on how the sex industry should be regulated, but it seems reasonable for policymakers to keep these complexities in mind, as the optimal sex trade regulation remains an open question in academia. Second, although a growing body of literature has explored the positive role of technology in improving police capabilities, this paper not only reinforces this perspective but also highlights unintended consequences on both the positive and negative sides. The adoption of new technology can dramatically change enforcement capabilities and further shape socioeconomic outcomes, even without legislative changes. It is essential to consider potential trade-offs between legitimacy, efficiency, and social costs in designing optimal policies within the constraints of actual enforcement capabilities.

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Online Appendix

A Data Appendix

A.1 China Administrative Penalty Database

A.1.1 Introducing the Database

Under China's Administrative Penalty Law, an administrative penalty is defined as an action by which an administrative authority imposes a sanction—either by restricting rights or imposing additional obligations—on individuals, legal entities, or organizations that have violated administrative regulations in accordance with the law. The China Administrative Penalty (CAP) Database, compiled by www.pkulaw.com, encompasses over 30 million records of administrative penalties issued by central and local governments across China from 2000 to 2022. We provide a brief introduction to the database, along with basic descriptive patterns. Figure A.1 presents screenshots illustrating the database structure.

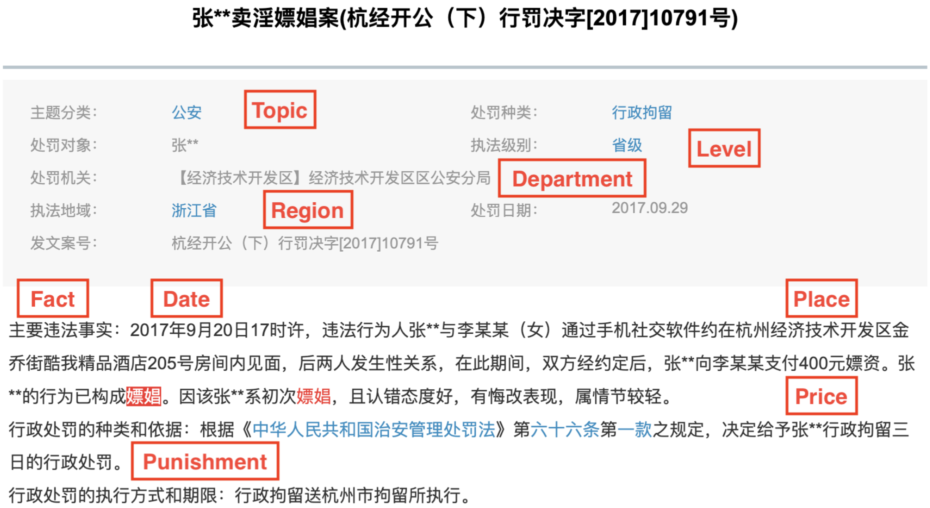
First, the number of administrative penalty records has surged since 2014, peaking in 2019 before declining. As illustrated in Panel A of Figure A.2, for the initial years of the database, there are only a few thousand records per year. However, for the years after 2014, this figure rises sharply, peaking at 5.5 million for 2019, before declining to 3.5 million for 2022. Two primary factors explain this rising and then declining trend. First, state capacity has expanded alongside China's economic growth, with the government investing significantly in enacting laws and regulations and enhancing enforcement authorities. The uneven spatial distribution shown in Panel B of Figure A.2 also reflects the influence of economic growth and state capacity: The number of records is markedly higher for eastern than for western regions, a pattern consistent with regional economic disparities. Second, government agencies are mandated to publicize administrative penalty records under transparency policies introduced prior to 2014. However, this shift toward transparency began to reverse after 2020, likely influenced by the COVID-19 pandemic and a general decrease in transparency measures.

Second, local governments account for a higher proportion of administrative penalties than governments at other levels, with significant variations across administrative levels. As shown in Panel A of Figure A.3, the total number of administrative penalty records rises substantially at lower administrative levels, largely because of the greater number of administrative units at these levels. For instance, nearly 20 million of the records, comprising 69% of the total, were issued by county-level governments. However, as shown in Panel B of Figure A.3, the number of province-level records per unit is, on average, the highest, followed by records at the prefecture and central levels, with county-level records being the least numerous per unit. Between 2000 and 2022, each provincial government issued approximately 36,000 administrative penalties, while each prefectural government issued about 23,000. Provincial and prefectural governments are particularly active in exercising administrative penalty authority, reflecting their extensive regulatory responsibilities and reach within local governance structures.

Third, a diverse array of government departments hold the authority to impose administrative penalties across various sectors. In total, 93 different types of government agencies issue administrative penalty records. Panel C of Figure A.3 highlights the top 20 departments with the highest number of penalty records, while Panel D categorizes these records by subject area. The data show that public-security-related penalties, primarily issued by the police, are by far the most prevalent, totaling over 13 million records and comprising 45.6% of all penalties. Following public security, market regulation and transportation represent the next largest categories, accounting for 19% and 12% of the penalty records, respectively. Departments overseeing fiscal and taxation matters and those tasked with land and urban development also represent significant shares, making up 7% and 6% of the total penalties, respectively.

