**Submitted to *MKTG551-1***
**manuscript 1**

# Perception of Rural Access to Healthcare: Information Assymetry, Distance-Effect, and Nudges

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There is a limited understanding of how spatial distance affects the perception of health access in rural communities. Partnering with a 3rd party service provider, we studied the impact of signalling trip time vs. distance and default choice on the rural patient's utilization of local primary care physicians. We find that signalling trip-time significantly improves healthcare utilization over distance signals. Similarly, we find that prompting potential patients to fill out a patient information card significantly increases health utilization. In our final study, we measure preventable hospitalizations in select counties where the 3rd party service provider was active, finding significantly lower hospitalizations in these counties over a 3-year span.
**DISCLAIMER:** The findings presented in this paper use simulated data, the only data that was used were county-level summary statistics to assist in simulation.

*Key words*: information-asymmetry; healh-access; nudges

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## 1. Introduction.

Access to healthcare in rural geographies has long been the focus of medical social science and policy research. Much of the literature has been concerned with the scarcity of resources (Douthit, Kiv, Dwolatzky, and Biswas (2015); Lam, Broderick, and Toor (2022)), exogenous differences in acceptance to healthcare (Coombs, Campbell, and Caringi (2022); Thomas, DiClemente, and Snell (2014)), and socioeconomic factors that limit utilization (Thomas et al. (2014); Nuako, Liu, Pham, Smock, James, Baker, Bierut, Colditz, and Chen (2022)).

Compared to their urban counterparts, rural geographies demonstrate much lower utilization of primary care physicians (PCPs), have a higher prevalence of preventable diseases like obesity and heart conditions (Nuako et al. 2022), and higher rates of government-sponsored insurance (Cyr et al. 2019). Rural geographies also face resource challenges at the service provider level, with 20% of the population residing in rural areas and only 9% of medical practitioners choosing to practice in these areas. However, primary care physicians have distributed themselves evenly among rural and urban communities (Rosenblatt and Hart 2000). Additionally, there are 2100 rural communities in the United States that fall under Health Professional Shortage Areas (HPSA), but rural communities see lower physician turnover in HSPA's relative to urban physicians (Pathman, Konrad, Dann, and

Koch 2004). As a result of higher number of HPSA's, rural patients must travel longer distances to receive care ($\frac{M_{rural}}{M_{urban}} = \frac{10.5mi}{4.4mi} = 2.386$). However, due to less traffic constraints characteristic of rural communities, travel times are not greater in rural areas by the same magnitude ($\frac{T_{rural}}{T_{urban}} = \frac{17min}{10.4min} = 1.634$) (Lam et al. 2022). McGrail, Humphreys, and Ward (2015) found that, once accounting for town clustering, travel times were not statistically different between a sparsely and closely-populated sample. This suggests that distance may be an inaccurate measure for determining health disparities.

In a review of the top-10 physician appointment service providers, all use distance to indicate proximity to the nearest physician. Research has shown that consumer's more easily access trip time from memory compared to distance when making frequent trips, with fewer errors in judgement, and have greater prioritization for time vs. distance when making trip decisions (Kang, Herr, and Page 2003). A reasonable assumption could be that this effect is magnified for infrequent or first-time trips (ie. Doctor's visits), where consumers lack a frame of reference to accurately make judgements. Continuing with this line of thought, potential patients in rural communities might not be aware of PCPs in their area because of greater spatial distance, whereas urban patients have greater accessibility to references because they are exposed more often in their respective communities. This information-assymmetry, where PCPs are present in a community but potential patients perceive a lack of access, might be a factor for low-utilization rates. This leads us to our first hypothesis:

**Hypothesis 1 (Time vs. Distance Perception)** *Appointment services which indicate travel time to the nearest PCP will lead to higher utilization rates of health services in rural communities.*

**Hypothesis 2** *An appointment service which indicates travel distance will have lower utilization rates of health services relative to indicating time proximity and the absence of travel metrics.*

Varying travel metrics represents a low-cost “nudge” to increase healthcare utilization. Choice architecture is described as a design framework for which decision makers are presented options, including the size3 of the a consideration set, presentation of options, and presence of defaults. Importantly, choice architecture and nudges do not alter the consumer's economic incentives, nor does it limit the options available to the consumer. Firms often relax the former requirement, implementing “bad” nudges, like presenting the most profitable option as the default-choice. However, there is a growing interest in using “good” nudges, or libertarian paternalism, as a tool to encourage consumers towards socially beneficial outcomes. “Libertarian paternalism is a relatively weak, soft, and non-intrusive type of paternalism becauses choices are not blocked, fenced off, or significantly burdened” (Thaler and Sunstein 2008). Governments have implemented teams of social scientists to introduce such nudges, with applications in increasing retirement contributes of military personnel or encouraging healthier eating habits (Benartzi, Beshears, Milkman, Sunstein, Thaler, Shankar, Tucker-Ray, Congdon, and Galing 2017). An important characteristic of nudges is that they can be implemented at a relatively low-cost. In a meta-analysis of nudge literature, Benartzi et al. (2017) concluded that nudges were far more effective at encouraging behavioral changes per dollar spent relative to traditional policy tools. Using traditional financial incentives Chetty, Friedman, Leth-Petersen, Nielsen, and Olsen (2014) estimate that for each Danish Krone spent in financial subsidies, there was only an increase of 1 cent in retirement savings. Conversely, Carroll, Choi, Laibson, Madrian, and Metrick (2009) find that, when prompting employees to enter their preferred contribution rate, there was an $100 increase in retirement savings per dollar spent.

