Title

Essays in Microeconomics

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Publication Date

2024

UNIVERSITY OF CALIFORNIA
SANTA CRUZ

ESSAYS IN APPLIED MICROECONOMICS

A dissertation submitted in partial satisfaction
of the requirements for the degree of

DOCTOR OF PHILOSOPHY

in

ECONOMICS

by

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June 2024

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Contents

Figures and Tables

List of Figures

List of Tables

Abstract

Essays in Applied Microeconomics

by

Ken Suzuki

This dissertation uses statistical causal inference methods to answer causal questions in the fields of health economics and economics of culture and institutions. The first chapter is an application of examiner designs in health economics. Despite efforts to reduce emergency department (ED) care and transition patients to alternative settings, there is limited evidence on its impact on patient outcomes. This chapter studies patients that call into a nurse advice line that are directed toward ED care in a rater instrumental variables design. Marginal patients are 5.3 percentage points more likely to be admitted as an inpatient within 3 days since triage and 3.7 percentage points more likely to have a second ED visit within 4 to 30 days. This chapter also shows increased outpatient utilization with no differences in short-term mortality, indicating efforts to divert patients to less acute settings are likely justified.

The second chapter examines the origins of the cross-nurse difference in average ED visit rates of similar patients quasi-randomly assigned to telephone triage nurses. Medical practitioners often make substantially different choices for similar patients. This chapter examines how variation in telephone triage practice style across nurses affect downstream patient healthcare utilization. While a triage decision-support tool standardizes the telephone triage process across nurses, the triage nurses can still exercise discretion through two potential margins: (i) overriding triage recommendations and (ii)

intensifying verbal communication to ensure patient compliance with triage disposition. I construct two nurse practice measures to quantify each nurse's average ED recommendation and verbal communication tendencies, exploiting quasi-random assignment of calls to nurses within call centers. My reduced-form estimates suggest that reassigning a call to a nurse with a higher ED recommendation and a longer call duration tendency increases the patient's probability of seeking in-person medical attention.

The third chapter investigates the causal effects of superstitious beliefs that discriminate against women born in the years of Goat in the Chinese zodiac. Translating the Goat-year superstition into a simple framework of partner search under the zodiac discrimination, I consider two empirical strategies to test for the presence of marriage discrimination: a regression discontinuity (RD) design and a difference-in-differences (DID) method. The RD design identifies the superstition effects if women born just before and after the Goat-new year had, on average, the same unobserved characteristics. However, I find that marriage and other socioeconomic outcomes exhibit persistent seasonal patterns, suggesting that the Goat and adjacent birth cohorts are not comparable without controlling for the unobserved differences. Our DID method controls for the unobserved differences across cohorts around the RD threshold and identifies the causal effects of the Goat-year superstition. Applying the DID method to the 1979 Goat-year women in a 1% sample of the 2000 Chinese Population Census, I find no statistical evidence of the superstition effects on marriage and other outcomes.

To the beauty and resilience of the wildlife in Santa Cruz,

Acknowledgements

My academic journey at UCSC would not have been possible without the support and encouragement of the people I met. First and foremost, I want to thank my advisor, Carlos Dobkin, for trusting me throughout my research career at UCSC, although many research seeds in my earlier years at UCSC did not flourish. Even when my ideas were unsuccessful, he always appreciated my attempts and nudged me into a new direction. Countless discussions with him have continuously broadened my perspectives.

I also want to sincerely thank my other committee members, Laura Giuliano and Justin Marion, for constructive feedback on my research work. Their support was indispensable in my building a research career in applied microeconomics. Beyond my committee, I appreciate all the comments I received from Michael Leung, Jon Robinson, George Bulman, Jeremy West, and Ajay Shenoy.

Moreover, I would also like to thank my coauthors of the first chapter, Liam Rose, Linda Diem Tran, and Anita Vashi. Liam has always shown me the importance of having policy-relevant research questions in health economics. Diem has generously provided me with the data. Anita has helped me interpret the analysis results from the perspective of medicine.

Finally, I am grateful to my friends throughout my time at UCSC. Knowing the research projects of my friends in health economics, Haedong Aiden Rho, Hyunjin Yun, Hamid Habibi, and Monica Shandal, has been exciting and motivating. My fellow Ph.D. students in the Economics Department have made my academic and nonacademic life in Santa Cruz enjoyable. I will never forget my time with them in discussions, food, and sports.

Chapter 1

What Does the Emergency Department Do?: Evidence from a Nurse Triage[1]

§ 1.1 Introduction

Health care in emergency departments (EDs) accounts for over 5% of US health care spending in addition to being the source of over 70% of inpatient admissions (Scott et al., 2021). While ED care is clearly beneficial to patients with the most acute symptoms, much of the care provided in EDs has been identified as a misallocation of resources and a potential source of cost savings. It has been estimated that over 30% of ED visits could be treated in less acute settings, making ED care the subject of intense focus from policymakers (Weinick et al., 2010; Vashi et al., 2019). As a result, substantial efforts have been undertaken to move patients to alternative care settings, including opening urgent care centers, improving access to primary care, expanding nurse advice lines,

[1]This chapter is based on a joint project with Liam Rose, Linda Diem Tran, and Anita Vashi. The views expressed herein are those of the authors and do not necessarily reflect those of the United States Department of Veterans Affairs or the Veterans Health Administrations.

offering provider incentives, and providing case managers from frequent ED utilizers.[2]

However, there is little to no evidence of the effect of ED care on patient outcomes. ED care plays a crucial role in the chain of survival for acute conditions such as myocardial infarction, stroke, and trauma, but less clear is how it affects patients on the margin. Those that receive care in EDs are fundamentally different from those that receive care in less acute settings, and patients that go to an ED are naturally more motivated to receive immediate care. Some patients may have had equivalent outcomes from less intensive care, while some individuals that avoided the ED may have benefited from the immediate attention. We avoid this issue by studying patients experiencing symptoms that call in to a nurse advice line. These lines provides individuals with access to medical personnel – most often a registered nurse – who can offer health-related information, advice, and guidance. Nurse advice lines have become ubiquitous across health care systems and insurance networks, including all major US insurance companies, national health systems such as the NHS, and state and federal health agencies. We examine patients in the Veterans Affairs health care system (VA). Using the rater instrumental variables design, we are able to identify patients that are convinced to go to the ED by virtue of the quasi-random assignment of nurses to patients that call into a VA nurse advice line.

We show that there is wide variation in the propensity of nurses to have patients visit the ED, even when accounting for patient location and when the call was placed. Our

[2]Evaluations of these efforts have shown mixed results (Flores-Mateo et al., 2012; Raven et al., 2016). Increases in primary care access successfully reduced ED visits in Uninsured and Medicaid populations (Sadowski et al., 2009; Retchin et al., 2009), but case management, individualized care plans, and information sharing were not consistently effective (Soril et al., 2015). Urgent care centers have been shown to be substitutes for EDs when examined through available hours (Allen et al., 2021) and opening and closing of clinics (Alexander et al., 2019), but may also increase overall health care spending through downstream hospital admissions (Currie et al., 2023).

first-stage estimate implies that changing a call assignment from a nurse at the 5th to a nurse at the 95th percentile of the ED propensity distribution increases the probability of ED visit by 10.5 percentage points, a 54 percent change from the 3-day ED visit rate of 19.4%. We show strong balance across observable covariates for patients assigned to nurses with different ED propensities.

Our IV results show that the marginal patient is 5.3 percentage points (pp) more likely to be admitted as an inpatient within 3 days since triage, relative to the sample mean of 2.3%. They are 3.7 pp more likely to have a second ED visit within 4 to 30 days and 32.7 pp less likely to have visit with a primary care within 3 days. However, unlike naive OLS estimates, we find no effect on 30-day mortality. These patients are also no less likely to call the nurse triage line again. Finally, we then show that compliers were more likely to have had ED visits and hospitalizations in the year preceding the call, as well as being more likely to have utilized telemedicine in the previous year.

This paper contributes to several strands of literature. First, it contributes to a very limited literature on ED effectiveness. It is well-established that ED overcrowding degrades the quality of treatment (Morley et al., 2018) , but Turner et al. (2020) and Hoe (2022) show that it has only small effect on revisits and no effect on downstream mortality. A review by Lauque et al. (2022) did not find an association between length of ED visit before inpatient admission and in-hospital mortality.

Second, it is related to previous on resource allocation in the health care sector. While there is great interest in productivity in the health care sector, patient allocation to different care settings is understudied. In post-acute care, Rose (2020) finds that skilled nursing facility (SNF) care reduces readmissions over home health or self-care, Werner et al. (2019) shows that home health care is associated with higher readmissions over SNFS, and Einav et al. (2023) finds that long-term care hospitals are equal or worse for patients outcomes than SNFs. There has been some work comparing ambulatory surgical centers (ACS) outpatient surgery performed at hospitals, with either better or equal outcomes at ACSs (Munnich and Parente, 2018; Aouad et al., 2019). There is additionally work comparing operative and non-operative management of certain conditions (Katz et al., 2013). These comparisons do not deal with acute care, however. Outcomes of visits that pass through the emergency department such as acute myocardial infraction (AMI) are often used as an indicator of hospital quality (e.g. Chandra et al. (2016)), but this does not touch on the impact of the emergency department itself. Chan and Chen (2022) and Silver (2021) study provider productivity within the ED, but can only consider patients within that setting.

There is a vast literature on overuse in the ED setting, with a number of different methods of classifying which care is not clinically appropriate for the ED (Weinick et al., 2010; Uscher-Pines et al., 2013; Sabbatini et al., 2014; Vashi et al., 2019). These works suggest that patients could be seen in lower acuity settings such as primary care or urgent care, but are unable to estimate the effects of reallocating patients away from the ED on patient outcomes. While Alexander et al. (2019) and Allen et al. (2021) both showed urgent care substituted for ED care, downstream outcomes were not available. Our work improves on this by examining how ED care affects outcomes for the marginal patient that might have otherwise been seen in either a lower acuity setting or at a later date.

Finally, this paper complements previous work on nurse triage lines. Previous work on this topic has shown mixed effects of nurse triage lines on utilization, with most not finding decreases in primary care or ED use (Lake et al., 2017; Boggan et al., 2020).

While this paper does not evaluate the effect of these lines directly, it does provide insight on the downstream effects of decisions by providers working in this setting.

In the next section we provide brief background about VA health care and the nurse triage program. This is followed by a presentation of our empirical strategy, results, and conclusion.

§ 1.2 Background

The VA operates one of the nation's largest health care systems, providing care to approximately 10 million veterans at 171 medical centers and 1113 outpatient facilities distributed across the country. To receive VA health care, an individual must have served and been honorably discharged from the military and qualify under at least one of three broad categories: have a disability connected to their service, have income below a set threshold, or have been discharged within the last five years.[3] In a given year, VA provides care to about one-third of US veterans, providing extensive service in a vertically-integrated system that includes primary care, mental health care, specialty care, acute care, and long-term care.

VA medical centers and outpatient clinics generally operate on a "hub-and-spoke" model, where regional medical centers work together with a number of nearby outpatient clinics. Medical centers are then geographically divided into 18 regional care systems known as Veteran Integrated Service Networks (VISNs). Historically, medical centers and VISNs developed their own call centers to serve as entry points for veterans and their

[3] VA uses the Department of Housing and Urban Development's annual geographic-based income limits, further allowing individuals to be 10% over the threshold if they agree to pay copays. Over 80% of enrolled veterans face no cost sharing.

families. The call centers provide frequently used administrative and clinical services. While services have differed somewhat among call centers, they all provided some form of assistance with appointment scheduling, enrollment questions, pharmacy services, and nurse triage.

Nurse triage services allow patients to speak with a Registered Nurse (RN) for evaluation of symptoms and disposition of health care concerns. The telephone triage process involves ranking veterans' health concerns according to urgency, and then using standardized physician-approved protocols to guide the nurse through a targeted medical history and provide uniform triage recommendations. These decision-support tools provide the nurse with recommendations for disposition (e.g. ED, urgent care, primary care, or self-care) and follow-up intervals (e.g. 911 now, 0–2 hours, 2–8 hours, 10–24 hours, within 3 days, within 2 weeks). While these recommendations are noted in the patients' medical record, the triage nurse does not initiate further contact with the patient to ensure the recommendation is followed.

In addition to providing services directly, VA also purchases care from non-VA providers. Importantly, this includes emergency care, with more than one-third of ED visits involving VA occurring at non-VA facilities. VA encourages enrollees that consider their life or health to be in danger to seek immediate medical attention, and prior approval is not required. Triage nurses are instructed to work with patients to direct them to the appropriate care location.

§ 1.3 Data

1.3.1 Overview

We construct our analysis sample by linking multiple sources of administrative data from the VA, including records of nurse triage cases, healthcare utilization, and patient demographics. This section sketches the most relevant information about our analysis sample and Appendix Table A1.1 describes our data cleaning and sample construction in further detail.

1.3.2 Data Sources and Sample Construction

Our sample construction starts with the universe of telephone triage cases received in all call centers across the US from July 1, 2018, to December 31, 2022. The triage records have information at the call level, including triage date-time (year, month, day, hour, and minute), patient ID, triage nurse ID, station (call center) ID, triage disposition (recommended follow-up location and timing), and free-entry notes.

To define the treatment variable of our analysis, we link the triage records to the ED utilization records that include patients' visits to both VA and non-VA EDs. Using patient ID, we search the ED utilization records for an ED visit made by the veteran within three days of the triage. In our main specification, the binary treatment is defined as an indicator of whether the veteran has an ED visit within three days of the triage.

Each call is further linked to the patient's prior healthcare utilization events at VA facilities (within 365 days of the triage), prior diagnoses (31 Elixhauser comorbidity indices), VA's benefits eligibility status (priority group indicators), and demographics (e.g., age, gender, marital status). We use those covariates for randomization and robustness checks.

Our primary outcomes include mortality, hospital admission, and subsequent health-care utilization events. We construct mortality indicators from 1 to 30 days of the triage call using the date of death in the VA veteran roster. We also measure whether the patient is admitted to the hospital through the ED and whether the patient calls the triage line again within a month since the index triage call.

To construct our main sample, we impose the following key restrictions (See Appendix Table A1.1). First, we drop the calls during non-business hours (before 8 am, after 4 pm, weekends, and holidays). Some call centers do not offer telephone triage during non-business hours and transfer calls to other call centers or non-VA contractors. Second, we remove calls from patients with the most recent prior triage call within 30 days to focus on the index triage incident. Third, we only retain calls received by nurses with at least 100 calls per year to reduce noise in our constructed measure of nurse ED tendencies.[4] Forth, we only keep calls in call-center-by-month-by-year cells with at least two nurses to focus on the calls that had a chance of being as good as randomly assigned to different nurses. Lastly, we select a subset of 28 call centers out of the remaining 72 call centers for which patient age is balanced across nurses within call-center-by-call-time cells. Specifically, we only include call centers for which the F-test of joint significance of nurse dummies fails to reject at the 10% significance level when we regress patient age on those nurse dummies and call-date-time dummies (day-of-week, hour-of-day, and month-year indicators).[5] With these restrictions, our baseline

[4]Anecdotally, some nurse managers stated that some nurses would only work nurse triage for short periods, or that nurse managers themselves would occasionally step in to field calls when needed.

[5]Chan et al. (2022) use a similar last step for their main sample construction to ensure quasi-random assignment of VA radiologists to veteran patients' chest X-ray exams. Similar to their study, although we expect nurse assignment to be as good as random in all call centers, our interviews with nurse managers suggest organizational and managerial structure can differ across call centers in ways that

sample consists of 320,145 calls (from 199,997 patients) received by 248 nurses at 28 call centers.

While our primary analysis only uses call-center-by-call-time fixed effects as the conditioning set, we use several patients' characteristics for randomization and robustness checks. We gather patients' prior healthcare utilization (within 365 days of the triage), prior diagnoses (31 Elixhauser comorbidity indices), VA benefits eligibility status (priority group indicators), and demographics (e.g., age, gender, marital status). Appendix Table A1.2 shows the list of all control variables.

Table 1.1 summarizes our sample of triage calls. The average call is from a near-elderly patient (average age = 61) with high rates of previous year healthcare utilization (primary care = 85%, VA ED = 25.2%, non-VA ED = 17.4%, telephone triage = 86%). Nurses recommend ED visit at a higher rate than the algorithm (29% vs. 26.5%). Roughly 20% of the calls result in at least one ED visit within 3 days since triage call, and 2.5% result in hospital admission through ED. Within 30 days of triage, 11.3% of the cases have another triage call, 62.1% have at least one primary care visit, and 0.5% result in patient mortality.

§ 1.4 Method

1.4.1 Overview

For call i, we consider a model to estimate the effect of ED visits on outcomes such as mortality:

call-center-by-call-date-time indicators may not perfectly absorb confounding variations.

$$Y_i = \beta_0 + \beta_1 D_i + \boldsymbol{X}_i' \boldsymbol{\pi} + u_i \tag{1.1}$$

$$D_i = \delta_0 + \delta_1 Z_i + \boldsymbol{X}_i' \boldsymbol{\rho} + e_i \tag{1.2}$$

where Y_i is the outcome of interest for call i, D_i is an indicator of whether the patient of call i visits ED within 3 days since triage call, X_i is a vector of call- and patient-level control variables, and u_i is an error term. The key challenge in estimating equation (1.1) is that patients who visit an ED differ from patients who do not in their underlying health conditions. For instance, patients with a life-threatening condition at the time of triage may be more likely to visit an ED after the triage call and experience a higher rate of mortality.

To estimate the effect of ED visits on patient health and utilization outcomes, we need an exogenous variation in ED visit decisions independent of the patient's underlying health conditions. Following the examiner design literature, our empirical strategy exploits as instrument Z_i the quasi-random assignment of triage nurses to calls and the variation in the propensity to have callers visit an ED across those nurses.

1.4.2 Instrumental Variable Construction

We construct a leave-one-out instrument by averaging ED visit indicators of other patients triaged by the same nurse, following the examiner design literature (Dahl et al., 2014; Dobbie et al., 2018; Silver and Zhang, 2022). In constructing this instrument, we focus on cross-nurse variation within cells defined by call center and call month-year interactions. Exploiting within-cell cross-nurse variation this way addresses potential concerns about non-random nurse assignment. Within call centers, average health conditions of healthcare users may not be uniformly distributed over time. For instance, patients who call in winter months may more likely have seasonal flu than patients who call in summer months. If certain nurses are more likely to work in summer than in winter, the simple leave-out average will be biased. Across call centers, there are level differences in patient ED utilization rates.

Specifically, for call i that is assigned to nurse j, we first obtain residual of ED visit status, denoted as D_i^*, before calculating the leave-one-out average. We partial out the conditioning set $\boldsymbol{X}_i$ from the ED visit indicator D_i using the following linear regression:

$$D_i^* = D_i - \boldsymbol{X}_i'\boldsymbol{\gamma} = Z_{ij} + \epsilon_i \tag{1.3}$$

where $\boldsymbol{X}_i$ includes call-center-by-call-month-by-year interactions. The residuals D_i^* include the nurse ED tendency Z_{ij} and idiosyncratic call-level error term ϵ_i.

Then we construct the leave-out ED tendency measure by averaging the residual ED visit status of all other patients but patient $k(i)$ assigned to nurse j in year $y(i)$:

$$Z_i = \frac{1}{K_{j,y(i)} - 1} \sum_{i'} \frac{1\{k(i') \neq k(i), j(i') = j, y(i') = y(i)\} D_{i'}^*}{n_{k(i'),j,y(i)}} \tag{1.4}$$

where $K_{j,y}$ is the number of patients assigned to nurse j in year y and $n_{k,j,y}$ is the total number of calls from patient k received by nurse j in year y.

1.4.3 Variation in Nurse ED Tendency and First-Stage Estimates

Figure 1.1 presents the distribution of our nurse ED tendency measure. In any given call-center-by-year cell, there are at least 2 distinct nurses, with the median cell including 5 nurses. All call-center-by-nurse-by-year cells contain more than 100 calls. The 5th to 95th percentile of our residualized, leave-out measure ranges from -0.063 to 0.063 with a standard deviation of 0.040. The solid line visually presents the first stage by a natural cubic spline regression of (residualized) ED visit indicator on our nurse ED tendency measure. The corresponding linear first-stage coefficient in equation (1.2) is 0.83, implying that changing a call assignment from the 5th to 95th percentile nurse increases the probability of ED visit by 10.5 percentage points, a 54 percent change from the 3-day ED visit rate of 19.4%. The first-stage effect is statistically significant, with a first-stage F-statistic of 2,266.5. Figure 1.2 shows that being assigned to a high ED tendency nurse persistently increases the probability of ED visit over 1 to 30 days since triage.

1.4.4 Instrument Validity

So far, we have shown that there is substantial variation across nurses in their tendency to have patients visit an ED. Our leave-out nurse ED tendency measure is a significant predictor of patient ED visit status. For the nurse ED tendency measure to be a valid instrument, it must satisfy exclusion and monotonicity conditions. We discuss those assumptions in the following sub-sections.

Quasi-Random Assignment and Exclusion

As we have discussed, our leave-out instrument is constructed to capture variations in the probability of having patients visit an ED across nurses within the same cell defined by call center and call month-year interaction. The quasi-randomness assumption requires that, within the cells, caller's potential outcomes are uncorrelated with the nurse assignment.

We empirically examine this assumption by testing if the leave-out nurse ED tendency measure is correlated with patient characteristics. To this end, we first obtain each call's predicted ED visit probability as an aggregate measure of patient characteristics by regressing ED visit indicator on a set of patient demographics (age, veteran status, mariatal status, gender, priority group, and rurality of residence), prior healthcare utilization and diagnosis measures as well as call-center-by-call-time fixed effects. We then examine if the predicted ED visit is correlated with our instrument, following the examiner design literature (Chan et al., 2023). In Figure 1.1, the dashed line presents a natural cubic spline regression of (residualized) predicted ED visit probability on our nurse ED tendency measure. The flat line indicates that our instrument is not meaningfully related with patient characteristics.

The exclusion restriction requires that nurse assignment affects caller's outcomes solely through the change in the probability of ED visit. A potential violation happens, for instance, if nurses with a higher ED propensity provide better (worse) health education over the phone, thereby decreasing (increasing) mortality. During triage, nurses only verbally interact with veterans over the phone and do not provide any substantial medical treatment. Moreover, nurses do not further follow-up the patient after the triage call to ensure the triage recommendation is followed. Taken together, nurses have limited scope to affect patient's outcomes beyond the post-triage ED visit probability.

Monotonicity

This study assumes monotonicity to identify and estimate an interpretable weighted average of individual-level treatment effects in the presence of heterogeneous ED treatment effects. Conventionally, literature in examiner design has often imposed pairwise monotonicity. In our context, the pairwise monotonicity requires that, for any pair of nurses j, j' within the same call-center-by-call-time cell, if nurse j has a higher propensity to have the calls visit an ED than nurse j' overall, any call i that visits an ED if triaged by nurse j' must visit an ED if triaged by nurse j.[6] Under this assumption, the 2SLS identifies the non-negatively weighted sum of the pairwise local average treatment effects (Imbens and Angrist, 1994).

However, this assumption fails for several potential reasons. For instance, nurse j may have a lower ED propensity than nurse j' for patients with a particular symptom, while j still has a higher ED propensity than nurse j' overall. Even if we construct nurse ED tendency within observed case types, the pairwise monotonicity still fails if nurse j's ED tendency relative to the other nurses changes in unobserved ways. Frandsen et al. (2023a) show that the 2SLS still identifies the non-negatively weighted sum of individual average treatment effects under the less restrictive assumption of average monotonicity. The average monotonicity assumption requires that the covariance between nurse-specific potential treatment status for call i ($D_i(j)$) and nurse overall propensity to have the callers visit an ED ($E\left[D_i(j)\right]$) are nonnegative.[7] This non-negative covariance requirement implies that the nurse ED tendencies calculated with all calls should positively correlate with caller's ED visit status for any subset of calls (Frandsen et al., 2023a).[8]

To empirically assess this implication, we examine if the first-stage relationship

[6] The pairwise monotonicity requires that, for any pair of nurses j, j' with $E\left[D_i(j)\right] \geq E\left[D_i(j')\right], D_i(j) \geq D_i(j')$ for all call i.

[7] Equivalently, the average ED propensity among nurses who would have case i visit an ED must be higher than or equal to the average ED propensity among nurses who would not.

[8] Average monotonicity is a condition defined at the individual call i level. The covariance must be taken for each call i using counterfactual assignment across different nurses. We cannot directly test the non-negative covariance requirement at the call level since each call is assigned to only one nurse. However, Sigstad (2023) shows robustness of average monotonicity using judicial panels where multiple judges decide each case. Sigstad (2023) shows that average monotonicity is violated in only 4% of US Supreme Court cases, whereas pairwise monotonicity is violated in 50%.

between the leave-out instrument and caller's ED visit status is positive for any subset of the calls defined by observable call characteristics, such as age above 60 vs. age below 60, following the prior literature (Dobbie et al., 2018; Chan et al., 2023). Table A1.3 presents that the first-stage coefficients are positive for all subsample pairs.

§ 1.5 Results

This section presents 2SLS estimates of the effect of having an ED visit within three days since triage on inpatient admission, primary care visits, repeat calls, and mortality. Following the design-based inference framework described by Abadie et al. (2023), we report standard errors clustered at the call center level since this is the level at which the call assignment to nurses happens.[9] In Appendix, we also report results with heteroskedasticity-robust standard errors and standard errors clustered at different levels.

1.5.1 Hospital Admission

Table 1.2 presents 2SLS estimates on the probability of hospital admission via ED within three days since the triage call. Hospital admission increases by 5.3 to 6.0 percentage points (pp) (sample average = 2.3%). Figure 1.3 presents the estimated effects on the probability of hospital admission via ED within 1 to 30 days since the triage call. The ED effect on admission persists over one month. On average, a call is approximately 5 pp more likely to experience a hospital admission within 30 days if it has a post-triage

[9] Ideally, we want to cluster standard errors at the work shift level as the group of nurses who worked during the same hours constitutes the natural unit of call randomization (Abadie et al., 2023; Frandsen et al., 2023b; Chyn et al., 2024). Unfortunately, we do not have nurse work shift data.

ED visit than if it does not.

1.5.2 Primary Care Visit

Post-triage healthcare in the ED can affect patient healthcare utilization in other facilities. Table 1.3 shows 2SLS estimates on the probability of having at least one primary care physician (PCP) visit within three days since triage. A post-triage ED visit decreases the probability of PCP utilization by 32.7 to 33.5 pp (sample average = 34.7%). Figure 1.4 shows 2SLS estimates on the probability of having at least one PCP visit within 1 to 30 days after triage. While the effects are waning in magnitude over time, the post-triage ED visit continues to depress the probability of visiting a PCP. On average, a call with an ED visit within three days is 24.3 pp less likely to have PCP utilization within 30 days of triage (sample average = 66.5%).

1.5.3 PCP Visits (between 4 and 30 days after triage)

The effects of ED on patient healthcare utilization at other facilities could differ across different points in time. While we find that having an ED visit decreases the probability of PCP utilization within 30 days since triage, the negative estimates can be a composite of multifaceted ED effects of potentially different signs, such as (i) the initial facility substitution from primary care to ED (short term), (ii) the effect of ED care on resolving patient healthcare needs (middle term), and (iii) the effect of ED care on follow-up care at primary care (middle term). While disentangling those channels is challenging, we attempt to tear out the middle-term effects ((ii) and (iii)) from the short-term effect of facility substitution ((i)).

Table 1.4 shows 2SLS estimates of the 3-day ED visit effect on the probability of having at least one primary care visit between 4 and 30 days since triage. While statistically insignificant, the 3-day ED visit decreases the probability of having a PCP visit in the middle term by 2.3 to 4.1 pp. Those estimates are much smaller than the estimates found in Figure 1.4, suggesting that the initial facility substitution channel primarily drives the estimates in Figure 1.4. While interpretation requires some caution, the middle-term reduction in PCP utilization suggests that the ED resolves patient healthcare needs more than it increases the onset of follow-up care in primary care.

1.5.4 Repeat ED Visits (between 4 and 30 days after triage)

Having an ED visit immediately after triage can increase patient attachment to healthcare at the ED, potentially leading to multiple ED visits in the middle term. We examine this possibility by estimating the 3-day ED visit effect on having another ED visit between 4 and 30 days since triage. While statistically imprecise, Table 1.5 suggests that having the first post-triage ED visit within three days increases the likelihood of another visit 4 to 30 days by 3.7 to 6.0 percentage points.

1.5.5 Repeat Call

Having multiple triage calls within the short- to mid-term is another possible indicator of a patient's unaddressed healthcare needs. Table 1.6 shows 2SLS estimates on the probability of a patient making a new triage call within three days since the original triage. While estimated effects are imprecise, patients who visit an ED within three days are 1.3 to 1.5 pp less likely to call the triage line again within three days since triage. Figure 1.5 reports 2SLS estimates on having a repeat call within 1 to 30 days. Overall, the effects are not distinguishable from zero.

1.5.6 Mortality

Finally, we examine the effect of an ED visit on patient mortality as an ultimate health outcome. Table 1.7 reports 2SLS estimates of the 3-day ED visit effect on mortality within three days since triage call. The estimate implies a 0.1 pp mortality reduction, although it is imprecisely estimated. Figure 1.6 presents the effects on mortality within 1 to 30 days since triage. The estimated ED effects on short- to mid-term mortality are not distinguishable from zero. Figure 1.7 shows the ED effects on 1-year mortality. We do not find any effect on long-term mortality, either.

§ 1.6 Robustness Checks

1.6.1 Extra Covariates

Figures 1.3-1.7 and Tables 1.2-1.7 show 2SLS estimates as we sequentially add controls to the models: (i) 4 prior utilization indicators (primary care, VA ED, non-VA ED, and inpatient), (ii) (i) + 5 prior utilization indicators (tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health), and (iii) (ii) + hold-out controls, including prior diagnosis (Elixhauser Comorbidity Index), demographics, and VA benefit eligibility. These controls address the concern that calls from sicker patients may be selected into triage nurses with higher propensity to induce ED visit. Overall, 2SLS estimates are not sensitive to the inclusion of these controls.

1.6.2 Granularity of Call-Center-by-Call-Time Fixed Effects

A possible threat to identification is that certain nurses may be more likely to work particular shift during which sicker patients are more likely to call. We address this possible threat by examining sensitivity of our 2SLS estimates to different sets of call-center-by-call-time fixed effects.[10] Appendix Tables A1.4-A1.9 show 2SLS estimates from specifications with different levels of call-center-by-call-time fixed effects. Overall, changing the granularity of call-center-by-call-time fixed effects does not affect the 2SLS estimates.

1.6.3 Instrument Types

In the main analysis, we construct the leave-one-patient-out instrument by averaging residualized ED visit indicators of other patients received by the same nurse in the same year. As robustness checks, we examine sensitivity of our 2SLS estimates to three alternative nurse ED tendency measures: (i) leave-one-call-out average, (ii) leave-one-patient-out average of non-residualized ED indicators, and (iii) leave-one-patient-out average of calls received by the same nurse from all years. Appendix Tables A1.10-A1.15 present 2SLS estimates from specifications with instruments constructed differently. Results across all instruments are similar to our preferred instrument.

[10]We examine four different sets of fixed effects specifications: (i) Call-Center-by-Month-Year, (ii) Call-Center-by-Month-Year + Call-Center-by-Day-of-Week, (iii) Call-Center-by-Year-by-Day-of-Week + Call-Center-by-Month-by-Day-of-Week + Call-Center-by-Day-of-Week-by-AM/PM, and (iv) Call-Center-by-Year-by-Day-of-Week + Call-Center-by-Month-by-Day-of-Week + Call-Center-by-Day-of-Week-by-Hour-of-Day

1.6.4 Standard Errors

Finally, we implement statistical inference with standard errors computed differently. Our main analysis reports standard errors clustered at the call-center level because this is the level at which nurses are effectively randomized (Abadie et al. 2023; Frandsen et al. 2023). Appendix Tables A1.16- A1.21 present the same set of results with standard errors clustered at the call-center-by-month-year level as well as heteroskedasticity-robust standard errors. The comparison of those results reveals that standard errors clustered at the call-center level are the most conservative among the three types of standard errors. Hence, the choice of standard errors does not affect our statistical inference results.

§ 1.7 Discussions

1.7.1 Complier Characteristics and Average Potential Outcomes

Our instrumental variable strategy identifies the local average treatment effects of ED visit among callers whose ED visit decision can be swayed by nurse assignment. We cannot identify treatment effects among the set of always-takers and never-takers: (i) calls with the most (least) severe health conditions to which all triage nurses would (would not) attempt to have them visit an ED regardless of those nurses' usual ED tendencies, and (ii) callers that would (or would not) visit an ED regardless of to which nurse they are assigned. This observation implies that the set of compliers consists of cases in the middle of the severity distribution. We estimate the average observable characteristics of compliers and contrast them to the overall averages of our analysis sample. Further, we estimate the average potential outcomes for compliers to understand our treatment effect estimates better.[11]

Table 1.8 shows average characteristics of compliers relative to the overall sample. Compliers have higher Elixhauser comorbidity scores and are more likely to have ED visits and inpatient admissions in the prior year than the overall sample. Appendix Figures A1.1-A1.4 present the average potential outcome estimates for hospital admissions, primary care visits, repeat calls, and mortality within 1 to 30 days since triage. The differences between panels (a) and (b) mirror the IV estimates shown in Figures 1.3-1.6. Appendix Figures A1.1 and A1.2 reveal that having a post-triage ED visit within 3 days immediately changes potential hospital admission and primary care visit trajectories within the next 30 days relative to the respective potential outcome trajectories under no ED visit. In contrast, Appendix Figures A1.3 and A1.4 show that potential outcome trajectories for repeat calls and mortality do not significantly differ between the states with and without ED visits.

1.7.2 Heterogeneous Treatment Effects

We examine whether having an ED visit affects outcomes differently across subpopulations. We split the analysis sample by age (65 and over vs. below 65), ED utilization in prior year, and inpatient admission in prior year.

[11] We estimate the average complier characteristics and average potential outcomes using Abadie's kappa method (Abadie, 2002, 2003). Specifically, we implement 2SLS regressions of the interaction between each covariate and ED visit indicator ($W_i \cdot D_i$) on ED visit indicator (D_i), where the right-hand-side ED visit indicator is instrumented by the leave-out average of ED visits. We similarly estimate the average potential outcomes with ED visit ($Y_i(1)$) among compliers, using 2SLS regressions of the interaction between each outcome and ED visit indicator ($Y_i \cdot D_i$) on ED visit indicator (D_i), where the right-hand-side ED visit indicator is instrumented by the leave-out average of ED visits. The average potential outcomes without ED visit ($Y_i(0)$) among compliers are similarly estimated by replacing D_i with $1 - D_i$.

Figures A1.5-A1.7 present 2SLS estimates of ED visit on the probability of having a hospital admission by subsample. The effects of ED visit on admission are larger among veterans who are older than 65, those with ED visits in the prior year, and those with inpatient admissions in the prior year. In contrast, Figures A1.8-A1.10 show that the ED effects on the probability of having a PCP visit are smaller among those sub-populations.

§ 1.8 Conclusion

This study examines the treatment effects of having an ED visit on patients' subsequent healthcare utilization and health outcomes, using the cross-nurse difference in their propensity to have patients visit an ED as an instrument. We find that a post-triage ED visit decreases primary care visits and increases hospital admissions and repeat ED visits within 30 days of triage. Our estimates suggest that the initial healthcare facility substitution primarily drives the considerable reduction in the probability of primary care utilization.

We interpret the 2SLS estimates as weighted average treatment effects among the complier subpopulation whose post-triage ED visit is swayed by the cross-nurse variation in triage practice (propensity to have patients visit an ED). Our average complier characteristics estimates suggest that the compliers are, on average, sicker than the overall sample.

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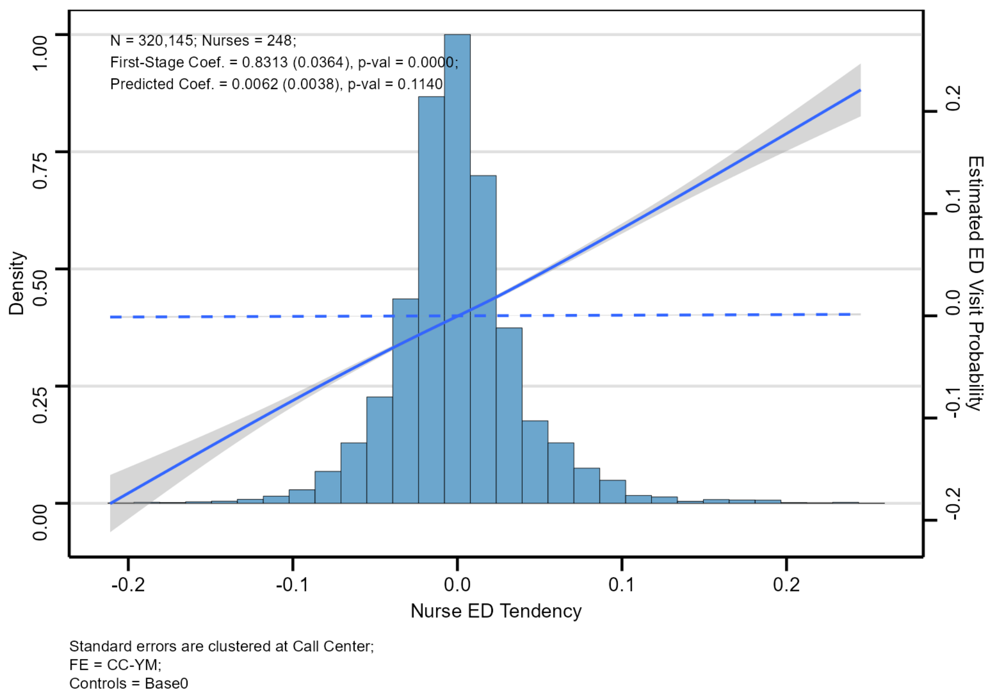
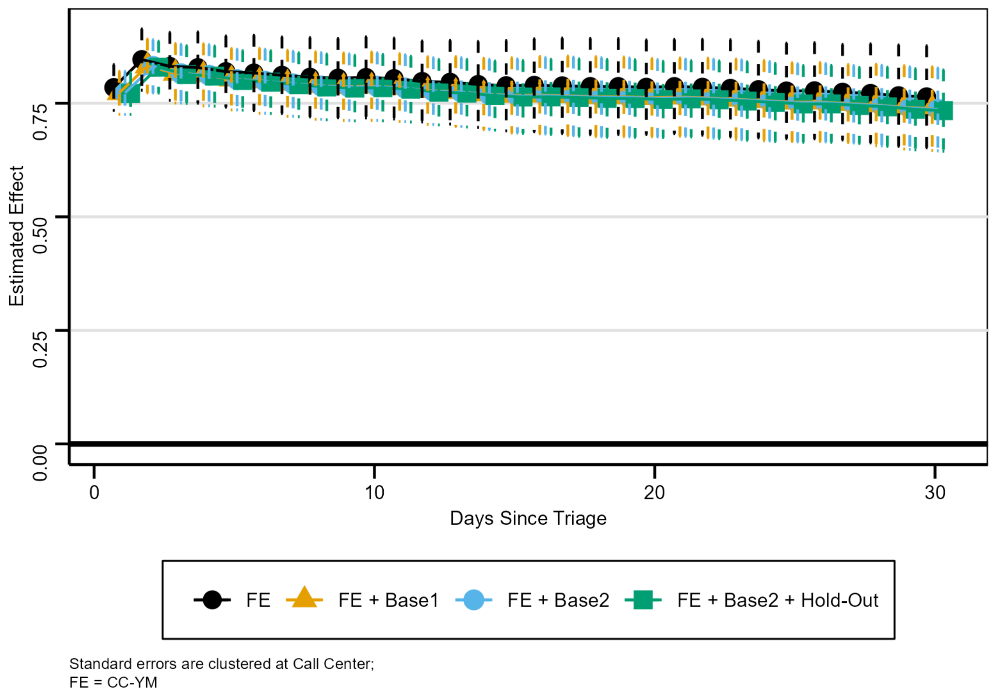
§ 1.10 Tables and Figures

Figure 1.1: Randomization Check and First Stage



Note: This figure shows the distribution of leave-one-patient-out average of ED visits as described in Section 1.4.2. The x-axis represents the leave-out average of ED visits. The left y-axis represents density, scaled to maximum of 1. The solid line visually presents the first-stage relationship between the leave-one-out average of ED visits and patient ED visit status within three days since triage. The dashed line visually presents a balance check by a natural cubic spline regression of (residualized) predicted ED visit probability (on the right y-axis) on the leave-out average of ED visits. The corresponding linear first-stage and linear balance regression slope estimates are displayed at the top of figure.