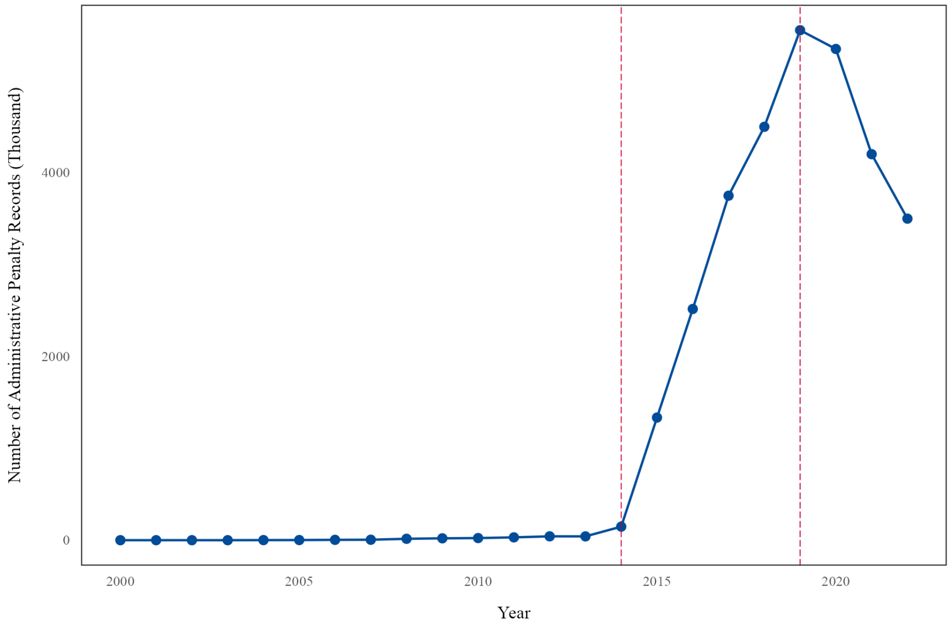
Finally, various types of punishments serve different roles in addressing violations by different subjects. As shown in Panel E of Figure A.3, fines and confiscations are the most frequently imposed penalties, comprising over 20 million records and accounting for 57% of all cases. This is followed by suspensions of licenses or production, and detention, which represent 16% and 7% of penalties, respectively. Note that authorities may apply multiple types of penalties simultaneously to address specific violations comprehensively. Panel F of Figure A.3 indicates a nearly balanced distribution of administrative penalties imposed on individuals versus legal entities.

(A) Screenshot of the Website

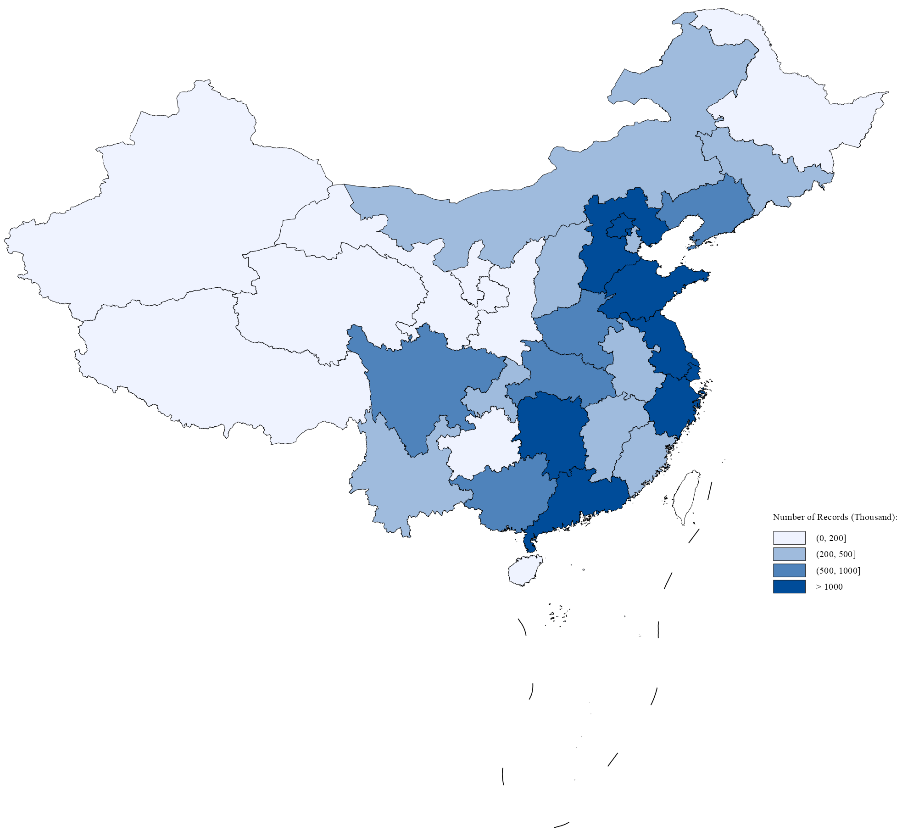


(B) An Example of a Sex Business Record

Figure A.1: Screenshots from the CAP Database

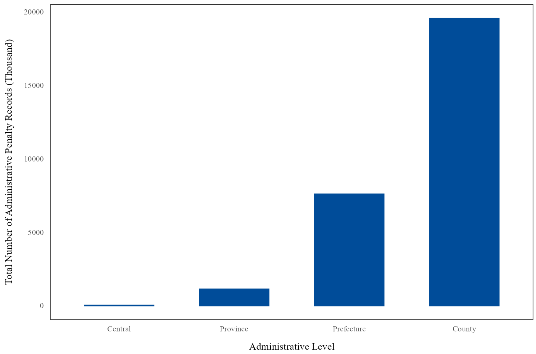


(A) Temporal Distribution from 2000 to 2022

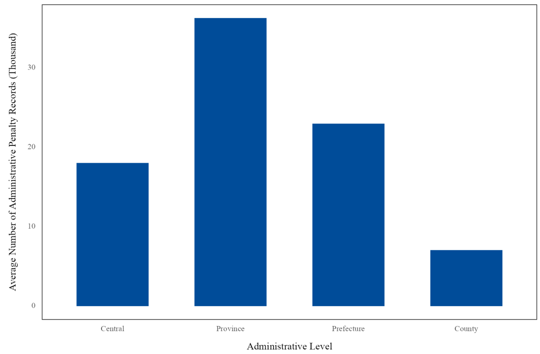


(B) Spatial Distribution Across Provinces, Summed from 2000 to 2022

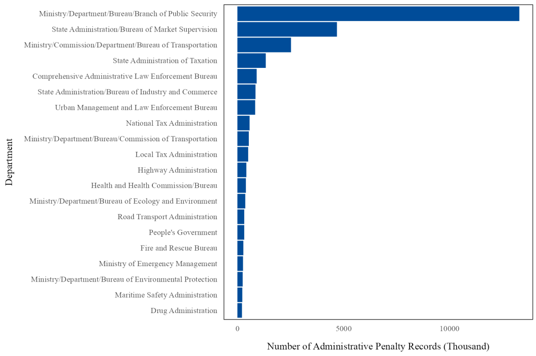
Figure A.2: Temporal and Spatial Distribution of Administrative Penalty Records