The size of the assortment set of options has a significant impact on the decision as well, with higher search costs moderated by the difficulty of the decision task, complexity of the choice set, the consumer's preference uncertainty, and their decision goal (Chernev, Böckenholt, and Goodman 2015). Primary care choice serves as a perfect parallel to the "choice overload" described above. Potential patients are exposed to a large set of primary care physicians (assortment size), with varying proximity, types of insurance accepted, and specialties (complexity). These consumers may not be aware of what constitutes a quality physician, both intrinsically and extrinsically (preference uncertainty). If, or when, a choice is made, the patient might worry that the physician has openings that fit their schedule, accept their particular insurance, or are non-judgmental to their lifestyle (difficulty of decision task). Nudges can help to minimize the cognitive effort of the decision goal by reducing the search costs of the potential patient when searching for a provider.

One might expect that these factors are exacerbated in rural communities, with a low perception of access and institutional knowledge, as well as lower socioeconomic status (SES). Mrkva et al. (2021) estimate that nudges were two to five times more effective at changing the behavior of low-SES consumers, and produced much greater social benefits compared to high-SES consumers. Nudges mediated the lack of domain-specific knowledge and health/financial numeracy. This suggests that rural Americans may be more susceptible to nudges with the goal of increasing health utilization and outcomes. Milkman, Beshears, Choi, Laibson, and Madrian (2011) found that nudges in the form of prompting respondents to indicate the time and date that they would receive their influenza vaccination significantly increased vaccination rates at a large employer. This leads us to our third hypothesis:

**Hypothesis 3** *Patients that are provided with a default-option of provider, with the 3$^{rd}$ party service's proprietary algorithm that ranks physicians on proximity and quality, will reduce the search costs and decision difficulty of potential patients. Prompting potential patients to fill out an information card with their appointment availability will increase utilization rates of local PCPs.*

Preventable hospitalizations, which are defined as hospitalizations easily preventable with early detection, preventative care, and lifestyle changes, greatly increase the strain on the healthcare system. McDermott and Jiang (2020) found that preventable hospitalizations resulted in $33.7 billion hospital costs in 2017, or 8.9% of total hospital costs in the U.S. Patients in primary care shortage areas, which primarily consist of rural areas, were found to be 1.82 times more likely to experience a preventable hospitalization (Parchman and Culler 1999). Meanwhile, primary care utilization has shown to greatly reduce these preventable hospitalizations, resulting in less strain on hospital groups and bettering health outcomes (Oh, Potter, Sabik, Trivedi, Wolinsky, and Wright (2022); Kronman, Ash, Freund, Hanchate, and Emanuel (2008)). This leads us to our final hypothesis:

**Hypothesis 4** *Preventable hospitalizations will decrease in counties with a service provider that uses travel-time and prompting nudges when scheduling appointments.*

## 2. Overview of Studies

We partner with a 3$^{rd}$ party service provider that specializes in routing patients to the nearest PCP and provides a convenient platform to schedule appointments. The service provider operates in select counties across America, including rural and urban areas, but has seen low-utilization in rural communities. The first study measures the effect of varying travel-related metrics on utilization of primary care resources. Similarly, the second study is concerned with measuring the effect of prompting potential patients to identify their general availability on PCP utilization. The final study measures preventable hospitalizations three-years after changes were made to the service-provider's patient portal, implementing the prior findings. The paper will proceed as follows: description of the methodology and results of the respective studies, a general discussion, policy implications, conclusion, and limitations.

## 3. Study 1.

### 3.1. Methodology.

**3.1.1. Randomization Procedure.** Two surveys, 3-months apart, were administered through Mechanical Turk (MTurk) and Qualtrics, indicating a study for rural American's to participate. Several screening questions were used to filter respondents, including attention tasks (to eliminate speeding). Respondents were again asked to indicate whether their residence was in a rural geography, with questions relating to the population of their town or city. Because we are interested in the probability of primary care utilization, we exclude respondents that indicate they currently have a primary care physician and have had an appointment in the last two years. This design may present endogeneity concerns, as we might expect that the sample is not representative of the rural population. To account for this, we stratify the sample on race and education; a demographic comparison is presented in Table 1.

MTurk recruitment reached 15125 people and occurred across 12 waves for each study. Upon completion of the screening task, respondents were paid \$0.15 and, if they passed the screening questions, were transferred to the primary survey. This survey asked respondents to reveal basic demographic information (ie. Race, gender, education, and income), as well as health behaviors, including smoking-status, exercise habits, preexisting conditions, and prior utilization of health services. Upon completion of the primary survey, respondents were paid \$1.00 and informed they would receive an additional \$1.50 if they were allowed to be redirected to the 3$^{\text{rd}}$ party service provider's website. The Qualtrics survey randomized respondents which indicated they would like to be redirected into three experimental conditions for Study 1.a (16% Control, 42% Time-Condition, 42% Distance-Condition), and two experimental conditions for Study 1.b (25% Control, 75% Prompt-Condition). Respondents were also provide a unique code to enter upon redirect, and again if they scheduled an appointment through the service provider. This code was used to track respondent's experimental condition, as well as filter respondents that did not interact with the website upon entry. A team of field-researchers were used to follow-up with physicians to record actual utilization.

Of these respondents, 2534 did not reside in rural geographies, 252 failed attention tasks, and 1123 indicated they currently have a PCP and have had an appointment in the prior two years. This resulted in a sample of 5315 respondents for Study 1.a and 5901 for Study 1.b. No respondents refused the redirect, however 235 and 412 respondents were eliminated from the Studies 1.a and 1.b, respectively, for lack of interaction with the web-page upon entry. The sample was stratified by race and education-level to address endogeneity concerns, resulting in a final sample of 1800 respondents per study.

**3.1.2. Data.** County-level statistics were obtained from Soc (2020, Social Determinants of Health Database). This data includes measures of prevention quality, percentage of physicians which offer medicaid, percentage of population uninsured, unemployment rates, percentage of population below poverty line, HPSA area, and mean hours worked by PCPs. This data is matched to the survey responses on zipcode.