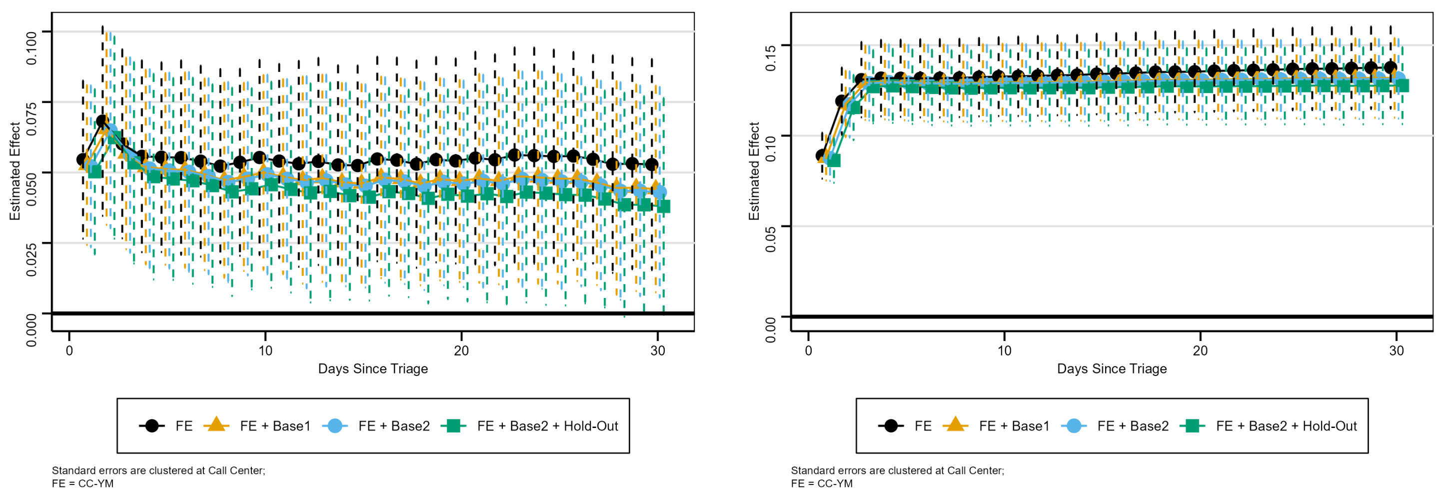
Figure 1.2: First Stage Effects (1-30 Days Since Triage)



Note: This figure shows the first-stage estimates of Equation (1.2). We regress indicators of whether the patient has an ED visit within 1 to 30 days since triage on the leave-one-patient-out average of ED visits within three days since triage constructed by the method described in Section 1.4.2.
Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2. Base 1 includes Prior Utilization 1 (primary care, VA ED, non-VA ED, and inpatient admission). Base 2 includes Prior Utilization 1 and Prior Utilization 2 (tele primary care, tele triage, mental health, clinical pharmacy, and tele mental health). The hold-out set includes patient demographics, prior diagnoses (Elixhauser comorbidity scores), and VA benefits eligibility. The 95% confidence intervals are constructed using standard errors clustered at the call-center level.

Figure 1.3: Effect of ED Visit on Hospital Admission through ED

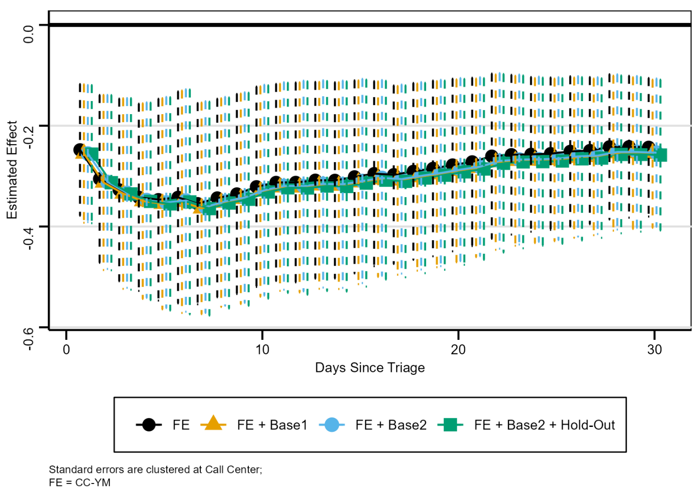
(a) IV (b) OLS



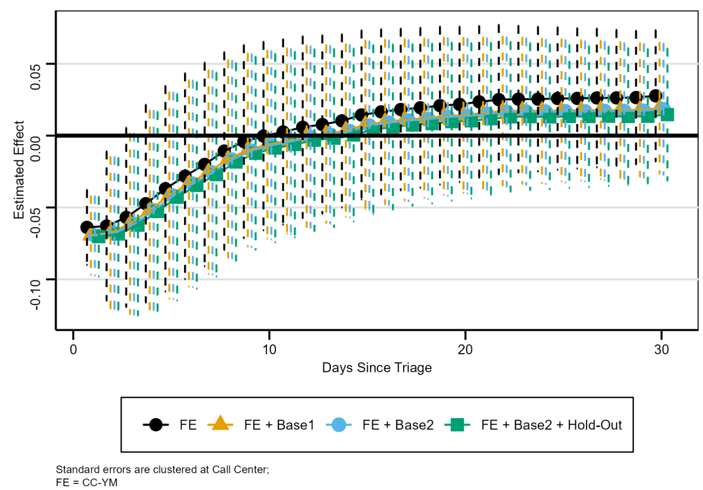
Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of hospital admission via ED within 1 to 30 days. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2. Base 1 includes Prior Utilization 1 (primary care, VA ED, non-VA ED, and inpatient admission). Base 2 includes Prior Utilization 1 and Prior Utilization 2 (tele primary care, tele triage, mental health, clinical pharmacy, and tele mental health). The hold-out set includes patient demographics, prior diagnoses (Elixhauser comorbidity scores), and VA benefits eligibility. The 95% confidence intervals are constructed using standard errors clustered at the call-center level.

Figure 1.4: Effect of ED Visit on Primary Care Visit

(a) IV



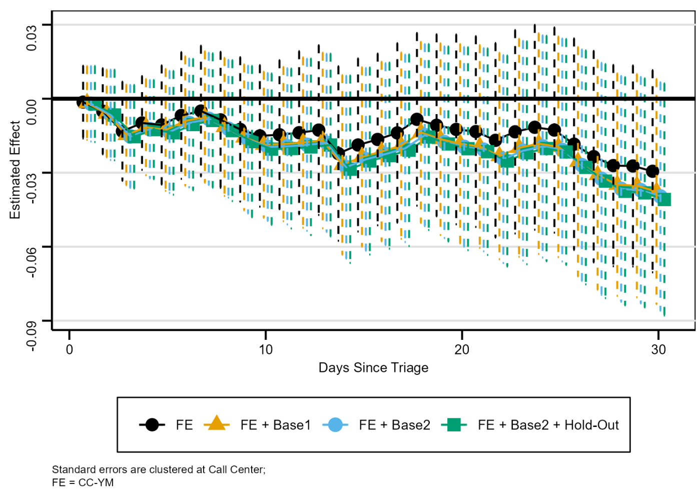
(b) OLS



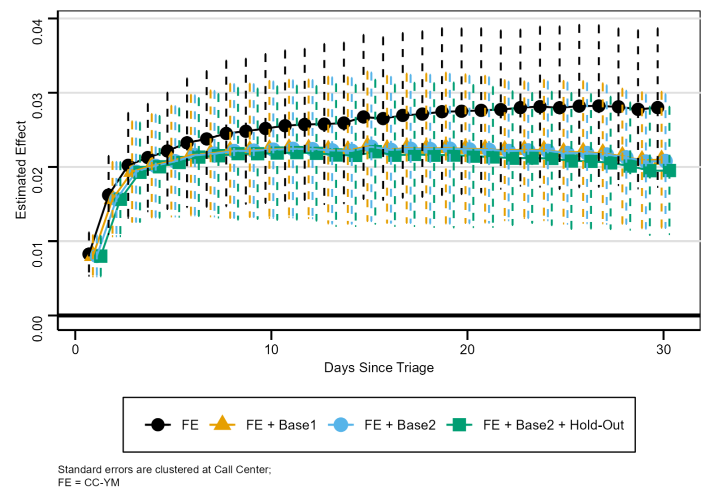
Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of primary care visit within 1 to 30 days. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2. Base 1 includes Prior Utilization 1 (primary care, VA ED, non-VA ED, and inpatient admission). Base 2 includes Prior Utilization 1 and Prior Utilization 2 (tele primary care, tele triage, mental health, clinical pharmacy, and tele mental health). The hold-out set includes patient demographics, prior diagnoses (Elixhauser comorbidity scores), and VA benefits eligibility. The 95% confidence intervals are constructed using standard errors clustered at the call-center level.

Figure 1.5: Effect of ED Visit on Repeat Call

(a) IV



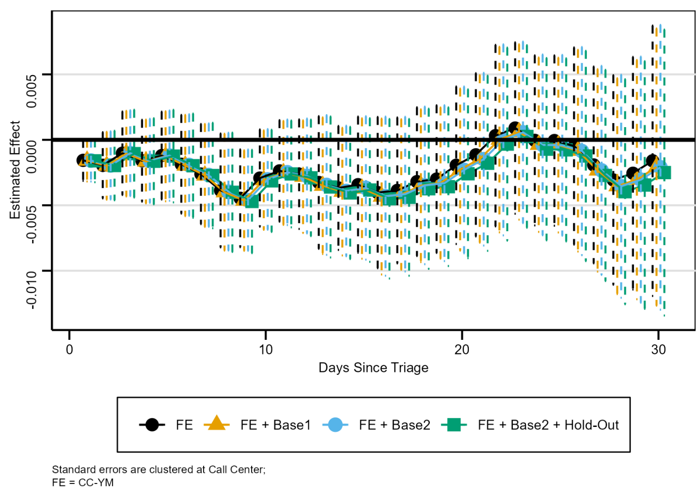
(b) OLS



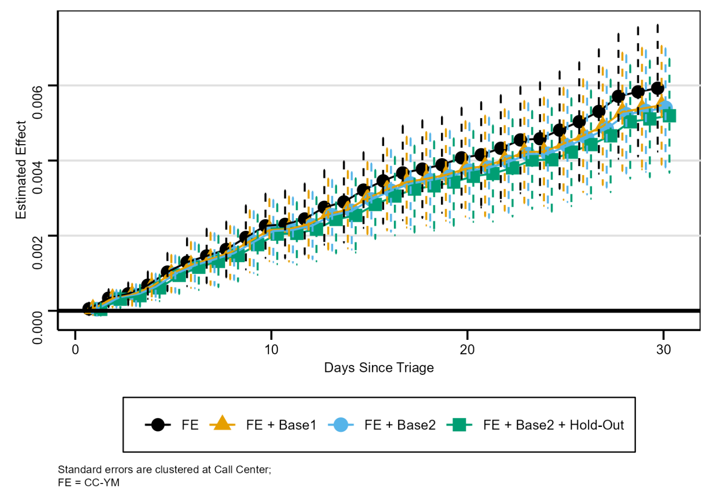
Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of repeat call within 1 to 30 days. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2. Base 1 includes Prior Utilization 1 (primary care, VA ED, non-VA ED, and inpatient admission). Base 2 includes Prior Utilization 1 and Prior Utilization 2 (tele primary care, tele triage, mental health, clinical pharmacy, and tele mental health). The hold-out set includes patient demographics, prior diagnoses (Elixhauser comorbidity scores), and VA benefits eligibility. The 95% confidence intervals are constructed using standard errors clustered at the call-center level.

Figure 1.6: Effect of ED Visit on Mortality

(a) IV



(b) OLS

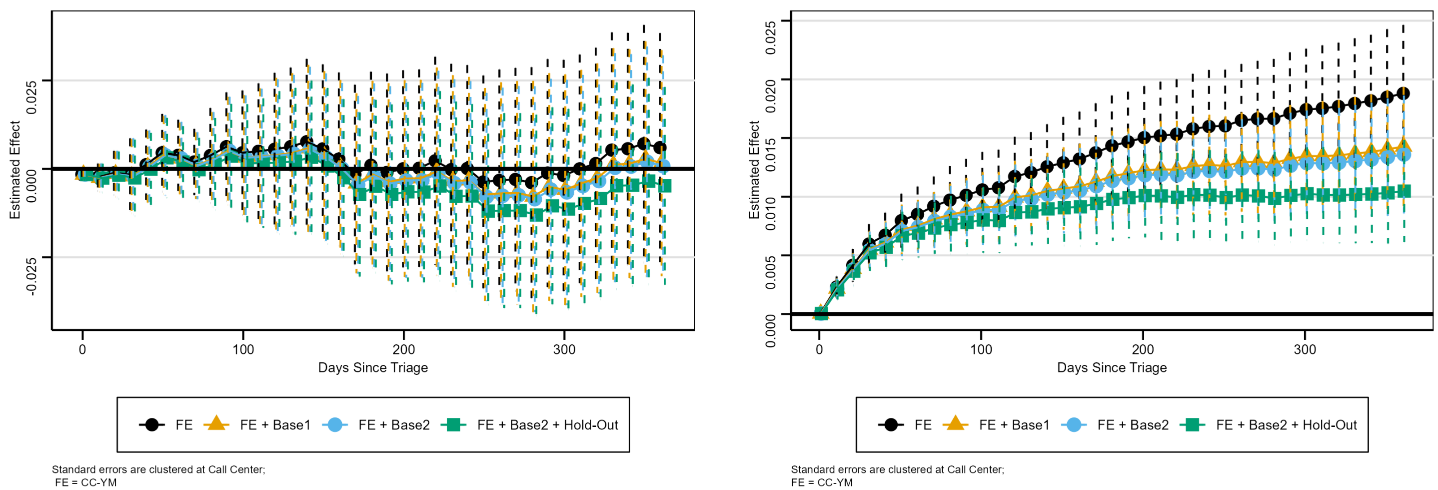


Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of death within 1 to 30 days. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2. Base 1 includes Prior Utilization 1 (primary care, VA ED, non-VA ED, and inpatient admission). Base 2 includes Prior Utilization 1 and Prior Utilization 2 (tele primary care, tele triage, mental health, clinical pharmacy, and tele mental health). The hold-out set includes patient demographics, prior diagnoses (Elixhauser comorbidity scores), and VA benefits eligibility. The 95% confidence intervals are constructed using standard errors clustered at the call-center level.

Figure 1.7: Effect of ED Visit on Mortality (1 Year)

(a) IV

(b) OLS



Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of death within 1, 11, ..., 361 days since triage. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2. Base 1 includes Prior Utilization 1 (primary care, VA ED, non-VA ED, and inpatient admission). Base 2 includes Prior Utilization 1 and Prior Utilization 2 (tele primary care, tele triage, mental health, clinical pharmacy, and tele mental health). The hold-out set includes patient demographics, prior diagnoses (Elixhauser comorbidity scores), and VA benefits eligibility. The 95% confidence intervals are constructed using standard errors clustered at the call-center level.

Table 1.1: Characteristics of Baseline Sample

Variables	Step 0	Step 1	Step 2	Step 3	Step 4	Step 5	Step 6	Step 7	Step 8
Algorithm ED	0.303	0.306	0.305	0.268	0.265	0.26	0.266	0.266	0.265
Nurse ED	0.358	0.355	0.354	0.286	0.282	0.277	0.285	0.284	0.29
Call Duration	7.844	14.214	14.126	13.202	13.13	12.789	12.502	12.508	11.605
Age	61.312	60.477	60.463	60.77	60.589	60.554	60.553	60.551	61.096
Veteran	0.995	0.995	0.995	0.994	0.994	0.994	0.994	0.994	0.993
Married	0.475	0.473	0.473	0.491	0.496	0.496	0.498	0.498	0.488
Male	0.859	0.852	0.852	0.854	0.855	0.855	0.854	0.854	0.861
White	0.676	0.666	0.666	0.675	0.674	0.674	0.661	0.661	0.634
Black	0.176	0.18	0.18	0.176	0.177	0.177	0.185	0.185	0.19
Hispanic	0.066	0.071	0.071	0.067	0.068	0.068	0.071	0.071	0.058
Rural County	0.185	0.181	0.181	0.194	0.195	0.195	0.191	0.191	0.255
Previous Year Primary Care	0.856	0.853	0.853	0.858	0.851	0.851	0.853	0.853	0.85
Previous Year VA ED	0.347	0.348	0.348	0.308	0.29	0.289	0.293	0.293	0.252
Previous Year Non VA ED	0.185	0.173	0.173	0.157	0.145	0.144	0.142	0.142	0.174
Previous Year Inpatient	0.133	0.13	0.13	0.107	0.099	0.098	0.099	0.099	0.084
Previous Year Tele Primary Care	0.699	0.699	0.699	0.688	0.676	0.673	0.677	0.677	0.702
Previous Year Tele Triage	0.874	0.896	0.896	0.881	0.874	0.894	0.898	0.898	0.86
Previous Year Mental Health	0.285	0.289	0.289	0.271	0.263	0.263	0.264	0.264	0.25
Previous Year Clinical Pharmacy	0.301	0.296	0.296	0.272	0.261	0.26	0.264	0.264	0.253
Previous Year Tele Mental Health	0.251	0.25	0.25	0.229	0.221	0.222	0.22	0.22	0.217
Elix Current Score	3.802	3.771	3.77	3.614	3.542	3.534	3.548	3.546	3.53
Elix Corrected Score	4.352	4.309	4.308	4.144	4.07	4.061	4.076	4.075	4.085
ED 1d	0.173	0.199	0.199	0.162	0.159	0.157	0.16	0.159	0.155
ED 2d	0.21	0.24	0.24	0.192	0.189	0.186	0.189	0.189	0.183
ED 3d	0.223	0.254	0.253	0.203	0.199	0.196	0.2	0.2	0.194
PCP 1d	0.151	0.158	0.158	0.171	0.162	0.164	0.164	0.164	0.193
PCP 2d	0.23	0.243	0.243	0.257	0.247	0.25	0.251	0.251	0.296
PCP 3d	0.279	0.294	0.294	0.303	0.293	0.295	0.297	0.297	0.347
Admit 1d	0.022	0.023	0.023	0.019	0.018	0.018	0.018	0.018	0.017
Admit 2d	0.033	0.035	0.035	0.026	0.025	0.024	0.024	0.024	0.023
Admit 3d	0.036	0.039	0.038	0.028	0.027	0.026	0.027	0.027	0.025
Repeat Call 1d	0.034	0.018	0.018	0.014	0.012	0.012	0.011	0.011	0.011
Repeat Call 2d	0.044	0.028	0.028	0.02	0.018	0.018	0.017	0.017	0.018
Repeat Call 3d	0.052	0.036	0.036	0.026	0.023	0.023	0.022	0.022	0.023
Mortality 1d	0.001	0	0	0	0	0	0	0	0
Mortality 2d	0.001	0	0	0	0	0	0	0	0
Mortality 3d	0.001	0.001	0.001	0	0	0	0	0	0
Calls	4,930,385	3,378,539	3,375,685	2,301,816	1,982,806	1,836,659	1,661,096	1,656,381	320,145
Patients	2,044,447	1,638,708	1,637,575	1,300,324	1,245,346	1,161,894	1,053,487	1,049,952	199,997
Nurses	6,866	3,888	3,881	3,518	3,058	1,421	1,293	1,290	248
Call Centers	101	101	101	101	101	96	74	72	28

Note: This table presents average call characteristics at each step of the sample restrictions to construct the analysis sample, detailed in Appendix Table A1.1. Algorithm ED and Nurse ED are triage indicators that equal 1 if call i is recommended ED. Veteran status, marital status, race and ethnicity, and rural residence are binary indicators. Previous year utilization indicators take 1 if the patient associated with call i had respective utilization events at least once in the past 365 days. Elixhauser scores are the total count of 31 comorbidity indices. "Current" scores count recorded diagnoses during a given fiscal year, whereas "corrected" scores look back at two fiscal years of recorded diagnoses and assign the patient the higher comorbidity count of those two years. Post-triage ED and PCP indicators measure ED and primary care physician visits within 1 to 3 days since triage.

Table 1.2: Effect of ED Visit (within 3 Days) on Hospital Admission (within 3 Days)

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.057	0.056	0.053
	(0.016)	(0.017)	(0.017)	(0.017)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.025	0.025	0.025	0.025
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table 1.3: Effect of ED Visit (within 3 Days) on Primary Care Visit (within 3 Days)

	(1)	(2)	(3)	(4)
ED Visit	-0.327	-0.335	-0.333	-0.335
	(0.088)	(0.092)	(0.092)	(0.093)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.347	0.347	0.347	0.347
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table 1.4: Effect of ED Visit (within 3 Days) on Primary Care Visit (between 4 and 30 Days)

	(1)	(2)	(3)	(4)
ED Visit	-0.023	-0.038	-0.036	-0.041
	(0.033)	(0.031)	(0.032)	(0.031)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.480	0.480	0.480	0.480
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table 1.5: Effect of ED Visit (within 3 Days) on Repeat ED Visit (between 4 and 30 Days)

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.040	0.039	0.037
	(0.043)	(0.035)	(0.035)	(0.035)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.099	0.099	0.099	0.099
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table 1.6: Effect of ED Visit (within 3 Days) on Repeat Call (within 3 Days)

	(1)	(2)	(3)	(4)
ED Visit	-0.013	-0.015	-0.015	-0.015
	(0.010)	(0.010)	(0.010)	(0.010)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.023	0.023	0.023	0.023
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table 1.7: Effect of ED Visit (within 3 Days) on Mortality (within 3 Days)

	(1)	(2)	(3)	(4)
ED Visit	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.000	0.000	0.000	0.000
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table 1.8: Complier Characteristics

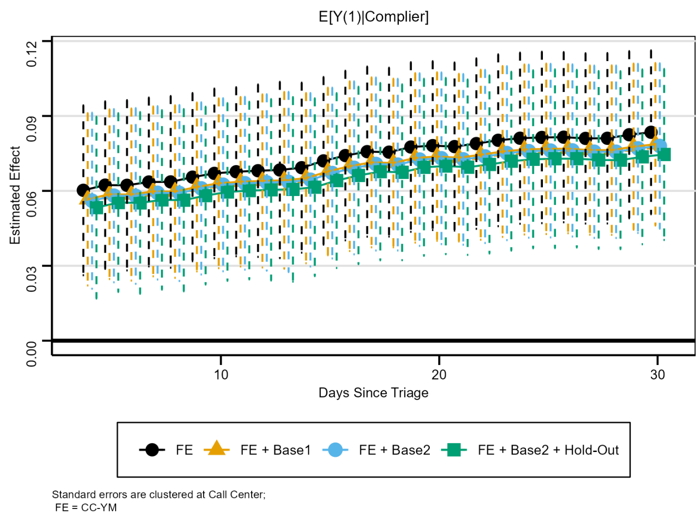
Variable	Compliers	SE	Overall Mean
Age	61.304	(0.859)	61.096
Veteran	1.000	(0.001)	0.993
Male	0.854	(0.026)	0.861
White	0.624	(0.085)	0.634
Black	0.277	(0.088)	0.190
Hispanic	0.020	(0.005)	0.058
Asian Other	0.036	(0.007)	0.068
Rural County	0.185	(0.046)	0.255
Priority Score Highly Disabled	0.473	(0.033)	0.486
Priority Score Low Moderate Disability	0.206	(0.016)	0.192
Priority Score Low Income	0.168	(0.013)	0.181
Priority Score Non Disabled Copay Required	0.141	(0.010)	0.122
Period of Service Vietnam	0.392	(0.036)	0.403
Period of Service Gulf	0.399	(0.029)	0.385
Period of Service Post Vietnam	0.154	(0.020)	0.153
Period of Service Korean	0.024	(0.006)	0.030
Period of Service Post Korean	0.024	(0.004)	0.018
Period of Service WW2	0.005	(0.002)	0.008
Service Connection 100	0.178	(0.019)	0.160
Service Connection 50 99	0.271	(0.018)	0.302
Service Connection 0 49	0.209	(0.017)	0.189
Service Connection No	0.327	(0.019)	0.329
Previous Year Primary Care	0.821	(0.027)	0.850
Previous Year VA ED	0.389	(0.037)	0.252
Previous Year Non VA ED	0.180	(0.026)	0.174
Previous Year Inpatient	0.124	(0.016)	0.084
Previous Year Tele Primary Care	0.717	(0.022)	0.702
Previous Year Tele Triage	0.917	(0.049)	0.860
Previous Year Mental Health	0.225	(0.017)	0.250
Previous Year Clinical Pharmacy	0.289	(0.022)	0.253
Previous Year Tele Mental Health	0.225	(0.017)	0.217
Elix Corrected Score	4.420	(0.161)	4.085

Note: This table presents average complier characteristics and the sample average among all calls in the analysis sample. Average complier characteristics are estimated using Abadie's kappa method. Specifically, we implement 2SLS regressions of the interaction between each covariate and ED visit indicator on ED visit indicator, where the right-hand-side ED visit indicator is instrumented by the leave-out average of ED visits. Regressions include call-center-by-call-time fixed effects. Standard errors are clustered at the call-center level.

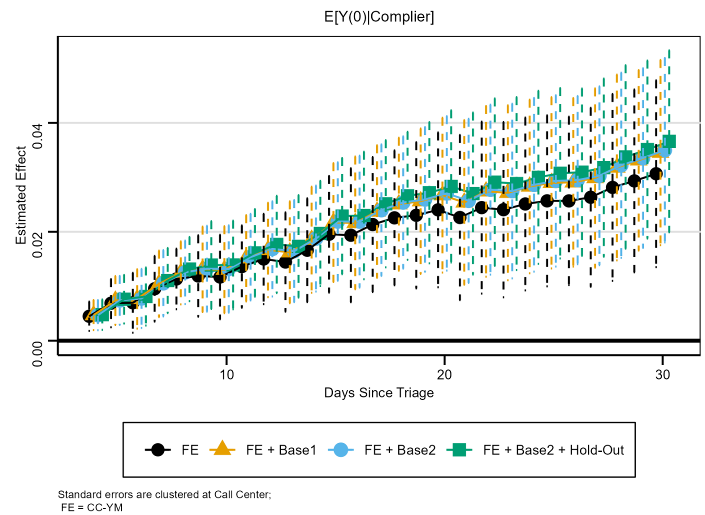
§ 1.11 Appendix Tables and Figures

Figure A1.1: Average Potential Hospital Admission through ED

(a) Admit(1)



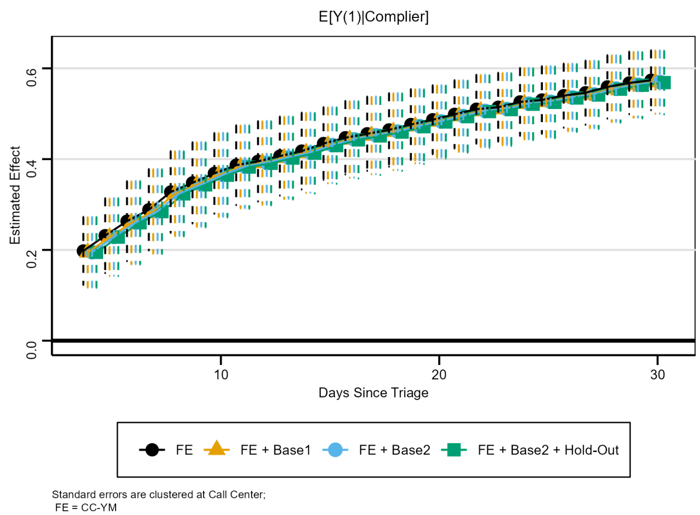
(b) Admit(0)



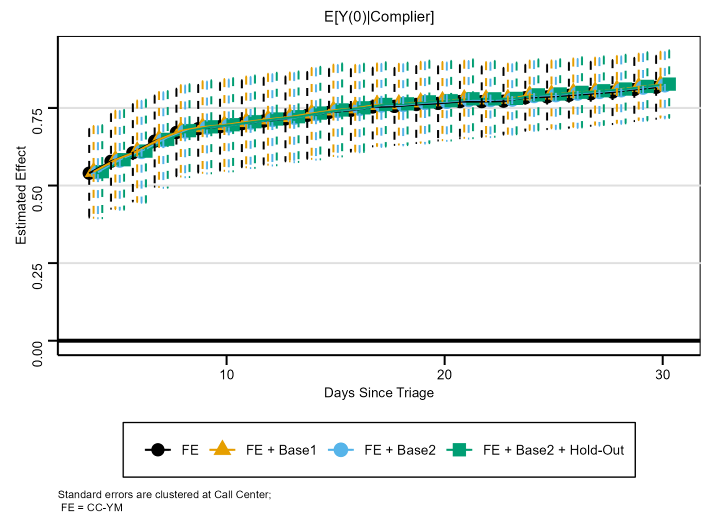
Note: This figure presents average potential hospital admission probabilities within 1 to 30 days among compliers. Average potential outcomes among compliers are estimated using Abadie's kappa method. Specifically, we estimate the average potential outcomes with ED visit among compliers ($E\left[Y(1) \mid \text{Compliers}\right]$), using 2SLS regressions of the interaction between each outcome and ED visit indicator ($Y \cdot D$) on ED visit indicator (D), where the right-hand-side ED visit indicator is instrumented by the leave-out average of ED visits. The average potential outcomes without ED visit among compliers are similarly estimated by replacing D with $1 - D$. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2. Standard errors are clustered at the call-center-by-month-year level.

Figure A1.2: Average Potential Primary Care Visit

(a) PCP(1)



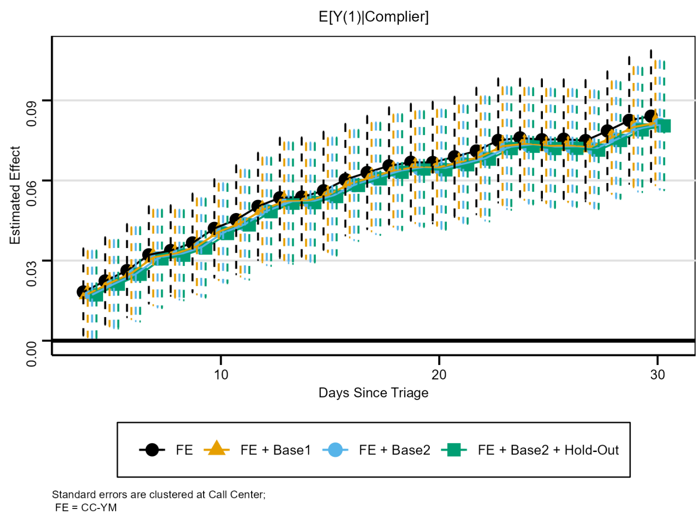
(b) PCP(0)



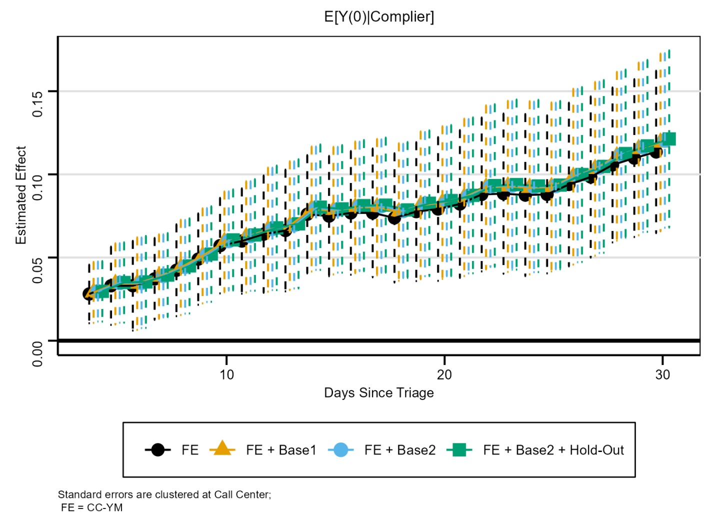
Note: This figure presents average potential primary care visit probabilities within 1 to 30 days among compliers. Average potential outcomes among compliers are estimated using Abadie's kappa method. Specifically, we estimate the average potential outcomes with ED visit among compliers ($E[Y(1) \mid \text{Compliers}]$), using 2SLS regressions of the interaction between each outcome and ED visit indicator ($Y \cdot D$) on ED visit indicator (D), where the right-hand-side ED visit indicator is instrumented by the leave-out average of ED visits. The average potential outcomes without ED visit among compliers are similarly estimated by replacing D with $1 - D$. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2. Standard errors are clustered at the call-center-by-month-year level.

Figure A1.3: Average Potential Repeat Call

(a) Repeat Call(1)



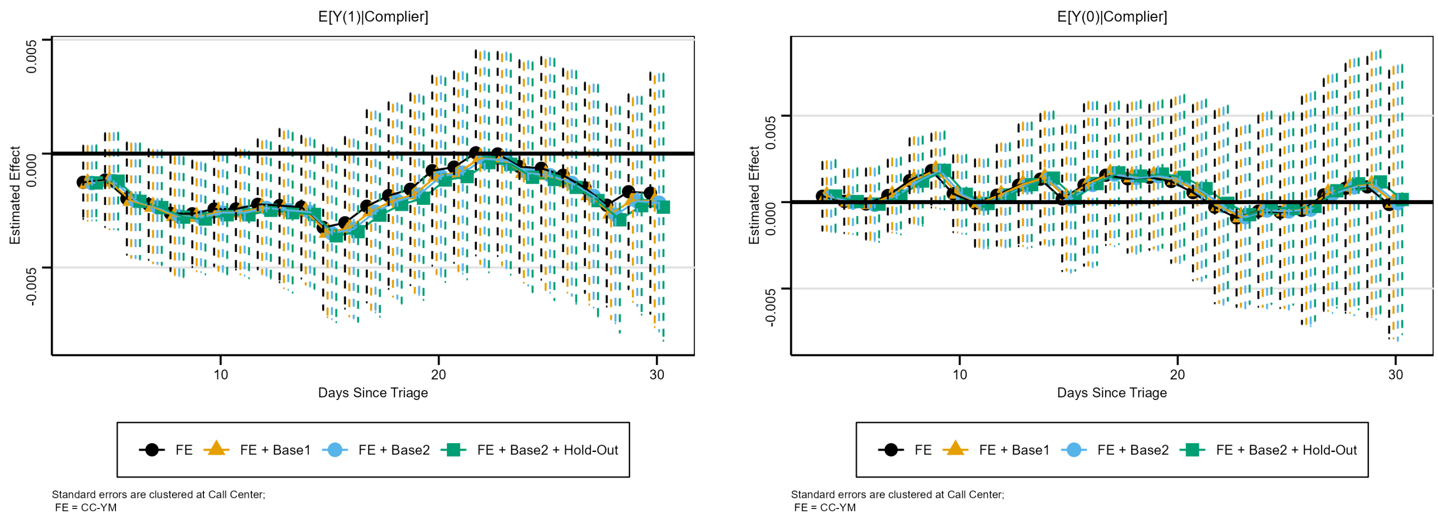
(b) Repeat Call(0)



Note: This figure presents average potential repeat call probabilities within 1 to 30 days among compliers. Average potential outcomes among compliers are estimated using Abadie's kappa method. Specifically, we estimate the average potential outcomes with ED visit among compliers ($E[Y(1) \mid \text{Compliers}]$), using 2SLS regressions of the interaction between each outcome and ED visit indicator ($Y \cdot D$) on ED visit indicator (D), where the right-hand-side ED visit indicator is instrumented by the leave-out average of ED visits. The average potential outcomes without ED visit among compliers are similarly estimated by replacing D with $1 - D$. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2. Standard errors are clustered at the call-center-by-month-year level.

Figure A1.4: Average Potential Mortality

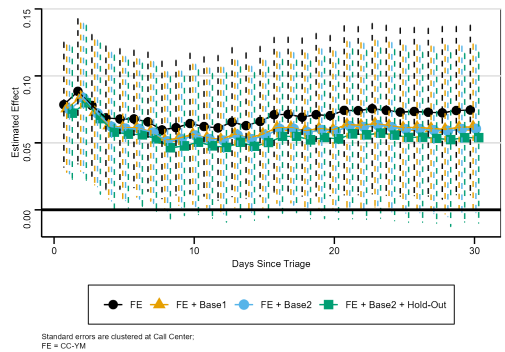
(a) Mortality(1) (b) Mortality(0)



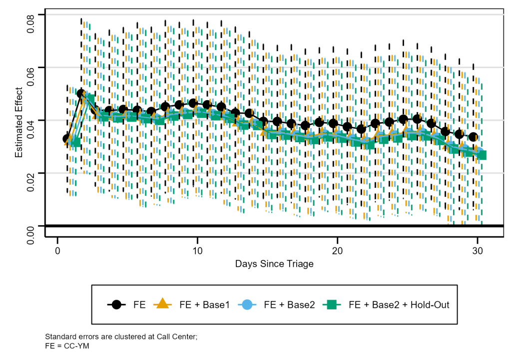
Note: This figure presents average potential mortality rates within 1 to 30 days among compliers. Average potential outcomes among compliers are estimated using Abadie's kappa method. Specifically, we estimate the average potential outcomes with ED visit among compliers ($E\left[Y(1) \mid \text{Compliers}\right]$), using 2SLS regressions of the interaction between each outcome and ED visit indicator ($Y \cdot D$) on ED visit indicator (D), where the right-hand-side ED visit indicator is instrumented by the leave-out average of ED visits. The average potential outcomes without ED visit among compliers are similarly estimated by replacing D with $1 - D$. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2. Standard errors are clustered at the call-center-by-month-year level.

Figure A1.5: Effect of ED Visit on Hospital Admission through ED (by Age)

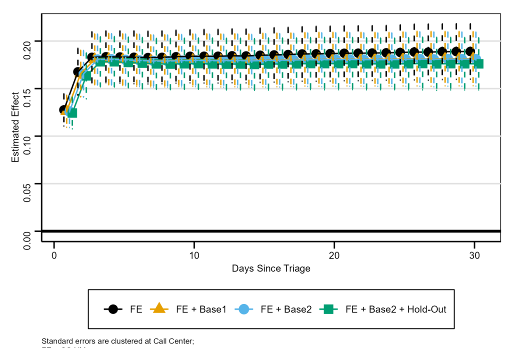
(a) IV (Age 65 and Over)



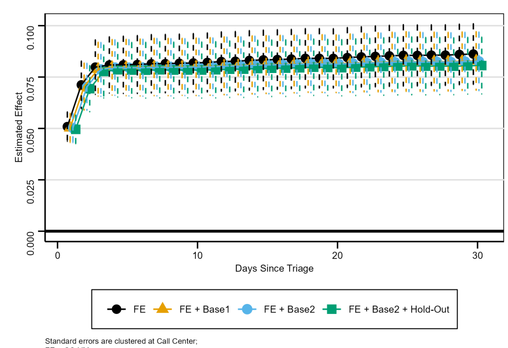
(b) IV (Age Below 65)



(c) OLS (Age 65 and Over)



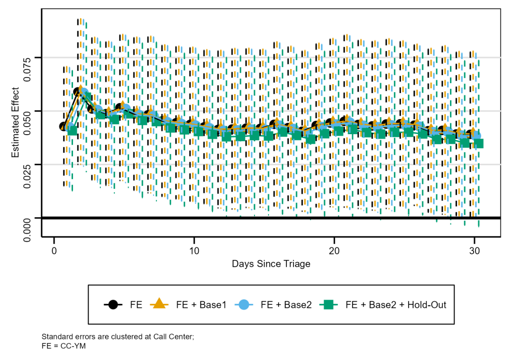
(d) OLS (Age Below 65)



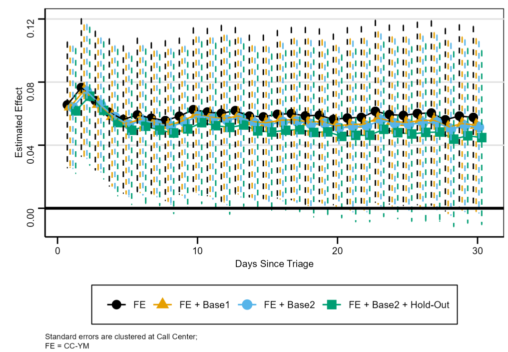
Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of hospital admission via ED within 1 to 30 days by patient age (over 65 or not). Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2.