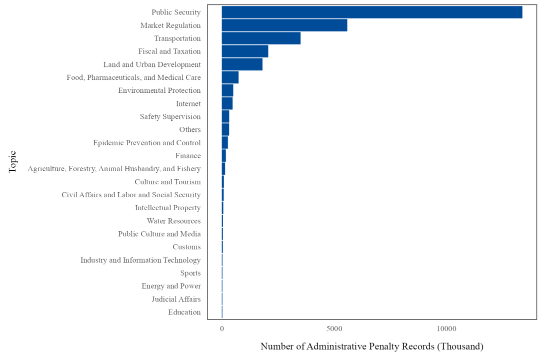
(A) By Administrative Level – Total



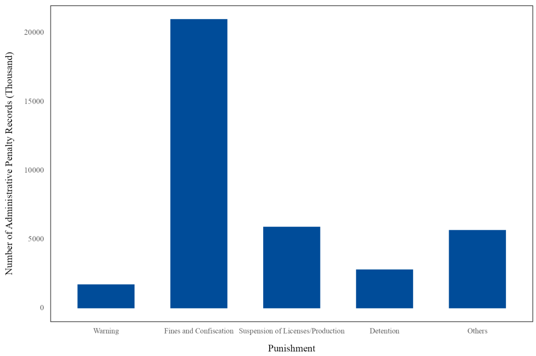
(B) By Administrative Level – Average



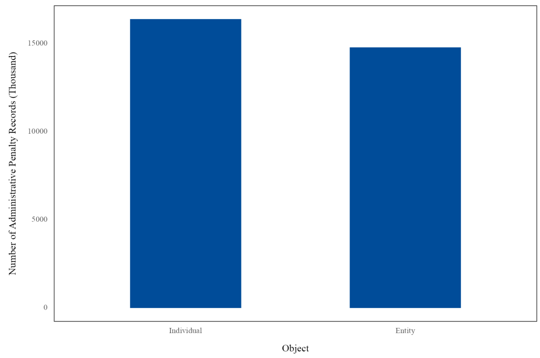
(C) By Department – Top 20



(D) By Topic



(E) By Punishment



(F) By Object

Figure A.3: Number of Administrative Penalty Records by Category, Summed from 2000 to 2022

Table A.1: Clauses Related to the Sex Industry

No.	Article
	Panel A. Criminal Law
358	Whoever organizes or forces anyone else into prostitution shall be sentenced to imprisonment of not less than five years but not more than ten years in addition to a fine; or be sentenced to imprisonment of not less than ten years or life imprisonment in addition to a fine or forfeiture of property if the circumstances are serious. Whoever organizes or forces any juvenile into prostitution shall be given a heavier penalty in accordance with the provisions of the preceding paragraph. Whoever commits the crime in the preceding two paragraphs and also commits murder, injuring, rape, kidnapping or any other crime shall be punished according to the provisions on the joinder of penalties for plural crimes. Whoever recruits or transports persons for an organizer of prostitution or otherwise assists in organizing prostitution shall be sentenced to imprisonment of not more than five years in addition to a fine; or if the circumstances are serious, be sentenced to imprisonment of not less than five years but not more than ten years in addition to a fine.
359	Those harboring prostitution or seducing or introducing others into prostitution are to be sentenced to five years or fewer in prison or put under limited incarceration or probation, in addition to paying a fine. If the case is serious, they are to be sentenced to five years or more in prison in addition to a fine. Those seducing young girls under 14 years of age into prostitution are to be sentenced to five years or more in prison in addition to a fine.
360	Those engaging in prostitution or visiting a whorehouse knowing that they are suffering from syphilis, clap, or other serious venereal diseases are to be sentenced to five years or fewer in prison or put under limited incarceration or probation, in addition to having to pay a fine.
361	Personnel of hotels, restaurants, entertainment industry, taxi companies, and other entities who take advantage of their entities' position to organize, force, seduce, harbor, or introduce others to prostitution are to be convicted and punished according to articles 358 and 359 of this law. Main persons in charge of the aforementioned entities who commit crimes stipulated in the above paragraph are to be severely punished.

362	Personnel of hotels, restaurants, entertainment industry, taxi companies, or other entities who inform law offenders and criminals while public security personnel are checking prostitution and whorehouse visiting activities, if the case is serious, are to be convicted and punished according to Article 310 of this law.
	Panel B. Public Security Administration Punishments Law
66	Anyone who whores or goes whoring shall be detained for not less than 10 days but not more than 15 days, and may be concurrently fined not more than 5,000 yuan. If the circumstances are relatively lenient, he (she) shall be detained for not more than 5 days or shall be fined not more than 500 yuan. Anyone who finds customers for any prostitute at a public place shall be detained for not more than 5 days or shall be fined not less than 500 yuan.
67	Anyone who induces, shelters, introduces any other person to prostitute shall be detained for not less than 10 days but not more than 15 days, and may be concurrently fined 5,000 yuan. If the circumstances are relatively lenient, he (she) shall be detained for not more than 5 days or shall be fined not 500 yuan.

Notes: This table lists the clauses related to the sex industry in the Criminal Law and Public Security Administration Punishments Law.

A.1.2 Filtering Illegal Activity Records

We identify illegal activity records through a systematic filtering process. First, we screen records using activity-specific keyword lists (see Table A.2) applied to both titles and text content. Next, we verify and exclude records containing false allegations. Finally, we retain only records involving a single, clearly identifiable illegal activity to ensure classification precision.

Table A.2: Keyword Lists to Filter Records

Illegal Activity	Keywords in Chinese
Sex Business	卖淫、嫖
Sexual Violence	猥亵、性骚扰、强奸
Marital Infidelity	出轨、婚外情、卖淫、嫖（基于离婚案件）
Domestic Violence	家暴、殴打妻子
Human Trafficking	拐卖、贩卖人口
Theft	偷、盗、窃
Robbery	抢劫

Notes: This table lists the keywords we use to filter records of interest.

A.1.3 Constructing the Variables

Our measures are constructed primarily through keyword searches, with additional validation performed with ChatGPT to ensure the accuracy of the extracted information. First, we extract the number of arrests from each record. In the CAP data, each record corresponds to a single arrest, whereas in the China Judgements Online (CJO) data, each record may involve multiple offenders, identified by means of defendant information. We then aggregate the total number of arrests to the prefecture–year level and normalize it by population per 100,000 people. On average, the number of arrests related to the sex industry is 3.22 per 100,000 people. On the basis of the reasons for punishment, we classify each arrest into three categories: sex organizer, sex worker, and sex buyer. We also calculate the number of arrests in each category per 100,000 people.