The service provider gathers data on the physicians in their database. Measures include the physician's Healthgrades$^{TM}$, utilization rates or availability, types of accepted insurance, and type of practice. This data is merged with the survey responses in a similar manner to the above.

**Table 1 Sample Summary Statistics**

| | Study 1a | Study 1b | Population | F-Value | $p$ |
|---|---|---|---|---|---|
| Age | 45.7 | 45.3 | 45.7 | .904 | .405 |
| Income | 55402 | 55060 | 54459 | 2.06 | .127 |
| Female | .498 | .511 | .509 | .397 | .672 |
| Education-Level | | | | .015 | .985 |
| Some HS | .354 | .379 | .350 | | |
| High School | .317 | .305 | .326 | | |
| Some College | .152 | .131 | .138 | | |
| Bachelors | .108 | .119 | .122 | | |
| Professional | .0683 | .0656 | .0641 | | |
| Race | | | | 1.95 | .135 |
| White | .566 | .570 | .601 | | |
| Hispanic | .165 | .170 | .151 | | |
| Black | .220 | .221 | .198 | | |
| Asian | .041 | .039 | .05 | | |
| Smoker | .312 | .330 | .325 | .306 | .736 |
| PCP Distance | 9.76 | 9.70 | 9.69 | .676 | .509 |
| $N$ | 1800 | 1800 | 119467 | | |

Sample was stratified on race and education level to ensure statistical power and representation of population

### 3.2. Empirical Design

Our econometric model starts from a level of basic analysis. We estimate an individual's *likelihood* to respond to the various treatment conditions and utilize local healthcare resources. The latent likelihood is a logit function of treatment conditions and covariates

$$\pi_i = \frac{e^{\mathcal{U}_{icp}}}{1 + e^{\mathcal{U}_{icp}}} \tag{1}$$

$$\mathcal{U}_{icp} = \alpha + \delta Trt_{icp} + X_i'\Gamma + \eta_p + \nu_c + \epsilon_{icp} \tag{2}$$

where $i$ indicates the survey respondent, $c$ indicates the county, and $p$ indicates the physician. $\pi_i$ is equal to 1 if the respondent booked and followed-through with their appointment, and 0 otherwise. $\delta$ is the coefficient corresponding to the relevant treatment group, with the control as the baseline estimate. $\Gamma$ is a vector of coefficients on the matrix of respondent characteristics. We include county and physician-level fixed effects to account for variation in county characteristics and physician quality, represented by $\eta$ and $\nu$ respectively. $\epsilon_{icp}$ is the error term from the Type 1 Extreme Value Distribution.

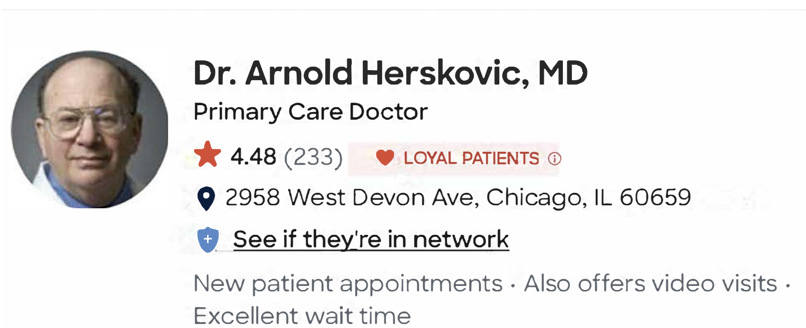
Correlations in the data aren't too concerning, with only 5 variables at the county-level over $abs_{corr} > 0.3$: unemployment rate, poverty line, PCP hours, prevention quality, and HPSA. Hence, we will drop poverty line, PCP hours, and HPSA from the county fixed effects. Education and Income are categorical variables, but are known to be correlated, so we will only include education as a regressor in the model.

### 3.3. Study 1.a: Location Cues

**3.3.1. Description.** This study varies the presence of travel-related metrics that indicate proximity to local PCP's when viewing the service provider's website. Respondents assigned to the control condition will view the exact page that the treatment conditions will view, including ratings and accepted insurance, but will only be exposed to an address. Respondents in the distance condition will view physicians ranked by proximity and quality, with an indication of the estimated distance in miles from their location. Similarly, the time-condition will be exposed to the estimated time to their present location.

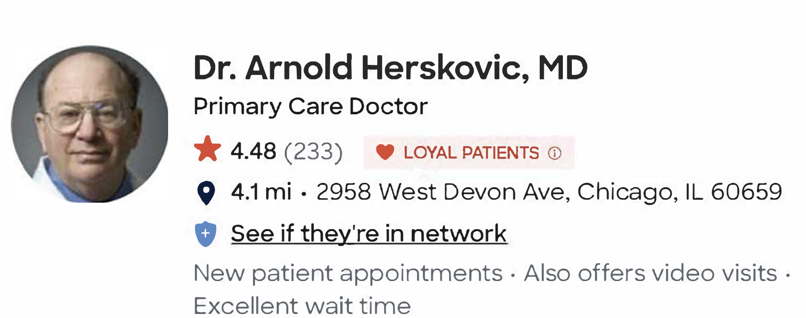
As a reminder to our hypotheses, we expect the coefficient for the distance treatment condition to have a negative coefficient relative to baseline for rural communities. This is inline with the reasoning in Kang et al. (2003), which consumers make poor time-to-distance calculations and rural respondents might make the assumption that the practitioner is out of reach. We also expect that the coefficient for the time treatment condition will have a positive sign relative to baseline, for the same reasons.