Figure A1.6: Effect of ED Visit on Hospital Admission through ED (by Prior ED Visit)

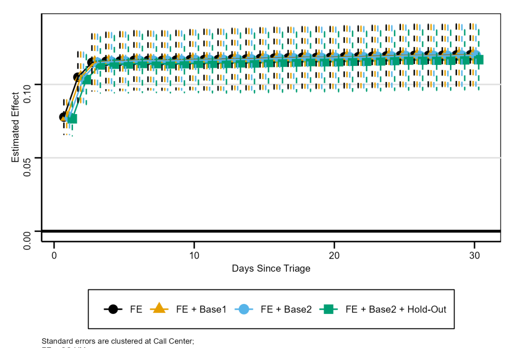
(a) IV (No Prior-Year ED Visit)



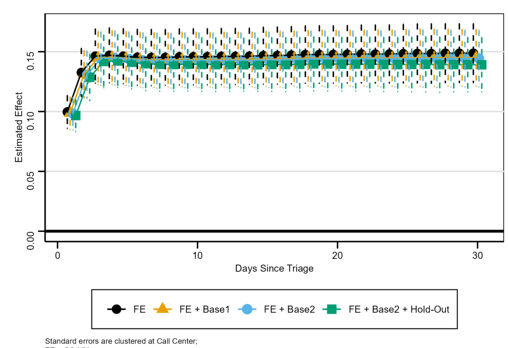
(b) IV (Prior-Year ED Visit)



(c) OLS (No Prior-Year ED Visit)



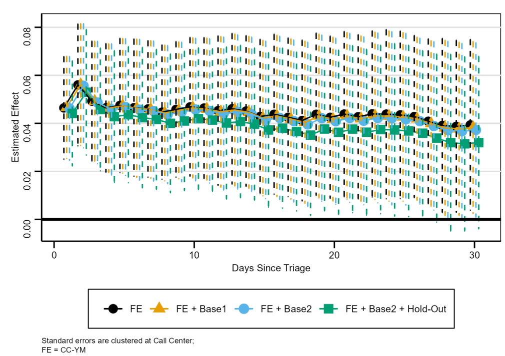
(d) OLS (Prior-Year ED Visit)



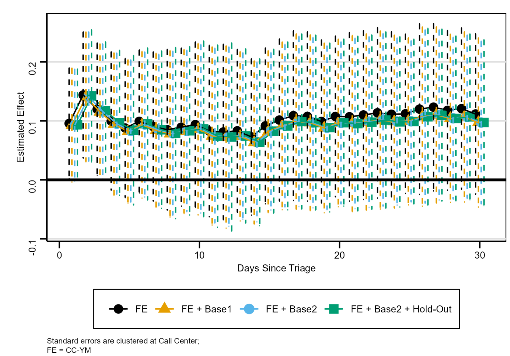
Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of hospital admission via ED within 1 to 30 days by prior year ED visit status. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2.

Figure A1.7: Effect of ED Visit on Hospital Admission through ED (by Prior Admission)

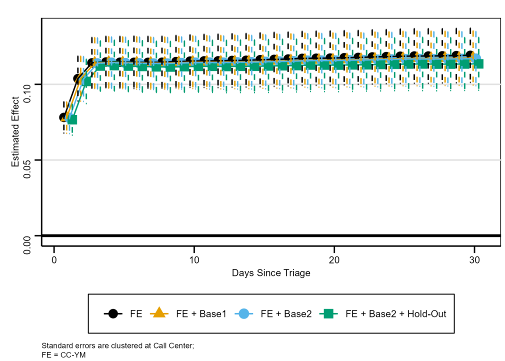
(a) IV (No Prior-Year Inpatient Admissions)



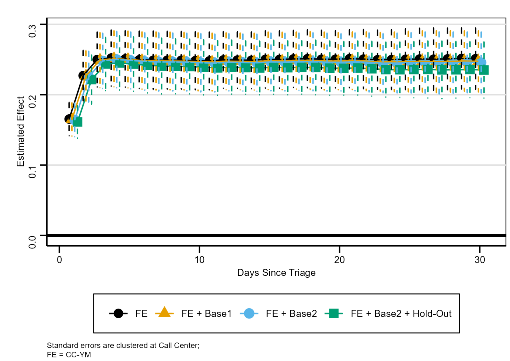
(b) IV (Prior-Year Inpatient Admissions)



(c) OLS (No Prior-Year Inpatient Admissions)



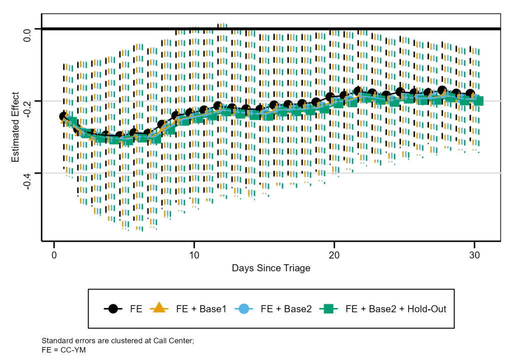
(d) OLS (Prior-Year Inpatient Admissions)



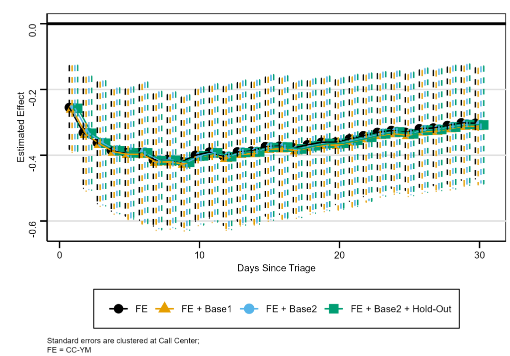
Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of hospital admission via ED within 1 to 30 days by prior year hospital admission status. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2.

Figure A1.8: Effect of ED Visit on Primary Care Visit (by Age)

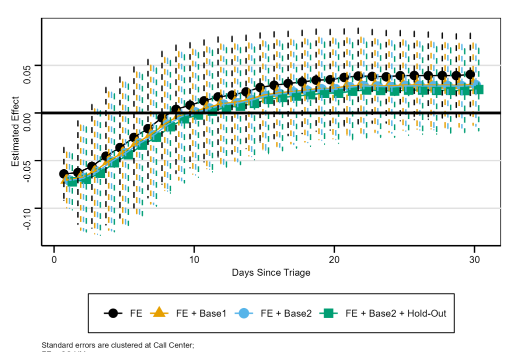
(a) IV (Age 65 and Over)



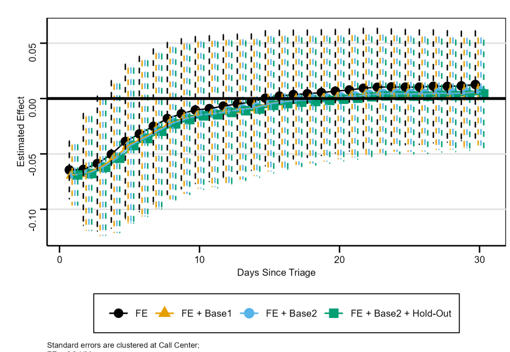
(b) IV (Age Below 65)



(c) OLS (Age 65 and Over)



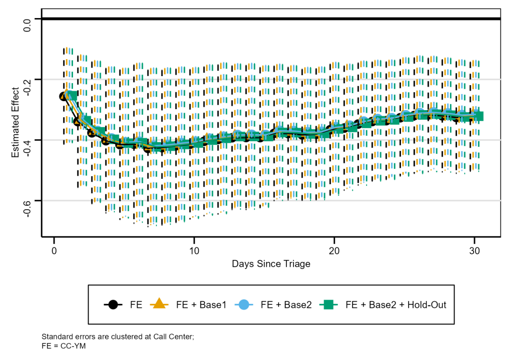
(d) OLS (Age Below 65)



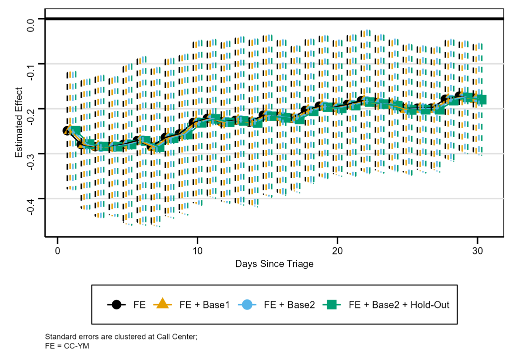
Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of primary care visit within 1 to 30 days by patient age (over 65 or not). Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2.

Figure A1.9: Effect of ED Visit on Primary Care Visit (by Prior ED Visit)

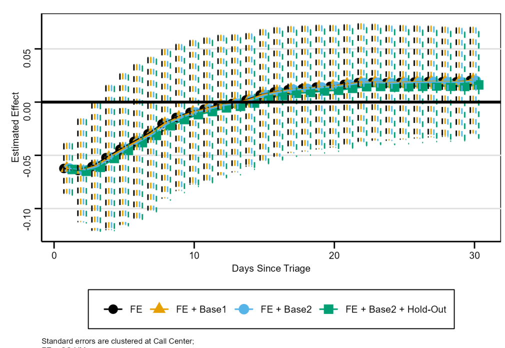
(a) IV (No Prior-Year ED Visit)



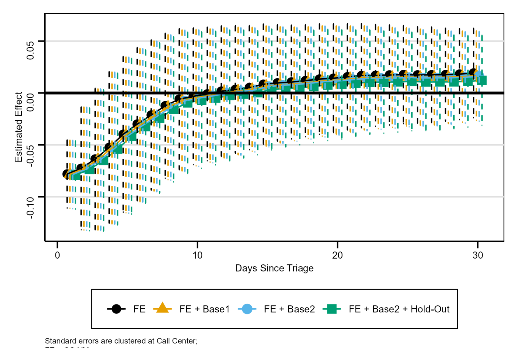
(b) IV (Prior-Year ED Visit)



(c) OLS (No Prior-Year ED Visit)



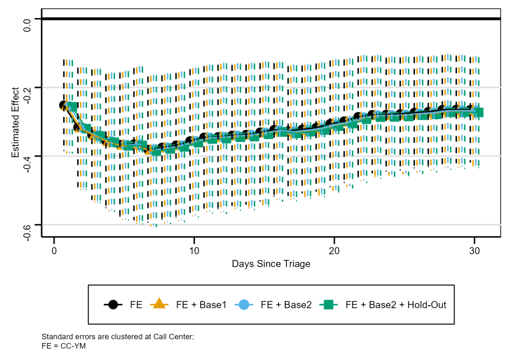
(d) OLS (Prior-Year ED Visit)



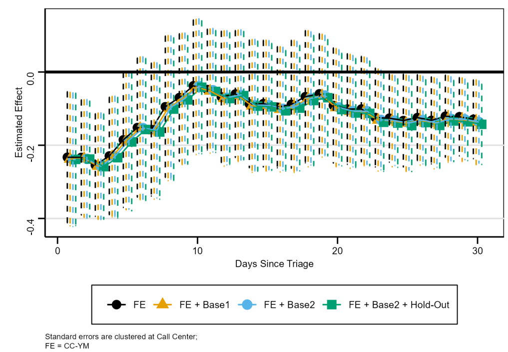
Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of primary care visit within 1 to 30 days by prior year ED visit status. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2.

Figure A1.10: Effect of ED Visit on Primary Care Visit (by Prior Admission)

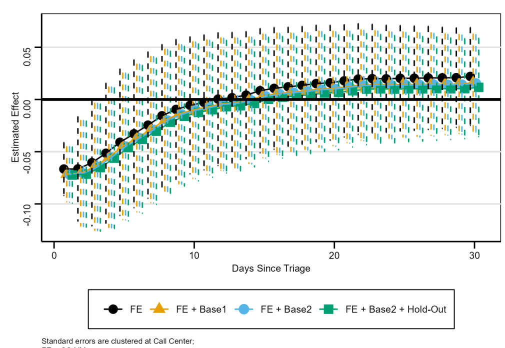
(a) IV (No Prior-Year Inpatient Admissions)



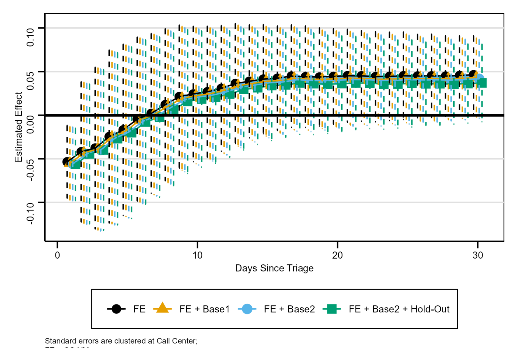
(b) IV (Prior-Year Inpatient Admissions)



(c) OLS (No Prior-Year Inpatient Admissions)



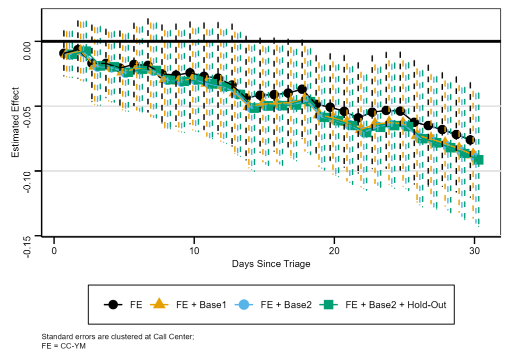
(d) OLS (Prior-Year Inpatient Admissions)



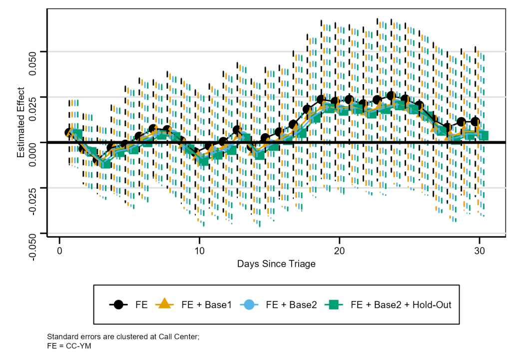
Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of primary care visit within 1 to 30 days by prior year hospital admission status. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2.

Figure A1.11: Effect of ED Visit on Repeat Call (by Age)

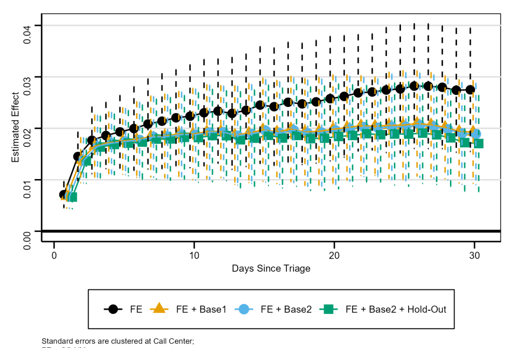
(a) IV (Age 65 and Over)



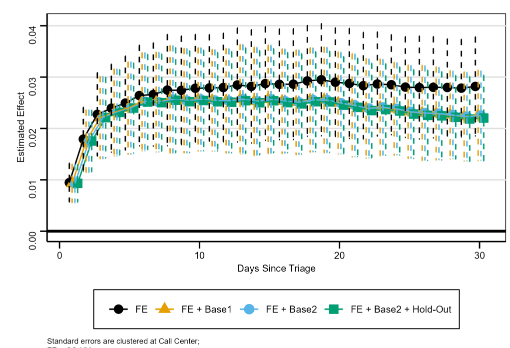
(b) IV (Age Below 65)



(c) OLS (Age 65 and Over)



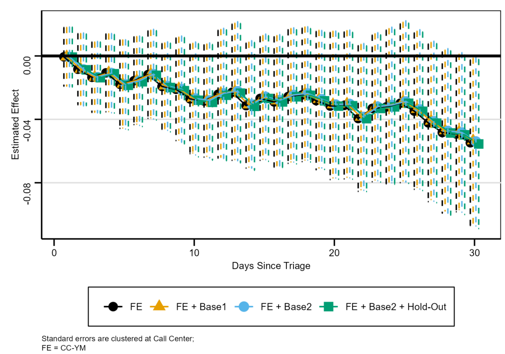
(d) OLS (Age Below 65)



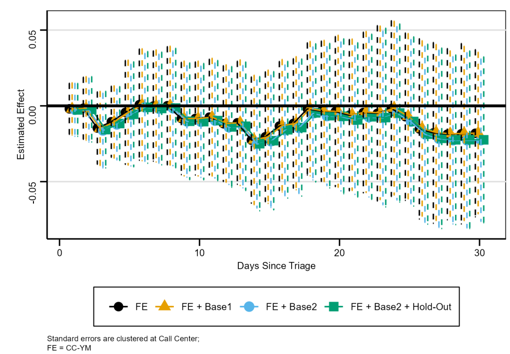
Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of repeat call within 1 to 30 days by patient age (over 65 or not). Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2.

Figure A1.12: Effect of ED Visit on Repeat Call (by Prior ED Visit)

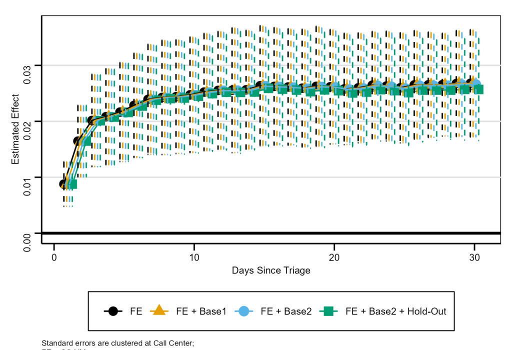
(a) IV (No Prior-Year ED Visit)



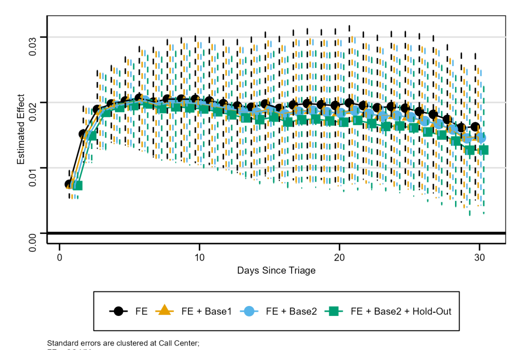
(b) IV (Prior-Year ED Visit)



(c) OLS (No Prior-Year ED Visit)



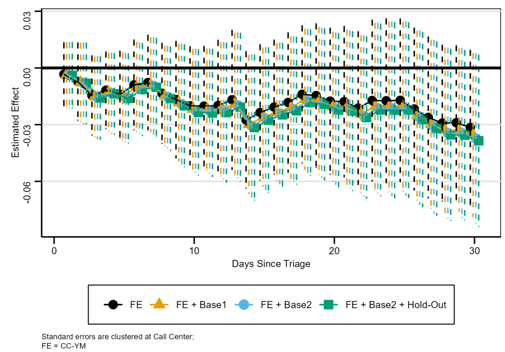
(d) OLS (Prior-Year ED Visit)



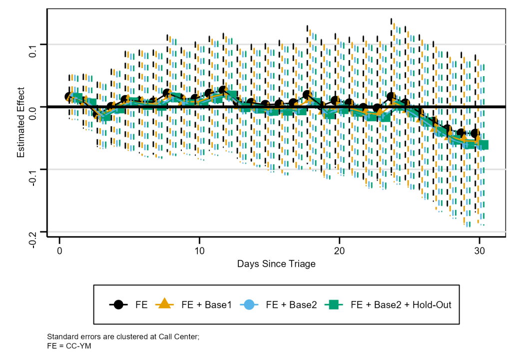
Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of repeat call within 1 to 30 days by prior year ED visit status. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2.

Figure A1.13: Effect of ED Visit on Repeat Call (by Prior Admission)

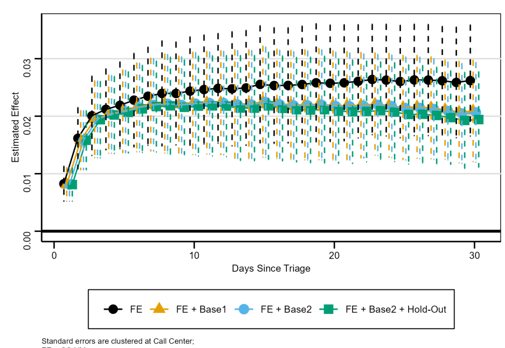
(a) IV (No Prior-Year Inpatient Admissions)



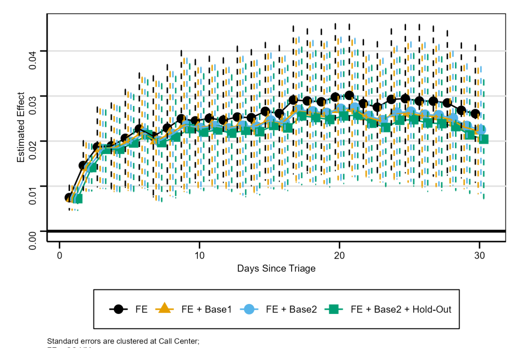
(b) IV (Prior-Year Inpatient Admissions)



(c) OLS (No Prior-Year Inpatient Admissions)



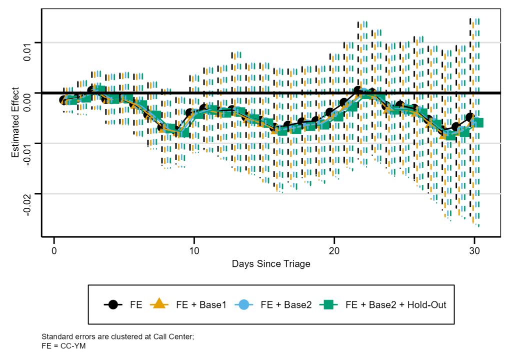
(d) OLS (Prior-Year Inpatient Admissions)



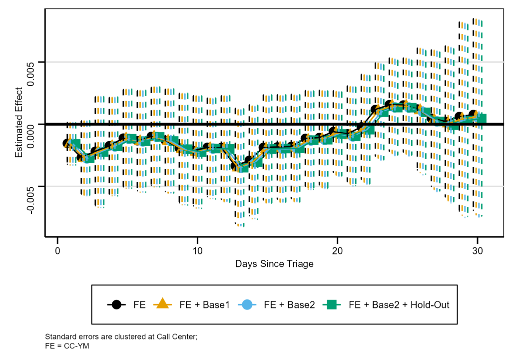
Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of repeat call within 1 to 30 days by prior year hospital admission status. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2.

Figure A1.14: Effect of ED Visit on Mortality (by Age)

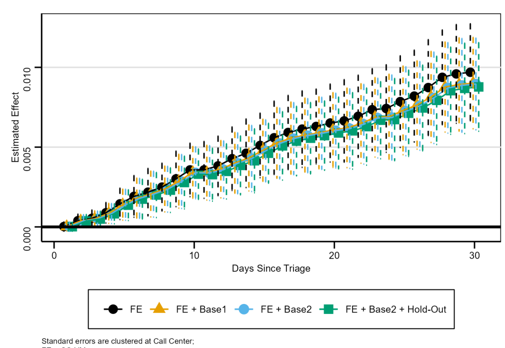
(a) IV (Age 65 and Over)



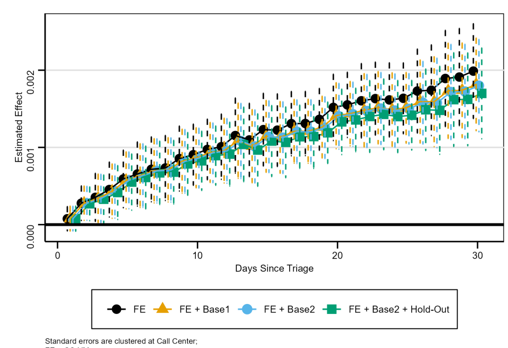
(b) IV (Age Below 65)



(c) OLS (Age 65 and Over)



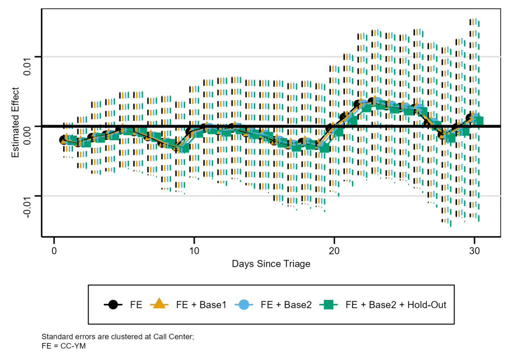
(d) OLS (Age Below 65)



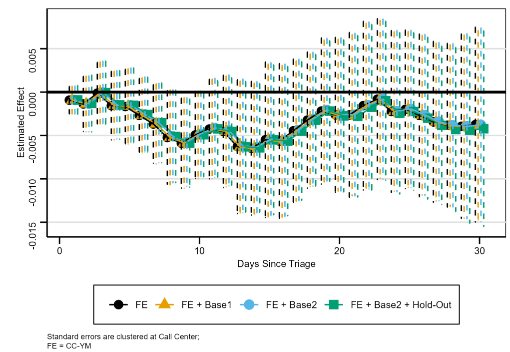
Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of death within 1 to 30 days by patient age (over 65 or not). Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2.

Figure A1.15: Effect of ED Visit on Mortality (by Prior ED Visit)

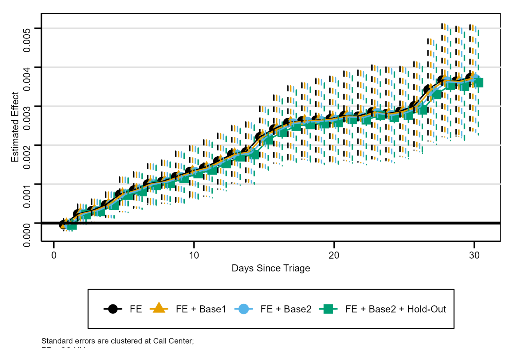
(a) IV (No Prior-Year ED Visit)



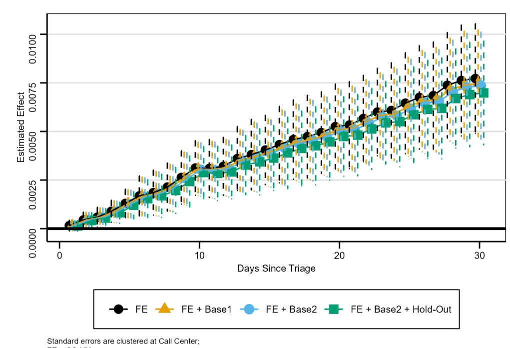
(b) IV (Prior-Year ED Visit)



(c) OLS (No Prior-Year ED Visit)



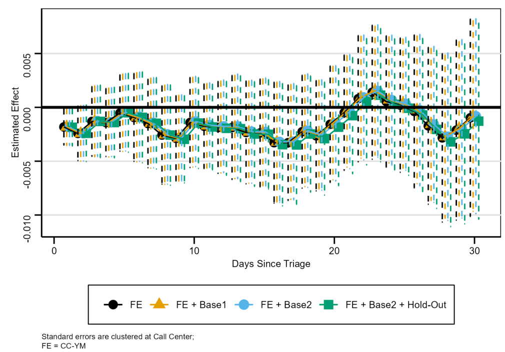
(d) OLS (Prior-Year ED Visit)



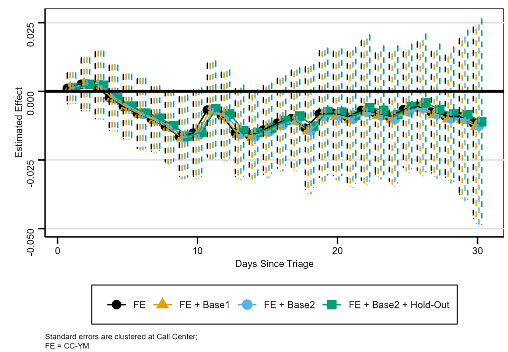
Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of death within 1 to 30 days by prior year ED visit status. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2.

Figure A1.16: Effect of ED Visit on Mortality (by Prior Admission)

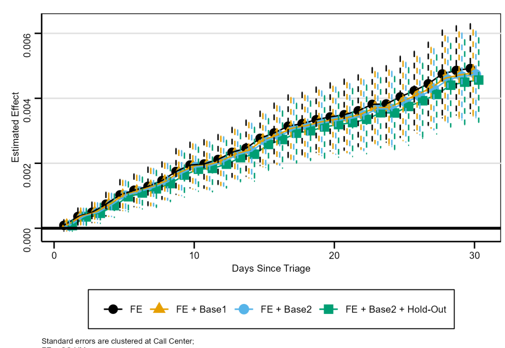
(a) IV (No Prior-Year Inpatient Admissions)



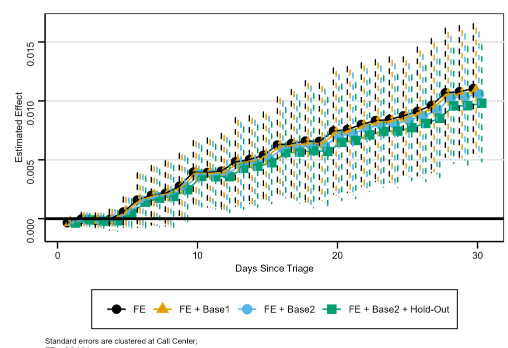
(b) IV (Prior-Year Inpatient Admissions)



(c) OLS (No Prior-Year Inpatient Admissions)



(d) OLS (Prior-Year Inpatient Admissions)



Note: This figure presents 2SLS and OLS estimates of the effect of having an ED visit (within 3 days since triage) on the probability of death within 1 to 30 days by prior year hospital admission status. Call-center-by-month-year fixed effects are included in all regressions. We sequentially add the sets of controls described in Appendix Table A1.2.

Table A1.1: Selection of Baseline Sample

Step	Description	Calls	Patients	Nurses	Call Centers
0	All symptom calls with complete IDs and dates.	4,930,385	2,044,447	6,866	101
1	Drop calls with incomplete triage disposition values (follow-up locations and intervals recommended by nurse & algorithm).	3,378,539	1,638,708	3,888	101
2	Drop calls from patients younger than 20 or older than 99; Drop calls from patients in inpatient facilities.	3,375,685	1,637,575	3,881	101
3	Drop calls received during non-business hours (before 8 am; after 4 pm; weekends; holidays).	2,301,816	1,300,324	3,518	101
4	Drop calls from patients with the most recent prior call within 30 days.	1,982,806	1,245,346	3,058	101
5	Drop calls received by nurses with less than or equal to 100 calls per year.	1,836,659	1,161,894	1,421	96
6	Drop calls received in call centers that experience consolidation during the analysis period.	1,661,096	1,053,487	1,293	74
7	Drop calls received in call centers with only one nurse per month-year.	1,656,381	1,049,952	1,290	72
8	Restrict to call centers for which patient age is balanced across nurses.	320,145	199,997	248	28

Notes: This table describes sample restriction steps to construct the analysis sample. The table lists the number of calls, patients, nurses, and call centers at each step. Table 1.1 presents average call characteristics at each step.

Table A1.2: List of Control Variables

Variables	Number of Indicators
Call-Center-by-Call-Time FEs	
Call-Center-by-Month-Year	1,017
Call-Center-by-Day-of-Week	139
Call-Center-by-Year-by-Day-of-Week	509
Call-Center-by-Month-by-Day-of-Week	1,640
Call-Center-by-Day-of-Week-by-AM	279
Call-Center-by-Day-of-Week-by-Hour-of-Day	1,118
Call-Center-by-Month-Year-by-Day-of-Week	5,055
Call-Center-by-Month-Year-by-Hour-of-Day	8,067
Prior Utilization 1	
Previous Year Primary Care	1
Previous Year VA ED	1
Previous Year Non VA ED	1
Previous Year Inpatient	1
Prior Utilization 2	
Previous Year Tele Primary Care	1
Previous Year Tele Triage	1
Previous Year Mental Health	1
Previous Year Clinical Pharmacy	1
Previous Year Tele Mental Health	1
Hold-Out Controls	
Age bins (5-year)	12
Male	1
Veteran (Y, N, NA)	2
Race (White, Black, Hispanic, Asian/Other, NA)	4
Rural County (Y, N)	1
Income Bin (1st-3rd terciles, NA)	3
Priority Score (High-, Low-/Moderate-, No-Disability; Low-Income; NA)	4
Service Connection (No SC, 0-49, 50-99, 100, NA)	4
Period of Service (Vietnam, Gulf, Post-Vietnam, Korean, Post-Korean, WW2, Other)	6
Elix Corrected Score bins (1-10, 11+, NA)	11

Note: This table lists control variables. Sets of indicators are constructed from each control. Column 2 shows the number of indicators generated from each control. Call-Center-by-Call-Month-Year FEs are included in the main analysis. Robustness checks use FEs at the different granularity. Prior Utilization 1, Prior Utilization 2, and Hold-Out Controls are sequentially added in robustness checks. All covariates are used to obtain the predicted ED visit probability for randomization check.

Table A1.3: Monotonicity Test

Variable	Subsample	Observations	Mean ED Visit	Estimate	SE
Age	65 and Over	153,906	0.199	0.827	(0.045)
Age	Below 65	166,239	0.189	0.834	(0.036)
Sex	Female	44,588	0.185	0.767	(0.032)
Sex	Male	275,557	0.195	0.842	(0.043)
Race Ethnicity	Asian/Other	21,637	0.148	0.889	(0.108)
Race Ethnicity	Black	60,701	0.226	0.844	(0.035)
Race Ethnicity	Hispanic	18,562	0.145	0.582	(0.109)
Race Ethnicity	White	202,870	0.195	0.833	(0.047)
Race Ethnicity	na	16,375	0.177	0.854	(0.059)
Rural County	No	238,525	0.195	0.843	(0.037)
Rural County	Yes	81,620	0.191	0.788	(0.055)
Priority Score	Group 1,4 Highly disabled	155,651	0.200	0.847	(0.037)
Priority Score	Group 2,3,6 Low/moderate disability	61,526	0.182	0.818	(0.064)
Priority Score	Group 5 Low-Income	57,962	0.200	0.763	(0.061)
Priority Score	Group 7-8 Non-Disabled, copayment required	39,105	0.174	0.961	(0.053)
Priority Score	na	5,901	0.221	0.508	(0.088)
Service Connection	1 No SC	105,225	0.189	0.836	(0.048)
Service Connection	2 SC 0-49	60,372	0.186	0.841	(0.073)
Service Connection	3 SC 50-99	96,548	0.193	0.825	(0.045)
Service Connection	4 SC 100	51,132	0.211	0.880	(0.054)
Service Connection	na	6,868	0.218	0.538	(0.084)
ED Visits in Prior Year	No	198,393	0.151	0.761	(0.048)
ED Visits in Prior Year	Yes	121,752	0.264	0.903	(0.032)
Inpatient Admissions in Prior Year	No	293,287	0.185	0.817	(0.037)
Inpatient Admissions in Prior Year	Yes	26,858	0.294	0.918	(0.067)
Elix Score	1 to 5	209,170	0.185	0.828	(0.039)
Elix Score	11+	12,319	0.244	0.794	(0.095)
Elix Score	6 to 10	47,673	0.263	0.892	(0.035)
Elix Score	na	50,983	0.154	0.744	(0.062)
Algorithm Recommendation	ED	84,961	0.437	0.618	(0.087)
Algorithm Recommendation	Non-ED	235,184	0.106	0.401	(0.071)

Note: This table presents monotonicity test results. We estimate first-stage coefficients on different subsamples of triage calls. Regressions include call-center-by-call-time fixed effects. Standard errors are clustered at the call-center level.