Second, we compute the share of arrests made on the basis of mobile payment records. Both the CAP and CJO datasets contain information about the facts and evidence of each case. Ideally, we identify cases involving mobile payment records using keywords such as ”mobile payment,” ”payment records,” ”Alipay,” and ”WeChat Pay.” For records lacking explicit mention, we determine whether the offender was arrested at the crime scene. We calculate the share of arrests involving mobile payment records by dividing the number of such arrests by the total number of arrests involving either mobile payments or on-site apprehensions. On average, approximately 18.95% of the arrests are made on the basis of mobile payment records.

Third, we calculate the share of arrests occurring in private crime scenes. Using a similar method, we label crime scenes as private if the sex business took place in homes or rented residential locations. Public crime scenes are defined as locations where police patrols are likely, such as clubs and massage parlors. We then calculate the share of arrests in private locations by dividing the number of private-location arrests by the total number of arrests in private or public locations. On average, 21.52% of the sex-industry-related arrests occurred in private locations.

Fourth, we extract data on the type and severity of punishments. Offenders involved in the sex industry could face one or more of four types of punishment: warnings, fines, confiscation of illegal gains, and detention. Detention is the most common penalty, applied in 85% of cases, and is followed by fines, issued in 32.6% of cases. Punishment severity varies, with the prefecture-level average fine being 18,646 yuan and the average detention lasting 534 days. Notably, the punishments in the CAP dataset are generally less severe than those in the CJO dataset.

Finally, we extract information on the price of sex services from case details. This information is critical for quantifying the reform’s impacts on the sex industry. We find that the prefecture-level average price for sex services in arrested cases is 433 yuan. However, since this price reflects only the sample that was arrested, the reader should exercise caution in making inferences about the broader market.

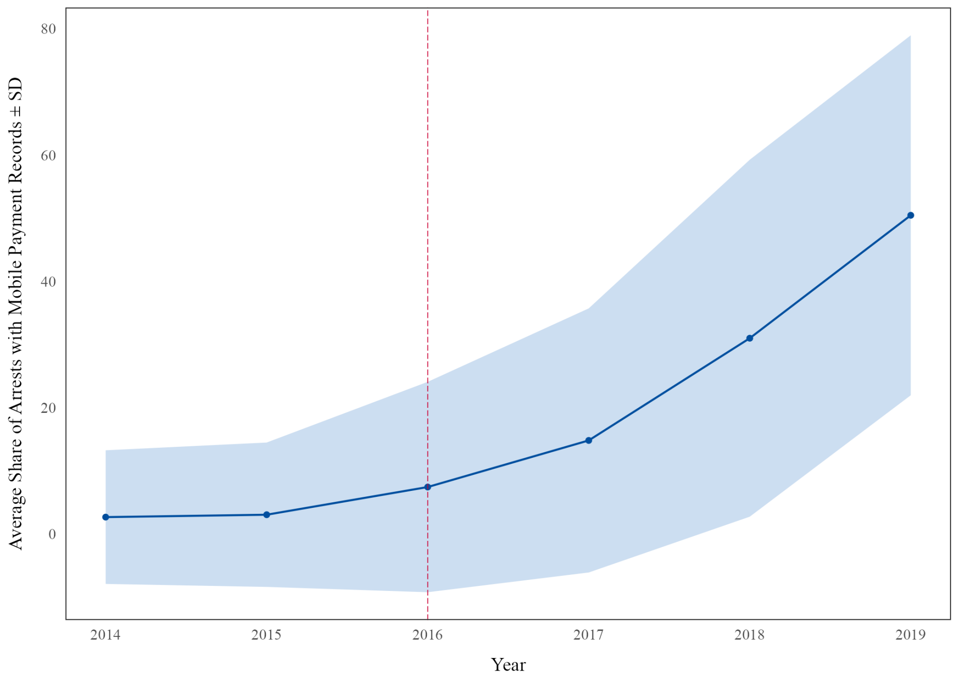


Figure A.4: Trend of Share of Arrests with Mobile Payment Records

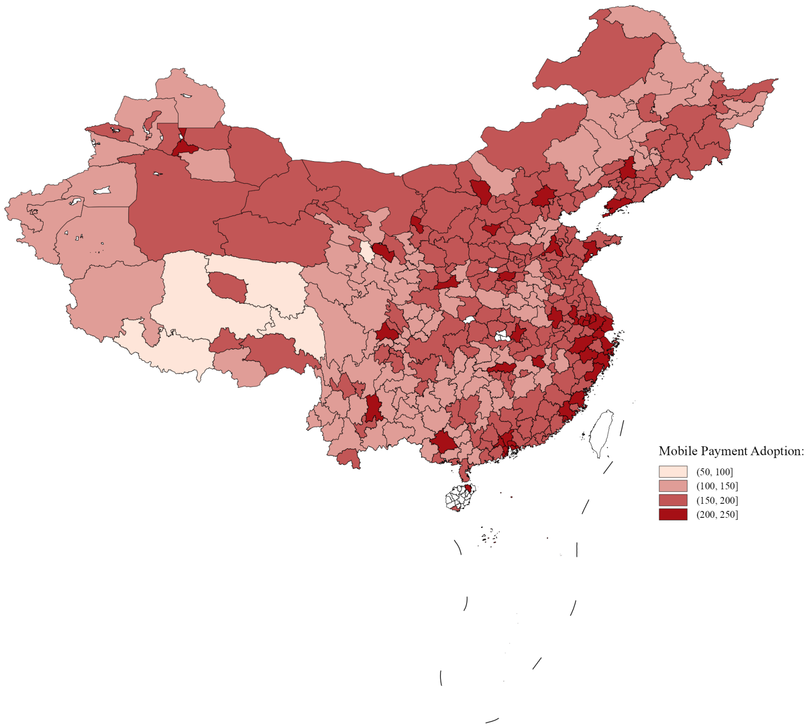
A.2 Digital Financial Inclusion Index of China