**Figure 1 Control Condition.**



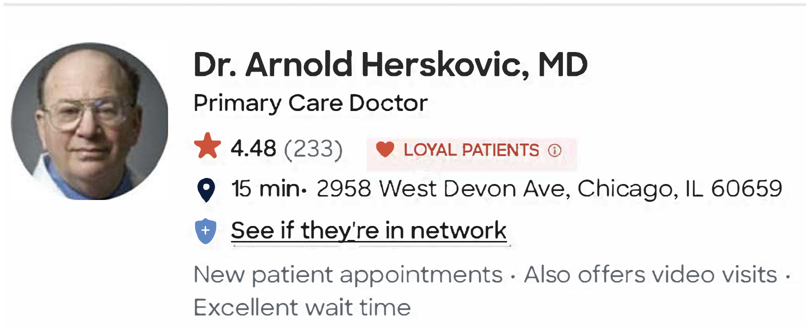

*Note.* Respondents will only be exposed to the address

**Figure 2 Distance Condition.**




*Note.* Respondents will be exposed to the estimated travel distance to the nearest PCP

**Figure 3** **Time Condition.**




*Note.* Respondents will be exposed to the estimated travel time to the nearest PCP

**3.3.2. Results.** From the model-free evidence of the data, we can see that respondents in the time treatment condition have much higher rates of utilization relative to respondents placed in the control and distance conditions ($\hat{\mu}_{control} = .268, \hat{\sigma}_{\theta_c} = .028$ ; $\hat{\mu}_{dist} = .267, \hat{\sigma}_{\theta_d} = .016$ ; $\hat{\mu}_{time} = .356, \hat{\sigma}_{\theta_t} = .017$). Table 2 shows, across all models, the time treatment condition has a greater log-odds of utilization relative to the other conditions, and is highly statistically significant. In the base logit model, respondents assigned to the Time treatment condition were 1.7 ($e^{0.53}$) times more likely to book an appointment though the website. When adding county-level fixed effects, this estimate jumps to 2.36 times as likely. The distance metric is not statistically significant in any model but is directionally consistent with our hypothesis. A reasonable assumption would be that consumers make errors in judgement equally when assessing travel time with only address and distance prompts. To test if there is any effect for the distance condition, I add an interaction term between the distance treatment and the distance to their nearest PCP. The analysis indicates that there is a significant reduction in the likelihood of booking, holding all other variables fixed, for each additional unit change in distance to PCP. This is consistent with Hypothesis 2. These findings suggest that potential patients in rural communities respond positively to location cues that indicate travel time, and negatively for cues that indicate travel distance.

There are additional coefficients of note in the logit model. The binary indicator for smoking-status coefficient is negative and marginally significant, and coefficients for Distance to PCP, time since last visit, and African American respondents are negative and highly significant. Conversely, coefficients for binary female indicator, education factors, age, and Asian respondents are statistically significant and positive. These results suggest that respondents who previously had poor health habits (ie. smokers and infrequent Doctor visits), are African American, or live further away from a local PCP are less likely to book an appointment through the website. Meanwhile, respondents with higher levels of education, are older, or biologically female are more likely to visit their local PCP.