Table A1.4: Effect of ED Visit (within 3 Days) on Hospital Admission (within 3 Days)

(a) CC-YM (Main)

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.057	0.056	0.053
	(0.016)	(0.017)	(0.017)	(0.017)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.025	0.025	0.025	0.025
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.4: (Cont.) Effect of ED Visit (within 3 Days) on Hospital Admission (within 3 Days)

(b) CC-YM + CC-DoW

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.057	0.056	0.054
	(0.016)	(0.017)	(0.017)	(0.018)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.025	0.025	0.025	0.025
FE: CC-by-YM	X	X	X	X
FE: CC-by-DoW	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.4: (Cont.) Effect of ED Visit (within 3 Days) on Hospital Admission (within 3 Days)

(c) CC-Y-DoW + CC-M-DoW + CC-DoW-AM

	(1)	(2)	(3)	(4)
ED Visit	0.062	0.059	0.058	0.055
	(0.017)	(0.017)	(0.017)	(0.018)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.025	0.025	0.025	0.025
FE: CC-by-Y-by-DoW	X	X	X	X
FE: CC-by-M-by-DoW	X	X	X	X
FE: CC-by-DoW-by-AM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.4: (Cont.) Effect of ED Visit (within 3 Days) on Hospital Admission (within 3 Days)

(d) CC-Y-DoW + CC-M-DoW + CC-DoW-HoW

	(1)	(2)	(3)	(4)
ED Visit	0.063	0.059	0.058	0.055
	(0.017)	(0.017)	(0.017)	(0.018)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.025	0.025	0.025	0.025
FE: CC-by-Y-by-DoW	X	X	X	X
FE: CC-by-M-by-DoW	X	X	X	X
FE: CC-by-DoW-by-HoD	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.5: Effect of ED Visit (within 3 Days) on Primary Care Visit (within 3 Days)

(a) CC-YM (Main)

	(1)	(2)	(3)	(4)
ED Visit	-0.327	-0.335	-0.333	-0.335
	(0.088)	(0.092)	(0.092)	(0.093)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.347	0.347	0.347	0.347
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.5: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (within 3 Days)

(b) CC-YM + CC-DoW

	(1)	(2)	(3)	(4)
ED Visit	-0.311	-0.319	-0.317	-0.319
	(0.089)	(0.093)	(0.093)	(0.094)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.347	0.347	0.347	0.347
FE: CC-by-YM	X	X	X	X
FE: CC-by-DoW	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.5: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (within 3 Days)

(c) CC-Y-DoW + CC-M-DoW + CC-DoW-AM

	(1)	(2)	(3)	(4)
ED Visit	-0.314	-0.320	-0.321	-0.324
	(0.081)	(0.085)	(0.085)	(0.086)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.347	0.347	0.347	0.347
FE: CC-by-Y-by-DoW	X	X	X	X
FE: CC-by-M-by-DoW	X	X	X	X
FE: CC-by-DoW-by-AM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.5: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (within 3 Days)

(d) CC-Y-DoW + CC-M-DoW + CC-DoW-HoW

	(1)	(2)	(3)	(4)
ED Visit	-0.311	-0.317	-0.317	-0.320
	(0.079)	(0.083)	(0.083)	(0.084)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.347	0.347	0.347	0.347
FE: CC-by-Y-by-DoW	X	X	X	X
FE: CC-by-M-by-DoW	X	X	X	X
FE: CC-by-DoW-by-HoD	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.6: Effect of ED Visit (within 3 Days) on Primary Care Visit (between 4 and 30 Days)

(a) CC-YM (Main)

	(1)	(2)	(3)	(4)
ED Visit	-0.023	-0.038	-0.036	-0.041
	(0.033)	(0.031)	(0.032)	(0.031)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.480	0.480	0.480	0.480
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.6: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (between 4 and 30 Days)

(b) CC-YM + CC-DoW

	(1)	(2)	(3)	(4)
ED Visit	-0.029	-0.043	-0.041	-0.047
	(0.035)	(0.033)	(0.033)	(0.032)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.480	0.480	0.480	0.480
FE: CC-by-YM	X	X	X	X
FE: CC-by-DoW	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.6: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (between 4 and 30 Days)

(c) CC-Y-DoW + CC-M-DoW + CC-DoW-AM

	(1)	(2)	(3)	(4)
ED Visit	-0.026	-0.040	-0.040	-0.045
	(0.033)	(0.031)	(0.031)	(0.031)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.480	0.480	0.480	0.480
FE: CC-by-Y-by-DoW	X	X	X	X
FE: CC-by-M-by-DoW	X	X	X	X
FE: CC-by-DoW-by-AM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.6: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (between 4 and 30 Days)

(d) CC-Y-DoW + CC-M-DoW + CC-DoW-HoW

	(1)	(2)	(3)	(4)
ED Visit	-0.027	-0.042	-0.041	-0.047
	(0.033)	(0.032)	(0.032)	(0.031)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.480	0.480	0.480	0.480
FE: CC-by-Y-by-DoW	X	X	X	X
FE: CC-by-M-by-DoW	X	X	X	X
FE: CC-by-DoW-by-HoD	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.7: Effect of ED Visit (within 3 Days) on Repeat ED Visit (between 4 and 30 Days)

(a) CC-YM (Main)

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.040	0.039	0.037
	(0.043)	(0.035)	(0.035)	(0.035)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.099	0.099	0.099	0.099
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.7: (Cont.) Effect of ED Visit (within 3 Days) on Repeat ED Visit (between 4 and 30 Days)

(b) CC-YM + CC-DoW

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.040	0.039	0.037
	(0.043)	(0.035)	(0.035)	(0.035)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.099	0.099	0.099	0.099
FE: CC-by-YM	X	X	X	X
FE: CC-by-DoW	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.7: (Cont.) Effect of ED Visit (within 3 Days) on Repeat ED Visit (between 4 and 30 Days)

(c) CC-Y-DoW + CC-M-DoW + CC-DoW-AM

	(1)	(2)	(3)	(4)
ED Visit	0.068	0.048	0.046	0.043
	(0.041)	(0.034)	(0.034)	(0.033)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.099	0.099	0.099	0.099
FE: CC-by-Y-by-DoW	X	X	X	X
FE: CC-by-M-by-DoW	X	X	X	X
FE: CC-by-DoW-by-AM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.7: (Cont.) Effect of ED Visit (within 3 Days) on Repeat ED Visit (between 4 and 30 Days)

(d) CC-Y-DoW + CC-M-DoW + CC-DoW-HoW

	(1)	(2)	(3)	(4)
ED Visit	0.069	0.048	0.047	0.044
	(0.040)	(0.033)	(0.033)	(0.032)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.099	0.099	0.099	0.099
FE: CC-by-Y-by-DoW	X	X	X	X
FE: CC-by-M-by-DoW	X	X	X	X
FE: CC-by-DoW-by-HoD	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.8: Effect of ED Visit (within 3 Days) on Repeat Call (within 3 Days)

(a) CC-YM (Main)

	(1)	(2)	(3)	(4)
ED Visit	-0.013	-0.015	-0.015	-0.015
	(0.010)	(0.010)	(0.010)	(0.010)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.023	0.023	0.023	0.023
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.8: (Cont.) Effect of ED Visit (within 3 Days) on Repeat Call (within 3 Days)

(b) CC-YM + CC-DoW

	(1)	(2)	(3)	(4)
ED Visit	-0.013	-0.015	-0.015	-0.016
	(0.010)	(0.010)	(0.010)	(0.010)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.023	0.023	0.023	0.023
FE: CC-by-YM	X	X	X	X
FE: CC-by-DoW	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.8: (Cont.) Effect of ED Visit (within 3 Days) on Repeat Call (within 3 Days)

(c) CC-Y-DoW + CC-M-DoW + CC-DoW-AM

	(1)	(2)	(3)	(4)
ED Visit	-0.014	-0.015	-0.016	-0.016
	(0.009)	(0.009)	(0.009)	(0.010)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.023	0.023	0.023	0.023
FE: CC-by-Y-by-DoW	X	X	X	X
FE: CC-by-M-by-DoW	X	X	X	X
FE: CC-by-DoW-by-AM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.8: (Cont.) Effect of ED Visit (within 3 Days) on Repeat Call (within 3 Days)

(d) CC-Y-DoW + CC-M-DoW + CC-DoW-HoW

	(1)	(2)	(3)	(4)
ED Visit	-0.013	-0.015	-0.016	-0.016
	(0.009)	(0.010)	(0.010)	(0.010)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.023	0.023	0.023	0.023
FE: CC-by-Y-by-DoW	X	X	X	X
FE: CC-by-M-by-DoW	X	X	X	X
FE: CC-by-DoW-by-HoD	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.9: Effect of ED Visit (within 3 Days) on Mortality (within 3 Days)

(a) CC-YM (Main)

	(1)	(2)	(3)	(4)
ED Visit	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.000	0.000	0.000	0.000
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.9: (Cont.) Effect of ED Visit (within 3 Days) on Mortality (within 3 Days)

(b) CC-YM + CC-DoW

	(1)	(2)	(3)	(4)
ED Visit	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.000	0.000	0.000	0.000
FE: CC-by-YM	X	X	X	X
FE: CC-by-DoW	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.9: (Cont.) Effect of ED Visit (within 3 Days) on Mortality (within 3 Days)

(c) CC-Y-DoW + CC-M-DoW + CC-DoW-AM

	(1)	(2)	(3)	(4)
ED Visit	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.000	0.000	0.000	0.000
FE: CC-by-Y-by-DoW	X	X	X	X
FE: CC-by-M-by-DoW	X	X	X	X
FE: CC-by-DoW-by-AM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.9: (Cont.) Effect of ED Visit (within 3 Days) on Mortality (within 3 Days)

(d) CC-Y-DoW + CC-M-DoW + CC-DoW-HoW

	(1)	(2)	(3)	(4)
ED Visit	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.000	0.000	0.000	0.000
FE: CC-by-Y-by-DoW	X	X	X	X
FE: CC-by-M-by-DoW	X	X	X	X
FE: CC-by-DoW-by-HoD	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.10: Effect of ED Visit (within 3 Days) on Hospital Admission (within 3 Days)

(a) Leave-One-Patient-Out (Main)

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.057	0.056	0.053
	(0.016)	(0.017)	(0.017)	(0.017)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.025	0.025	0.025	0.025
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.10: (Cont.) Effect of ED Visit (within 3 Days) on Hospital Admission (within 3 Days)

(b) Leave-One-Call-Out

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.057	0.056	0.053
	(0.016)	(0.016)	(0.017)	(0.017)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.025	0.025	0.025	0.025
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.10: (Cont.) Effect of ED Visit (within 3 Days) on Hospital Admission (within 3 Days)

(c) Non-Residualized

	(1)	(2)	(3)	(4)
ED Visit	0.063	0.060	0.059	0.056
	(0.017)	(0.017)	(0.017)	(0.018)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.025	0.025	0.025	0.025
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.10: (Cont.) Effect of ED Visit (within 3 Days) on Hospital Admission (within 3 Days)

(d) All Years Pooled

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.057	0.057	0.055
	(0.016)	(0.016)	(0.016)	(0.017)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.025	0.025	0.025	0.025
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.11: Effect of ED Visit (within 3 Days) on Primary Care Visit (within 3 Days)

(a) Leave-One-Patient-Out (Main)

	(1)	(2)	(3)	(4)
ED Visit	-0.327	-0.335	-0.333	-0.335
	(0.088)	(0.092)	(0.092)	(0.093)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.347	0.347	0.347	0.347
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.11: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (within 3 Days)

(b) Leave-One-Call-Out

	(1)	(2)	(3)	(4)
ED Visit	-0.325	-0.333	-0.331	-0.333
	(0.088)	(0.092)	(0.091)	(0.092)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.347	0.347	0.347	0.347
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.11: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (within 3 Days)

(c) Non-Residualized

	(1)	(2)	(3)	(4)
ED Visit	-0.317	-0.325	-0.324	-0.326
	(0.086)	(0.090)	(0.090)	(0.091)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.347	0.347	0.347	0.347
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.11: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (within 3 Days)

(d) All Years Pooled

	(1)	(2)	(3)	(4)
ED Visit	-0.305	-0.311	-0.312	-0.314
	(0.094)	(0.097)	(0.096)	(0.097)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.347	0.347	0.347	0.347
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.12: Effect of ED Visit (within 3 Days) on Primary Care Visit (between 4 and 30 Days)

(a) Leave-One-Patient-Out (Main)

	(1)	(2)	(3)	(4)
ED Visit	-0.023	-0.038	-0.036	-0.041
	(0.033)	(0.031)	(0.032)	(0.031)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.480	0.480	0.480	0.480
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.12: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (between 4 and 30 Days)

(b) Leave-One-Call-Out

	(1)	(2)	(3)	(4)
ED Visit	-0.025	-0.039	-0.038	-0.043
	(0.033)	(0.031)	(0.031)	(0.030)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.480	0.480	0.480	0.480
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.12: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (between 4 and 30 Days)

(c) Non-Residualized

	(1)	(2)	(3)	(4)
ED Visit	-0.027	-0.043	-0.041	-0.046
	(0.033)	(0.031)	(0.032)	(0.030)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.480	0.480	0.480	0.480
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.12: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (between 4 and 30 Days)

(d) All Years Pooled

	(1)	(2)	(3)	(4)
ED Visit	-0.010	-0.022	-0.023	-0.027
	(0.035)	(0.034)	(0.034)	(0.034)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.480	0.480	0.480	0.480
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.13: Effect of ED Visit (within 3 Days) on Repeat ED Visit (between 4 and 30 Days)

(a) Leave-One-Patient-Out (Main)

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.040	0.039	0.037
	(0.043)	(0.035)	(0.035)	(0.035)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.099	0.099	0.099	0.099
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.13: (Cont.) Effect of ED Visit (within 3 Days) on Repeat ED Visit (between 4 and 30 Days)

(b) Leave-One-Call-Out

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.040	0.039	0.036
	(0.043)	(0.035)	(0.035)	(0.035)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.099	0.099	0.099	0.099
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.13: (Cont.) Effect of ED Visit (within 3 Days) on Repeat ED Visit (between 4 and 30 Days)

(c) Non-Residualized

	(1)	(2)	(3)	(4)
ED Visit	0.058	0.037	0.035	0.033
	(0.044)	(0.036)	(0.036)	(0.035)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.099	0.099	0.099	0.099
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.13: (Cont.) Effect of ED Visit (within 3 Days) on Repeat ED Visit (between 4 and 30 Days)

(d) All Years Pooled

	(1)	(2)	(3)	(4)
ED Visit	0.059	0.042	0.040	0.038
	(0.037)	(0.030)	(0.030)	(0.030)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.099	0.099	0.099	0.099
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.14: Effect of ED Visit (within 3 Days) on Repeat Call (within 3 Days)

(a) Leave-One-Patient-Out (Main)

	(1)	(2)	(3)	(4)
ED Visit	-0.013	-0.015	-0.015	-0.015
	(0.010)	(0.010)	(0.010)	(0.010)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.023	0.023	0.023	0.023
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.14: (Cont.) Effect of ED Visit (within 3 Days) on Repeat Call (within 3 Days)

(b) Leave-One-Call-Out

	(1)	(2)	(3)	(4)
ED Visit	-0.014	-0.015	-0.016	-0.016
	(0.010)	(0.010)	(0.010)	(0.010)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.023	0.023	0.023	0.023
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.14: (Cont.) Effect of ED Visit (within 3 Days) on Repeat Call (within 3 Days)

(c) Non-Residualized

	(1)	(2)	(3)	(4)
ED Visit	-0.010	-0.011	-0.012	-0.012
	(0.010)	(0.011)	(0.011)	(0.011)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.023	0.023	0.023	0.023
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.14: (Cont.) Effect of ED Visit (within 3 Days) on Repeat Call (within 3 Days)

(d) All Years Pooled

	(1)	(2)	(3)	(4)
ED Visit	-0.015	-0.016	-0.017	-0.017
	(0.010)	(0.010)	(0.010)	(0.010)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.023	0.023	0.023	0.023
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.15: Effect of ED Visit (within 3 Days) on Mortality (within 3 Days)

(a) Leave-One-Patient-Out (Main)

	(1)	(2)	(3)	(4)
ED Visit	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.000	0.000	0.000	0.000
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.15: (Cont.) Effect of ED Visit (within 3 Days) on Mortality (within 3 Days)

(b) Leave-One-Call-Out

	(1)	(2)	(3)	(4)
ED Visit	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.000	0.000	0.000	0.000
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.15: (Cont.) Effect of ED Visit (within 3 Days) on Mortality (within 3 Days)

(c) Non-Residualized

	(1)	(2)	(3)	(4)
ED Visit	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.000	0.000	0.000	0.000
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.15: (Cont.) Effect of ED Visit (within 3 Days) on Mortality (within 3 Days)

(d) All Years Pooled

	(1)	(2)	(3)	(4)
ED Visit	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.000	0.000	0.000	0.000
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.16: Effect of ED Visit (within 3 Days) on Hospital Admission (within 3 Days)

(a) SE Clustered at Call Center (Main)

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.057	0.056	0.053
	(0.016)	(0.017)	(0.017)	(0.017)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.025	0.025	0.025	0.025
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.16: (Cont.) Effect of ED Visit (within 3 Days) on Hospital Admission (within 3 Days)

(b) SE Clustered at Call-Center-by-Month-Year

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.057	0.056	0.053
	(0.010)	(0.010)	(0.010)	(0.010)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.025	0.025	0.025	0.025
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call-center-by-month-year level.

Table A1.16: (Cont.) Effect of ED Visit (within 3 Days) on Hospital Admission (within 3 Days)

(c) Heteroskedasticity-Robust SE

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.057	0.056	0.053
	(0.009)	(0.009)	(0.009)	(0.009)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.025	0.025	0.025	0.025
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Heteroskedasticity-robust standard errors are reported.

Table A1.17: Effect of ED Visit (within 3 Days) on Primary Care Visit (within 3 Days)

(a) SE Clustered at Call Center (Main)

	(1)	(2)	(3)	(4)
ED Visit	-0.327	-0.335	-0.333	-0.335
	(0.088)	(0.092)	(0.092)	(0.093)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.347	0.347	0.347	0.347
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.17: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (within 3 Days)

(b) SE Clustered at Call-Center-by-Month-Year

	(1)	(2)	(3)	(4)
ED Visit	-0.327	-0.335	-0.333	-0.335
	(0.031)	(0.032)	(0.032)	(0.032)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.347	0.347	0.347	0.347
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call-center-by-month-year level.

Table A1.17: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (within 3 Days)

(c) Heteroskedasticity-Robust SE

	(1)	(2)	(3)	(4)
ED Visit	-0.327	-0.335	-0.333	-0.335
	(0.025)	(0.025)	(0.025)	(0.026)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.347	0.347	0.347	0.347
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Heteroskedasticity-robust standard errors are reported.

Table A1.18: Effect of ED Visit (within 3 Days) on Primary Care Visit (between 4 and 30 Days)

(a) SE Clustered at Call Center (Main)

	(1)	(2)	(3)	(4)
ED Visit	-0.023	-0.038	-0.036	-0.041
	(0.033)	(0.031)	(0.032)	(0.031)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.480	0.480	0.480	0.480
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.18: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (between 4 and 30 Days)

(b) SE Clustered at Call-Center-by-Month-Year

	(1)	(2)	(3)	(4)
ED Visit	-0.023	-0.038	-0.036	-0.041
	(0.027)	(0.028)	(0.028)	(0.028)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.480	0.480	0.480	0.480
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call-center-by-month-year level.

Table A1.18: (Cont.) Effect of ED Visit (within 3 Days) on Primary Care Visit (between 4 and 30 Days)

(c) Heteroskedasticity-Robust SE

	(1)	(2)	(3)	(4)
ED Visit	-0.023	-0.038	-0.036	-0.041
	(0.027)	(0.028)	(0.027)	(0.027)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.480	0.480	0.480	0.480
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Heteroskedasticity-robust standard errors are reported.

Table A1.19: Effect of ED Visit (within 3 Days) on Repeat ED Visit (between 4 and 30 Days)

(a) SE Clustered at Call Center (Main)

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.040	0.039	0.037
	(0.043)	(0.035)	(0.035)	(0.035)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.099	0.099	0.099	0.099
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.19: (Cont.) Effect of ED Visit (within 3 Days) on Repeat ED Visit (between 4 and 30 Days)

(b) SE Clustered at Call-Center-by-Month-Year

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.040	0.039	0.037
	(0.019)	(0.018)	(0.018)	(0.018)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.099	0.099	0.099	0.099
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call-center-by-month-year level.

Table A1.19: (Cont.) Effect of ED Visit (within 3 Days) on Repeat ED Visit (between 4 and 30 Days)

(c) Heteroskedasticity-Robust SE

	(1)	(2)	(3)	(4)
ED Visit	0.060	0.040	0.039	0.037
	(0.018)	(0.018)	(0.018)	(0.018)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.099	0.099	0.099	0.099
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Heteroskedasticity-robust standard errors are reported.

Table A1.20: Effect of ED Visit (within 3 Days) on Repeat Call (within 3 Days)

(a) SE Clustered at Call Center (Main)

	(1)	(2)	(3)	(4)
ED Visit	-0.013	-0.015	-0.015	-0.015
	(0.010)	(0.010)	(0.010)	(0.010)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.023	0.023	0.023	0.023
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.20: (Cont.) Effect of ED Visit (within 3 Days) on Repeat Call (within 3 Days)

(b) SE Clustered at Call-Center-by-Month-Year

	(1)	(2)	(3)	(4)
ED Visit	-0.013	-0.015	-0.015	-0.015
	(0.007)	(0.008)	(0.008)	(0.008)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.023	0.023	0.023	0.023
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call-center-by-month-year level.

Table A1.20: (Cont.) Effect of ED Visit (within 3 Days) on Repeat Call (within 3 Days)

(c) Heteroskedasticity-Robust SE

	(1)	(2)	(3)	(4)
ED Visit	-0.013	-0.015	-0.015	-0.015
	(0.008)	(0.008)	(0.008)	(0.008)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.023	0.023	0.023	0.023
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Heteroskedasticity-robust standard errors are reported.

Table A1.21: Effect of ED Visit (within 3 Days) on Mortality (within 3 Days)

(a) SE Clustered at Call Center (Main)

	(1)	(2)	(3)	(4)
ED Visit	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.000	0.000	0.000	0.000
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table A1.21: (Cont.) Effect of ED Visit (within 3 Days) on Mortality (within 3 Days)

(b) SE Clustered at Call-Center-by-Month-Year

	(1)	(2)	(3)	(4)
ED Visit	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.000	0.000	0.000	0.000
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call-center-by-month-year level.

Table A1.21: (Cont.) Effect of ED Visit (within 3 Days) on Mortality (within 3 Days)

(c) Heteroskedasticity-Robust SE

	(1)	(2)	(3)	(4)
ED Visit	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	320,145	320,145	320,145	320,145
Dependent Variable Mean	0.000	0.000	0.000	0.000
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of ED visit (within 3 days since triage). All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Heteroskedasticity-robust standard errors are reported.

Chapter 2

How Do Telephone Triage Nurses Affect Patient ED Utilization?[1]

§ 2.1 Introduction

Human decision-making agents often make substantially different choices for the same problems. Physicians differ in their tendency to prescribe certain treatments for observably similar patients (Phelps, 2000; Grytten and Sørensen, 2003; Epstein and Nicholson, 2009; Chandra et al., 2011; Van Parys and Skinner, 2016; Molitor, 2018). Bail judges differ in their propensity to grant pretrial release for similar defendants (Dobbie et al., 2018; Kleinberg et al., 2018). Disability examiners diverge in their generosity of granting disability claims (Maestas et al., 2013; French and Song, 2014; Dahl et al., 2014; Autor et al., 2019). Such cross-agent variation and its consequences on individuals' outcomes draws interest from researchers and policymakers. For researchers, these judge-fixed effects designs have become an increasingly popular way to find the effect

[1] The views expressed herein are those of the author and do not necessarily reflect those of the United States Department of Veterans Affairs or the Veterans Health Administrations.

of treatments that could otherwise not be randomized (Chyn et al., 2024). For policymakers, there is often interest in reducing the variation in agent tendencies in order to ensure equitable outcomes.

This study analyzes variations in practice styles across nurses in a telephone triage call center. These triage lines are nearly ubiquitous across the US and other developed countries, providing patients with the opportunity to receive prompt medical advice and better determine the appropriate level of care. Most often, these triage lines operate by having registered nurses evaluate patient symptoms and recommend the appropriate healthcare disposition following a decision-support algorithm. While these decision-support tools standardizes the triage process, nurses can still exercise discretion through a few channels. First, nurses are often allowed to override the triage disposition recommended by the algorithm. Second, nurses can intensify verbal communication (e.g., changing voice, tone, or word choices) to ensure patient compliance with the triage recommendations. The two margins of nurse discretion generate variations in triage styles across nurses.

This paper exploits quasi-random assignment of calls to nurses within call centers to isolate nurse heterogeneity in triage styles from patient heterogeneity in health conditions. For this, I examine nurse triage lines operated by the US Department of Veterans Affairs (VA). These lines are available nationally to VA enrollees, but VA operationalizes them with nurses grouped into location-based call centers. For the most part, these lines work similarly to lines provided by major insurance companies, private health care groups, and national health care systems such as the NHS. Callers are routed to an available nurse, and the nurse walks the caller through a series of questions aided by the decision-support algorithm. At the end of the call, the nurse can answer medical questions and provide a recommendation for follow-up care, including self-care, an in-person visit with a provider, or immediate medical attention. As such, I construct a measure of nurse tendency to recommend an immediate emergency department (ED) visit and a verbal communication intensity measure using telephone triage data from the call centers. Then, I examine whether variations in those nurse tendency measures translate to variations in patient post-triage healthcare utilization. Under quasi-random assignment, differences in patient post-triage outcomes across nurses can be interpreted as counterfactuals when patients were assigned to a nurse with a different triage style.

My nurse triage tendency measures reveal substantial cross-nurse variation in ED recommendations and verbal communication for on-average similar patients. After accounting for call-center-by-time effects, the average ED recommendation propensity differs by 25.5 percentage points (sample average = 29%), and the average call duration differs by 9.2 minutes (sample average = 10.6 minutes) between two nurses in the 5th and 95th percentile of the respective measure. Those heterogeneous triage styles translate to differences in patient healthcare utilization outcomes. My reduced-form estimates suggest that patients are more likely to visit an ED if triaged by nurses who tend to recommend ED more and talk longer. Reassigning a call to a nurse with a 10 percentage points higher average ED recommendation propensity and a 10 minutes longer average call duration increases the probability of having an ED visit within 3 days by 6.5 percentage points.

This study contributes to several strands of literature. First, it is related to a large body of literature on practice variation in health care. While existing studies document evidence of substantial variation in physicians' practice styles and consequences in patients' outcomes, less is known about practice variation among other healthcare professions and whether such variation affects patient outcomes. Among a few exceptions, Chan et al. (2022) propose a framework to identify agent heterogeneity in skill and preferences when cases are quasi-randomly assigned. Applying their framework to VA radiologist data, they report that a large component of cross-radiologist variation in pneumonia diagnosis rates is attributable to differences in diagnostic skill. I similarly exploit quasi-random allocation of incoming calls within VA call centers and document heterogeneous triage patterns across VA triage nurses.

Second, this paper is also related to the growing literature on the role of human decision-makers when they oversee predictive algorithms (Hoffman et al., 2018; Kleinberg et al., 2018; Frankel, 2021; Mullainathan and Obermeyer, 2022; Agarwal et al., 2023; Angelova et al., 2023). Prior studies report that human decision-makers often underperform algorithmic recommendations. Mullainathan and Obermeyer (2022) compare physicians' diagnoses for heart attacks to machine learning predictions of the probability of heart attack. Examining cases where physicians deviate from predicted risk, they show that physicians over- and under-diagnose heart attacks. Angelova et al. (2023) report that bail judges differ in their performance in assessing defendants' pretrial misconduct risk and that 90% of the judges underperform a predictive algorithm in risk assessment.

The rest of this paper proceeds as follows. The rest of this paper proceeds as follows. Section 2.2 provides the background of VA health care and the telephone program. Section 2.3 describes data sources and analysis sample construction. Section 2.4 presents a reduced-form analysis framework. Section 2.5 presents results. Section 2.6 discusses methodological implications for examiner design. Section 2.7 concludes.

§ 2.2 Background

The Veterans Health Administration (VHA) of the US Department of Veterans Affairs (VA) operates the nation's largest integrated healthcare delivery system, with around 170 medical centers and over 1,000 outpatient facilities. The VA provides inpatient and outpatient care to around 11 million enrollees, with approximately 6 million users annually. The VA reimburses for ED care at any hospital in emergencies, and about 5-10 percent of the VA health spending is devoted to this benefit, driven mainly by subsequent inpatient admissions. The VA divides the US into 18 operational regions (VISNs), with each VISN subdivided into local units called "stations." A station typically has a tertiary care hospital, ED, medical centers, and outpatient clinics.

The VA is known as an early adopter of telehealth, providing many medical services remotely. Across the US, the VA operates nurse advice lines that allow patients to call for many reasons, from appointment-making to directions. Five VISNs have a centralized call center for each, whereas the other VISNs have multiple call centers at a (or a group of) station(s) level. Some call centers are open 24/7/365, while others are closed during non-business hours (typically before 8 AM and after 4 PM).

When a veteran patient calls for triage, the next available nurse is assigned to assess the patient's health care needs and recommend appropriate follow-up care. The triage nurse evaluates patient symptoms using a decision-support algorithm, common to all nurses and call centers.[2] The algorithm standardizes the triage process. First, the nurse talks to the patient and gathers and inputs basic patient information into the algorithm, such as age, gender, chief complaint, and pain scale. Second, the algorithm prompts

[2] The algorithm is proprietary software developed by a third-party contractor and is similar to the software used in other nurse triage lines.

clinical questions based on the initial inputs. Third, the nurse further communicates with the patient and enters the patient's responses to those clinical questions into the algorithm. Fourth, the algorithm recommends a follow-up location (e.g., ED, Urgent Care, Clinic, Home) and interval (Now-911, Now, 2-8h, 12-24h, 24-48h). Appendix Figures A2.8a-A2.8f provide an example of the algorithm screen at each stage (DSHI Systems).

While the decision-support algorithm standardizes the triage process, nurses can still exercise discretion through two key channels. First, nurses are allowed to override the triage disposition recommended by the algorithm. Second, nurses can intensify verbal communication (e.g., changing voice, tone, or word choices) to ensure patient compliance with the triage recommendations. Overall, the nurses tend to recommend more intensive care than the algorithm. While the nurses recommend ED to 29 percent of the calls, the algorithm recommends ED to 26.5 percent. The triage records reveal that some nurses explicitly issue a warning message to ensure patient compliance (e.g., "You can experience serious health consequences if you do not seek immediate care.").

§ 2.3 Data

I construct my analysis sample from multiple VA administrative data sources, including nurse triage records, healthcare utilization records from emergency departments and primary care facilities, and patient demographics from the VA patient roster. Appendix Table A2.1 describes sample restriction steps and the number of calls, patients, nurses, and call centers at each step.

2.3.1 Data Sources and Sample Construction

I start with the universe of telephone triage cases received in all call centers across the US from July 1, 2018, to December 31, 2022. The triage records have information at the call level, including triage date-time (year, month, day, hour, and minute), patient ID, triage nurse ID, station (call center) ID, triage disposition (recommended follow-up location and timing), call duration (in minutes), and free-entry notes.

This study imposes several sample restrictions to construct the analysis sample. First, I restrict the sample to the calls received during weekday business hours (between 8 am and 4 pm), as some call centers do not offer telephone triage during non-business hours and transfer calls to other call centers or non-VA contractors. Second, I focus on the index triage cases by removing calls from patients with the most recent prior triage within the past 30 days. Third, I restrict the sample to the calls triaged by nurses with at least 100 calls per year to reduce noise in nurse triage practice measures. Fourth, I drop calls in call-center-by-month-by-year cells with only one nurse to ensure that calls had a chance of being as good as randomly assigned to different nurses. Lastly, I select a subset of 28 call centers from the remaining 72 call centers where patient age is balanced across nurses. Specifically, I regress patient age on nurse dummies and call-time dummies (day-of-week, hour-of-day, and month-year indicators) for each call center. Then, I only retain call centers for which the F-test of joint significance of nurse dummies fails to reject at the 10% significance level.[3] After those restrictions, the analysis sample consists of 319,830 calls (from 199,841 patients) received by 248

[3] Chan et al. (2022) use a similar last step for their main sample construction to ensure quasi-random assignment of VA radiologists to veteran patients' chest X-ray exams. Similar to their study, although we expect nurse assignment to be as good as random in all call centers, our interviews with nurse managers suggest organizational and managerial structure can differ across call centers in ways that call-center-by-call-date-time indicators may not perfectly absorb confounding variations.

nurses at 28 call centers.

The triage sample is linked to the primary outcomes of interest: post-triage healthcare utilization at emergency departments and primary care facilities. Using VA and non-VA ED visit records, I construct indicators of patients having at least one ED visit within 1 to 30 days of the triage call. Primary care visit indicators are similarly constructed using records from VA-affiliated primary care facilities.

For randomization and robustness checks, I gather patients' prior healthcare utilization (within 365 days of the triage), prior diagnoses (31 Elixhauser comorbidity indices), VA's benefits eligibility status (priority group indicators), and demographics (e.g., age, gender, marital status). Appendix Table A2.2 lists all control variables used for randomization and robustness checks.

Table 2.1 presents average call characteristics at each step of the sample restrictions. In the analysis sample (Step 9), nurses recommend ED visit at a higher rate than the algorithm (29% vs. 26.5%). The average call duration is 10.6 minutes. The average call is from near-elderly patient (average age = 61) with high rates of previous year healthcare utilization (primary care = 85%, VA ED = 25.2%, non-VA ED = 17.4%, telephone triage = 86%). Roughly 19.4% of the calls result in at least one ED visit within 3 days since triage call, and 34.7% of the calls result in primary care visit.

§ 2.4 Method

2.4.1 Overview

This study quantifies variations in practice styles across telephone triage nurses for similar patients and analyzes how such variations affect patient healthcare utilization.

Once assigned to a call, the nurse evaluates the patient's symptoms and recommends the appropriate healthcare disposition using a decision-support algorithm. Although the decision-support tool standardizes the triage protocol, the nurse can still exercise discretion through (i) overriding the triage disposition recommended by the algorithm and (ii) intensifying verbal communication (e.g., changing voice, tone, or word choices) to ensure patient compliance with the triage recommendations. In what follows, I propose a method to capture cross-nurse variations in the two margins of nurse discretion and estimate how those variations affect patient post-triage utilization outcomes.

2.4.2 Reduced-Form Model

I consider a reduced-form model that explains patient ED visit by assigned nurse's average triage styles. Specifically, for call i from patient $k(i)$ received by nurse j, suppose that patient $k(i)$'s ED visit is determined by nurse- and call-specific factors:

$$\begin{aligned} ED_{k(i)} &= \lambda_j^{Visit} + \alpha_i^{Visit} \\ &= \lambda_j^{Visit} + \boldsymbol{X}_i' \gamma^{Visit} + u_i^{Visit} \end{aligned} \tag{2.1}$$

where $ED_{k(i)}$ is an indicator of whether patient $k(i)$ has at least one ED visit (within 3 days) after triage, and λ_j^{Visit} is nurse j's average tendency to have patients visit an ED. Call-specific factor α_i^{Visit} is divided into observable variables $\boldsymbol{X}_i$ (call-center-by-call-time fixed effects) and an unobserved error term u_i^{Visit}.

As discussed above, nurse j can exercise her discretion in triage by (i) adjusting the triage recommendation and (ii) changing verbal communication. I rewrite the nurse j effect in Equation (2.1) as a function of nurse j's average ED recommendation and average verbal communication intensity as follows:

$$\lambda_j^{Visit} = f\left(\lambda_j^{Rec}, \lambda_j^{Verbal}\right) \tag{2.2}$$

where λ_j^{Rec} and λ_j^{Verbal} are nurse j's average ED recommendation and verbal communication intensity, respectively. Plugging this into Equation (2.1), I have a reduced-form model as follows:

$$ED_{k(i)} = f\left(\lambda_j^{Rec}, \lambda_j^{Verbal}\right) + \boldsymbol{X}_i' \boldsymbol{\gamma}^{Visit} + u_i^{Visit} \tag{2.3}$$

where the function $f(\cdot, \cdot)$ captures counterfactual ED visit status under different combinations of the two triage practice values.

Nurses can intensify their verbal communication with a patient by several different ways, such as voice, tone, or word choices (e.g., "you would face serious health consequences if you do not visit ED"). In what follows, I use call duration (in minutes) as a proxy measure for verbal treatment.

2.4.3 Construction of Nurse Triage Tendency Measures

I empirically quantify nurse triage practice styles using my analysis sample described in Section 2.3. While the sample average of ED recommendation indicators (call duration) among all the calls assigned to each nurse is an unbiased estimate of the nurse's ED recommendation (call duration) tendency, with a finite number of cases per nurse, including call i itself generates a correlation between the nurse triage style measure and call-specific determinants of ED visit, resulting in a bias in reduced-form estimate. To avoid this finite sample bias problem, I construct leave-one-out averages of residual ED recommendations and call duration at the nurse level.

Specifically, for call i that is assigned to nurse j, I first obtain residual of nurse ED recommendation indicator, denoted as D_i^{Rec*}, before calculating the leave-one-out average. I partial out the conditioning set $\boldsymbol{X}_i$ from ED recommendation indicator D_i^{Rec} using the following linear regression:

$$D_i^{Rec*} = D_i^{Rec} - \boldsymbol{X}_i'\boldsymbol{\gamma}^{Rec} = \lambda_j^{Rec} + u_i^{Rec} \tag{2.4}$$

where $\boldsymbol{X}_i$ includes call-center-by-call-month-year interactions. The residuals D_i^{Rec*} include the nurse effect λ_j^{Rec} and idiosyncratic call-level error term u_i^{Rec}.

Then, I construct the leave-out ED recommendation tendency measure by averaging the residuals of all other patients but patient $k(i)$ assigned to nurse j in year $y(i)$:

$$\lambda_{j,-k(i)}^{Rec} = \frac{1}{K_{j,y(i)} - 1} \sum_{i'} \frac{1\left\{k(i') \neq k(i), j(i') = j, y(i') = y(i)\right\} D_{i'}^{Rec*}}{n_{k(i'),j,y(i)}} \tag{2.5}$$

where $K_{j,y}$ is the number of patients assigned to nurse j in year y and $n_{k,j,y}$ is the total number of calls from patient k received by nurse j in year y.

I use call duration as a proxy measure of verbal communication intensity. A leave-out average of call duration $\lambda_{j,-k(i)}^{Dur}$ is similarly calculated by replacing ED recommendation indicator with call duration in Equations (2.4) and (2.5).[4]

2.4.4 Empirical Model for Reduced-Form Analysis

The reduced-form model in Equation (2.3) considers how the nurse ED recommendation and verbal communication (proxied by call duration) tendencies affect patient ED

[4] In constructing the leave-out average of call duration, I drop 315 observations with call duration longer than the 99.9th percentile of the duration distribution (72 minutes). The maximum value of the dropped observations is 96,421 minutes. This extreme value is likely due to system error.

utilization. As the main empirical specification, I specify the function $f(\cdot,\cdot)$ with a linear function of the two leave-out measures and their interaction as follows:

$$ED_{k(i)} = \theta_0 + \theta_1 \lambda^{Dur}_{j,-k(i)} + \theta_2 \lambda^{Rec}_{j,-k(i)} + \theta_3 \lambda^{Dur}_{j,-k(i)} \cdot \lambda^{Rec}_{j,-k(i)} + \boldsymbol{X}_i' \boldsymbol{\gamma}^{Visit} + u_i^{Visit} \tag{2.6}$$

where the interaction term allows the partial effect of one measure to depend on the other. $\theta_3 > 0$ implies that patients are more likely to visit an ED if they are assigned to a nurse who is more likely to recommend ED and has longer call durations, compared to a nurse who is less likely to recommend ED and tends to have shorter call durations.

2.4.5 2SLS Estimation

I further estimate the treatment effects of ED recommendation and call duration using the constructed leave-out nurse averages of ED recommendation ($\lambda^{Rec}_{j,-k(i)}$), call duration ($\lambda^{Dur}_{j,-k(i)}$), and their interaction ($\lambda^{Rec}_{j,-k(i)} \cdot \lambda^{Dur}_{j,-k(i)}$) as instruments. I specify the model as follows:

$$\begin{aligned} ED_{k(i)} &= g\left(D_i^{Dur}, D_i^{Rec}, \boldsymbol{X}_i, u_i\right) \\ &= \beta_0 + \beta_1 D_i^{Dur} + \beta_2 D_i^{Rec} + \beta_3 D_i^{Dur} \cdot D_i^{Rec} + \boldsymbol{X}_i' \boldsymbol{\Pi} + u_i \end{aligned} \tag{2.7}$$

where the interaction term of the two treatment variables allows the partial effect of one treatment to depend on the other:

$$\frac{\partial ED_{k(i)}}{\partial D_i^{Dur}} = \beta_1 + \beta_3 D_i^{Rec} \tag{2.8}$$

$$\frac{\partial ED_{k(i)}}{\partial D_i^{Rec}} = \beta_2 + \beta_3 D_i^{Dur}. \tag{2.9}$$

Allowing for multiple treatments complicates instrumental variable assumptions for the local average treatment effect (LATE) interpretation under treatment effect heterogeneity (Angrist and Imbens, 1995; Heckman et al., 2006; Mogstad et al., 2021). I assume a stronger assumption of constant treatment effect to interpret 2SLS estimates. Given this caveat, I consider this 2SLS analysis to provide secondary evidence that supports the reduced-form analysis.

2.4.6 Variation in Nurse Triage Tendency Measures

Figure 2.2a and 2.2b show the distributions of the leave-out call duration and ED recommendation measures, which exhibit substantial variations across nurses at those margins. After accounting for call-center-by-time effects, the leave-out average of (residualized) call duration ranges from -4.648 to 4.563, with a standard deviation of 2.850. The leave-out average of (residualized) ED recommendation propensity ranges from -0.123 to 0.132, with a standard deviation of 0.077.

The solid line visually presents the reduced-form relationship between each leave-out measure and patient ED visit status within three days since triage. The corresponding linear reduced-form coefficient for the call duration measure is 0.0027, implying that reassigning a call from the 5th to the 95th percentile nurse increases the probability of having an ED visit by 2.5 percentage points, a 12.8% increase from the 3-day ED visit rate of 19.4%. The linear reduced-form coefficient for the ED recommendation measure is 0.344, suggesting that reassigning a call from the 5th to the 95th percentile nurse increases the probability of having an ED visit by 8.8 percentage points, a 45.2% increase from the 3-day ED visit rate of 19.4%.

Figure 2.2 presents a scatter plot of the leave-out call duration and the leave-out

ED recommendation measures averaged at the nurse level. Each dot represents a nurse by the combination of the two triage-style measures. While nurses substantially differ in the combination of the two triage-style measures, the two measures are positively correlated. The correlation coefficient of the two measures (calculated at the call level) is 0.277, which implies that nurses with a higher propensity to recommend ED tend to talk longer.

2.4.7 Balance Check

This study exploits quasi-random assignment of calls to nurses within the call-center-by-call-time cells to isolate nurse heterogeneity in practice styles from patient heterogeneity in health conditions. This strategy fails if some nurses receive systematically different patients than other nurses. While I do not hear any evidence of systematic sorting mechanisms from qualitative interviews with VA nurse managers, I empirically examine this quasi-randomness assumption by testing if the two leave-out nurse practice measures are correlated with patient characteristics.

Specifically, I examine whether a composite measure of patient characteristics is correlated with the constructed leave-out nurse triage tendency measures, following the examiner design literature (Chan et al., 2023). For each call, I first obtain fitted ED visit probability using a linear regression of ED visit indicator on a set of patient demographics (age, veteran status, marital status, gender, priority group, and rurality of residence), prior healthcare utilization and diagnosis measures as well as call-center-by-call-time fixed effects. Then, I examine if the predicted ED visit is correlated with the two nurse practice measures. In Figure 2.2a, the dashed line presents a natural cubic spline regression of (residualized) predicted ED visit probability on the leave-out call duration measure. Figure 2.2b similarly visualizes the relationship between the predicted ED visit and the leave-out ED recommendation measure. The flat lines imply that the two measures are not meaningfully related with patient's underlying health conditions, supporting quasi-random assignment of calls to nurses.