Table A.3: Digital Financial Inclusion Indicator System

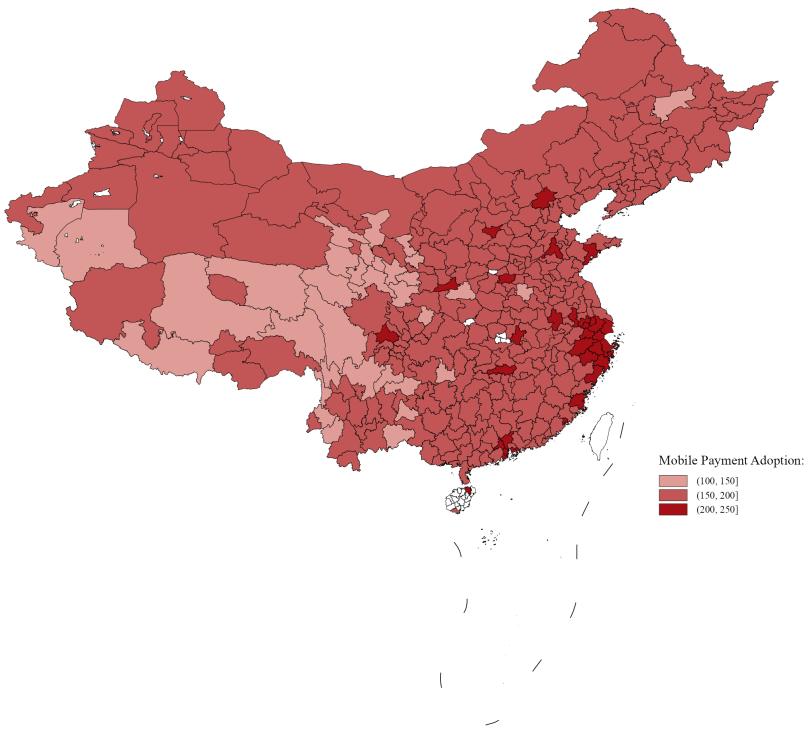
Primary Dimension	Secondary Dimension	Specific Indicator
Coverage Breadth	Account Coverage Rate	Number of Alipay accounts per 10,000 people
		Proportion of Alipay accounts with linked bankcards
		Average number of bankcards linked to each Alipay account
Usage Depth	Payment Services	Average number of payments per user
		Average payment amount per user
		Proportion of highly active users (50 or more transactions per year) among users active at least once a year
	Money Market Fund Services	Average number of Yu'e Bao purchases per user
		Average amount of Yu'e Bao purchases per user
		Number of Yu'e Bao purchasers per 10,000 Alipay users
	Credit Services	Number of Alipay users with internet consumer loans per 10,000 users
		Average number of loans per user
		Average loan amount per user
		Number of Alipay users with internet microenterprise loans per 10,000 users
		Average number of loans for microentrepreneurs
		Average loan amount for microentrepreneurs
	Insurance Services	Number of insured Alipay users per 10,000 users
		Average number of insurance policies per user
		Average insurance amount per user

	Investment Services	Number of Alipay users participating in internet investment and wealth management per 10,000 users Average number of investments per user Average investment amount per user
	Credit Services	Average credit inquiries per natural person Number of Alipay users utilizing credit-based services (including finance, accommodation, travel, social, etc.) per 10,000 users
Digitalization Level	Mobile Payment	Proportion of mobile payments in total transactions Proportion of mobile payment amount in total transaction volume
	Affordability	Average loan interest rate for microentrepreneurs Average loan interest rate for individuals
	Credit-based Payment	Proportion of Huabei payments in total transactions Proportion of Huabei payment amount in total transaction volume Proportion of transactions exempt from deposit via Sesame Credit (compared to cases requiring a deposit) Proportion of exempted payment amount via Sesame Credit (compared to cases requiring a deposit)
	Convenience	Proportion of QR code payments in total transactions Proportion of QR code payment amount in total transaction volume

Notes: This table lists the components of the Digital Financial Inclusion Index of China (DFIIC).



Panel A: Main Measurement – Digital Financial Index (Breadth)



Panel B: Alternative Measurement – Digital Financial Index

Figure A.5: Spatial Distribution of Mobile Payment Adoption Across Prefectures

Table A.4: Balance Tests

	(1) MPA	(2) Male Sex Ratio	(3) MPA × Male Sex Ratio
Male Sex Ratio	1.483		
	(1.564)		
GDP Per Capita (ln)	20.456***	-0.205*	-19.374
	(3.716)	(0.124)	(20.445)
GDP Growth Rate	-127.358***	1.663	220.552
	(29.508)	(1.490)	(245.609)
Share of Secondary Sector in GDP	11.329	-0.228	-46.506
	(23.142)	(0.627)	(97.974)
Share of Tertiary Sector in GDP	92.992***	-0.338	-24.438
	(32.591)	(0.714)	(116.197)
Fiscal Revenue Per Capita (ln)	11.723**	0.029	12.378
	(4.734)	(0.095)	(15.539)
Fiscal Expenditure Per Capita (ln)	-8.882**	0.162	21.618
	(3.703)	(0.104)	(17.218)
Observations	285	285	285
R-squared	0.754	0.039	0.029

Notes: This table reports the results of balance tests. *MPA* indicates mobile payment adoption, measured by the coverage breadth index from 2015. *Male Sex Ratio* is a dummy variable indicating whether the ratio of men to women in one prefecture is above the median. Other prefecture-level characteristics are from *China City Statistical Yearbook*.

B Robustness Checks

In this appendix, we provide robustness checks for the main results. These checks are classified into four types: using alternative measurements of mobile payment adoption, accounting for WeChat adoption, using alternative samples excluding outliers, and addressing data coverage concerns. In the first set of robustness checks, we (i) use binary treatment as a proxy for mobile payment adoption and (ii) use total DFIIC intensity as a measure of mobile payment adoption. Second, we account for WeChat adoption to address potential measurement concerns. Third, we (i) exclude Dongguan City to account for its effect as the ”sin city” of China and (ii) exclude Hangzhou City and Shenzhen City, the headquarters of Alipay and WeChat Pay, respectively. Finally, to reduce the concern about data coverage, we (i) control for data coverage trend, (ii) use only the CAP dataset, and (iii) use only the CJO dataset.