**Table 2** **Study 1.a Regression Results**

| Regressors | Logit | | | | Probit | | |
|---|---|---|---|---|---|---|---|
| | Model 1 | Model 2 | Model 3 | Model 4 | Model 5 | Model 6 | Model 7 |
| (Intercept) | $2.98^{***}$ | $2.98^{***}$ | $3.49^{**}$ | $3.49^{**}$ | $1.68^{***}$ | $1.68^{***}$ | $1.91^{**}$ |
| | (0.51) | (0.51) | (1.12) | (1.12) | (0.277) | (0.277) | (0.66) |
| TrtDist | $-0.03$ | 0.04 | $-0.05$ | $-0.03$ | 0.02 | 0.02 | $-0.13$ |
| | (0.09) | (0.09) | (0.12) | (0.12) | (0.04) | (0.04) | (0.62) |
| TrtTime | $0.53^{***}$ | $0.86^{***}$ | $0.55^{*}$ | $0.65^{**}$ | $0.49^{***}$ | $0.49^{***}$ | $0.40^{**}$ |
| | (0.14) | (0.16) | (0.22) | (0.19) | (0.09) | (0.09) | (0.15) |
| Smoker | $-0.19^{*}$ | $-0.32^{*}$ | $-0.19$ | $-0.31^{*}$ | $-0.20^{*}$ | $-0.20^{*}$ | $-0.20^{*}$ |
| | (0.10) | (0.15) | (0.10) | (0.14) | (0.09) | (0.09) | (0.09) |
| PCP Distance | $-0.47^{***}$ | $-0.32^{***}$ | $-0.53^{***}$ | $-0.34^{***}$ | $-0.19^{***}$ | $-0.19^{***}$ | $-0.20^{***}$ |
| | (0.01) | (0.03) | (0.08) | (0.10) | (0.02) | (0.02) | (0.06) |
| Female | $0.37^{**}$ | $0.41^{*}$ | $0.38^{**}$ | $0.42^{*}$ | 0.22 | 0.22 | $0.22^{*}$ |
| | (0.12) | (0.19) | (0.11) | (0.18) | (0.11) | (0.11) | (0.11) |
| Educ: High School | 0.25 | $0.55^{*}$ | 0.25 | $0.55^{*}$ | 0.29 | 0.29 | 0.30 |
| | (0.16) | (0.24) | (0.16) | (0.24) | (0.15) | (0.15) | (0.15) |
| Educ: Some College | $0.56^{***}$ | $0.81^{***}$ | $0.57^{***}$ | $0.81^{***}$ | $0.43^{***}$ | $0.43^{***}$ | $0.43^{***}$ |
| | (0.09) | (0.15) | (0.09) | (0.15) | (0.10) | (0.10) | (0.10) |
| Educ: Bachelor | $0.80^{*}$ | $1.26^{*}$ | $0.80^{*}$ | $1.26^{**}$ | $0.69^{*}$ | $0.69^{*}$ | $0.69^{*}$ |
| | (0.35) | (0.51) | (0.34) | (0.48) | (0.30) | (0.30) | (0.29) |
| Educ: Professional | $0.95^{***}$ | $1.41^{***}$ | $0.96^{***}$ | $1.42^{***}$ | $0.77^{***}$ | $0.77^{***}$ | $0.77^{***}$ |
| | (0.16) | (0.34) | (0.16) | (0.33) | (0.22) | (0.22) | (0.21) |
| Age | $0.02^{***}$ | $0.02^{***}$ | $0.02^{***}$ | $0.02^{***}$ | $0.01^{***}$ | $0.01^{***}$ | $0.01^{***}$ |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Last Visit | $-0.38^{***}$ | $-0.49^{***}$ | $-0.38^{***}$ | $-0.49^{***}$ | $-0.28^{***}$ | $-0.28^{***}$ | $-0.28^{***}$ |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.02) | (0.02) | (0.01) |
| Hispanic | $-0.02$ | $-0.04$ | $-0.02$ | $-0.05$ | $-0.00$ | $-0.00$ | $-0.00$ |
| | (0.02) | (0.05) | (0.02) | (0.05) | (0.03) | (0.03) | (0.03) |
| Black | $-0.07^{***}$ | $-0.07$ | $-0.07^{***}$ | $-0.07$ | $-0.04$ | $-0.04$ | $-0.03$ |
| | (0.01) | (0.05) | (0.02) | (0.06) | (0.02) | (0.02) | (0.03) |
| Asian | $0.41^{***}$ | $0.40^{***}$ | $0.41^{***}$ | $0.40^{***}$ | $0.23^{***}$ | $0.23^{***}$ | $0.23^{***}$ |
| | (0.01) | (0.04) | (0.00) | (0.05) | (0.02) | (0.02) | (0.03) |
| TrtDist*PCP Dist | | | $-0.35^{**}$ | $-0.38^{**}$ | | | $-.22^{**}$ |
| | | | (0.12) | (0.12) | | | (0.07) |
| TrtTime*PCP Dist | | | 0.09 | 0.02 | | | 0.01 |
| | | | (0.05) | (0.06) | | | (0.04) |
| County FE | | ✓ | | ✓ | | ✓ | ✓ |
| Physician FE | | ✓ | | ✓ | | ✓ | ✓ |
| Clustered SE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Num. obs. | 1800 | 1800 | 1800 | 1800 | 1800 | 1800 | 1800 |
| Deviance | 1667.44 | 1294.76 | 1666.37 | 1294.65 | 1293.84 | 1293.84 | 1293.74 |
| Log Likelihood | $-833.72$ | $-647.38$ | $-833.18$ | $-647.32$ | $-646.92$ | $-646.92$ | $-646.87$ |
| Pseudo $R^2$ | 0.23 | 0.32 | 0.23 | 0.32 | 0.32 | 0.32 | 0.32 |
| Num. groups: idCounty | | 40 | | 40 | | 40 | 40 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Standard Errors are clustered at race and education-level.

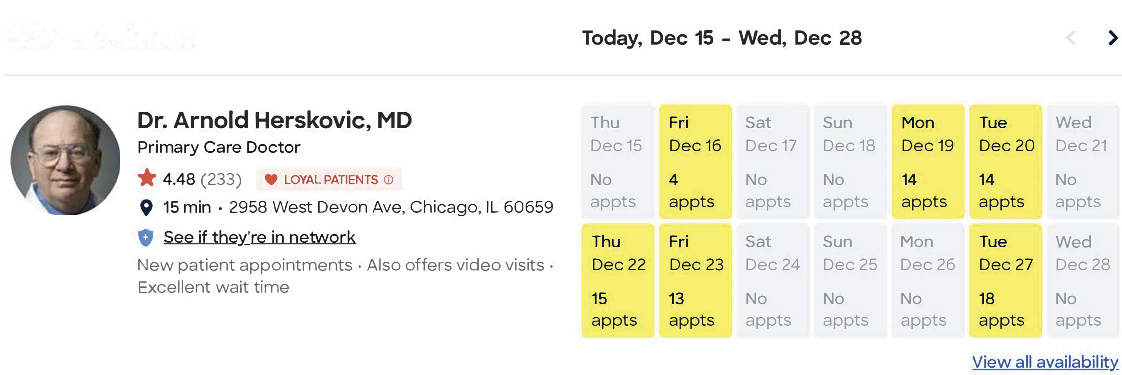
Finally, as a robustness check, we include a probit specification of our model (Models 5, 6, and 7). The probit specification is defined as follows,

$$Pr(\mathcal{U}_{icp} = 1) = \Phi(\alpha + \delta Trt_{icp} + X_i'\Gamma + \eta_p + \nu_c + \epsilon_{icp}) \tag{3}$$

where $\Phi$ is the standard normal CDF of the same model parameters as the logit specification. The main coefficients of interest do not change significantly. The time treatment condition remains highly statistically significant, and indicates respondents are 1.62 times more likely to visit their local PCP compared to the baseline. Interestingly, the coefficient for the distance treatment condition flips signs, but remains statistically insignificant. The interaction term remains statistically significant at a reduced magnitude in Model 7.

### 3.4. Study 1.b: Default-Choice and Prompting Nudges

**Figure 4** **Time Condition.**




*Note.* Respondents are matched to a default primary care physician on proximity and rank. They are presented with the updated time condition, as well as a calendar to indicate physician availability. Upon selection of a date, the respondent fills out an information card for follow-up by the provider.

**3.4.1. Description.** Following the conclusion of Study 1.a, the service provider modified the user experience of their website to include travel time statistics for users visiting their site. In order to assess whether additional nudges, like default-choice and prompts, positively affect outcomes, the service provider performs an A/B test displaying Figure 3 for users in the control condition and a new design outlined in Figure 4.