§ 2.5 Results

2.5.1 Reduced-Form Estimates of Nurse Triage Measures

ED Visit

Table 2.3a presents the reduced-form effects of the two nurse triage tendency measures on the probability of ED visit within 3 days since triage. Columns (1), (3), and (4) reveal that, while a 10-minute difference in average call duration translates to an increase in the ED visit probability by 0.027 percentage points when the call duration measure is the sole regressor, the call duration measure becomes insignificant when the two triage measures are included in the regression simultaneously. In Columns (2) and (3), the estimates imply that reassigning a call to a nurse with a 1 percentage point higher ED recommendation tendency increases the probability of having an ED visit by approximately 0.34 percentage points. In Column (4), the positive interaction effect estimate implies that nurses with a higher ED recommendation propensity and a longer call duration tendency are more likely to have patients visit an ED. Reassigning a call to a nurse with a 10 percentage points higher average ED recommendation propensity and a 10 minutes longer average call duration increases the probability of having an ED visit within 3 days by 6.5 percentage points ($0.0647 = 0.001 \times 1 + 0.345 \times 0.1 + 0.292 \times 1 \times 0.1$).

Appendix Figures A2.2a, A2.3a, A2.4a, and A2.5a present the reduced-form estimates of a visit within 1 to 30 days since triage. While the effects of the two triage style measures are gradually waning, the estimates suggest that talking to a nurse with more aggressive triage styles persistently increases the probability of visiting an ED within 30 days.

Primary Care Visit

Table 2.3b shows the reduced-form estimates on the probability of having at least one primary care visit. In Column (1), while the estimate is imprecise, the call duration tendency is estimated to have a small negative effect. Columns (2) and (3) reveal that reassigning a call to a nurse with a 1 percentage point higher ED recommendation propensity decreases the probability of having a primary care visit by 0.128 percentage points. In Column (4), the interaction term has a positive coefficient estimate, implying that nurses with a higher ED recommendation and a longer average call duration are likely to have patients visit a primary care physician. Appendix Figures A2.2c, A2.3c, A2.4c, and A2.5c show the reduced-form estimates of a visit within 1 to 30 days since triage. Appendix Figure A2.2c shows that, while the average call duration measure has negative estimated effects on the probability of having a primary care visit immediately after triage when included as the sole regressor, the effect disappears over time. In Appendix Figure A2.5c, the estimates from the specification with the interaction term do not differ over time.

2.5.2 2SLS Estimations of Nurse Recommendation and Call Duration Effects

This study further implements 2SLS estimation of call duration and nurse ED recommendation effects on patient healthcare utilization using the leave-out average call duration, the leave-out average ED recommendation, and their interaction as instruments. Appendix Table A2.3 presents first-stage estimates.

ED Visit

Table 2.4a presents 2SLS estimates of the effects of call duration and nurse ED recommendation on the probability of having an ED visit. The positive coefficients on the ED recommendation indicator and the interaction term imply that nurse ED recommendation increases patients' ED visit probability regardless of call duration. With call duration fixed at 10 minutes, a call recommended an ED visit is more likely to visit an ED by 38.6 ($0.386 = 0.029 + 0.357 \times 1$) percentage points than a call recommended a non-ED facility. The negative coefficient on call duration suggests that an increase in call duration has a differential impact on ED visits, depending on whether the nurse recommends an ED visit. An extra 10 minutes of call time increases the ED visit probability by 25.2 ($0.252 = -0.105 + 0.357 \times 1$) percentage points for patients recommended ED. In contrast, it decreases the ED visit probability by 10.5 ($-0.105 = -0.105 + 0.357 \times 0$) percentage points for patients recommended a non-ED facility. Appendix Figures A2.2b, A2.3b, A2.4b, and A2.5b present the 2SLS estimates within 1 to 30 days since triage.

Primary Care Visit

Table 2.4b shows 2SLS estimates of the effects of call duration and ED recommendation on the probability of having a primary care visit. The estimates imply that the partial effect of nurse ED recommendation depends on call duration. While nurse ED recommendation increases the probability of having a primary care visit if the call duration is longer than 16 minutes ($1.594 = 0.322/0.202$), it decreases the probability of having a primary care visit if the call duration is shorter than 16 minutes. Likewise, the partial effect of call duration depends on whether the assigned nurse recommends ED. An extra 10 minutes of call time increases the primary care visit probability by 14.4 ($0.144 = -0.058 + 0.202 \times 1$) percentage points for patients recommended ED. In contrast, it decreases the primary care visit probability by 5.8 ($-0.058 = -0.058+0.202\times 0$) percentage points for patients recommended a non-ED facility. Appendix Figures A2.2d, A2.3d, A2.4d, and A2.5d show the 2SLS estimates within 1 to 30 days since triage.

§ 2.6 Discussion

The reduced-form results suggest that nurse practice styles – represented by the combination of triage recommendation and call duration tendencies – meaningfully affect healthcare utilization outcomes for similar patients. The estimates on the interaction of the two triage style measures are positive for both ED and primary care utilization outcomes. This finding suggests that nurses with more aggressive treatment styles on the two margins are more effective at getting patients to followup care than nurses with less aggressive styles. The 2SLS results align with this finding. Patients are more likely to have some healthcare visit (ED or primary care) when they receive a more intensive healthcare recommendation (ED) combined with a longer call duration.

Those findings have several methodological implications in research designs that exploit quasi-random agent assignment as instruments for multiple treatments.[5] First, the reduced-form analysis reveals that nurse triage styles potentially affect multiple utilization outcomes (e.g., ED and primary care). A researcher might naively define treatment using a single indicator of having a post-triage ED visit (vs. no visit) and estimate treatment effect with nurse assignment as an instrument. However, if some nurses are more likely to have patients utilize healthcare services in general than others, ignoring healthcare utilization at other facilities (e.g., primary care, urgent care) violates the exclusion restriction.[6]

Second, this study finds that the two practice style measures are positively correlated. Literature examining the causal interpretation of 2SLS with multiple instruments (for a single treatment) points out that using one instrument without conditioning on the others violates the exogeneity condition for LATE unless the instruments are mutually independent (Carneiro et al., 2011; Mogstad et al., 2021). In the present context, using nurse ED recommendation propensity ($E\left[D_i^{Rec}(j)\right]$) as a sole instrument for the patient's ED visit without conditioning on nurse verbal communication style ($E\left[D_i^{Dur}(j)\right]$) violates the LATE assumption.[7] To ensure the LATE interpretation under treatment effect het-

[5]For this context, such research design views the reduced form in this paper as the first stage to estimate the effect of ED care on subsequent patient outcomes.

[6]Some frontier studies in judges design literature attempt to identify interpretable treatment effect parameters with 2SLS under multiple treatments (Bhuller and Sigstad, 2024; Humphries et al., 2023; Chyn et al., 2024). Humphries et al. (2023) discuss conditions under which a 2SLS that controls for non-focal treatment propensities can identify causal effects of the treatment of interest.

[7]This problem arises in this context because the decision-maker for the ED visit (patient) and the decision-maker for the ED recommendation (nurse) do not coincide. In this respect, this design departs from the standard judges design where the judge is the only decision-maker for treatment (e.g., pretrial release). More broadly, this issue will likely arise when agents "nudge" subjects into (a single) treatment using multiple channels.

erogeneity, researchers must either use the propensity score ($E\left[ED_{k(i)}(j)\right]$ estimated by the leave-one-out average of the ED visit indicator of the patients assigned to each nurse) as a single collapsed instrument or control for nurse practice styles in all other dimensions.

§ 2.7 Conclusion

This paper examines the origins of the cross-nurse difference in average ED visit rates of similar patients quasi-randomly assigned to VA telephone triage nurses. I consider two margins of nurse discretion in the VA triage process as potential sources of the cross-nurse variation in patients' ED utilization rates: (i) upgrading triage recommendation to ED and (ii) intensifying verbal communication to ensure patient compliance with the triage disposition. I construct the leave-one-patient-out measures of nurse ED recommendation propensity and call duration tendency. Then, I document that the two measures exhibit substantial variations across nurses. My reduced-form estimates suggest that nurses with a higher ED recommendation and a longer call duration lead patients to see some healthcare provider (in ED or primary care) after triage.

There are several avenues for potential extension. First, while this paper examines the cross-nurse variation in the propensity to recommend ED, future research can examine the efficacy of having human nurses in the triage process by explicitly quantifying and examining cross-nurse variation in the propensity to deviate from the algorithm recommendation. Appendix Figure A2.5 plots the nurse-level average of ED recommendation conditional on algorithm ED recommendation against the nurse-level average of ED recommendation conditional on algorithm non-ED recommendation. Those nurses at the top-left corner can approximate a counterfactual state with no nurse discretion (at least in triage disposition), as they never deviate from the algorithm recommendations.[8] Future research must thoroughly explore conditions to identify the efficacy of having human nurses relative to the algorithm from observed differences in average patient outcomes between nurses who deviate and nurses who never deviate.

Second, examining the underlying causes of cross-nurse variations in their triage practice is imperative. While this study finds substantial variations in nurse triage recommendations, it has yet to address whether they reflect cross-nurse differences in diagnostic skills or preferences. From an econometrics perspective, Chan et al. (2022) argue that cross-examiner differences in diagnostic skills violate a strict form of monotonicity assumption in research design that exploits the quasi-random assignment of examiners as an instrument (judges design). From a policy perspective, understanding the causes is crucial if policymakers want to deliver more uniform services by decreasing cross-nurse variation in triage recommendations. Future research must explore methods to distinguish the causes of cross-nurse variations.[9]

Third, while this study uses call duration as a proxy for verbal communication, future research can consider constructing alternative verbal communication measures by applying natural language processing methods to text data in triage records.[10] Identifying particular communication styles that encourage (or discourage) healthcare utilization at

[8]This approximation resembles the "identification at infinity" method. For instance, Arnold et al. (2022) extrapolate (unobserved) pretrial misconduct rates of the detained, exploiting quasi-random case assignment to "supremely lenient" judges who release nearly all defendants.

[9]Existing studies propose methods to isolate skill from preferences using cross-examiner variation in misclassification (confusion matrix). For instance, Chan et al. (2022) find substantial cross-radiologist variation in false negative rates in pneumonia diagnoses (the share of each radiologist's patients initially not diagnosed with pneumonia but diagnosed with pneumonia within the next 10 days), attributing a large portion of cross-radiologist variation in pneumonia diagnoses to differences in diagnostic skills.

[10]VA triage nurses typically leave free-entry summaries of triage conversations in records.

more granular levels (e.g., word choices) can be informative in considering effective telephone triage operations.

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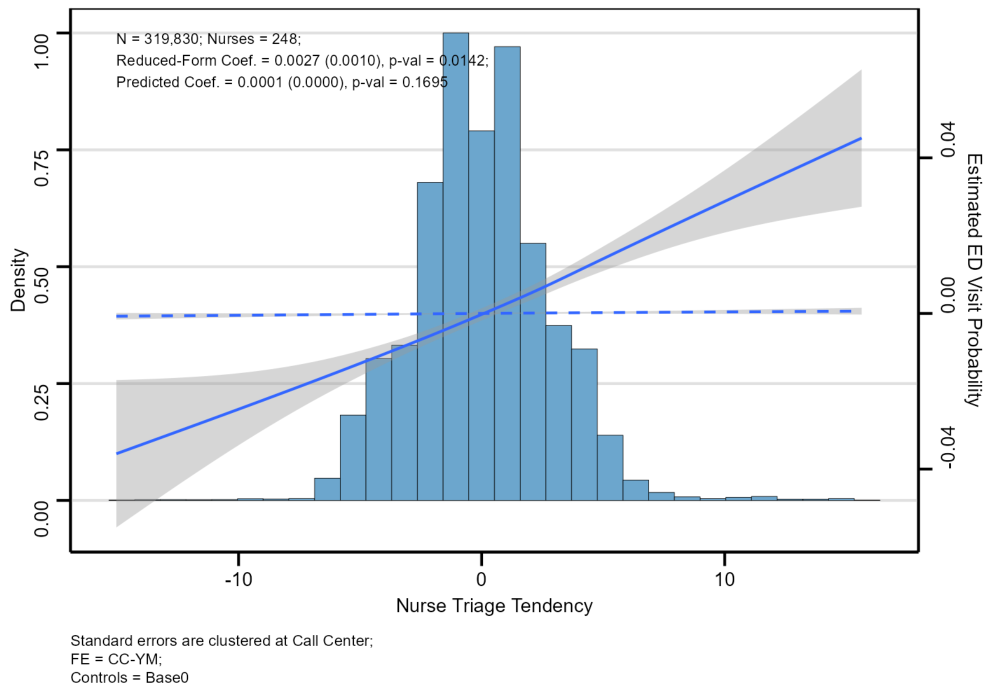
§ 2.9 Tables and Figures

Figure 2.1: Variations in Leave-Out Triage Measures

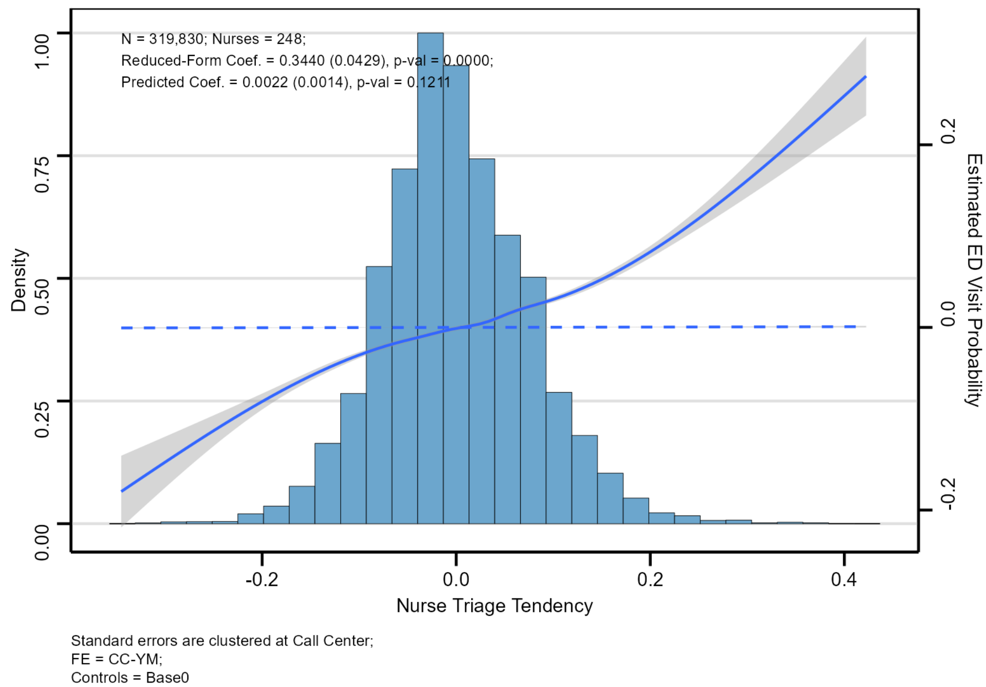
(a) Leave-Out Call Duration (in Minutes)



Note: This figure shows the distribution of leave-one-patient-out average of residual call duration as described in Section 2.4.3. The x-axis represents the leave-out average of residual call duration (unit = minutes). The left y-axis represents density, scaled to maximum of 1. The solid line visually presents the reduced-form relationship between the leave-one-out call duration and patient ED visit status within three days since triage. The dashed line visually presents a balance check by a natural cubic spline regression of (residualized) predicted ED visit probability (on the right y-axis) on the leave-out call duration measure. The corresponding linear reduced-form and linear balance regression slope estimates are displayed at the top of figure.

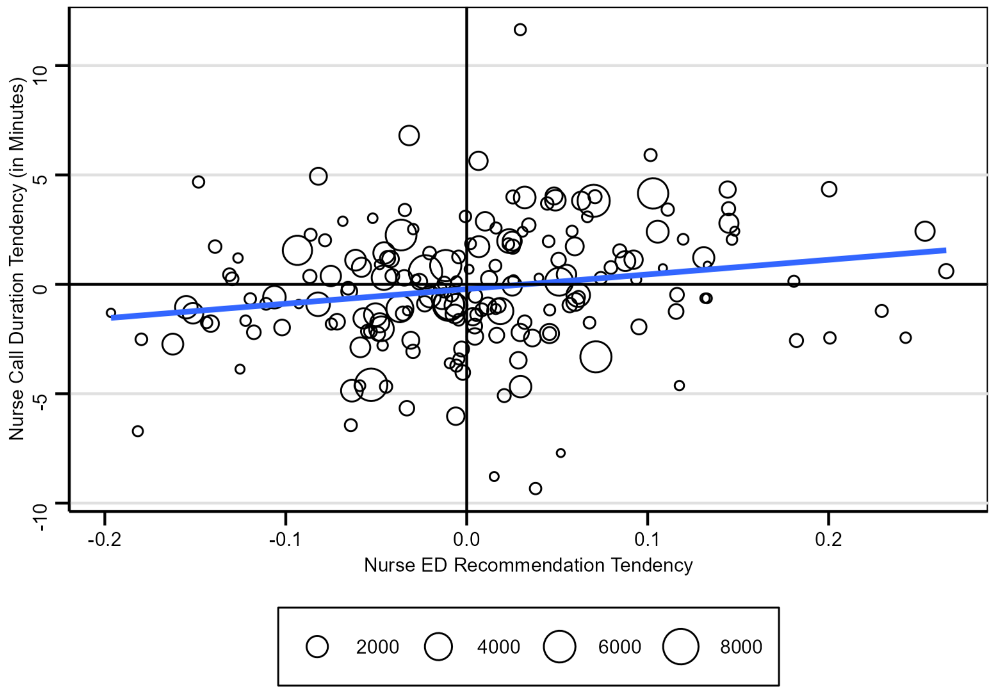
Figure 2.1: (Cont.) Variations in Leave-Out Triage Measures

(b) Leave-Out ED Recommendation



Note: This figure shows the distribution of leave-one-patient-out average of residual nurse ED recommendation as described in Section 2.4.3. The x-axis represents the leave-out average of residual nurse ED recommendation. The left y-axis represents density, scaled to maximum of 1. The solid line visually presents the reduced-form relationship between the leave-one-out nurse ED recommendation and patient ED visit status within three days since triage. The dashed line visually presents a balance check by a natural cubic spline regression of (residualized) predicted ED visit probability (on the right y-axis) on the leave-out nurse ED recommendation measure. The corresponding linear reduced-form and linear balance regression slope estimates are displayed at the top of figure.

Figure 2.2: Nurse ED Recommendation vs. Call Duration Tendency Measures



Note: This figure presents the relationship between the leave-out average of residual nurse ED recommendations and the leave-out average of residual call duration. I further average Equation (2.5) at the nurse level to have each point in this figure represent one nurse. The point size reflects the total number of calls each nurse triaged during the sample period. The solid line overlaid on the points is an unweighted regression of the call duration measure (averaged at the nurse level) on the ED recommendation measure (averaged at the nurse level).

Table 2.1: Characteristics of Baseline Sample

Variables	Step 0	Step 1	Step 2	Step 3	Step 4	Step 5	Step 6	Step 7	Step 8	Step 9
Algorithm ED	0.303	0.306	0.305	0.268	0.265	0.26	0.266	0.266	0.265	0.265
Nurse ED	0.358	0.355	0.354	0.286	0.282	0.277	0.285	0.284	0.29	0.29
Call Duration	7.844	14.214	14.126	13.202	13.13	12.789	12.502	12.508	11.605	10.645
Age	61.312	60.477	60.463	60.77	60.589	60.554	60.553	60.551	61.096	61.095
Veteran	0.995	0.995	0.995	0.994	0.994	0.994	0.994	0.994	0.993	0.993
Married	0.475	0.473	0.473	0.491	0.496	0.496	0.498	0.498	0.488	0.488
Male	0.859	0.852	0.852	0.854	0.855	0.855	0.854	0.854	0.861	0.861
White	0.676	0.666	0.666	0.675	0.674	0.674	0.661	0.661	0.634	0.634
Black	0.176	0.18	0.18	0.176	0.177	0.177	0.185	0.185	0.19	0.19
Hispanic	0.066	0.071	0.071	0.067	0.068	0.068	0.071	0.071	0.058	0.058
Rural County	0.185	0.181	0.181	0.194	0.195	0.195	0.191	0.191	0.255	0.255
Previous Year Primary Care	0.856	0.853	0.853	0.858	0.851	0.851	0.853	0.853	0.85	0.85
Previous Year VA ED	0.347	0.348	0.348	0.308	0.29	0.289	0.293	0.293	0.252	0.252
Previous Year Non VA ED	0.185	0.173	0.173	0.157	0.145	0.144	0.142	0.142	0.174	0.174
Previous Year Inpatient	0.133	0.13	0.13	0.107	0.099	0.098	0.099	0.099	0.084	0.084
Previous Year Tele Primary Care	0.699	0.699	0.699	0.688	0.676	0.673	0.677	0.677	0.702	0.702
Previous Year Tele Triage	0.874	0.896	0.896	0.881	0.874	0.894	0.898	0.898	0.86	0.86
Previous Year Mental Health	0.285	0.289	0.289	0.271	0.263	0.263	0.264	0.264	0.25	0.25
Previous Year Clinical Pharmacy	0.301	0.296	0.296	0.272	0.261	0.26	0.264	0.264	0.253	0.252
Previous Year Tele Mental Health	0.251	0.25	0.25	0.229	0.221	0.222	0.22	0.22	0.217	0.217
Elix Current Score	3.802	3.771	3.77	3.614	3.542	3.534	3.548	3.546	3.53	3.53
Elix Corrected Score	4.352	4.309	4.308	4.144	4.07	4.061	4.076	4.075	4.085	4.085
ED 1d	0.173	0.199	0.199	0.162	0.159	0.157	0.16	0.159	0.155	0.155
ED 2d	0.21	0.24	0.24	0.192	0.189	0.186	0.189	0.189	0.183	0.183
ED 3d	0.223	0.254	0.253	0.203	0.199	0.196	0.2	0.2	0.194	0.194
PCP 1d	0.151	0.158	0.158	0.171	0.162	0.164	0.164	0.164	0.193	0.193
PCP 2d	0.23	0.243	0.243	0.257	0.247	0.25	0.251	0.251	0.296	0.296
PCP 3d	0.279	0.294	0.294	0.303	0.293	0.295	0.297	0.297	0.347	0.347
Calls	4,930,385	3,378,539	3,375,685	2,301,816	1,982,806	1,836,659	1,661,096	1,656,381	320,145	319,830
Patients	2,044,447	1,638,708	1,637,575	1,300,324	1,245,346	1,161,894	1,053,487	1,049,952	199,997	199,841
Nurses	6,866	3,888	3,881	3,518	3,058	1,421	1,293	1,290	248	248
Call Centers	101	101	101	101	101	96	74	72	28	28

Note: This table presents average call characteristics at each step of the sample restrictions to construct the analysis sample, detailed in Appendix Table A2.1. Algorithm ED and Nurse ED are triage indicators that equal 1 if call i is recommended ED. Veteran status, marital status, race and ethnicity, and rural residence are binary indicators. Previous year utilization indicators take 1 if the patient associated with call i had respective utilization events at least once in the past 365 days. Elixhauser scores are the total count of 31 comorbidity indices. "Current" scores count recorded diagnoses during a given fiscal year, whereas "corrected" scores look back at two fiscal years of recorded diagnoses and assign the patient the higher comorbidity count of those two years. Post-triage ED and PCP indicators measure ED and primary care physician visits within 1 to 3 days since triage.

Table 2.2: Reduced-Form Effects of Leave-Out Nurse Triage Measures

(a) LHS = ED Visit (Within 3 Days)

	(1)	(2)	(3)	(4)
Leave-out Call Duration (in 10 mins)	0.027		0.002	0.001
	(0.010)		(0.007)	(0.008)
Leave-out Nurse ED Recommendation		0.344	0.342	0.345
		(0.043)	(0.042)	(0.040)
Leave-out Nurse ED Recommendation × Leave-out Call Duration (in 10 mins)				0.292
				(0.075)
Observations	319,830	319,830	319,830	319,830
Dependent Variable Mean	0.194	0.194	0.194	0.194
FE: CC-by-YM	X	X	X	X

Note: This table reports reduced-form estimates of the effect of nurse triage practice measures. Leave-one-out averages of nurse ED recommendation and call duration are constructed as described in Method section. The nurse ED recommendation measure is in probability (1 unit = 100 percentage points). The call duration measure is scaled (1 unit = 10 minutes). All specifications include call-center-by-call-time fixed effects. Standard errors are clustered at the call center level.

Table 2.2: (Cont.) Reduced-Form Effects of Leave-Out Nurse Triage Measures

(b) LHS = PCP Visit (Within 3 Days)

	(1)	(2)	(3)	(4)
Leave-out Call Duration (in 10 mins)	-0.009		0.000	0.000
	(0.010)		(0.010)	(0.009)
Leave-out Nurse ED Recommendation		-0.128	-0.128	-0.127
		(0.051)	(0.056)	(0.055)
Leave-out Nurse ED Recommendation × Leave-out Call Duration (in 10 mins)				0.176
				(0.132)
Observations	319,830	319,830	319,830	319,830
Dependent Variable Mean	0.347	0.347	0.347	0.347
FE: CC-by-YM	X	X	X	X

Note: This table reports reduced-form estimates of the effect of nurse triage practice measures. Leave-one-out averages of nurse ED recommendation and call duration are constructed as described in Method section. The nurse ED recommendation measure is in probability (1 unit = 100 percentage points). The call duration measure is scaled (1 unit = 10 minutes). All specifications include call-center-by-call-time fixed effects. Standard errors are clustered at the call center level.

Table 2.3: 2SLS Estimates of ED Recommendation and Call Duration

(a) LHS = ED Visit (Within 3 Days)

	(1)	(2)	(3)	(4)
Call Duration (in 10 mins)	-0.105	-0.105	-0.105	-0.104
	(0.024)	(0.025)	(0.024)	(0.024)
Nurse ED Recommendation	0.029	0.025	0.028	0.029
	(0.075)	(0.078)	(0.077)	(0.075)
Nurse ED Recommendation × Call Duration (in 10 mins)	0.357	0.357	0.355	0.353
	(0.076)	(0.077)	(0.076)	(0.074)
Observations	319,830	319,830	319,830	319,830
Dependent Variable Mean	0.194	0.194	0.194	0.194
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of nurse ED recommendation and call duration. Nurse ED recommendation is a binary indicator (= 1 if recommended ED). Call duration is scaled (1 unit = 10 minutes). These treatment variables are instrumented by the leave-out average of nurse ED recommendation, the leave-out average of call duration, and their interaction. All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

Table 2.3: (Cont.) 2SLS Estimates of ED Recommendation and Call Duration

(b) LHS = PCP Visit (Within 3 Days)

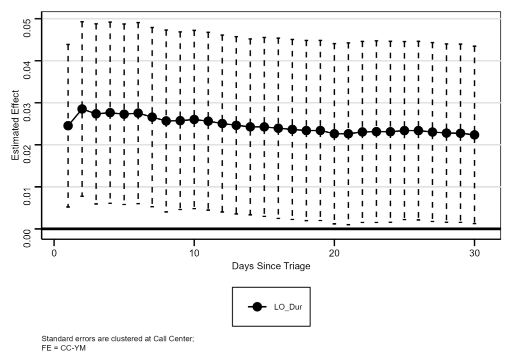
	(1)	(2)	(3)	(4)
Call Duration (in 10 mins)	-0.058	-0.058	-0.056	-0.056
	(0.052)	(0.052)	(0.050)	(0.050)
Nurse ED Recommendation	-0.322	-0.320	-0.317	-0.318
	(0.190)	(0.186)	(0.183)	(0.182)
Nurse ED Recommendation × Call Duration (in 10 mins)	0.202	0.200	0.195	0.195
	(0.172)	(0.169)	(0.166)	(0.166)
Observations	319,830	319,830	319,830	319,830
Dependent Variable Mean	0.347	0.347	0.347	0.347
FE: CC-by-YM	X	X	X	X
Prior Utilization 1		X	X	X
Prior Utilization 2			X	X
Hold-Out Controls				X

Note: This table reports IV estimates of the effect of nurse ED recommendation and call duration. Nurse ED recommendation is a binary indicator (= 1 if recommended ED). Call duration is scaled (1 unit = 10 minutes). These treatment variables are instrumented by the leave-out average of nurse ED recommendation, the leave-out average of call duration, and their interaction. All specifications include call-center-by-call-time fixed effects. Control variables in all specifications are described in Appendix. Prior utilization 1 includes previous year VA ED, non-VA ED, and inpatient admission. Prior utilization 2 includes previous year tele-primary care, tele-triage, mental health, clinical pharmacy, and tele-mental health. Hold-out controls include demographics, socioeconomic status, combat history, eligibility for benefits, and prior diagnoses (Elixhauser scores). Standard errors are clustered at the call center level.

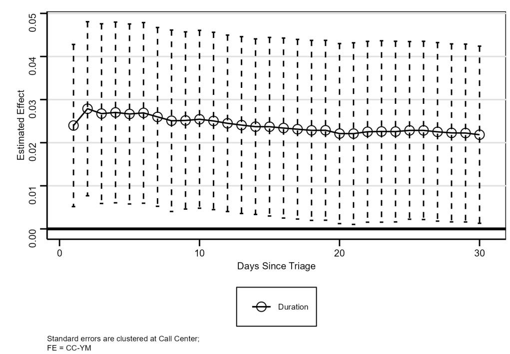
§ 2.10 Appendix Tables and Figures

Figure A2.1: 2SLS and Reduced-Form (IV = Leave-Out Call Duration)

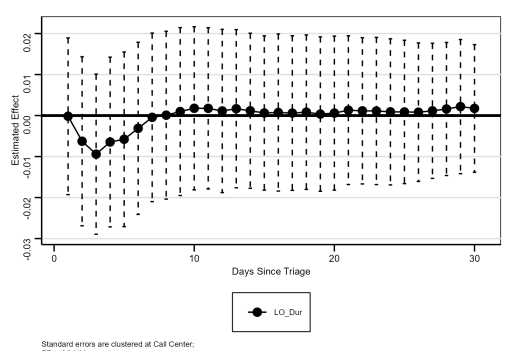
(a) Reduced Form: Y = ED Visit



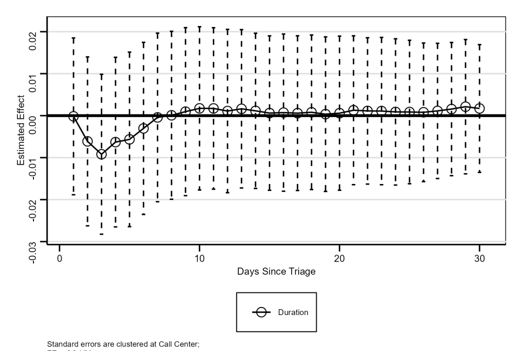
(b) 2SLS: Y = ED Visit



(c) Reduced Form: Y = PCP Visit



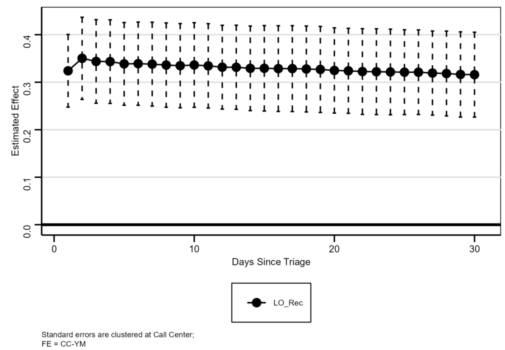
(d) 2SLS: Y = PCP Visit



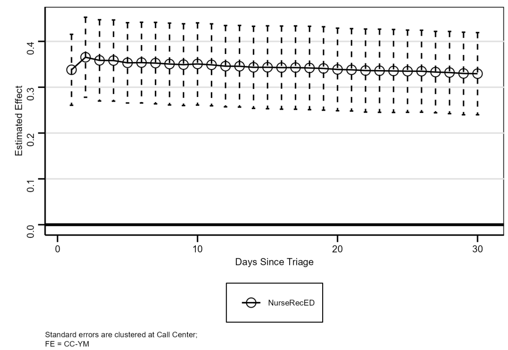
Note: Panels (a) and (c) show the reduced-form estimates of Equation (2.6) with the leave-out average of call duration as the sole regressor. Panels (b) and (d) similarly present the 2SLS estimates of Equation (2.7) with call duration as the sole treatment variable instrumented by the leave-out average of call duration. The outcomes are indicator variables of whether the patient has an ED (PCP) visit within 1 to 30 days since triage.

Figure A2.2: 2SLS and Reduced-Form (IV = Leave-Out ED Recommendation)

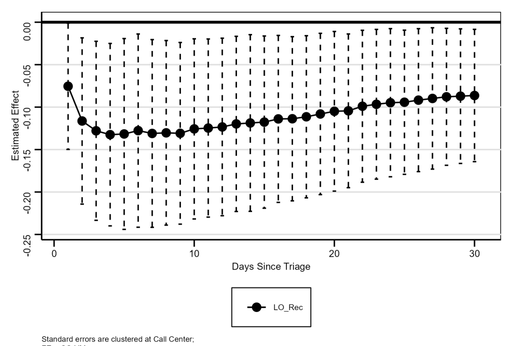
(a) Reduced Form: Y = ED Visit



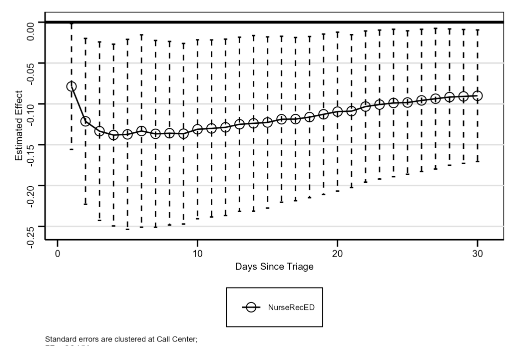
(b) 2SLS: Y = ED Visit



(c) Reduced Form: Y = PCP Visit



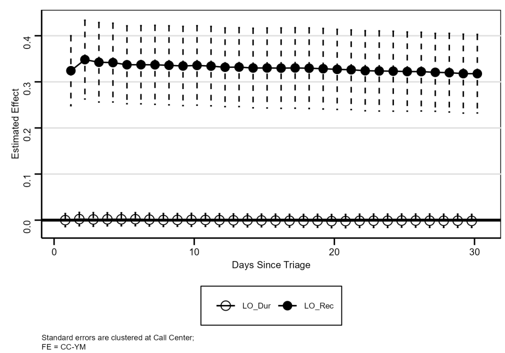
(d) 2SLS: Y = PCP Visit



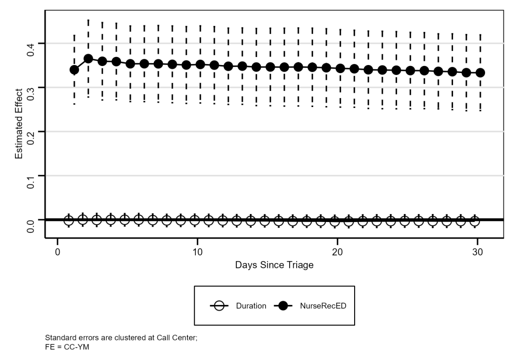
Note: Panels (a) and (c) show the reduced-form estimates of Equation (2.6) with the leave-out average of nurse ED recommendation as the sole regressor. Panels (b) and (d) similarly present the 2SLS estimates of Equation (2.7) with nurse ED recommendation as the sole treatment variable instrumented by the leave-out average of nurse ED recommendation. The outcomes are indicator variables of whether the patient has an ED (PCP) visit within 1 to 30 days since triage.

Figure A2.3: 2SLS and Reduced-Form (IV = Leave-Out ED Recommendation and Leave-Out Call Duration)

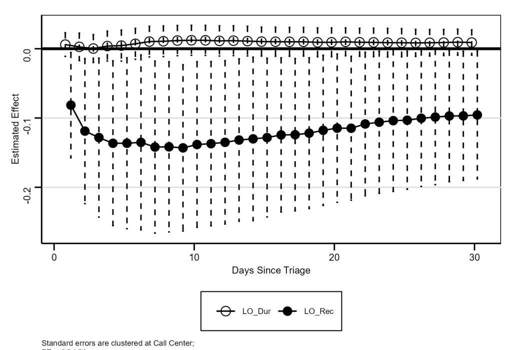
(a) Reduced Form: Y = ED Visit



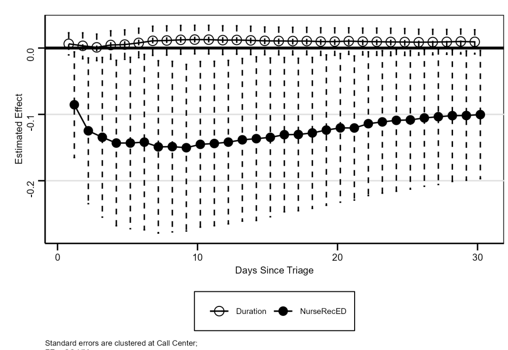
(b) 2SLS: Y = ED Visit



(c) Reduced Form: Y = PCP Visit



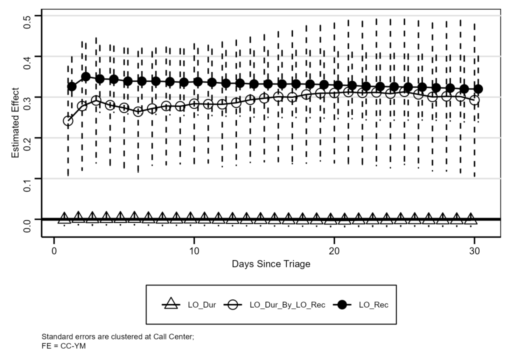
(d) 2SLS: Y = PCP Visit



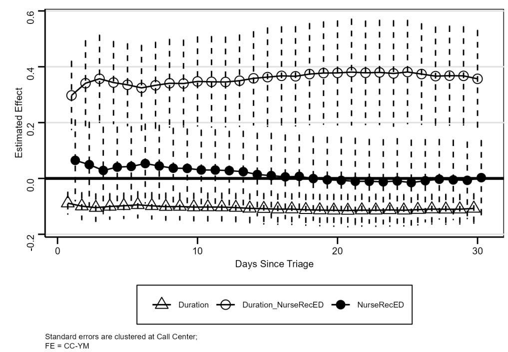
Note: Panels (a) and (c) show the reduced-form estimates of Equation (2.6) with the leave-out average of nurse ED recommendation and the leave-out average of call duration as two regressors. Panels (b) and (d) similarly present the 2SLS estimates of Equation (2.7) with call duration and nurse ED recommendation as two treatment variables instrumented by the leave-out average of nurse ED recommendation and the leave-out average of call duration. The outcomes are indicator variables of whether the patient has an ED (PCP) visit within 1 to 30 days since triage.