B.1 Alternative Measurement: Binary Treatment

Table B.1: Real-Name Reform and Police Enforcement in the Sex Industry – DID

	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	4.082***	3.596**	5.308**	5.009**	0.472	-0.104
× Post	(1.567)	(1.577)	(2.359)	(2.362)	(0.529)	(0.066)
Observations	1,710	1,710	1,086	1,086	1,710	1,710
R-squared	0.756	0.759	0.717	0.718	0.924	0.921
Preshock Mean Y	1.043	1.043	4.160	4.160	12.294	1.205
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.2: Real-Name Reform and Police Enforcement in the Sex Industry – DDD

	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	6.159***	6.268***	13.829***	13.526***	1.617	-0.113
× Post × Male Sex Ratio	(2.349)	(2.392)	(4.365)	(4.375)	(1.066)	(0.126)
Observations	1,710	1,710	1,086	1,086	1,710	1,710
R-squared	0.769	0.771	0.722	0.722	0.924	0.922
Preshock Mean Y	1.043	1.043	4.160	4.160	12.294	1.205
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DDD results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.3: Real-Name Reform and Sexual Violence – DID

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.213**	0.176**	0.197***	0.164***	0.035	0.034
× Post	(0.089)	(0.084)	(0.056)	(0.051)	(0.043)	(0.044)
Observations	1,710	1,710	1,710	1,710	1,710	1,710
R-squared	0.775	0.778	0.698	0.703	0.794	0.795
Preshock Mean Y	0.344	0.344	0.076	0.076	0.252	0.252
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.4: Real-Name Reform and Sexual Violence – DDD

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.427***	0.435***	0.265***	0.265***	0.149*	0.155**
× Post × Male Sex Ratio	(0.145)	(0.147)	(0.091)	(0.092)	(0.076)	(0.076)
Observations	1,710	1,710	1,710	1,710	1,710	1,710
R-squared	0.783	0.785	0.709	0.713	0.797	0.797
Preshock Mean Y	0.344	0.344	0.076	0.076	0.252	0.252
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov×Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DDD results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

B.2 Alternative Measurement: Total DFIIC

Table B.5: Real-Name Reform and Police Enforcement in the Sex Industry – DID

	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption × Post	0.265***	0.252***	0.116*	0.107*	0.039	-0.008*
	(0.079)	(0.078)	(0.064)	(0.064)	(0.029)	(0.004)
Observations	1,710	1,710	1,086	1,086	1,710	1,710
R-squared	0.769	0.770	0.716	0.717	0.924	0.922
Preshock Mean Y	1.043	1.043	4.160	4.160	12.294	1.205
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.6: Real-Name Reform and Police Enforcement in the Sex Industry – DDD

	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.328***	0.329***	0.280***	0.269**	0.061	-0.005
$\times$ Post $\times$ Male Sex Ratio	(0.093)	(0.094)	(0.107)	(0.107)	(0.044)	(0.005)
Observations	1,710	1,710	1,086	1,086	1,710	1,710
R-squared	0.788	0.789	0.719	0.720	0.925	0.923
Preshock Mean Y	1.043	1.043	4.160	4.160	12.294	1.205
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov$\times$Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DDD results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.7: Real-Name Reform and Sexual Violence – DID

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.015***	0.014***	0.012***	0.011***	0.003**	0.003**
× Post	(0.004)	(0.004)	(0.003)	(0.003)	(0.001)	(0.001)
Observations	1,710	1,710	1,710	1,710	1,710	1,710
R-squared	0.786	0.787	0.717	0.720	0.796	0.797
Preshock Mean Y	0.344	0.344	0.076	0.076	0.252	0.252
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov×Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.8: Real-Name Reform and Sexual Violence – DDD

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.018***	0.018***	0.012***	0.012***	0.006***	0.006***
× Post × Male Sex Ratio	(0.005)	(0.005)	(0.004)	(0.004)	(0.002)	(0.002)
Observations	1,710	1,710	1,710	1,710	1,710	1,710
R-squared	0.797	0.798	0.733	0.736	0.800	0.800
Preshock Mean Y	0.344	0.344	0.076	0.076	0.252	0.252
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DDD results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

B.3 WeChat Adoption

Table B.9: Real-Name Reform and Police Enforcement in the Sex Industry – DID

	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption × Post	6.780***	6.536***	1.955	1.789	0.732	-0.127
	(2.463)	(2.441)	(1.261)	(1.266)	(0.698)	(0.134)
Observations	1,560	1,560	1,020	1,020	1,560	1,560
R-squared	0.776	0.778	0.714	0.714	0.925	0.923
Preshock Mean Y	1.101	1.101	4.097	4.097	12.548	1.226
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.10: Real-Name Reform and Police Enforcement in the Sex Industry – DDD

	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	10.991***	10.803***	4.558**	4.444*	1.540	-0.022
× Post × Male Sex Ratio	(3.531)	(3.481)	(2.264)	(2.263)	(1.240)	(0.214)
Observations	1,560	1,560	1,020	1,020	1,560	1,560
R-squared	0.796	0.798	0.715	0.716	0.926	0.923
Preshock Mean Y	1.101	1.101	4.097	4.097	12.548	1.226
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov×Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DDD results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.11: Real-Name Reform and Sexual Violence – DID

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.351***	0.337***	0.292***	0.278***	0.070**	0.071**
× Post	(0.108)	(0.102)	(0.083)	(0.077)	(0.033)	(0.033)
Observations	1,560	1,560	1,560	1,560	1,560	1,560
R-squared	0.783	0.785	0.725	0.730	0.786	0.787
Preshock Mean Y	0.313	0.313	0.071	0.071	0.225	0.225
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.12: Real-Name Reform and Sexual Violence – DDD

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.506***	0.496***	0.372***	0.362***	0.134***	0.134***
× Post × Male Sex Ratio	(0.157)	(0.152)	(0.122)	(0.116)	(0.052)	(0.051)
Observations	1,560	1,560	1,560	1,560	1,560	1,560
R-squared	0.794	0.796	0.742	0.746	0.789	0.790
Preshock Mean Y	0.313	0.313	0.071	0.071	0.225	0.225
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DDD results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