Users assigned to the treatment condition will be redirected to the website, enter their unique code, and be asked if they'd like to share their location. Respondents that answer "No" are dropped from the analysis. The 3$^{\text{rd}}$ party service provider uses their proprietary algorithm to match the respondent with a physician, weighted on proximity and physician ratings. This is the default-choice for the respondent. They have the option to view other physicians in their area by selecting an option labeled “See more.” in order to not limit the choice set for the consumers. In addition to the default-choice, consumers are presented with a series of available dates that the physician has openings. Upon selection of an available date, the respondent is redirected to another page to fill out an information form that indicates their availability on that date, as well as typical availability on weekdays. The information form allows physicians to follow-up should they cancel or miss their appointment, in order to schedule subsequent appointments.

**3.4.2. Results.** The model-free results are encouraging for two reasons: (1) they confirm our findings from Study 1.a, with an even higher mean rate of utilization under the control condition – which has added the travel time metric, and (2) the treatment under prompting and default-choice nudges has marginally higher utilization rates than the control [$\hat{\mu}_{control} = 0.374(0.023)$; $\hat{\mu}_{treat} = 0.414(0.013)$]. The logit model includes demographic and health regressors as before, but coefficient estimates are similar and, thus, are not shown in Table 3 for simplicity. Standard errors are clustered on race and education-level, as before, these were the chosen variables to stratify the sample. The coefficient for the treatment condition is significant ($p < .01$) and positive in the base logit

**Table 3 Study 1.b Regression Results.**

| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| (Intercept) | $1.55^{***}$ | $1.55^{***}$ | $1.57^{**}$ | $1.57^{**}$ |
| | (0.39) | (0.36) | (0.43) | (0.43) |
| Treated | $0.34^{**}$ | $0.34^{***}$ | $0.38^{**}$ | $0.38^{***}$ |
| | (0.13) | (0.07) | (0.13) | (0.06) |
| Indiv. Variables | ✓ | ✓ | ✓ | ✓ |
| County FE | | | ✓ | ✓ |
| Physician FE | | | ✓ | ✓ |
| Clustered SE | | ✓ | | ✓ |
| Num. obs. | 1800 | 1800 | 1800 | 1800 |
| Deviance | 2037.01 | 2037.01 | 1934.87 | 1934.87 |
| Log Likelihood | −1018.51 | −1018.51 | −967.43 | −967.43 |
| Pseudo $R^2$ | 0.15 | 0.15 | 0.16 | 0.16 |
| Num. groups: idCounty | | | 40 | 40 |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Standard errors are clustered at race and education-level. Individual demographic and health variables are included in the model, but their coefficients are similar to Study 1.a’s Regression results.

specification. Once we include county-level and physician fixed effects, the treatment coefficient is highly significant at $p < .001$. The estimates indicate that respondents exposed to the default-choice and prompting nudges are 1.46 ($e^{0.34}$) times more likely than those in the control condition to book their appointments and visit their local PCP.

As before, we include a robustness check using the probit specification, and find no changes in the significance of the treated coefficient, nor their sign and magnitude.

# 4. Study 2: Impact on Preventable Hospitalizations

## 4.1. Identification Strategy

I am interested in measuring the causal effect of the 3rd party provider's implementation of nudges on preventable hospitalizations. Identification of the causal effect relies on the assignment of treatment in counties that are similar in characteristics. If the counties are not comparable, the concern is that there are exogenous factors that affect the rate of preventable hospitalizations other than the treatment, leading to biased estimates.

We address this endogeneity concern by randomly assigning treatment to forty counties in Montana and South Dakota, where the sample is stratified on unemployment rate and mean prevention quality. Montana is the treatment state of interest, where all counties selected in South Dakota are randomly assigned to control. The reason for this is two-fold: (1) South Dakota and Montana are similar in characteristics; both having a large proportion of rural inhabitants, similar employment, income, and education-level statistics, and they share a border, and (2) the research design provides us a level of control to ensure that there are not spillovers. One of the key assumptions that we make when estimating the difference-in-difference is that there are no spillover effects between treated and control counties. We are confident that this assumption holds, as the service provider had not previously operated in either state prior to treatment, and physicians must be registered through the provider in order to appear on the website. Thus, South Dakota should not have any physicians populating the provider's website. There may be concerns over patients traveling out of state to use the service, but, because the provider collects addresses of all patients when booking appointments, we were able to refute any evidence of cross-state travel. The second key assumption that we make for our difference-in-difference design is that the treated and control counties share parallel trends in their rates of preventable hospitalizations prior to treatment. To assess the validity of this assumption, I explore the trend pre-treatment and post-treatment in Figure 5.

To account for any time-invariant differences between the treated and control counties, we include a set of county-level fixed effects. A challenge to the identification strategy would be if there were any exogenous shocks in the counties under stud, such as hospital closures or new prevalence of disease that serve as a catalyst to future preventable hospitalizations. We include time-period fixed effects index at the county-level to absorb any differences in medical outcomes that are correlated with the utilization of the service and rate of preventable hospitalizations.

Finally, we must address the question of external validity. The difference-in-difference design allows us to estimate the causal effect of the service for a narrow sub-population of the United States. One might expect that rural residents of South Dakota and Montana differ significantly from rural residents elsewhere in the country, say Alabama. Additionally, it would be difficult to extend these findings to the total US population inclusive of suburban and urban inhabitants.

When drawing conclusions from these findings, recognize that the results may not translate to a more representative sample.