Figure A2.4: 2SLS and Reduced-Form (IV = Leave-Out ED Recommendation and Leave-Out Call Duration)

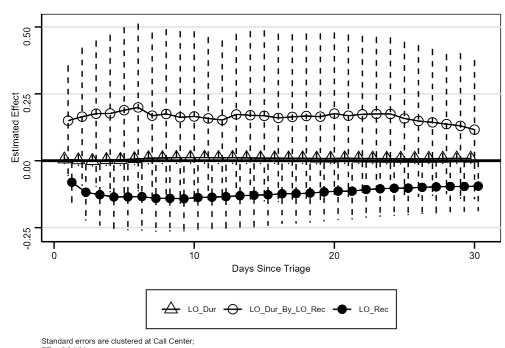
(a) Reduced Form: Y = ED Visit



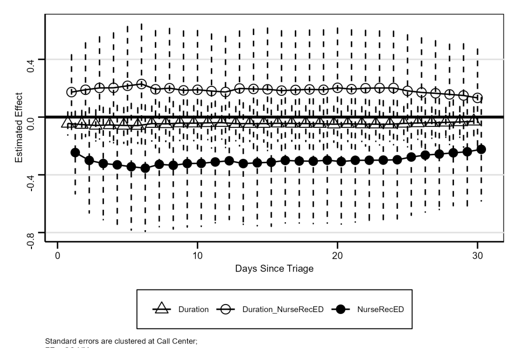
(b) 2SLS: Y = ED Visit



(c) Reduced Form: Y = PCP Visit

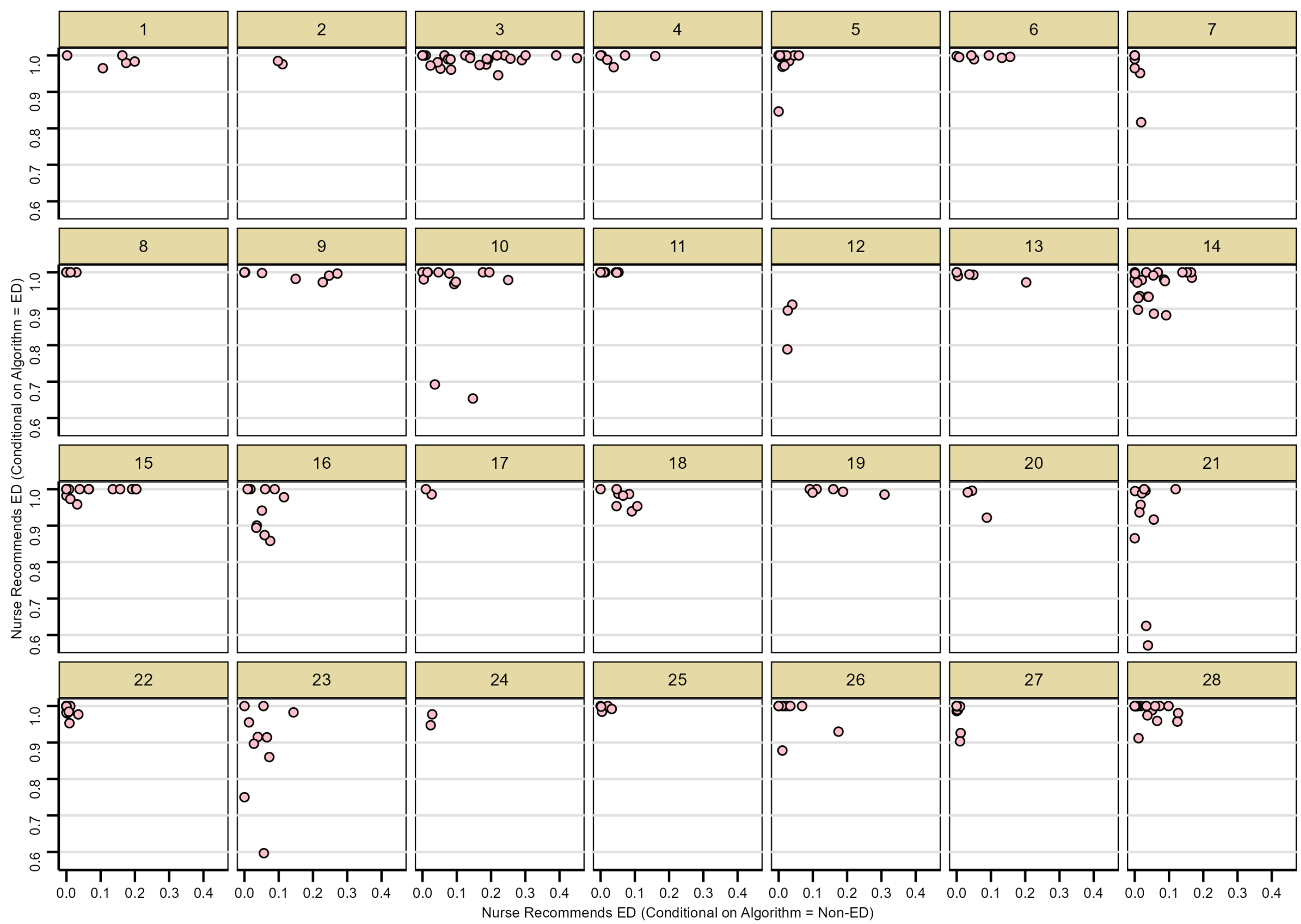


(d) 2SLS: Y = PCP Visit



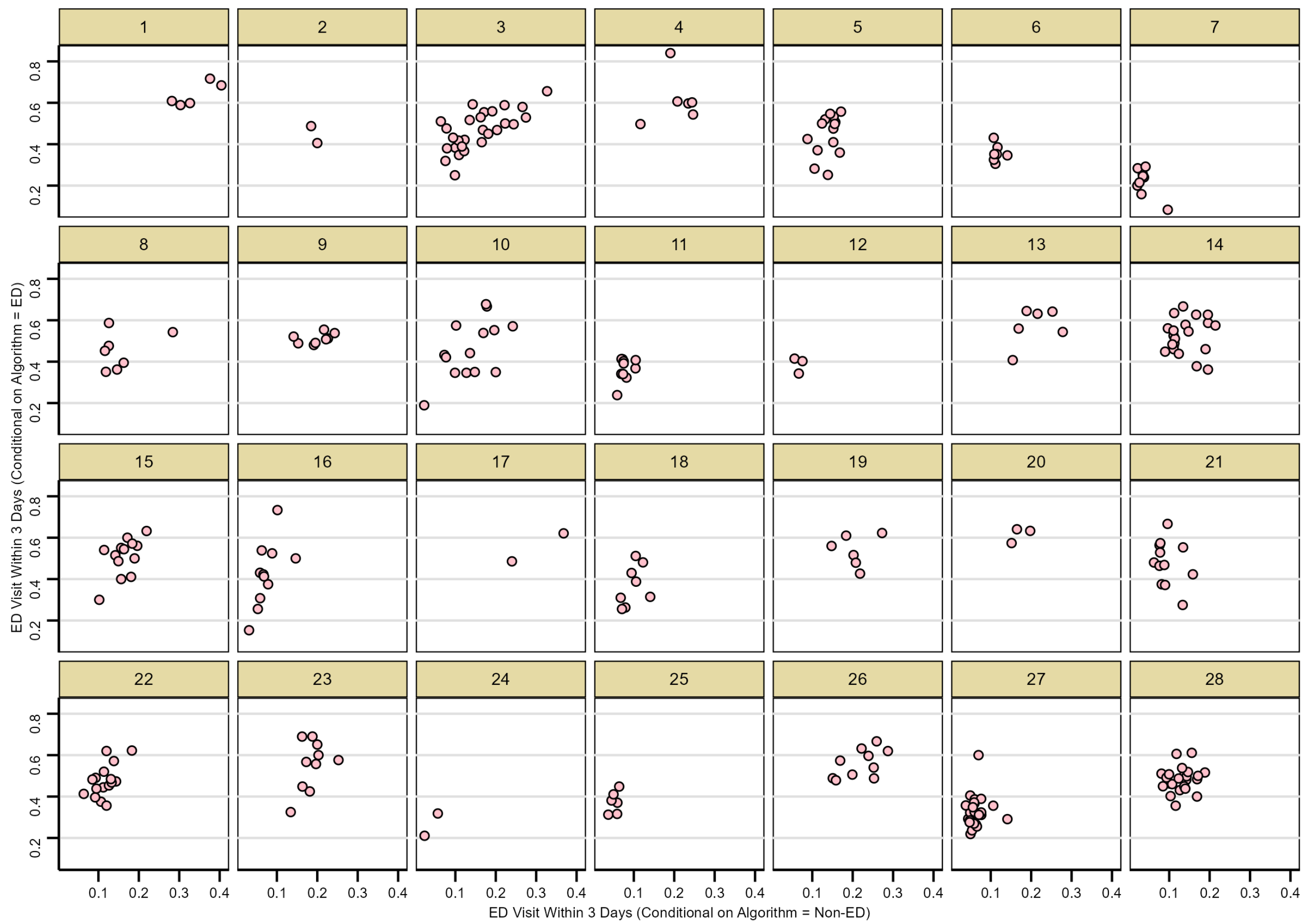
Note: Panels (a) and (c) show the reduced-form estimates of Equation (2.6) with the leave-out average of nurse ED recommendation, the leave-out average of call duration, and their interaction as three regressors. Panels (b) and (d) similarly present the 2SLS estimates of Equation (2.7) with call duration, nurse ED recommendation, and their interaction as three treatment variables instrumented by the two leave-out measures and their interaction. The outcomes are indicator variables of whether the patient has an ED (PCP) visit within 1 to 30 days since triage.

Figure A2.5: Nurse-Level Average of ED Recommendation, Conditional on Algorithm Recommendation



Note: This figure plots the nurse-level average of ED recommendation conditional on algorithm ED recommendation (y-axis) against the nurse-level average of ED recommendation conditional on algorithm non-ED recommendation (x-axis). Each point represents one nurse. Each cell represents one call center. The top-left corner is the nurse who never deviates from the algorithm recommendations.

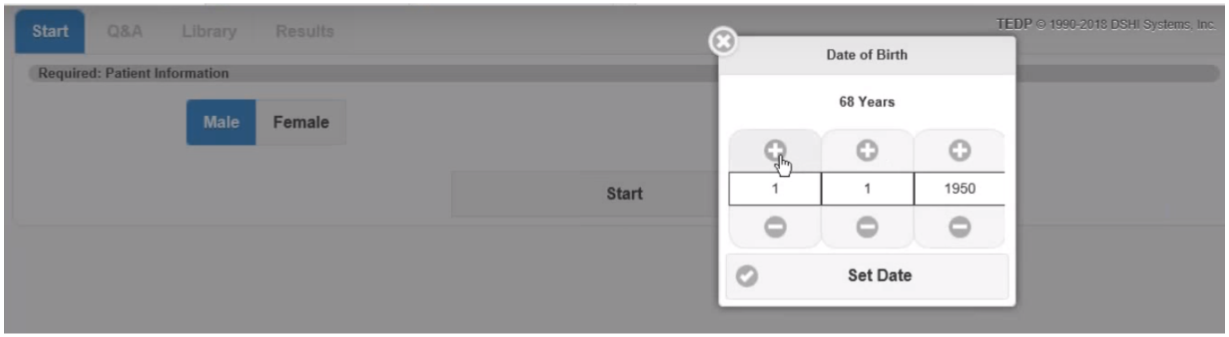
Figure A2.6: Nurse-Level Average of ED Visit, Conditional on Algorithm Recommendation



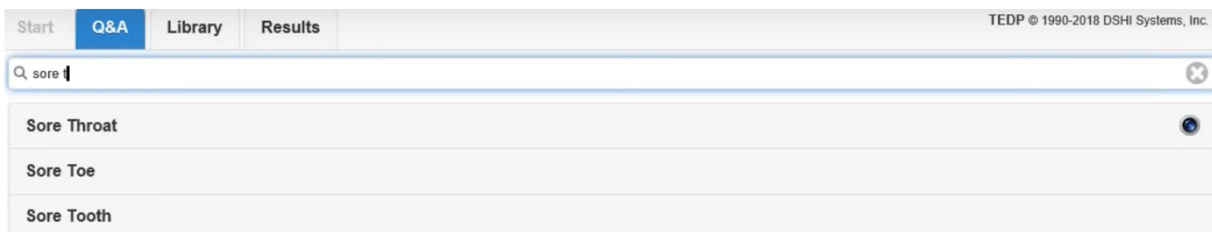
Note: This figure plots the nurse-level average of ED visits conditional on algorithm ED recommendation (y-axis) against the nurse-level average of ED visits conditional on algorithm non-ED recommendation (x-axis). Each point represents one nurse. Each cell represents one call center.

Figure A2.7: Triage Algorithm Screen

(a) Initial Inputs (Age and Gender)



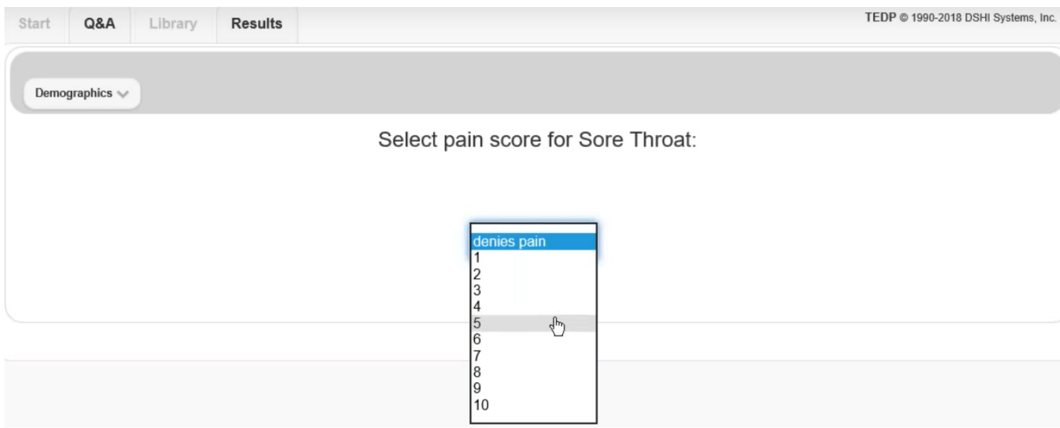
(b) Initial Inputs (Chief Complaint)



Note: These figures show part of the triage algorithm screen used by VA triage nurses. Panels A2.8a and A2.8b display screens for the initial inputs (patient's age, gender, and chief complaint).

Figure A2.7: (Cont.) Triage Algorithm Screen

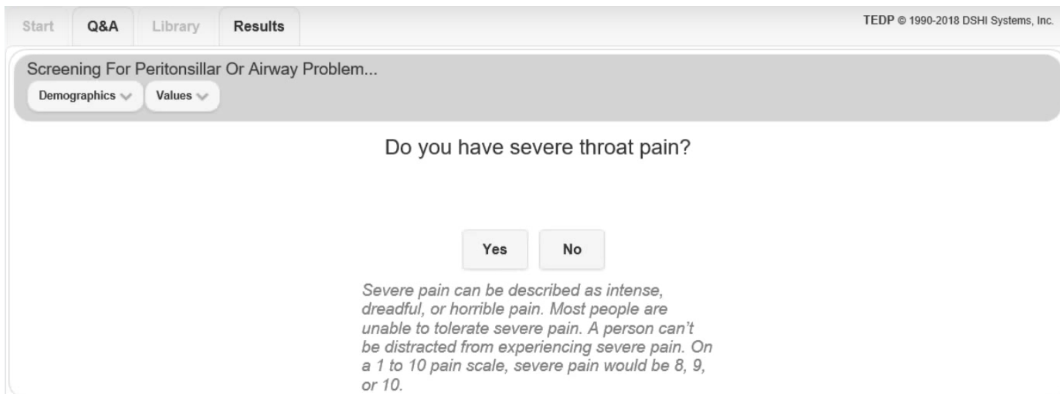
(c) Initial Inputs (Pain Scale)



Note: These figures show part of the triage algorithm screen used by VA triage nurses. Panel A2.8c displays a screen for the initial inputs (pain scale).

Figure A2.7: (Cont.) Triage Algorithm Screen

(d) Symptom Questions

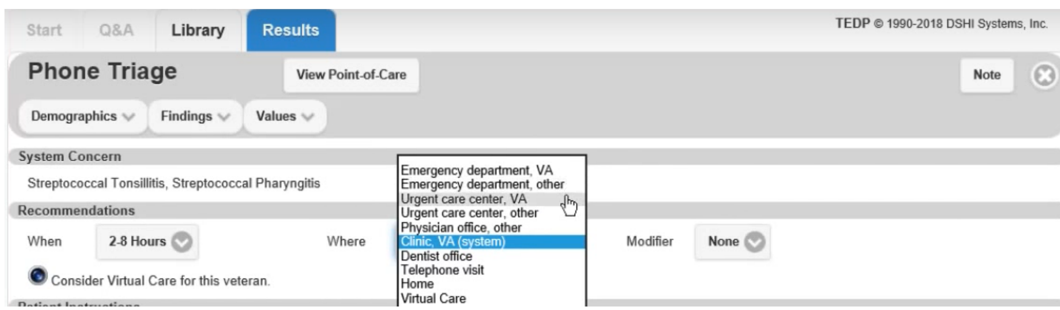


Note: These figures show part of the triage algorithm screen used by VA triage nurses. Panel A2.8d displays a symptom question. The algorithm prompts a sequence of symptom-related questions based on the initial inputs. The nurse feeds patient's response into the algorithm.

Figure A2.7: (Cont.) Triage Algorithm Screen

(e) Algorithm Recommendation

(f) Nurse Recommendation



Note: These figures show part of the triage algorithm screen used by VA triage nurses. Panels A2.8e and A2.8f display triage dispositions selected by the algorithm and nurse. The nurse is allowed to change triage dispositions from algorithm recommendations.

Table A2.1: Selection of Baseline Sample

Step	Description	Calls	Patients	Nurses	Call Centers
0	All symptom calls with complete IDs and dates.	4,930,385	2,044,447	6,866	101
1	Drop calls with incomplete triage disposition values (follow-up locations and intervals recommended by nurse & algorithm).	3,378,539	1,638,708	3,888	101
2	Drop calls from patients younger than 20 or older than 99; Drop calls from patients in inpatient facilities.	3,375,685	1,637,575	3,881	101
3	Drop calls received during non-business hours (before 8 am; after 4 pm; weekends; holidays).	2,301,816	1,300,324	3,518	101
4	Drop calls from patients with the most recent prior call within 30 days.	1,982,806	1,245,346	3,058	101
5	Drop calls received by nurses with less than or equal to 100 calls per year.	1,836,659	1,161,894	1,421	96
6	Drop calls received in call centers that experience consolidation during the analysis period.	1,661,096	1,053,487	1,293	74
7	Drop calls received in call centers with only one nurse per month-year.	1,656,381	1,049,952	1,290	72
8	Restrict to call centers for which patient age is balanced across nurses.	320,145	199,997	248	28
9	Drop calls with call duration longer than the 99.9th percentile of the duration distribution.	319,830	199,841	248	28

Notes: This table describes sample restriction steps to construct the analysis sample. The table lists the number of calls, patients, nurses, and call centers at each step. Table 2.1 presents average call characteristics at each step.

Table A2.2: List of Control Variables

Variables	Number of Indicators
Call-Center-by-Call-Time FEs	
Call-Center-by-Month-Year	1,017
Call-Center-by-Day-of-Week	139
Call-Center-by-Year-by-Day-of-Week	509
Call-Center-by-Month-by-Day-of-Week	1,640
Call-Center-by-Day-of-Week-by-AM	279
Call-Center-by-Day-of-Week-by-Hour-of-Day	1,118
Call-Center-by-Month-Year-by-Day-of-Week	5,055
Call-Center-by-Month-Year-by-Hour-of-Day	8,067
Prior Utilization 1	
Previous Year Primary Care	1
Previous Year VA ED	1
Previous Year Non VA ED	1
Previous Year Inpatient	1
Prior Utilization 2	
Previous Year Tele Primary Care	1
Previous Year Tele Triage	1
Previous Year Mental Health	1
Previous Year Clinical Pharmacy	1
Previous Year Tele Mental Health	1
Hold-Out Controls	
Age bins (5-year)	12
Male	1
Veteran (Y, N, NA)	2
Race (White, Black, Hispanic, Asian/Other, NA)	4
Rural County (Y, N)	1
Income Bin (1st-3rd terciles, NA)	3
Priority Score (High-, Low-/Moderate-, No-Disability; Low-Income; NA)	4
Service Connection (No SC, 0-49, 50-99, 100, NA)	4
Period of Service (Vietnam, Gulf, Post-Vietnam, Korean, Post-Korean, WW2, Other)	6
Elix Corrected Score bins (1-10, 11+, NA)	11

Note: This table lists control variables. Sets of indicators are constructed from each control. Column 2 shows the number of indicators generated from each control. Call-Center-by-Call-Month-Year FEs are included in the main analysis. Robustness checks use FEs at the different granularity. Prior Utilization 1, Prior Utilization 2, and Hold-Out Controls are sequentially added in robustness checks. All covariates are used to obtain the predicted ED visit probability for randomization check.

Table A2.3: First-Stage Effects of Leave-Out Nurse Triage Measures

(a) LHS: Nurse ED Recommendation

	(1)	(2)	(3)	(4)
Leave-out Call Duration (in 10 mins)	0.078		0.006	0.006
	(0.024)		(0.002)	(0.002)
Leave-out Nurse ED Recommendation		0.959	0.953	0.953
		(0.013)	(0.014)	(0.014)
Leave-out Nurse ED Recommendation × Leave-out Call Duration (in 10 mins)				-0.032
				(0.034)
Observations	319,830	319,830	319,830	319,830
Dependent Variable Mean	0.290	0.290	0.290	0.290
FE: CC-by-YM	X	X	X	X

Note: This table reports first-stage estimates of the effect of nurse triage practice measures. Leave-one-out averages of nurse ED recommendation and call duration are constructed as described in Method section. The nurse ED recommendation measure is in probability (1 unit = 100 percentage points). The call duration measure is scaled (1 unit = 10 minutes). All specifications include call-center-by-call-time fixed effects. Standard errors are clustered at the call center level.

Table A2.3: (Cont.) First-Stage Effects of Leave-Out Nurse Triage Measures

(b) LHS: Call Duration

	(1)	(2)	(3)	(4)
Leave-out Call Duration (in 10 mins)	10.233		10.228	10.227
	(0.051)		(0.054)	(0.055)
Leave-out Nurse ED Recommendation		10.564	0.067	0.068
		(3.484)	(0.161)	(0.159)
Leave-out Nurse ED Recommendation × Leave-out Call Duration (in 10 mins)				0.160
				(0.674)
Observations	319,830	319,830	319,830	319,830
Dependent Variable Mean	10.645	10.645	10.645	10.645
FE: CC-by-YM	X	X	X	X

Note: This table reports first-stage estimates of the effect of nurse triage practice measures. Leave-one-out averages of nurse ED recommendation and call duration are constructed as described in Method section. The nurse ED recommendation measure is in probability (1 unit = 100 percentage points). The call duration measure is scaled (1 unit = 10 minutes). All specifications include call-center-by-call-time fixed effects. Standard errors are clustered at the call center level.

Table A2.3: (Cont.) First-Stage Effects of Leave-Out Nurse Triage Measures

(c) LHS: Call Duration × Nurse ED Recommendation

	(1)	(2)	(3)	(4)
Leave-out Call Duration (in 10 mins)	3.703		3.035	3.015
	(0.266)		(0.215)	(0.218)
Leave-out Nurse ED Recommendation		11.963	8.848	8.909
		(1.385)	(0.951)	(0.906)
Leave-out Nurse ED Recommendation ×				8.244
Leave-out Call Duration (in 10 mins)				(2.426)
Observations	319,830	319,830	319,830	319,830
Dependent Variable Mean	3.055	3.055	3.055	3.055
FE: CC-by-YM	X	X	X	X

Note: This table reports first-stage estimates of the effect of nurse triage practice measures. Leave-one-out averages of nurse ED recommendation and call duration are constructed as described in Method section. The nurse ED recommendation measure is in probability (1 unit = 100 percentage points). The call duration measure is scaled (1 unit = 10 minutes). All specifications include call-center-by-call-time fixed effects. Standard errors are clustered at the call center level.

Chapter 3

Is the Goat-Year Birth Really Ominous?

§ 3.1 Introduction

The marriage market in China has recently drawn the interest of many researchers across fields as the sex ratio imbalance has intensified marriage market competition over partner search. Previous studies have examined the impact of China's surplus of males and the intensified partner search competition on marital and non-marital outcomes (Mu and Xie, 2014; Wei and Zhang, 2011; Nie, 2020). Using the 2000 Census data, a recent demographic research projects that the male-to-female ratio of potential first-marriage partners will increase from 1.5 to 1.8 between 2020 and 2030 (Jiang et al., 2014). Given the likely grave consequences of these phenomena, it is imperative to understand the origins of the sex ratio imbalance from many angles.

While many studies have attributed China's sex ratio imbalance to the One-Child Policy (OCP) under widely prevailing families' preferences for having a son, the driving forces of China's demographic and fertility trends can be multidimensional. Interaction between the Communist ideology, economic growth, and traditional family values have created a unique marriage pattern in China since the Communist Revolution in 1949. Previous studies have noted that, while the 1950 Marriage Law stipulated free-choice marriages and equal rights between husbands and wives, families have kept a traditional patriarchal and patrilineal family system until recently, resulting in sex-selective fertility decisions during the OCP period (Huang and Zhou, 2015; Mu and Xie, 2014). On the other hand, post-reform economic development since 1978 has contributed to narrowing gender gaps in educational attainment and labor market outcomes, potentially empowering women in marriage and intra-family decision making. To predict future fertility trajectory in post-OCP China, understanding the influence of changing gender and family norm on people's behaviors is pressing.

This study examines gender discrimination based on the Chinese zodiac, one overlooked cultural factor that potentially affects Chinese families' fertility and marriage decisions. East Asian societies have a tradition of expressing a year using one of the twelve animals (rat, ox, tiger, rabbit, Dragon, Snake, Horse, Goat, monkey, rooster, dog, and pig). From the ancient past, people have developed superstitious beliefs attached to the zodiac signs. Some zodiac signs are associated with bad luck. People in China, South Korea, and Japan had a similar cultural superstition: Women born in the year of Goat (China), Horse (South Korea) and Fire-horse (Japan) bring misfortune to their husbands. Anecdotal evidence suggests that women born in the year of Goat (or Sheep synonymously referring to the same animal sign) face discrimination in the marriage market in China. A news article reports Chinese parents' grief that their daughter's marriage was repeatedly turned down for her zodiac sign despite her diploma from a prestigious school and resident status (hukou) in Beijing[1]. While few studies have inves-

[1]China Daily, July 14, 2017, "'Chain of scorn' in Chinese-style blind date" https://www.chinadaily.com.cn/china/2017-07/14/content_30116525.htm (accessed November

tigated the gender discrimination against Chinese women of the Goat zodiac sign, there is scholarly evidence that the zodiac-based gender discrimination influenced marital and familial decisions in Japan, which underscores the importance of the zodiac superstition's behavioral impact. Rohlfs et al. (2010) analyze how Japanese parents responded to the most recent Fire-Horse years (1846, 1906, and 1966) in the course of Japan's economic and social development over the period. Calculating male and female cohort size and male-to-female ratio for the three Fire-Horse years and surrounding years, they find that the male-to-female ratios for the 1846 and 1906 cohorts are considerably higher than that for surrounding cohorts, suggesting the incidence of sex-selective infanticide. On the other hand, cohort size in 1966 significantly dropped by 21% to 24% for both males and females, indicating couples' sex-blind child avoidance through abstinence, contraception, or abortion (Rohlfs et al., 2010). Although the investigation into the fate of the 1966 Fire-horse women over lifecycle is nascent, several studies have compared later outcomes of the 1966 Fire-horse women to those of the surrounding cohorts (Akabayashi, 2008; Yamada, 2013; Shimizutani and Yamada, 2014). Shimizutani and Yamada (2014) find that the 1966 Fire-horse women are more likely to be divorced, have lower educational attainment, and earn lower own and household income in middle age than women in the surrounding cohorts.

Following the spirits of the preceding papers about Japan's Fire-horse superstition, this paper investigates the effects of the gender-discriminatory zodiac superstition on various socioeconomic outcomes of women born in the year of Goat. Using a 1% sample of the 2000 Chinese Population Census, this study investigates the causal effects of the Goat-year superstition on marriage outcomes using econometric methods. While

2, 2021)

previous papers on the zodiac effects compare individuals born in a particular year to those born in the surrounding years, this comparison raises two concerns. First, including birth cohorts far away from the target zodiac year reduces comparability. Second, comparing birth cohorts born just before the target zodiac year to those born just after the beginning of the zodiac year is susceptible to confounding by seasonality. This paper applies a Difference-in-Differences (DID) method to two adjacent new years to address these issues. The 1979 Goat-year birth effect is estimated by subtracting the difference in marriage rates between those born at the beginning of 1978 and the end of 1977 from the difference between those born at the beginning of 1979 and the end of 1978. This method is robust to the cyclical seasonality under the assumption that seasonal trends around the two new-year thresholds are similar. The DID estimates of the Goat-year effect on marriage are close to zero and statistically insignificant. Moreover, this study graphically presents the evolution of key demographic variables across birth cohorts, such as cohort size, male-to-female ratios, marriage, educational attainment, and labor market outcomes. While these outcomes exhibit seasonality within years and respond to natural and political events, such as the Great Chinese Famine and the OCP, these plots do not show any clear descriptive evidence of the Goat-year effect.

This paper contributes to the vibrant literature on the demographic trend and marriage market in China. Determinants of marriage and fertility trends can be multidimensional. While the OCP came to an end in 2015, China's total fertility rates have hovered around 1.6 in the late 2010s, far below the replacement-level fertility (The World Bank) [2] . If couples prioritize child's quality over quantity as the economy grows, the lower fertility rate will persist in the future, foreshadowing grave socioeconomic consequences in

[2] The World Bank, "Fertility rate, total (births per woman) - China" https://data.worldbank.org/indicator/SP.DYN.TFRT.IN?locations=CN (accessed November 6, 2021).

many fields. This study's finding of the no zodiac superstition effects on fertility and marriage might be a blessing for policymakers in the society suffering from population aging. At the same time, this finding raises a new question: why the similar zodiac superstition affects people's actual behaviors in one country while it does not materialize in other societies.

This study adds a new finding to the burgeoning literature of the Chinese zodiac. Among the traditional cultures worldwide, the Chinese zodiac is one of the most influential cultural institutions, affecting the social and economic lives of vast East Asian populations and their diasporas. Besides Japan's Fire-horse superstition, many demographic and economic studies have examined the impact of the year of Dragon, which is traditionally associated with auspiciousness and success. The literature has reported a surge in the number of births in the 1988 and 2000 years of Dragon in Hong Kong, Taiwan, Singapore, Malaysia, and the United States (Vere, 2008; Goodkind, 1991; Wong and Yung, 2005; Johnson and Nye, 2011; Sim, 2015). While most previous studies have focused on Chinese diasporas, Mocan and Yu (2020) report the 2000 and 2012 Dragon-year effects on the number of marriages, the number of births, and Dragon-year children's educational outcomes in mainland China. The finding of this paper contributes to the ongoing scholarly debates on the roles of the zodiac in the lives of the mainland Chinese population in the late 20th century, during which the country underwent dramatic social and economic changes.

This study's finding of the absence of the Goat-year superstition effects has a crucial methodological implication in quantitative studies using econometric research designs. Recent economic literature has utilized cultural beliefs as a source of an exogenous shock to the primary independent variable of interest to estimate the causal effects on outcomes.

For instance, Becker and Woessmann (2009) exploit the Protestant Reformation as an exogenous shock to literacy, a proxy for human capital, and estimate returns to human capital using a distance to Luther's city of Wittenberg as an instrumental variable. Similary, Lee (2005) and Zhang and Zhang (2015) use the zodiac discrimination against the Horse- and Goat-year women as an instrument for marriage and estimate the marriage effect on women's labor force participation in South Korea and China. This study claims that Zhang and Zhang (2015)'s first-stage effect of the Goat-year birth on women's marriage is likely due to specification errors rather than actual superstition effects. This finding highlights the challenge of using a subtle cultural institution as an instrument and alerts researchers who use culture as part of research designs.

More broadly, this paper contributes to the emerging literature about the impact of cultural beliefs on human behaviors. Recent studies investigate a broad range of cultural phenomena, such as systematic increases and decreases in the number of births in China according to auspicious and inauspicious days in traditional Chinese astrology (Huang et al., 2021), similar birth patterns among Chinese Americans in California (Almond et al., 2015), price premium and discount for houses with lucky and unlucky address numbers in Canadian neighborhoods with a large immigrant population (Fortin et al., 2014), and the influence of witchcraft beliefs on various elements of social capital in Sub-Saharan Africa (Gershman, 2016).

The rest of the paper proceeds as follows. Section 2 translates the Goat-year superstition into a simple partner search framework and derives econometric models. Section 3 describes the sample construction from the 2000 Chinese Population Census. Section 4 presents the estimated Goat-Year effects on marriage and other socioeconomic outcomes. Lastly, section 5 discusses implications of our findings for studies using culture as research designs and concludes our study.

§ 3.2 Conceptual Framework and Empirical Models

We set up a simple conceptual framework to translate the zodiac superstition into empirical models. Consider a partner search in the marriage market where male participants care about female participants' zodiac sign along with other characteristics. Female participants with the Goat sign are less preferred to those with other zodiac signs because of the discriminatory zodiac superstition. As we present in Figure 3.1, women born in goat years have to spend more monetary and non-monetary costs to find a marriage partner than otherwise comparable women born in other zodiac years. Due to the higher search costs, goat-year women would have a lower probability of being married. Formally,

$$\Pr\left(Married_i \mid X_i, \epsilon_i, Goat_i = 1\right) < \Pr\left(Married_i \mid X_i, \epsilon_i, Goat_i = 0\right) \tag{3.1}$$

where $Married_i$ is an indicator for marriage, X_i and ϵ_i are observable and unobservable determinants of marriage, and $Goat_i$ is an indicator of goat-year birth.

An ideal statistical test for Eq (3.1) would randomly assign the Goat (treatment) sign and another zodiac (control) sign to female participants in the marriage market and compare the marriage rates between the two groups. Some previous studies about partner search implement online field experiments to elicit preferences over partners: Experimenters create fictitious profiles of potential partners with various personal traits on matching platforms and compare the number of "visits" across different profiles[3].

[3] For instance, Ong, Yang, and Zhang (2020) elicit participants' preferences over partners' income and educational attainment using an online experiment in investigating the reported difficulties of elite women in finding a partner ("leftover women" phenomenon) in China.

However, artificially designed experiments have limitations in reproducing the complex decision-making process in the actual marriage market. Hence, we propose and estimate econometric models that leverage quasi-random variation in the zodiac sign, exploiting the access to micro-level census data.

First, we consider a regression discontinuity (RD) model using age in months as a running variable. This RD model compares the 1979 Goat-year women with the women born in 1978 (Horse) or 1980 (Monkey), assuming that these women in the adjacent cohorts are comparable except for the zodiac signs. This research design shares a similar identification assumption as previous studies about the Fire-Horse superstition in Japan: Yamada (2013) and Shimizutani and Yamada (2014) nonparametrically compare the marriage rates of Japanese women in Fire-Horse and the adjacent cohorts. In addition to the simple comparison of the marriage rates across cohorts, we estimate a linear RD specification to control for the age difference between cohorts using a flexible function of age:

$$Married_i = \theta_0 + \theta_1 Goat_i + f\left(age_i\right) + X_i'\Gamma + \varepsilon_i \tag{3.2}$$

where $Goat_i$ equals one if a woman i is born in the 1979 goat year and zero otherwise. $f\left(age_i\right)$ is a polynomial function of age whose terms fully interact with the goat dummy. Assuming that whether an individual i is born in the goat year or adjacent year is as good as randomly determined, the coefficient of the goat-year dummy captures the causal effects of the zodiac superstition on marriage.

While the RD method increases comparability of women by restricting the sample to the Goat and surrounding cohorts, a potential threat to this method is the systematic relationship between the timing of birth and later outcomes. Recent studies show that season of birth and later outcomes have significant correlations by parental selection in the US (Buckles et al., 2021). Suppose parents who give birth at the beginning of a year are systematically different from those who give birth at the end of a year. For instance, such a difference may arise from differential peak seasons across occupations. In that case, their children may experience different childhood environments, resulting in different marriage outcomes. Unobserved differences in childhood environments violate the local randomness assumption of the RD approach.

We deal with this potential shortcoming of the RD strategy due to the relationship between season of birth and marriage outcome by combining the RD and difference-in-differences (DID) method. The RD estimates are biased if the women born in the Goat- and surrounding years have unobservable characteristics (e.g., childhood environments) that affect marriage outcomes. To control for the unobserved differences across cohorts, we assume that the difference in marriage rates due to the difference in the timing of birth (net of the goat-year effect) is the same across adjacent cohorts. This assumption excludes the possibility that the difference in childhood environments between December-(the end of a year) and March-(the beginning of a year) born women changes across years. It also rules out the possibility that the one-year age difference across adjacent cohorts causes any difference in the timing-of-birth effect on marriage. Under the assumption, the difference in marriage rates between women born before and after the 1977-1978 (Snake-Horse) new-year cutoff would provide a counterfactual for the difference at the 1978-1979 (Horse-Goat) threshold in the absence of the goat-year superstition. Constructing two overlapping pairs of adjacent two years (1977-1978 and

1978-1979), we estimate the following DID model:

(3.3)

$$Married_i = \psi_0 + \psi_1 Pair78\_79_i + \psi_2 NewYear_i + \psi_3 Pair78\_79_i \cdot NewYear_i + X_i'\delta + \nu_i$$

where $Pair78\_79_i$ equals one if a unit i is in the 1978-1979 pair and equals zero if i is in the 1977-1978 pair. $NewYear_i$ is an indicator that equals one if a unit i's birth year is the second year within each pair (1978 for the 1977-1978 pair and 1979 for the 1978-1979 pair). The covariates X_i include a polynomial function of age and dummy variables for ethnicity, province of residence, and educational attainment.

In Eq (3.3), the Goat-year superstition effects are captured by the coefficient on the interaction of the two dummy variables. The 1978-1979 pair dummy absorbs the average difference in marriage rates between the 1978-1979 and the 1977-1978 pairs. The new-year indicator captures the difference between December-(the end of a year) and March-(the beginning of a year) born women common to both the 1978-1979 and the 1977-1978 pairs. The interaction term captures any effects specific to the Goat-year women in the 1978-1979 pair. This coefficient is essentially equivalent to the difference between the two RD coefficients, which we would obtain by running the RD model in Eq (3.2) separately for the 1977-1978 and 1978-1979 pairs.

§ 3.3 Data

We obtain a 1% sample of the Chinese Population Census from IPUMS International (2020). As of January 2022, the Census rounds of 1982, 1990, and 2000 are available for academic purposes. This study constructs the analysis sample from the 2000 Census to conduct the econometric analysis proposed in the previous section. The 2000 Census contains critical demographic information, including year and month of birth, sex, marital status, ethnicity, educational attainment, employment status, and location of residence at the prefecture-level.

Our regression discontinuity (RD) and difference-in-differences methods compare the women born in 1978 (Horse), 1979 (Goat), and 1980 (Monkey). To this end, we first restrict the sample to these three female cohorts. They were at ages 20-22 when the National Bureau of Statistics of the People's Republic of China collected the Census survey from November 1-10, 2000. Second, we further limit the sample to women whose marital status is either "single" or "first-marriage" to avoid complexity arising from potentially different mechanisms between first marriage and remarriage[4].

While the Census collects birth month in the solar calendar, the zodiac sign in China revolves according to the lunar calendar. Hence, we convert the lunar calendar into the solar calendar and assign a corresponding zodiac sign to each birth cohort at the solar month level[5]. The Horse sign is attached to individuals born in February 1978-January 1979. The Goat sign is attached to February 1979-January 1980, and the Monkey sign to February 1980-January 1981. As we do not have date-level birth information, some individuals born in January and February are misclassified into the wrong zodiac cohort. In order to avoid our results being affected by this inconsistency, our regression analysis excludes individuals born in January and February[6]. We present analysis results with

[4]The marital status in the Census has five categories (single, first marriage, remarried, divorced, and widowed). Among the three analysis cohorts, the shares of remarried, divorced, and widowed women are 0.14%, 0.17%, and 0.03%, respectively.

[5]The exact dates for each zodiac year are the following: February 7, 1978 to January 27, 1979 (Horse), January 28, 1979 to February 15, 1980 (Goat), and February 16, 1980 to February 4, 1981 (Monkey).

[6]Previous zodiac studies (e.g., Zhang and Zhang, 2015) similarly exclude individuals born in January and February to avoid misclassification.

January and February in Appendix.

Table 3.1 presents summary statistics of marriage and other variables of the women in the final sample. Rural women have higher marriage and labor force participation rates[7] than urban counterparts. On the other hand, urban women tend to have higher educational attainment than rural counterparts. Given the large underlying differences in these socioeconomic characteristics, we separately analyze the rural and urban samples in the following sections.