B.4 Alternative Sample: Excluding Outliers

Table B.13: Real-Name Reform and Police Enforcement in the Sex Industry – DID

	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.145***	0.143**	0.090**	0.085*	0.033*	-0.005***
× Post	(0.054)	(0.056)	(0.045)	(0.045)	(0.020)	(0.002)
Observations	1,692	1,692	1,068	1,068	1,692	1,692
R-squared	0.790	0.790	0.715	0.715	0.915	0.898
Preshock Mean Y	0.936	0.936	4.060	4.060	11.763	1.120
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.14: Real-Name Reform and Police Enforcement in the Sex Industry – DDD

	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.192***	0.193***	0.204**	0.198**	0.051*	-0.003
× Post × Male Sex Ratio	(0.067)	(0.067)	(0.087)	(0.088)	(0.030)	(0.003)
Observations	1,692	1,692	1,068	1,068	1,692	1,692
R-squared	0.806	0.806	0.717	0.718	0.916	0.899
Preshock Mean Y	0.936	0.936	4.060	4.060	11.763	1.120
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DDD results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.15: Real-Name Reform and Sexual Violence – DID

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.008***	0.008***	0.006***	0.006***	0.002**	0.002**
× Post	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Observations	1,692	1,692	1,692	1,692	1,692	1,692
R-squared	0.811	0.811	0.742	0.744	0.800	0.801
Preshock Mean Y	0.340	0.340	0.073	0.073	0.250	0.250
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov×Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.16: Real-Name Reform and Sexual Violence – DDD

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.010***	0.011***	0.007***	0.007***	0.004**	0.004**
× Post × Male Sex Ratio	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	1,692	1,692	1,692	1,692	1,692	1,692
R-squared	0.818	0.819	0.756	0.758	0.803	0.803
Preshock Mean Y	0.340	0.340	0.073	0.073	0.250	0.250
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov×Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DDD results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

B.5 Data Coverage: Controlling for Coverage Trend

Table B.17: Real-Name Reform and Police Enforcement in the Sex Industry – DID

	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.191***	0.182***	0.087**	0.082**	0.029	-0.005
× Post	(0.057)	(0.057)	(0.041)	(0.041)	(0.022)	(0.003)
Data Coverage Before 2014	2.540	2.523*	0.296	0.153	-0.308	-0.029
× Post	(1.568)	(1.521)	(2.519)	(2.539)	(0.765)	(0.070)
Observations	1,710	1,710	1,086	1,086	1,710	1,710
R-squared	0.774	0.775	0.717	0.717	0.924	0.922
Preshock Mean Y	1.043	1.043	4.160	4.160	12.294	1.205
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.18: Real-Name Reform and Police Enforcement in the Sex Industry – DDD

	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.233***	0.238***	0.181**	0.174**	0.045	-0.003
× Post × Male Sex Ratio	(0.067)	(0.068)	(0.079)	(0.080)	(0.033)	(0.004)
Data Coverage Before 2014	2.084	1.736	-0.387	-0.185	-0.811	-0.007
× Post × Male Sex Ratio	(2.371)	(2.305)	(4.628)	(4.628)	(1.420)	(0.115)
Observations	1,710	1,710	1,086	1,086	1,710	1,710
R-squared	0.793	0.794	0.719	0.720	0.925	0.923
Preshock Mean Y	1.043	1.043	4.160	4.160	12.294	1.205
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DDD results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.19: Real-Name Reform and Sexual Violence – DID

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.011***	0.011***	0.009***	0.008***	0.003**	0.003**
× Post	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
Data Coverage Before 2014	0.071	0.073	0.060	0.064	-0.012	-0.015
× Post	(0.106)	(0.104)	(0.064)	(0.063)	(0.051)	(0.051)
Observations	1,710	1,710	1,710	1,710	1,710	1,710
R-squared	0.789	0.790	0.723	0.725	0.797	0.797
Preshock Mean Y	0.344	0.344	0.076	0.076	0.252	0.252
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.20: Real-Name Reform and Sexual Violence – DDD

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.013***	0.014***	0.009***	0.009***	0.005***	0.005***
× Post × Male Sex Ratio	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)
Data Coverage Before 2014	-0.134	-0.155	-0.008	-0.020	-0.112	-0.118
× Post × Male Sex Ratio	(0.190)	(0.189)	(0.109)	(0.108)	(0.097)	(0.096)
Observations	1,710	1,710	1,710	1,710	1,710	1,710
R-squared	0.800	0.801	0.738	0.741	0.801	0.802
Preshock Mean Y	0.344	0.344	0.076	0.076	0.252	0.252
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov×Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DDD results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

B.6 Data Coverage: Only CAP Dataset

Table B.21: Real-Name Reform and Police Enforcement in the Sex Industry – DID

	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.192***	0.184***	0.001	-0.012	0.028	-0.005
× Post	(0.061)	(0.061)	(0.083)	(0.079)	(0.021)	(0.003)
Observations	1,710	1,710	186	186	1,710	1,710
R-squared	0.758	0.759	0.767	0.769	0.924	0.922
Preshock Mean Y	0.617	0.617	1.245	1.245	12.294	1.205
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov×Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.22: Real-Name Reform and Police Enforcement in the Sex Industry – DDD

	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.238***	0.241***	0.025	0.020	0.041	-0.003
× Post × Male Sex Ratio	(0.071)	(0.072)	(0.091)	(0.088)	(0.031)	(0.004)
Observations	1,710	1,710	186	186	1,710	1,710
R-squared	0.778	0.780	0.770	0.772	0.925	0.922
Preshock Mean Y	0.617	0.617	1.245	1.245	12.294	1.205
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DDD results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.23: Real-Name Reform and Sexual Violence – DID

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.007***	0.006***	0.006***	0.006***	0.000**	0.000**
× Post	(0.002)	(0.002)	(0.002)	(0.002)	(0.000)	(0.000)
Observations	1,710	1,710	1,710	1,710	1,710	1,710
R-squared	0.683	0.686	0.681	0.684	0.532	0.535
Preshock Mean Y	0.018	0.018	0.015	0.015	0.003	0.003
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.24: Real-Name Reform and Sexual Violence – DDD