### 4.2. Methodology.

**4.2.1. Data** Data were collected from various sources. Preventable hospitalizations, incidence rates, and other medical statistics were collected by county from the HCAI Patient Discharge dataset. Prior to treatment, the mean rate of preventable hospitalizations for treated and control counties were 112.97 ($\sigma_T = 10.507$) and 114.121 ($\sigma_C = 6.154$) respectively. County-level statistics were gathered from the United States Census, including employment statistics, education levels, and rates of economic growth. Finally, we combined these measures with the existing data from the Social Determinants of Health (SDOH) database, outlined above.

**4.2.2. Randomization Procedure** As mentioned in the identification strategy, counties were randomized into treatment with eight counties in South Dakota comprising the control group were matched with 32 randomly sampled counties in Montana on similar characteristics. These samples were stratified on prevention quality and unemployment rate.

### 4.3. Model and Results

**4.3.1. Descriptive Analysis** Let $H_{ict}$ denote the (log) count of preventable hospitalizations in county $c$ at time $t$. We use the log for Ordinary Least Squares estimation, as the count follows a poisson distribution. Let $T_p$ represent the pre-treatment periods, where $p \in \{0, 1, 2\}$. $D_{ct}$ is a dummy variable denoting treatment status at time of intervention, given by:

$$D_{ict} = \begin{cases} 1 & \text{if } i = 1 \text{ and } t > T_p \\ 0 & \text{otherwise} \end{cases} \tag{4}$$

Suppose that we have,

$$\mathbb{E}[H_{0ct}|c, t, D_{ct}] = \mathbb{E}[H_{0ct}|c, t] = \gamma_c + \lambda_t \tag{5}$$

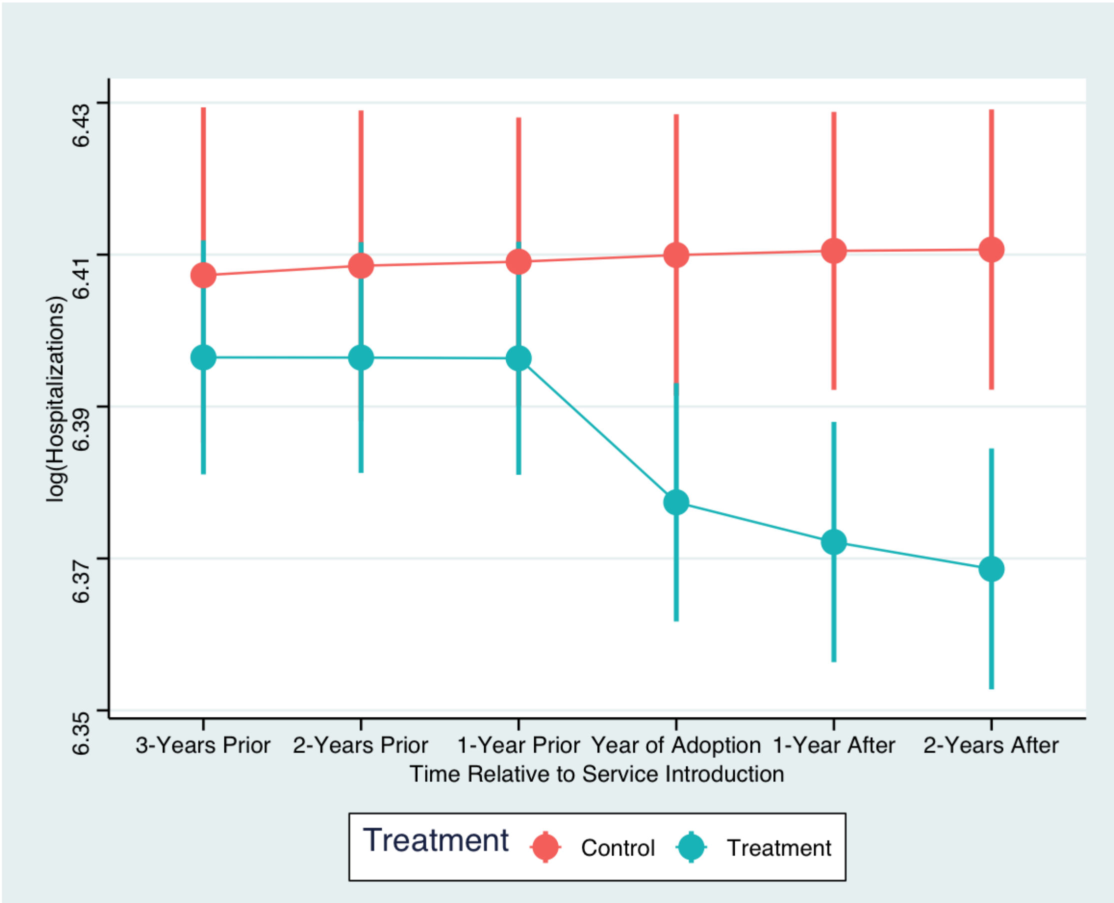
$$\mathbb{E}[H_{1ct} - H_{0ct}|c, t, D_{ct}] = \delta \tag{6}$$

$$H_{ict} = \gamma_c + \lambda_t + \delta D_{ct} + \epsilon_{ict} \tag{7}$$

Where $\gamma_c$ captures the county effects, $\lambda_t$ captures the time effects, and $\epsilon_{ict}$ is the error term with $\mathbb{E}[\epsilon_{ict}|c, t] = 0$. The causal parameter of interest, $\delta$, is obtained by the analog of sample means. In other-words, we are measuring the average treatment effect on the treated with the assumption that, in the counterfactual scenario where no counties are exposed to treatment, unobserved differences are constant overtime, and $\delta$ captures the ATT. The sample difference-in-differences is given by:

$$\begin{gathered}(\mathbb{E}[H_{ict}|c \in MT_c, t = 2019] - \mathbb{E}[H_{ict}|c \in MT_c, t = 2022]) - \\ (\mathbb{E}[H_{ict}|c \in SD_c, t = 2019] - \mathbb{E}[H_{ict}|c \in SD_c, t = 2022]) = \delta\end{gathered} \tag{8}$$

**Figure 5** **Trend of Preventable Hospitalizations**




*Note.* Hospitalizations represents the log of preventable hospitalizations. At the end of period 2, the service provider began operating in treated counties. Parallel trends assumption is held.