§ 3.4 Results

3.4.1 The Goat-Year Effects on Marriage Outcomes

Figures 3.2 and 3.3 show marriage rates of rural women born in 1978 (Horse), 1979 (Goat), and 1980 (Horse) as of November 2000. We superimpose a quadratic fit on each plot. The 1978-1979 (Horse-Goat) comparison exhibits a dip in the marriage rate for January-February cohorts compared to the surrounding birth-month cohorts. The marriage profile appears to be continuous at the new-year threshold in the 1979-1980 plot. Figures 3.4 and 3.5 presents the same marriage profiles for urban women. For urban women, the new-year dip in the marriage rates is visible for both the 1978-1979 and 1979-1980 pairs. Overall, the RD estimates from Eq (3.2) confirm the new-year dip in the graphical analysis, while the magnitude and statistical significance are somewhat

[7] We consider a person employed if her work status ("worked for income last week") is either "yes" or "no, due to training, vacation, or seasonal holidays." The Census asks activity status for persons whose work status is "no, for other reasons." We consider a person unemployed if her activity status is either "had never worked before and is looking for a job" or "lost last job and is looking for a job." We consider the remaining individuals (in school, keeping household, retired, or disabled) as not in the labor force.

sensitive to the degree of age polynomial. Tables 3.2 and 3.3 show that the estimated dip at the 1978-1979 new year ranges -2.5 to -6.8 and -4.8 to -8.2 percentage points for rural and urban women, respectively (columns (1), (3), and (5)). By contrast, the estimated discontinuity at the 1979-1980 new year has different signs across age specifications and is statistically insignificant (columns (2), (4), and (6)).

Although the RD estimates indicate a decline in the marriage rate at the beginning of the 1979 Goat year, we expect these negative estimates to be affected by the season-of-birth effects. Figure 3.6 presents the marriage-cohort profile across the 1970-1983 birth cohorts. The marriage rate discontinuously drops at multiple new-year thresholds. Hence, the negative RD estimates at the 1978-1979 new year likely include the cyclical seasonality effects.

Under the DID assumption explained in the previous section, DID estimates net out the seasonality effects by subtracting the marriage gap at the 1977-1978 threshold from the gap at the 1978-1979 threshold. Figures 3.7 and 3.8 stack the marriage-cohort profiles for pairs of two adjacent years. A quadratic function of age fits each profile of March-December birth-month cohorts. We excluded January and February birth-month cohorts as we cannot determine their zodiac sign by birth information at the month level. Overall, March-December marriage-age profiles are parallel, indicating season-of-birth effects on marriage are roughly stationary across pairs of two adjacent years. The observed parallel patterns indirectly support our assumption for a DID method: In the absence of the zodiac superstition, the marriage gap at the 1978-1979 (Horse-Goat) threshold would have been the same as the marriage gap at the 1977-1978 (Snake-Horse) threshold.

Table 3.4 presents the DID estimates for rural and urban women from Eq (3.3). The estimate is nearly zero and statistically insignificant across specifications (row 1). The null effect indicates that, contrary to the zodiac discrimination hypothesis, after the model accounts for the cyclical marriage discontinuity at the new-year threshold, there is no extra effect on marriage for those born at the beginning of the 1979 goat year. The estimates for other dummy variable terms match the observed patterns in the marriage-age profiles. The marriage rate of the 1978-1979 cohort pair is approximately 4.4-5.3 percentage points lower than that for the 1977-1978 cohort pair, reflecting the one-year age difference. While the estimated size of cyclical discontinuity at the threshold varies across specifications, those born just after the new year in 1978 are 3.3-11.7 percentage points less likely to marry than those born at the end of 1977.

3.4.2 The Goat-Year Effects on Other Outcomes

The DID results show no statistical evidence of the Goat-year discrimination effects on marriage rates of the 1979 Goat-year women. However, the zodiac superstition may affect other socioeconomic outcomes. Demographic and economic studies have documented a sharp decline in the number of births in the 1966 Fire-Horse year in Japan, attributing parents' sex-blind child avoidance to the similar discriminatory beliefs attached to Fire-Horse women (Kaku and Matsumoto, 1975). Analyzing the Census records from Japan, Rohlfs et al. (2010) show a skewed sex ratio of the 1846 birth cohort born in the earlier round of the Fire-Horse year. They attribute the increased male-to-female ratio of the 1846 cohort to sex-selective infanticide in the year before modern contraceptives became available. For socioeconomic outcomes, Shimizutani and Yamada (2014) report that the 1966 Fire-Horse women have lower educational attainment and income than women in the surrounding cohorts. Given these episodes of the neighboring country, this study investigates if we can find any statistical evidence of the zodiac influences in socioeconomic outcomes of the 1979 Goat-year cohort.

Figures 3.9-3.12 show the histogram of yearly and monthly birth cohorts (population pyramids) based on the 2000 Population Census. The yearly birth cohort bin is the share of one yearly cohort to the total population, whereas the monthly cohort bin is the fraction of one monthly cohort to the total population born in 1978 (Horse), 1979 (Goat), and 1980 (Monkey). While the population shares drastically shrink for cohorts born during the Great Famine (around age 40 in 2000) and for cohorts born after the implementation of the One-Child Policy (around age 20 in 2000), there is no comparable decline for the 1979 men and women. The monthly birth cohort share exhibits seasonal fluctuations, but the 1979 Goat-year cohorts show neither noticeable declines in shares nor irregular seasonal patterns compared to the 1978 and 1980 birth cohorts.

Similarly, we plot the male-to-female ratio across yearly and monthly birth cohorts in Figures 3.13-3.16. As numerous other papers point out (e.g., Jiang et al., 2014), we can notice the dramatic increase in the male-to-female ratio since the 1980s. However, the male-to-female ratio for the 1979 Goat-year cohort does not show any noticeable dip compared to the surrounding cohorts. The male-to-female ratio across monthly birth cohorts exhibits no irregular drop, either. The ratio hovers around one throughout the late 1970s, including the 1979 Goat year.

Although Figures 3.9-3.16 do not show any evidence for sex-blind and sex-selective child avoidance, parents may invest less in education for their Goat-year daughters if they expect returns to be lower due to the zodiac discrimination. In order to investigate the Goat-year discrimination effects on human capital formation, we similarly plot education and labor market profiles across birth cohorts.

Figures 3.17-3.20 plot the educational attainment across the 1970-1983 birth cohorts. We break down the sample into four mutually exclusive educational attainment levels (primary or less, middle school, high school, and higher education) and compute the share of each education level to the cohort size. The educational attainment profiles exhibit distinct seasonal patterns: Women born in the 4th quarter tend to achieve higher educational attainment than those born in the 1st-3rd quarters. The 1979 Goat-year women follow the same patterns as the other cohorts. Lastly, Figures 3.21-3.24 show the labor force participation rate, unemployment rate, and employment-to-population ratio across the 1970-1983 birth cohorts. These labor market indicators do not show any discontinuous change for the 1979 Goat-year cohort, either. Overall, these graphical analyses do not show evidence that parents made differential human capital investment decisions between the Goat and non-Goat cohorts.

§ 3.5 Discussion and Conclusion

This paper investigated whether Chinese women born in the 1979 Goat year experienced marriage-market discrimination due to the superstitious beliefs stigmatizing them as a source of misfortune to the husband. While the RD estimates show a discontinuous drop in the marriage rate at the new year of the 1978-1979 year transition, we argued that this drop is likely due to the systematic differences in parental and childhood background between the end of the year and the beginning of the year birth cohorts. Once we account for the season-of-birth effects using the DID strategy, we find no statistical evidence of the Goat-year superstition effects for the 1979 Goat-year women. While investigating the mechanism behind the absence of the Goat-year effects is beyond the scope of our analysis, one possibility is the ban on superstitious activities during the 1960s-1970s. Mocan and Yu (2020) note that the Chinese government banned any activity connected with superstition during the Cultural Revolution (1966-1976). Having experienced the Cultural Revolution, families of the 1979 Goat-year women might expect the superstition to be no obstacle in the marriage market. As we explained in the previous section, we do not find any statistical evidence of the 1979 Goat year effects on the cohort size and the male-to-female ratio. The absence of parental reaction to the unlucky year in China contrasts with Janapese parents' sex-selective and sex-blind child avoidance behaviors in the 1846, 1906, and 1966 Fire-Horse years that Rohlfs et al. (2010) report.

The null effects of the zodiac superstition also speak to research designs that use culture and institution as a source of exogenous variation. Zhang and Zhang (2015) use the 1979 Goat-year birth as an instrumental variable for marriage in analyzing the marriage effects on Chinese women's labor force participation. While they show sizable first-stage zodiac effects on marriage, we believe that their first-stage estimate is biased due to an inappropriate polynomial age specification. Zhang and Zhang (2015) regress the marriage dummy on the Goat-year dummy, age, age-squared, and other covariates using the cohorts born between 1970 and 1983. To show the specification problem, we present a marriage-cohort profile for the 14 cohorts and superimpose a quadratic fit for the profile in Figure 3.25. The quadratic age function does not trace the S-shaped profile well, generating a wide gap between the predicted and actual marriage rates. Given that there is only one Goat year (1979) in this sample, the first-stage regression translates this gap into a negative coefficient estimate on the Goat-year dummy variable, which cannot be interpretable as the superstition effects. Table 3.5 presents the Goat-year effect estimates from Zhang and Zhang (2015)'s first-stage specification. While the estimates using all 1970-1983 cohorts (columns (1) and (6)) are large in magnitude and statistically significant, the estimates shrink toward zero as we exclude cohorts far away from the 1979 Goat-year cohort. This contradictory finding highlights the importance of careful checks for confounding factors when researchers use a subtle cultural institution as a research design.

This paper leaves several issues for future research. First, the absence of the Goat-year effects is sharply contrasting to the influence of male preferences in the skewed male-to-female ratio. Understanding why one cultural institution affects family fertility behaviors while others do not is important in making demographic policies. Second, while we found the systematic relationship between the timing of birth and later outcomes (marriage and education), to our knowledge, less is known about what causes these seasonal patterns in China. Given that many research designs rest on variation in the timing of birth, demographic and economic researchers need more knowledge on the driving force of the seasonality.

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§ 3.7 Tables and Figures

Figure 3.1: Conceptual Model of Partner Search with Zodiac Discrimination

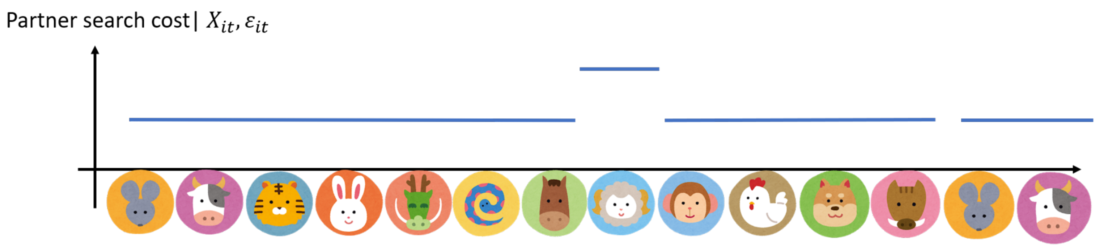

Figure 3.2: Marriage-Cohort Profile for Rural Women (Horse-Goat)

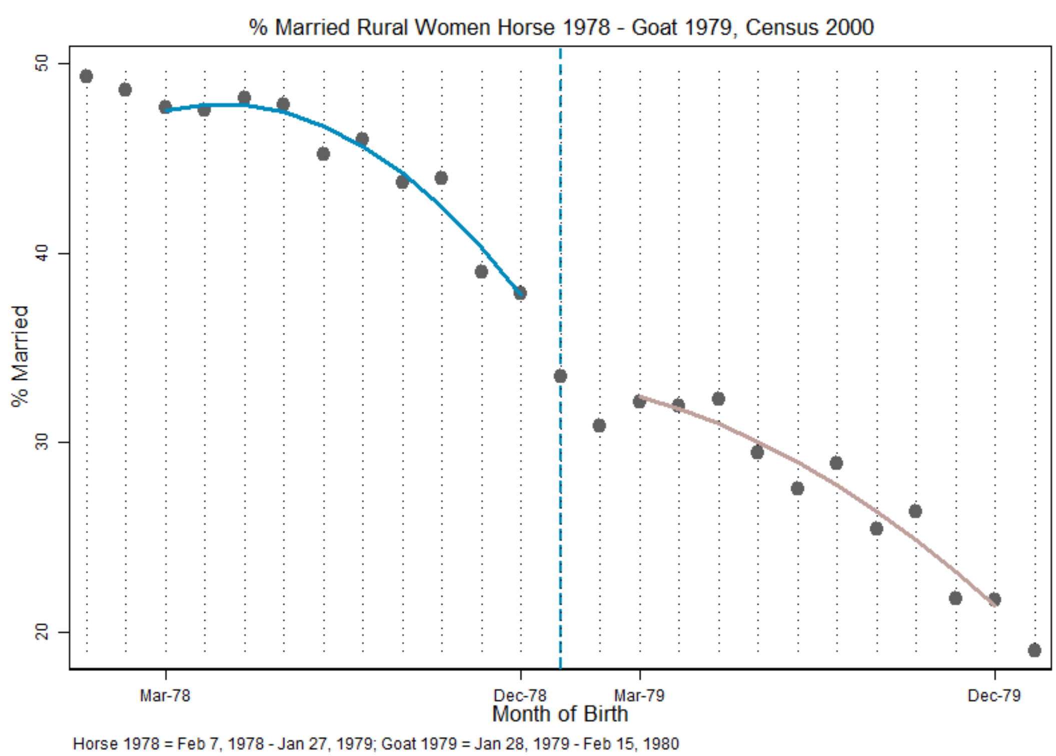

Figure 3.3: Marriage-Cohort Profile for Rural Women (Goat-Monkey)

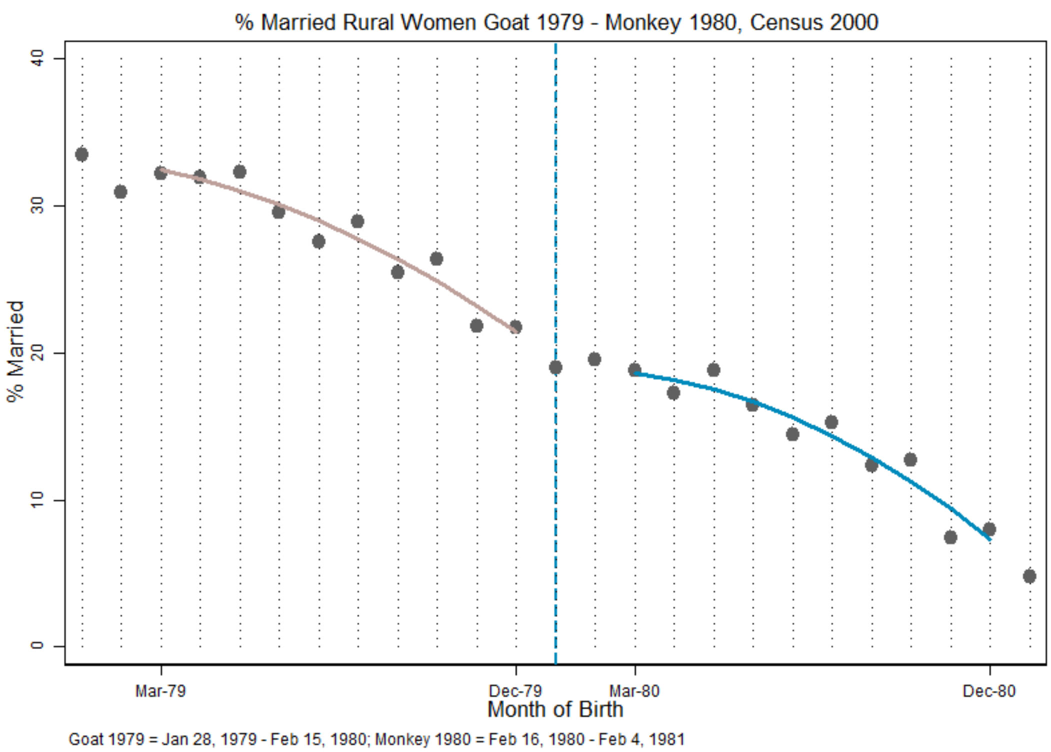

Figure 3.4: Marriage-Cohort Profile for Urban Women (Horse-Goat)

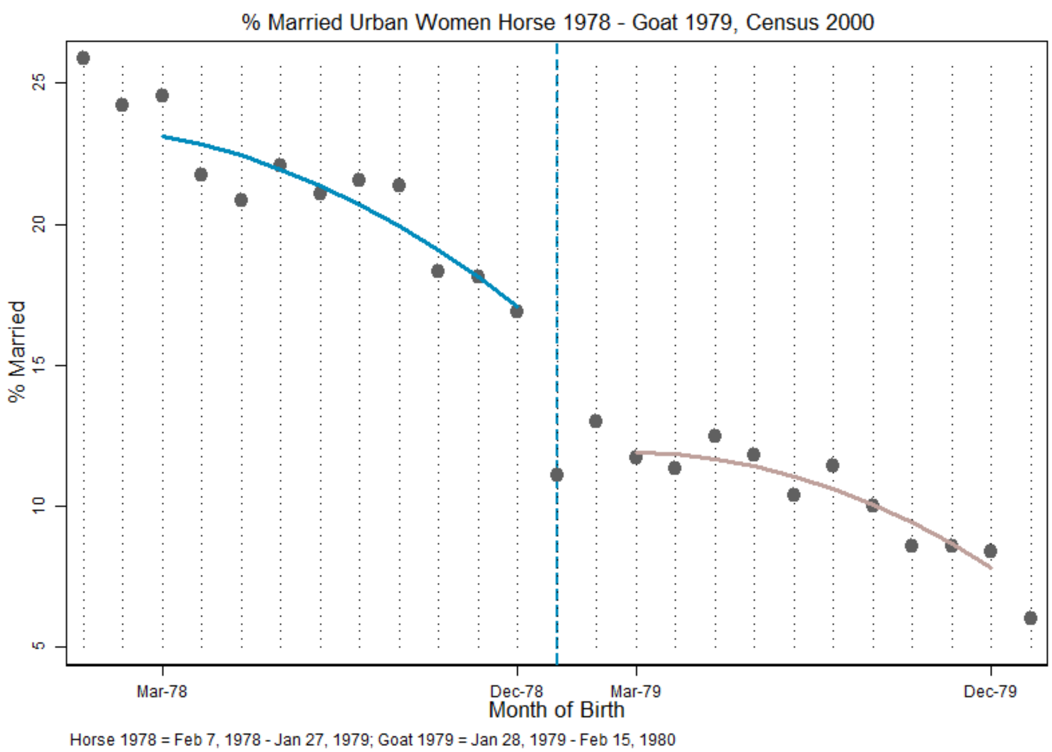

Figure 3.5: Marriage-Cohort Profile for Urban Women (Goat-Monkey)