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.007***	0.007***	0.007***	0.007***	0.000**	0.000**
× Post × Male Sex Ratio	(0.003)	(0.003)	(0.003)	(0.003)	(0.000)	(0.000)
Observations	1,710	1,710	1,710	1,710	1,710	1,710
R-squared	0.703	0.706	0.700	0.704	0.544	0.546
Preshock Mean Y	0.018	0.018	0.015	0.015	0.003	0.003
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DDD results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

B.7 Data Coverage: Only CJO Dataset

Table B.25: Real-Name Reform and Police Enforcement in the Sex Industry – DID

	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.008**	0.007**	0.083*	0.076*	0.028	-0.005
× Post	(0.003)	(0.004)	(0.044)	(0.044)	(0.021)	(0.003)
Observations	1,710	1,710	1,062	1,062	1,710	1,710
R-squared	0.783	0.784	0.740	0.741	0.924	0.922
Preshock Mean Y	0.425	0.425	4.510	4.510	12.294	1.205
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.26: Real-Name Reform and Police Enforcement in the Sex Industry – DDD

	Police Enforcement in Sex Industry				Placebo Tests	
	Number of Sex Business Arrests Per 100,000		Share of Arrests with Mobile Payment Records (%)		Number of Arrests Per 100,000	
					Theft	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.007	0.007	0.168**	0.159*	0.041	-0.003
× Post × Male Sex Ratio	(0.004)	(0.004)	(0.084)	(0.085)	(0.031)	(0.004)
Observations	1,710	1,710	1,062	1,062	1,710	1,710
R-squared	0.785	0.785	0.742	0.743	0.925	0.922
Preshock Mean Y	0.425	0.425	4.510	4.510	12.294	1.205
Control Vars	No	Yes	No	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DDD results of the real-name reform on police enforcement in the sex industry and placebo tests. Columns (1)–(2) focus on the effect on the number of sex business arrests per 100,000. Columns (3)–(4) examine the effect on the share of arrests involving mobile payment records. Columns (5)–(6) report the results of placebo tests using the number of arrests for theft or robbery per 100,000 as outcomes. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.27: Real-Name Reform and Sexual Violence – DID

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.005***	0.005***	0.003***	0.003***	0.002**	0.003**
× Post	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Observations	1,710	1,710	1,710	1,710	1,710	1,710
R-squared	0.823	0.823	0.732	0.734	0.796	0.797
Preshock Mean Y	0.326	0.326	0.060	0.060	0.249	0.249
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov×Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DID results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table B.28: Real-Name Reform and Sexual Violence – DDD

	Number of Arrests for Sexual Violence Per 100,000					
	Sexual Violence		Sexual Harassment		Rape	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.006***	0.006***	0.002**	0.002**	0.004***	0.004***
× Post × Male Sex Ratio	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	1,710	1,710	1,710	1,710	1,710	1,710
R-squared	0.825	0.826	0.735	0.737	0.799	0.800
Preshock Mean Y	0.326	0.326	0.060	0.060	0.249	0.249
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the DDD results of the real-name reform on the number of arrests for sexual violence. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual harassment per 100,000. Columns (5)–(6) report the effect on the number of arrests for rape per 100,000. Control variables include the logarithms of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. We also control for all two-way interaction terms in all columns. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

C Additional Results

Table C.1: Real-Name Reform and Police Enforcement for Different Sex Industry Participants

	Number of Sex Business Arrests Per 100,000					
	Sex Organizer		Sex Worker		Sex Buyer	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	0.057***	0.057***	0.068***	0.064***	0.076***	0.071***
× Post	(0.018)	(0.019)	(0.025)	(0.024)	(0.023)	(0.022)
Observations	1,710	1,710	1,710	1,710	1,710	1,710
R-squared	0.726	0.726	0.734	0.735	0.767	0.769
Preshock Mean Y	0.520	0.520	0.243	0.243	0.279	0.279
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the results of the real-name reform on the number of arrests for different types of sex businesses. Columns (1)–(2) focus on the effect on the number of arrests for pimp per 100,000. Columns (3)–(4) examine the effect on the number of arrests for prostitution per 100,000. Columns (5)–(6) report the effect on the number of arrests for clients per 100,000. Control variables include the logarithm of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table C.2: Real-Name Reform and Different Types of Punishments for Sex Business Violations

	Share of Fines (%)		Share of Expropriation (%)		Share of Detention (%)	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Payment Adoption	-0.052*	-0.050*	0.035	0.034	0.023	0.020
× Post	(0.030)	(0.029)	(0.053)	(0.054)	(0.022)	(0.022)
Observations	1,206	1,206	1,206	1,206	1,206	1,206
R-squared	0.869	0.870	0.586	0.586	0.651	0.653
Preshock Mean Y	92.688	92.688	24.854	24.854	97.144	97.144
Control Vars	No	Yes	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Prov×Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the results of the real-name reform on the share of different types of punishments for sex businesses. Columns (1)–(2) focus on the effect on the share of fines. Columns (3)–(4) examine the effect on the share of expropriation. Columns (5)–(6) report the effect on the share of detention. Control variables include the logarithm of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.

Table C.3: Real-Name Reform and Sexual Violence Involving Sex Workers and Other Women

	Number of Arrests for Sexual Violence Per 100,000			
	Sex Workers		Other Women	
	(1)	(2)	(3)	(4)
Mobile Payment Adoption	0.0001	0.0001	0.0114***	0.0109***
× Post	(0.0000)	(0.0000)	(0.0029)	(0.0026)
Observations	1,710	1,710	1,710	1,710
R-squared	0.3696	0.3710	0.7903	0.7913
Preshock Mean Y	0.003	0.003	0.332	0.332
Control Vars	No	Yes	No	Yes
Prefecture FE	Yes	Yes	Yes	Yes
Prov×Year FE	Yes	Yes	Yes	Yes

Notes: This table reports the results of the real-name reform on the number of arrests for sexual violence involving sex workers and other women. Columns (1)–(2) focus on the effect on the total number of arrests for sexual violence involving sex workers per 100,000. Columns (3)–(4) examine the effect on the number of arrests for sexual violence involving other women per 100,000. Control variables include the logarithm of GDP per capita, fiscal revenue per capita, and fiscal expenditure per capita. Standard errors are clustered at the prefecture level. * significant at the 10% level. ** significant at the 5% level. *** significant at the 1% level.