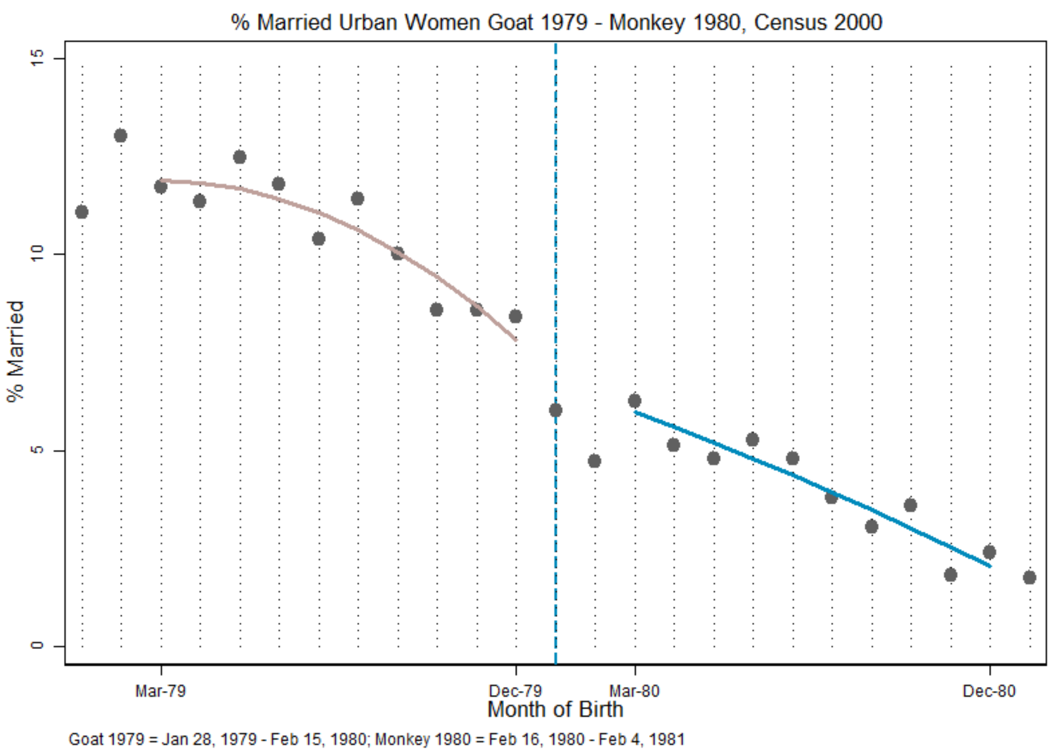

Figure 3.6: Marriage-Cohort Profiles for Rural Women 1970-1983

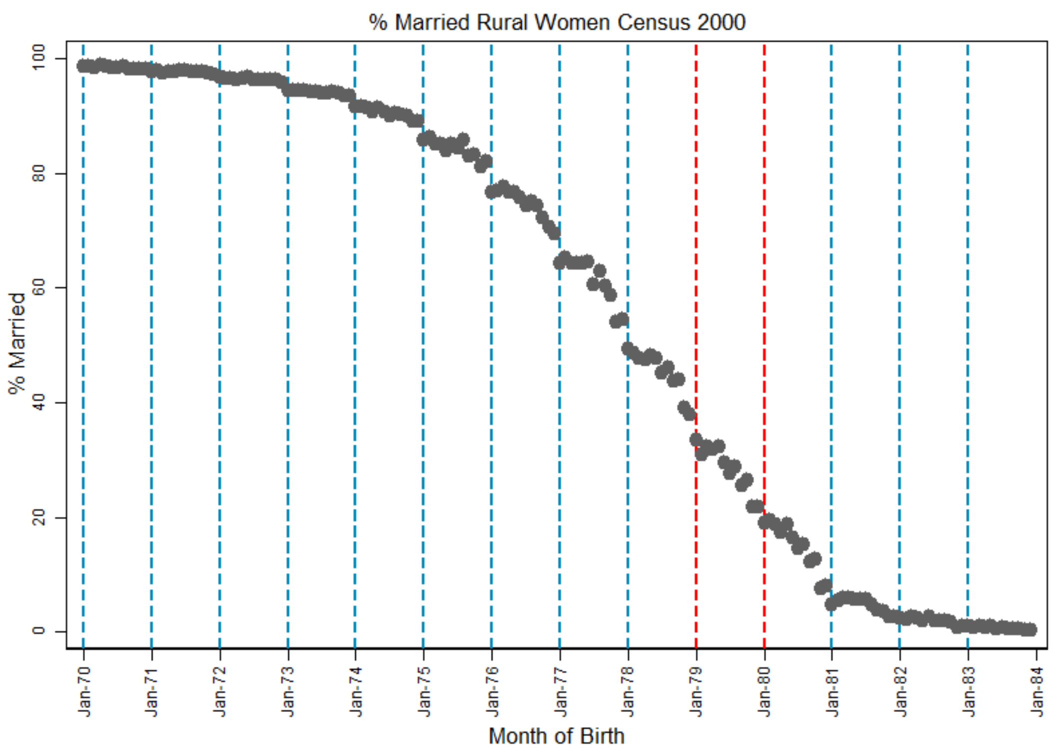

Figure 3.7: Stacked Marriage-Cohort Profiles for Rural Women 1976-1982

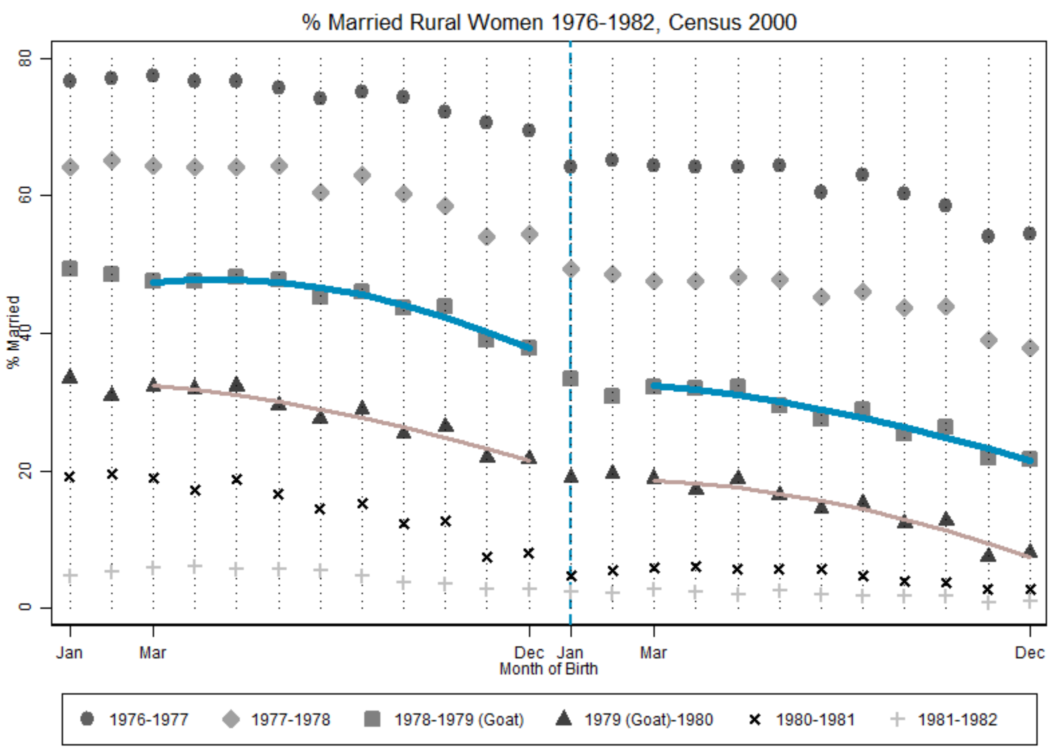

Figure 3.8: Stacked Marriage-Cohort Profiles for Urban Women 1976-1982

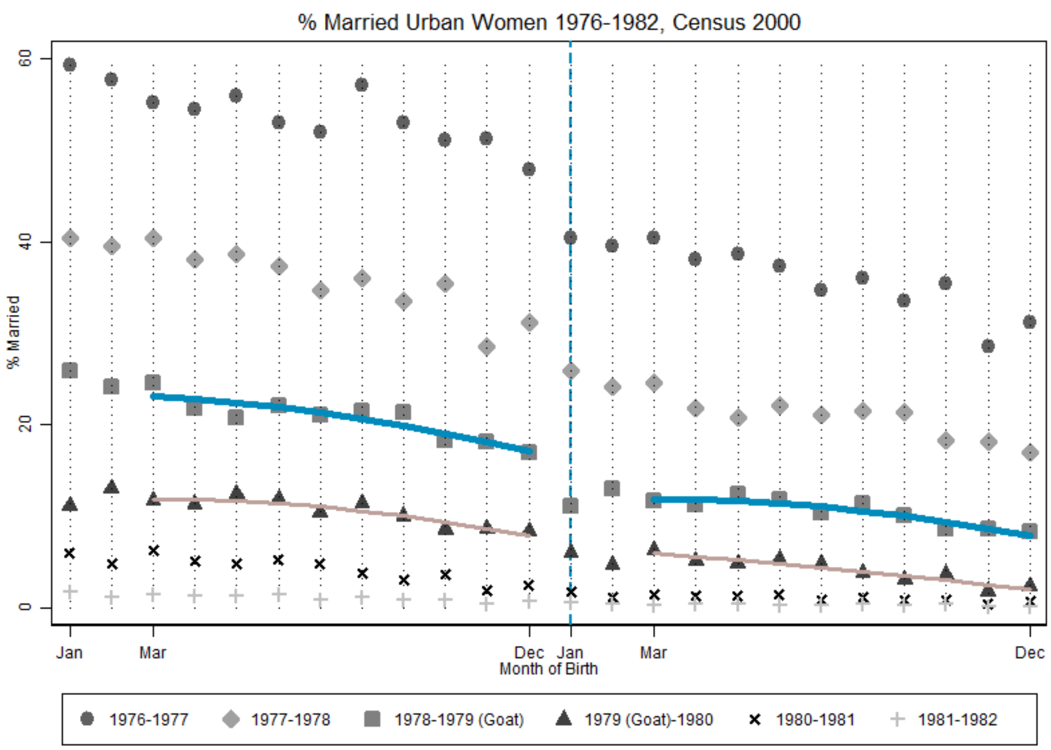

Figure 3.9: Population Pyramid for Rural Women

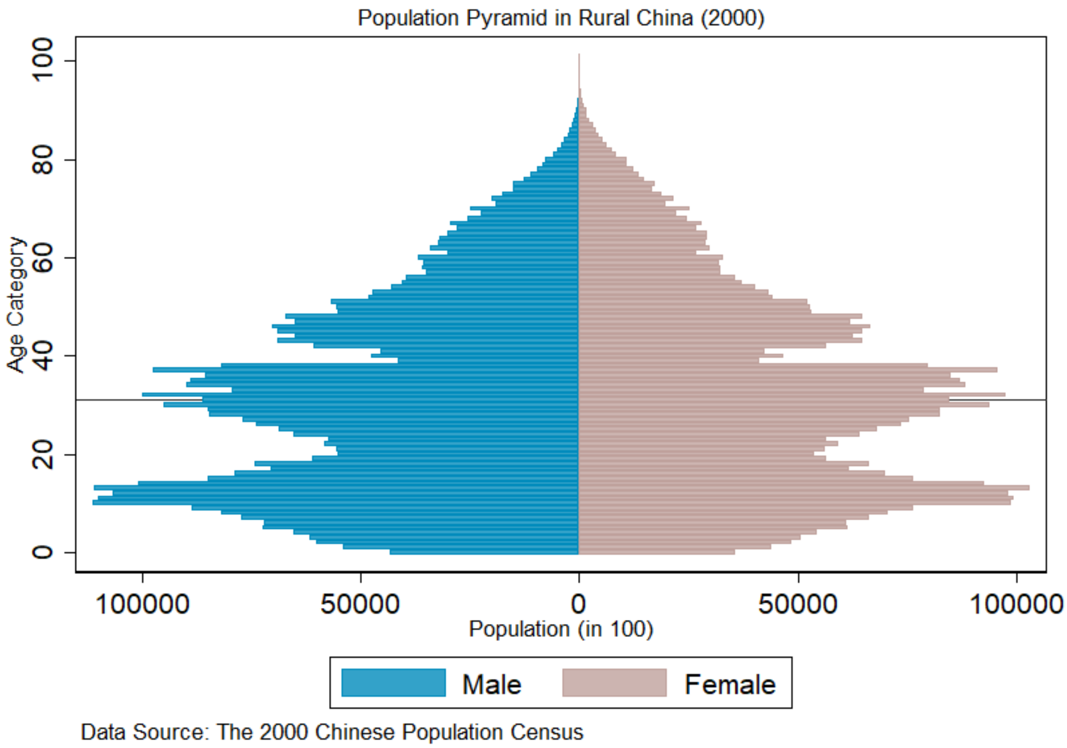

Figure 3.10: Population Pyramid for Rural Women around the 1979 Goat Year

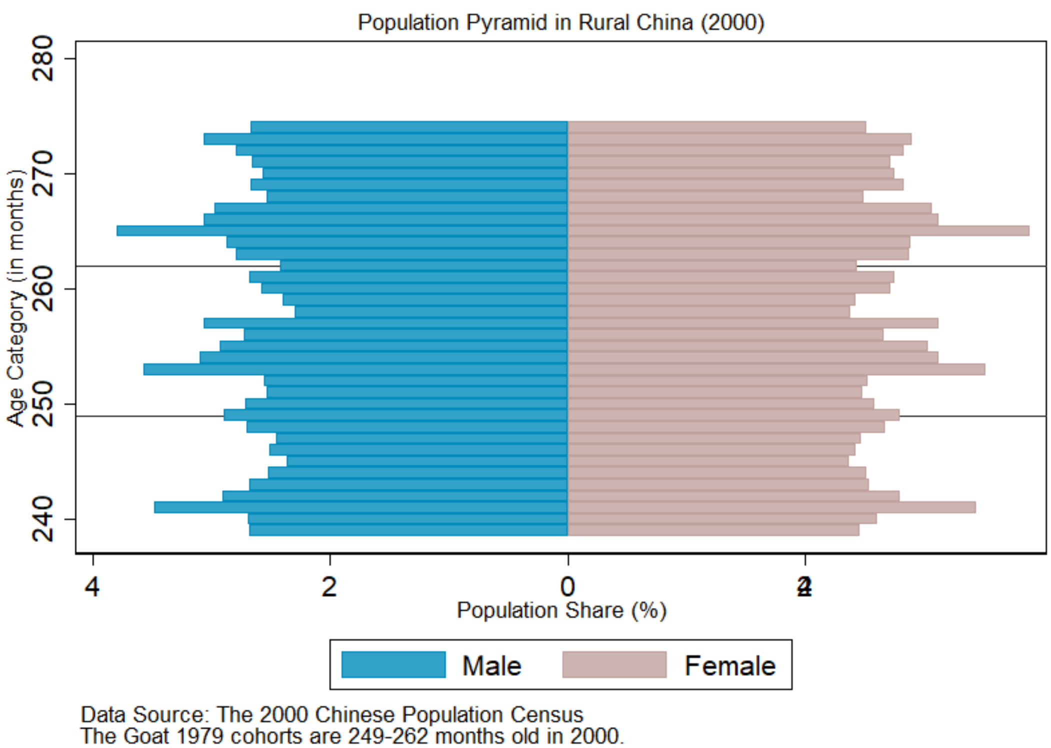

Figure 3.11: Population Pyramid for Urban Women

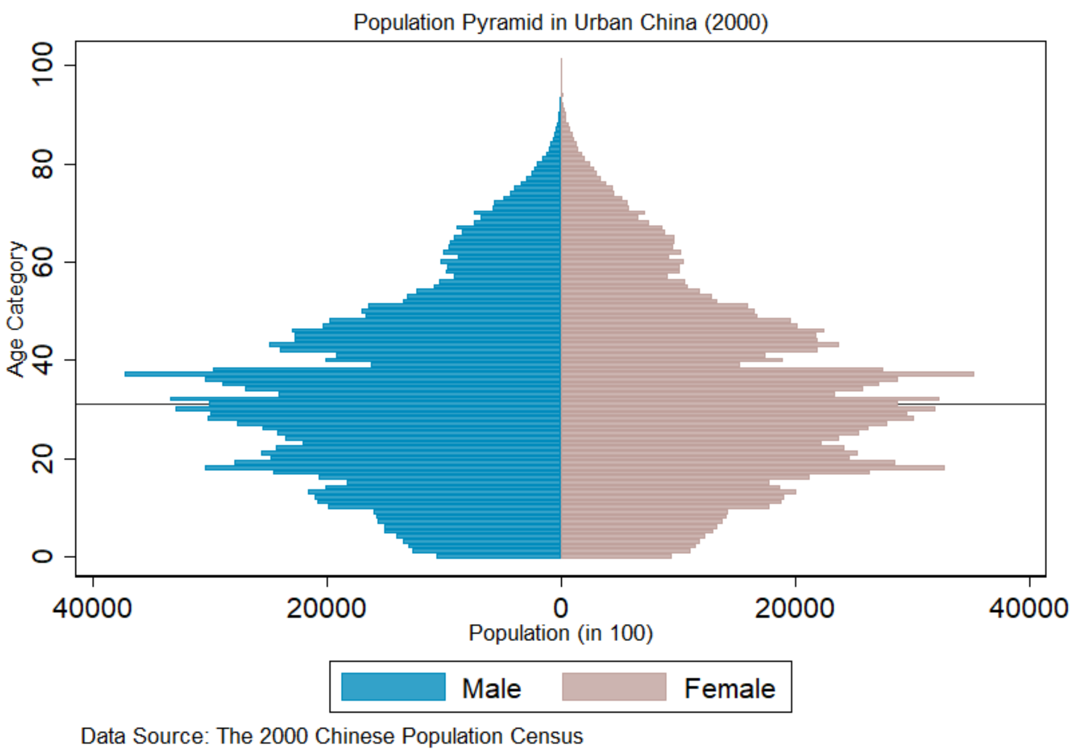

Figure 3.12: Population Pyramid for Urban Women around the 1979 Goat Year

Figure 3.13: Male-to-Female Ratio for the Rural Sample

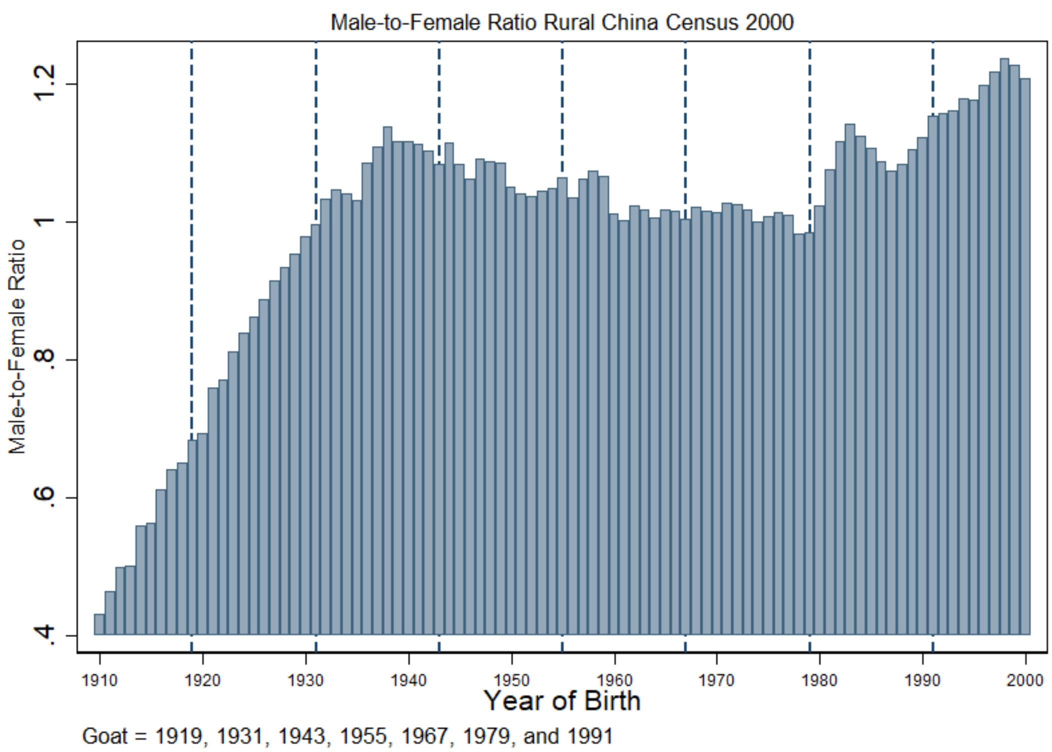

Figure 3.14: Male-to-Female Ratio for Rural Goat-Year Women

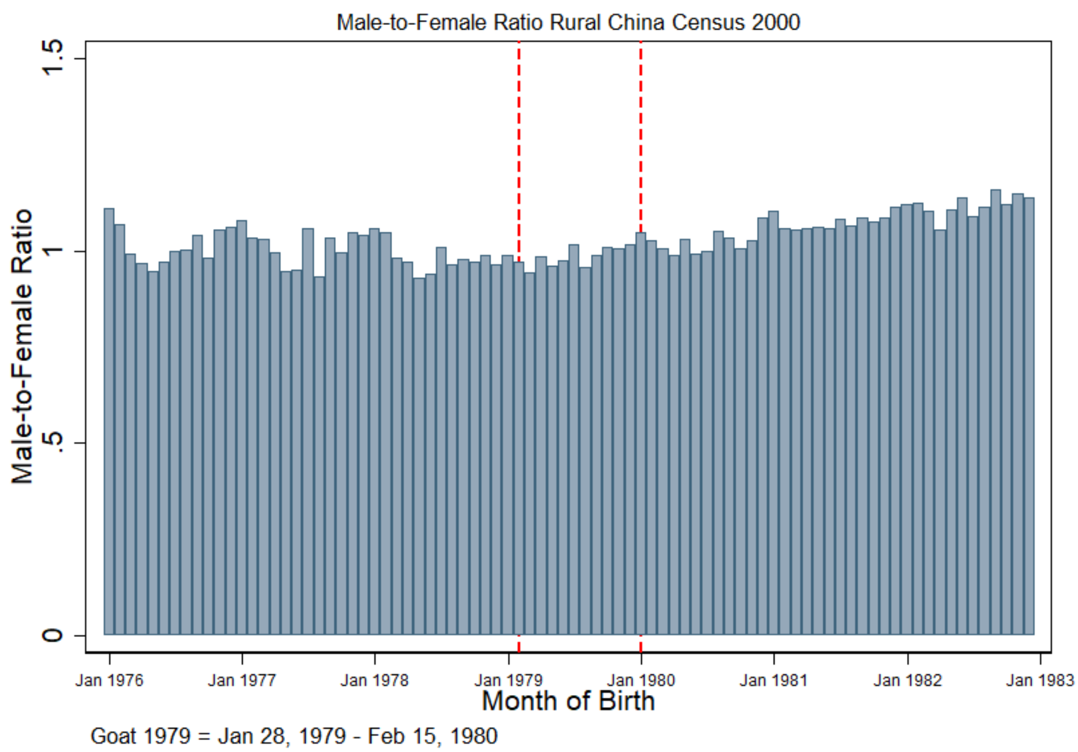

Figure 3.15: Male-to-Female Ratio for the Urban Sample

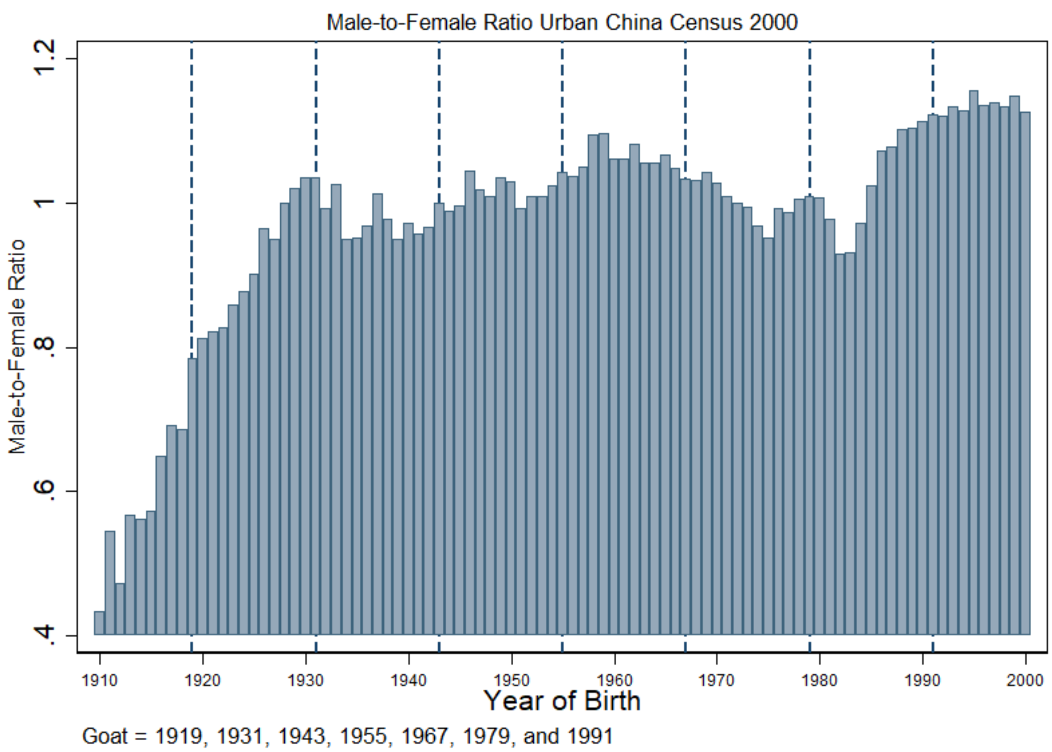

Figure 3.16: Male-to-Female Ratio for Urban Goat-Year Women

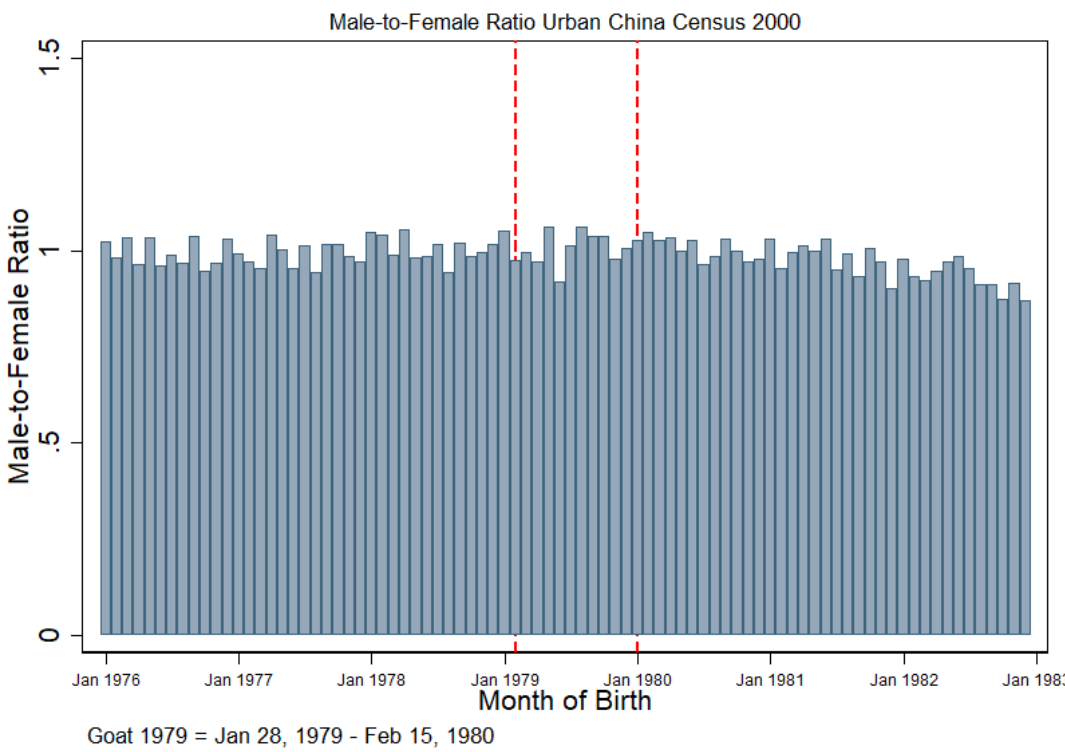

Figure 3.17: Educational Attainment for Rural Women

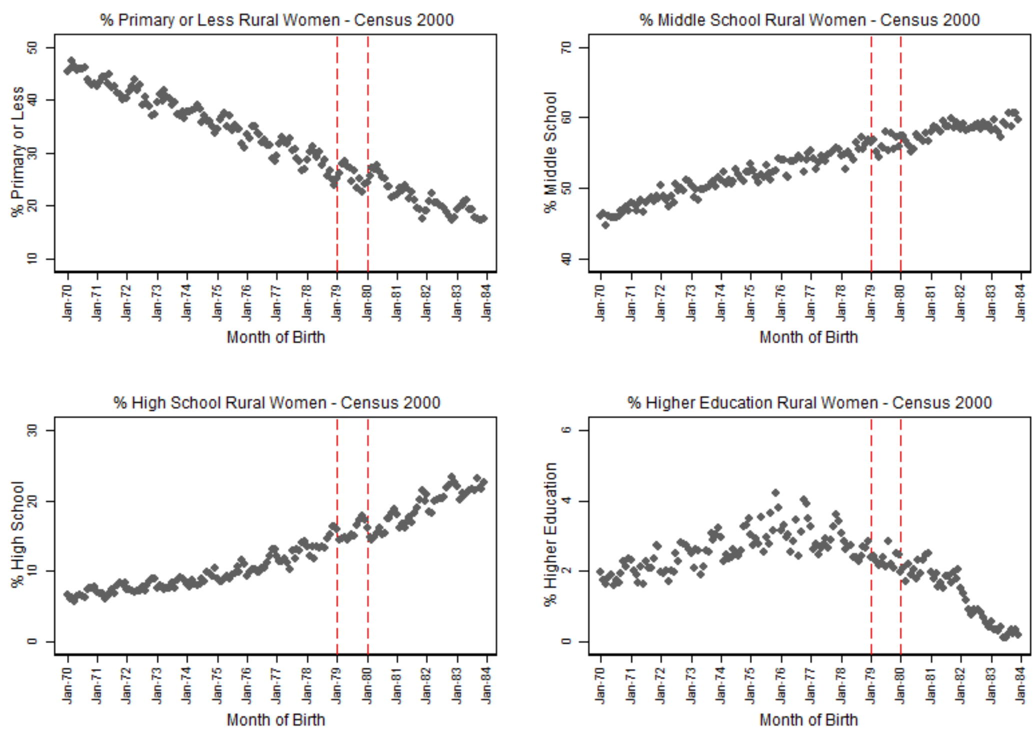

Figure 3.18: Educational Attainment for Rural Goat-Year Women

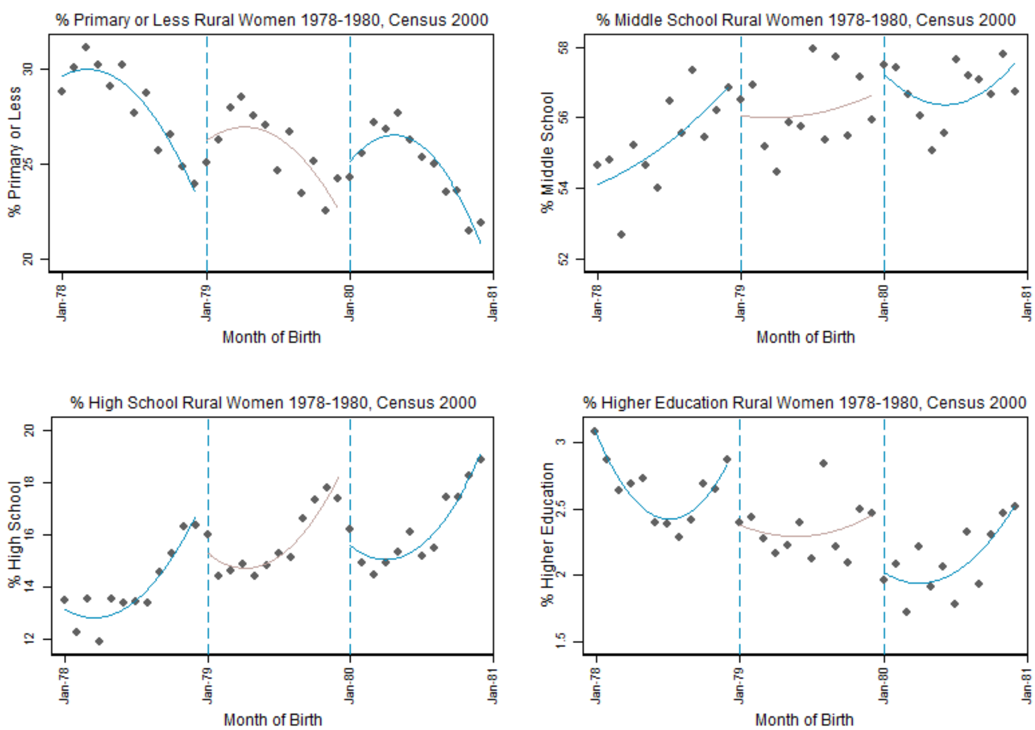

Figure 3.19: Educational Attainment for Urban Women

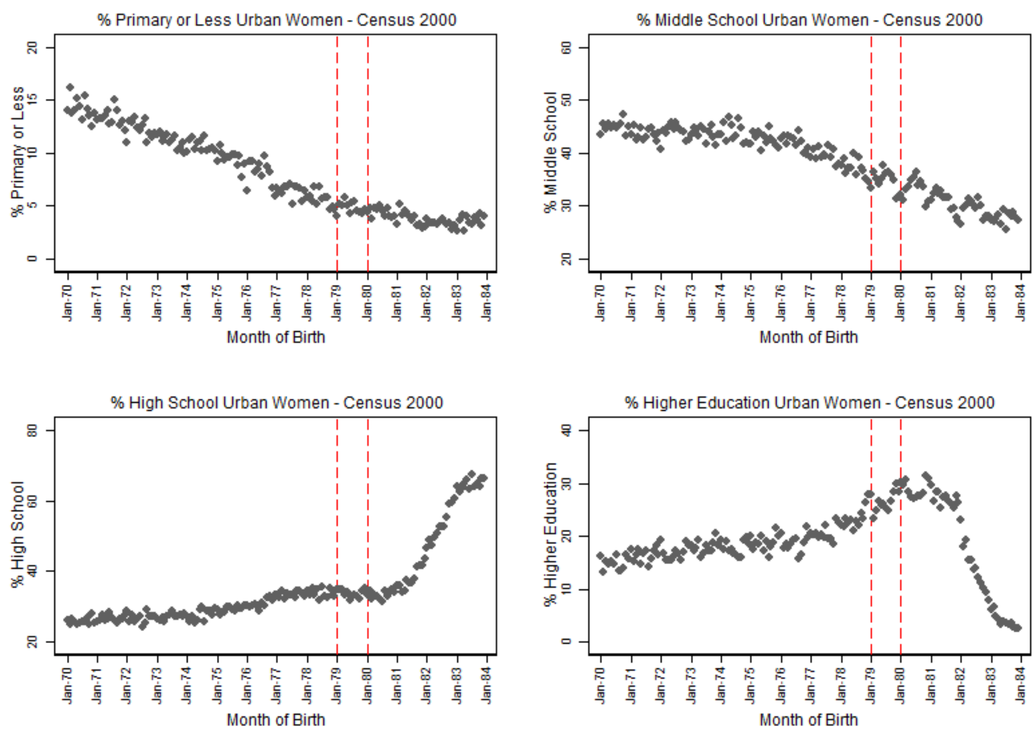

Figure 3.20: Educational Attainment for Urban Goat-Year Women

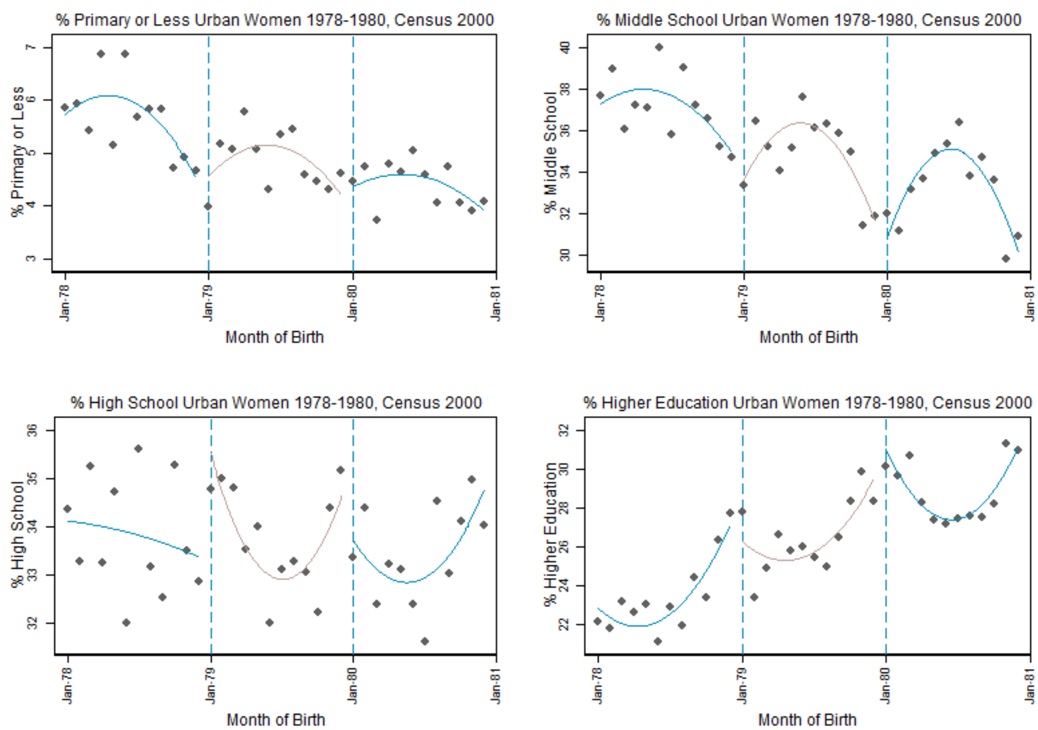

Figure 3.21: Labor Market Profiles for Rural Women

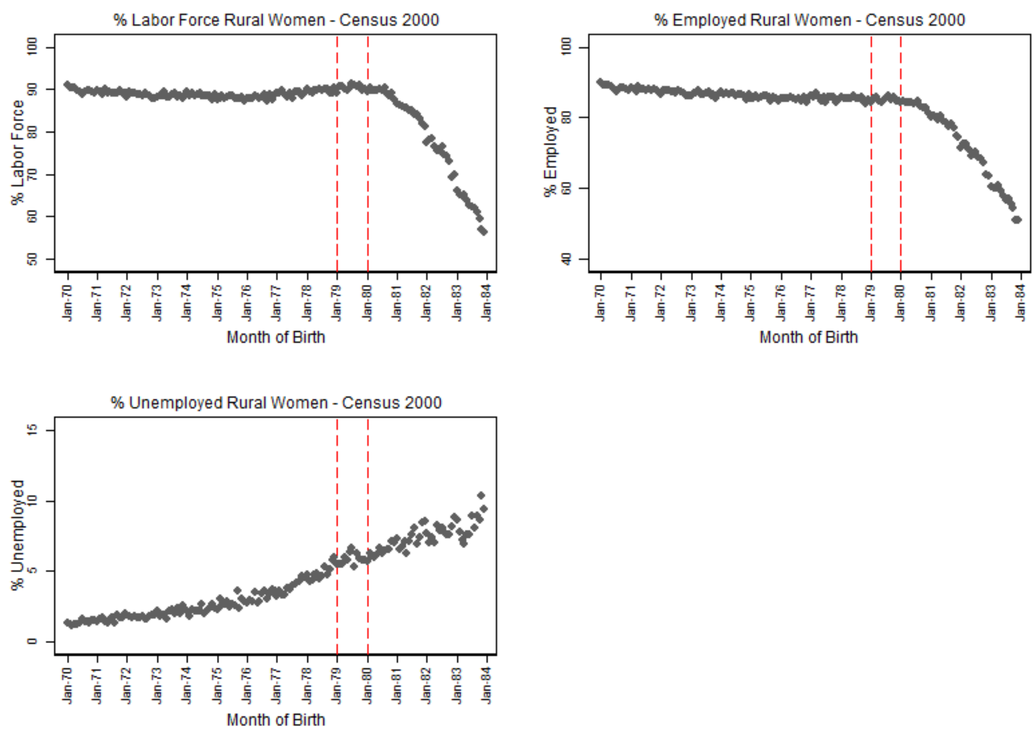

Figure 3.22: Labor Market Profiles for Rural Goat-Year Women

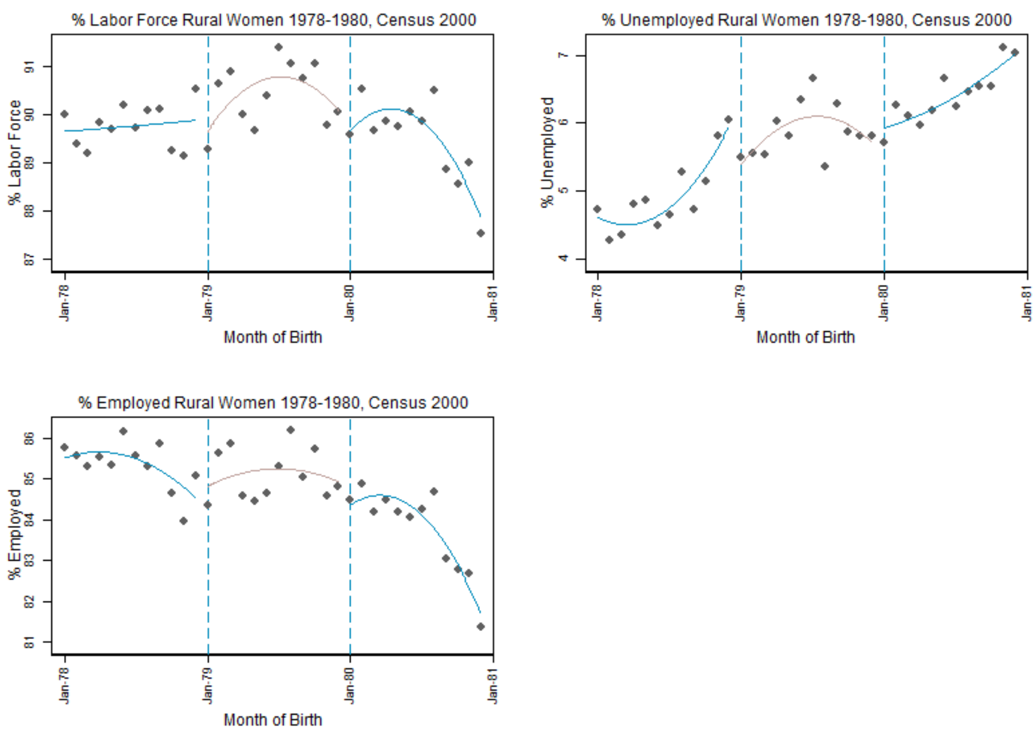

Figure 3.23: Labor Market Profiles for Urban Women

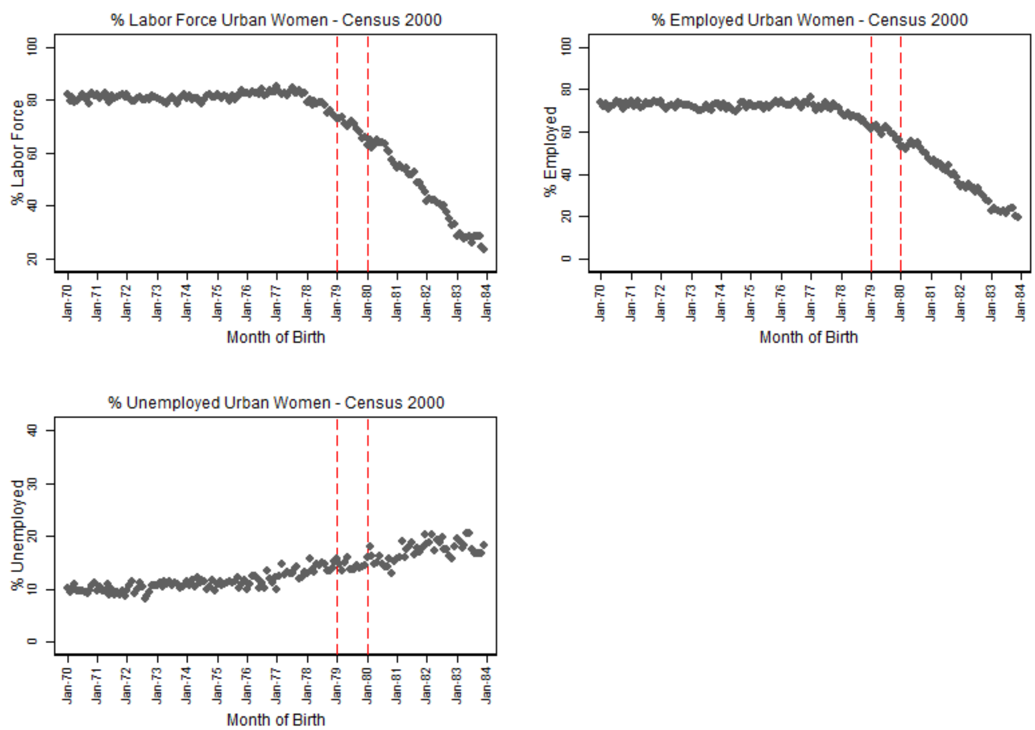

Figure 3.24: Labor Market Profiles for Urban Goat-Year Women

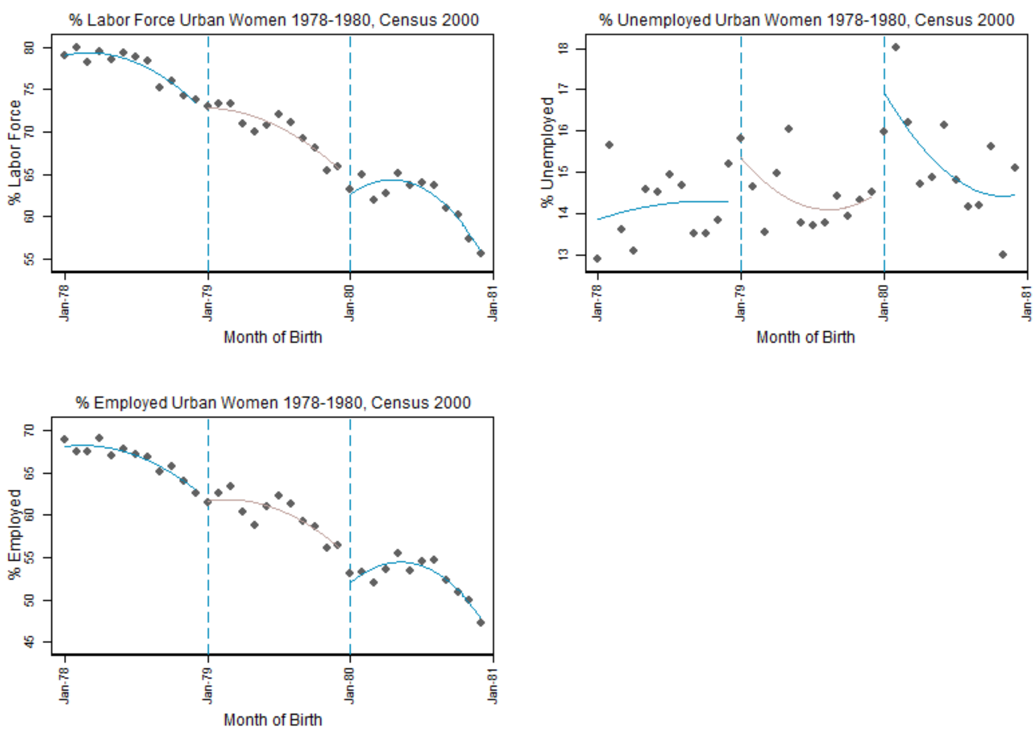

Figure 3.25: Marriage-Cohort Profiles with Quadratic Fit

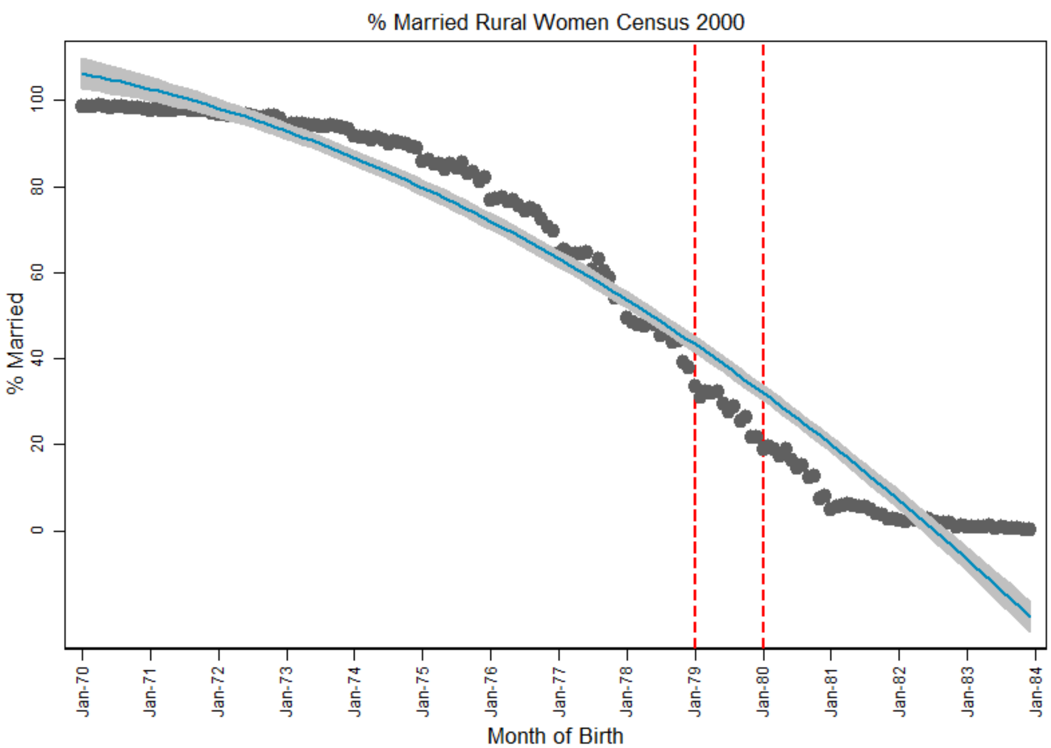

Table 3.1: Summary Statistics

	Rural	Urban
Married	0.293	0.116
	(0.455)	(0.320)
Age	21.319	21.270
	(0.847)	(0.843)
Han	0.876	0.941
	(0.330)	(0.236)
Primary School or Less	0.260	0.049
	(0.439)	(0.216)
Middle School	0.561	0.351
	(0.496)	(0.477)
High/Technical School	0.155	0.336
	(0.362)	(0.472)
Higher Education	0.024	0.264
	(0.152)	(0.441)
Labor Force	0.899	0.693
	(0.301)	(0.461)
Employed	0.847	0.593
	(0.360)	(0.491)
Unemployed	0.052	0.100
	(0.222)	(0.300)
Observations	141429	62717

Table 3.2: RD Estimates of the Goat-Year Effects on Marriage (Rural)

	(1)	(2)	(3)	(4)	(5)	(6)
	78-79(HG)	79-80(GM)	78-79(HG)	79-80(GM)	78-79(HG)	79-80(GM)
Goat	-0.039***	-0.014	-0.025	-0.029	-0.068	-0.131
	(0.013)	(0.014)	(0.025)	(0.036)	(0.057)	(0.103)
Constant	0.348***	0.179***	0.340***	0.180***	0.331***	0.180***
	(0.021)	(0.015)	(0.023)	(0.015)	(0.029)	(0.015)
Observations	96984	91757	96984	91757	96984	91757
R-squared	0.187	0.177	0.187	0.177	0.187	0.177
Age Polynomial	2	2	3	3	4	4
Province FE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table 3.3: RD Estimates of the Goat-Year Effects on Marriage (Urban)

	(1)	(2)	(3)	(4)	(5)	(6)
	78-79(HG)	79-80(GM)	78-79(HG)	79-80(GM)	78-79(HG)	79-80(GM)
Goat	-0.054***	-0.005	-0.048*	0.034	-0.082	0.081
	(0.014)	(0.014)	(0.028)	(0.036)	(0.062)	(0.103)
Constant	0.266***	0.145***	0.238***	0.147***	0.288***	0.149***
	(0.014)	(0.009)	(0.018)	(0.009)	(0.028)	(0.009)
Observations	42177	42090	42177	42090	42177	42090
R-squared	0.145	0.103	0.145	0.103	0.145	0.104
Age Polynomial	2	2	3	3	4	4
Province FE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table 3.4: DID Estimates of the Goat-Year Effects on Marriage

	(1) Rural	(2) Rural	(3) Rural	(4) Urban	(5) Urban	(6) Urban
Pair_1978-1979=1 × New Year=1	-0.002	0.016	0.013	0.003	-0.001	-0.001
	(0.010)	(0.016)	(0.016)	(0.012)	(0.020)	(0.020)
Pair_1978-1979=1	-0.049***	-0.053***	-0.051***	-0.044***	-0.044***	-0.044***
	(0.007)	(0.011)	(0.011)	(0.010)	(0.015)	(0.015)
New Year=1	-0.061***	-0.078***	-0.117***	-0.049***	-0.045***	-0.033
	(0.008)	(0.012)	(0.019)	(0.010)	(0.016)	(0.024)
Age	0.009***	0.008***	0.009***	0.008***	0.008***	0.008***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Age^2	-0.000	0.000	0.001***	0.000***	0.000***	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age^3		0.000	-0.000		0.000	-0.000
		(0.000)	(0.000)		(0.000)	(0.000)
Age^4			-0.000***			-0.000
			(0.000)			(0.000)
New Year=1 × Age	-0.003**	-0.009**	-0.040***	-0.002	0.000	0.005
	(0.001)	(0.004)	(0.009)	(0.002)	(0.005)	(0.011)
New Year=1 × Age^2	-0.000**	-0.001**	-0.007***	-0.000	0.000	0.001
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.002)
New Year=1 × Age^3		-0.000	-0.000***		0.000	0.000
		(0.000)	(0.000)		(0.000)	(0.000)
New Year=1 × Age^4			0.000*			0.000
			(0.000)			(0.000)
Han	-0.075***	-0.075***	-0.075***	-0.008	-0.008	-0.008
	(0.003)	(0.003)	(0.003)	(0.006)	(0.006)	(0.006)
Primary or Less	0.140***	0.140***	0.140***	0.140***	0.140***	0.140***
	(0.003)	(0.003)	(0.003)	(0.008)	(0.008)	(0.008)
High/Technical School	-0.187***	-0.187***	-0.187***	-0.151***	-0.151***	-0.151***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Higher Education	-0.305***	-0.304***	-0.304***	-0.266***	-0.266***	-0.266***
	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
Constant	0.612***	0.614***	0.599***	0.406***	0.407***	0.403***
	(0.005)	(0.006)	(0.007)	(0.008)	(0.010)	(0.010)
Observations	195319	195319	195319	81745	81745	81745
R-squared	0.114	0.114	0.114	0.134	0.134	0.134
Province FE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses.

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table 3.5: Sensitivity of the Goat-Year Effect to Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Rural	Rural	Rural	Rural	Rural	Urban	Urban	Urban	Urban	Urban
Goat	-0.120***	-0.017***	-0.007***	-0.004*	-0.019***	-0.127***	-0.018***	-0.007***	-0.001	-0.012***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)
Age	0.026***	-0.018***	-0.025***	-0.027***	0.036***	0.008***	-0.037***	-0.047***	-0.053***	-0.010
	(0.000)	(0.000)	(0.001)	(0.002)	(0.010)	(0.000)	(0.000)	(0.001)	(0.002)	(0.011)
Age^2	-0.000***	0.000***	0.000***	0.000***	-0.000**	-0.000***	0.000***	0.000***	0.000***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Han	-0.009***	-0.014***	-0.021***	-0.025***	-0.028***	0.004	0.002	0.000	-0.001	-0.004
	(0.001)	(0.002)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)
Primary School or Less	0.044***	0.077***	0.088***	0.100***	0.110***	0.061***	0.103***	0.111***	0.118***	0.122***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.004)	(0.005)	(0.006)	(0.008)
High/Technical School	-0.084***	-0.096***	-0.110***	-0.131***	-0.155***	-0.070***	-0.098***	-0.110***	-0.121***	-0.129***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)
Higher Education	-0.164***	-0.242***	-0.250***	-0.239***	-0.241***	-0.168***	-0.196***	-0.196***	-0.197***	-0.202***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Constant	-4.071***	1.464***	2.257***	2.299***	-5.815***	-1.642***	4.032***	5.293***	6.176***	0.677
	(0.013)	(0.040)	(0.082)	(0.227)	(1.217)	(0.022)	(0.059)	(0.114)	(0.284)	(1.393)
Observations	791076	455903	346958	237091	141429	317056	196744	153306	106062	62717
R-squared	0.614	0.472	0.392	0.299	0.215	0.598	0.399	0.306	0.212	0.147
Age Polynomial	2	2	2	2	2	2	2	2	2	2
Support	1970-1983	1975-1983	1976-1982	1977-1981	1978-1980	1970-1983	1975-1983	1976-1982	1977-1981	1978-1980
Province FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

§ 3.8 Appendix Tables

Table A3.1: RD Estimates of the Goat-Year Effects on Marriage with January and February Cohorts (Rural)

	(1)	(2)	(3)	(4)	(5)	(6)
	78-79(HG)	79-80(GM)	78-79(HG)	79-80(GM)	78-79(HG)	79-80(GM)
Goat	-0.040***	-0.014**	-0.035***	-0.012	-0.037**	-0.007
	(0.008)	(0.007)	(0.011)	(0.010)	(0.016)	(0.014)
Constant	0.342***	0.177***	0.332***	0.180***	0.334***	0.180***
	(0.018)	(0.013)	(0.018)	(0.013)	(0.018)	(0.013)
Observations	119731	109448	119731	109448	119731	109448
R-squared	0.191	0.178	0.191	0.178	0.191	0.178
Age Polynomial	2	2	3	3	4	4
Province FE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table A3.2: RD Estimates of the Goat-Year Effects on Marriage with January and February Cohorts (Urban)

	(1)	(2)	(3)	(4)	(5)	(6)
	78-79(HG)	79-80(GM)	78-79(HG)	79-80(GM)	78-79(HG)	79-80(GM)
Goat	-0.029***	-0.018***	0.001	-0.030***	-0.001	-0.033**
	(0.009)	(0.006)	(0.012)	(0.009)	(0.017)	(0.013)
Constant	0.236***	0.139***	0.219***	0.141***	0.216***	0.143***
	(0.011)	(0.008)	(0.011)	(0.009)	(0.011)	(0.009)
Observations	51999	50192	51999	50192	51999	50192
R-squared	0.147	0.105	0.148	0.105	0.148	0.105
Age Polynomial	2	2	3	3	4	4
Province FE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table A3.3: DID Estimates of the Goat-Year Effects on Marriage with January and February Cohorts

	(1) Rural	(2) Rural	(3) Rural	(4) Urban	(5) Urban	(6) Urban
Pair_1978-1979=1 × New Year=1	-0.022***	-0.012	-0.014	-0.002	0.013	0.012
	(0.008)	(0.012)	(0.012)	(0.011)	(0.015)	(0.015)
Pair_1978-1979=1	-0.021***	-0.026***	-0.023**	-0.022**	-0.034***	-0.032**
	(0.006)	(0.009)	(0.009)	(0.009)	(0.013)	(0.013)
New Year=1	-0.044***	-0.050***	-0.076***	-0.034***	-0.042***	-0.048***
	(0.006)	(0.008)	(0.011)	(0.008)	(0.011)	(0.014)
Age	0.011***	0.010***	0.013***	0.010***	0.008***	0.010***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Age^2	-0.000	-0.000	0.001***	0.000**	0.000**	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age^3		0.000	-0.000***		0.000	-0.000
		(0.000)	(0.000)		(0.000)	(0.000)
Age^4			-0.000***			-0.000***
			(0.000)			(0.000)
New Year=1 × Age	-0.004***	-0.005*	-0.038***	-0.002	-0.002	-0.015**
	(0.001)	(0.003)	(0.005)	(0.001)	(0.003)	(0.007)
New Year=1 × Age^2	-0.000	-0.000	-0.007***	-0.000	-0.000	-0.003***
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
New Year=1 × Age^3		-0.000	-0.000***		-0.000	-0.000**
		(0.000)	(0.000)		(0.000)	(0.000)
New Year=1 × Age^4			0.000***			0.000**
			(0.000)			(0.000)
Han	-0.075***	-0.075***	-0.074***	-0.007	-0.007	-0.007
	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)
Primary or Less	0.141***	0.141***	0.141***	0.145***	0.145***	0.145***
	(0.002)	(0.002)	(0.002)	(0.007)	(0.007)	(0.007)
High/Technical School	-0.185***	-0.185***	-0.185***	-0.149***	-0.149***	-0.148***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Higher Education	-0.300***	-0.300***	-0.300***	-0.260***	-0.260***	-0.259***
	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
Constant	0.596***	0.598***	0.579***	0.394***	0.397***	0.387***
	(0.005)	(0.005)	(0.006)	(0.007)	(0.008)	(0.008)
Observations	221824	221824	221824	93101	93101	93101
R-squared	0.115	0.115	0.116	0.134	0.134	0.134
Province FE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses.

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$