**Table 4** **Difference-in-Difference**

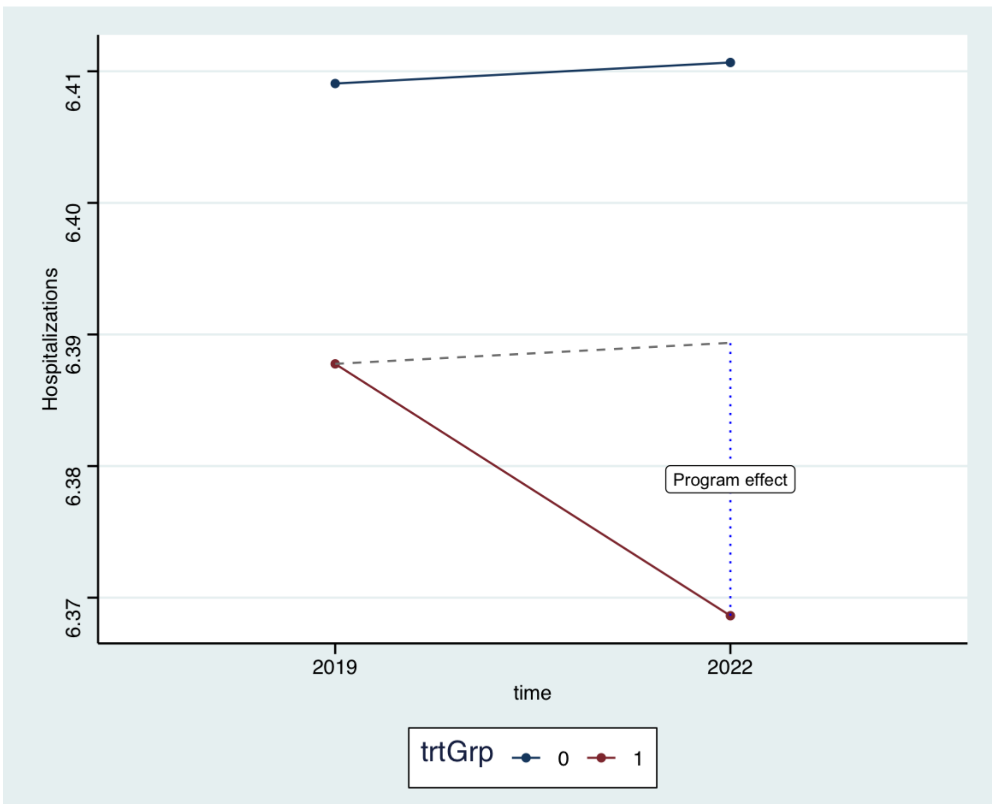
| | Treated | Control | Trt - Cont |
|---|---|---|---|
| Before | 6.396 | 6.409 | $-.013$ |
| After | 6.369 | 6.411 | $-.042$ |
| Change | $-.028$ | .002 | $-.029$ |

Sample analog of means. The difference-in-difference estimate is .029 reduction in preventable hospitalizations. Since we are working with logs, this is a 2.9% reduction.

where $MT_c$ and $SD_c$ are the sets of counties assigned to treatment and control conditions, respectively.

The non-parametric results 4 indicate a reduction of (log) preventable hospitalizations, with the treated counties having 12.25 fewer hospitalizations per year. This might seem an insignificant amount, but this reflects a 2.9% reduction of preventable hospitalizations in treated counties. In a hypothetical scenario where these results extend to the entire US Population, using the 2017 figure in McDermott and Jiang (2020), the implementation of time-proximity and default-choice nudges in appointment providers would result in a \$989.02 million reduction in costs incurred on hospitals. Even using a significantly conservative estimate of .0029%, this would still result in a reduction of \$9.9$M$ USD. Meanwhile, the service provider incurred costs of \$25$k$ for website design and programmatic alterations, recovering costs with-in 3-months of launch. Unfortunately we do not have access to statistics for individual hospital costs, so we cannot calculate figures for the total reduction of costs attributable to the treatment.

**Figure 6** **Effect Size**




**4.3.2. Regression DiD** Next we estimate the causal effect of the treatment using a regression difference-in-differences design. We begin our model similarly to before, with,

$$\mathbb{E}[H_{0ct}|\varsigma_c, \lambda_t, c, t, D_{ct}] = \mathbb{E}[H_{0ct}|\varsigma_c, \lambda_t, c, t] \tag{9}$$

$$\mathbb{E}[H_{1ct} - H_{0ct}|\varsigma_c, \lambda_t, c, t, D_{ct}] = \delta \tag{10}$$

where $\varsigma_c$ represents the time-invariant county fixed-effects, $\lambda_t$ denotes time-varying market effects, $\alpha$ is a county-specific intercept pre-treatment, and $\delta$ is still the causal parameter of interest. Our model follows cleanly as,

$$H_{ict} = \alpha + \varsigma_c + \lambda_t + \delta(D_{1ct} \cdot \tau) + \epsilon_{ict} \tag{11}$$

**Table 5 Study 2 Regression Results.**

| | Model 1 | Model 2 |
|---|---|---|
| (Intercept) | $6.38^{***}$ | $6.38^{***}$ |
| | (0.004) | (0.004) |
| Time | 0.002 | 0.002 |
| | (0.004) | (0.001) |
| Treated*Time | $-0.021^{***}$ | $-0.021^{***}$ |
| | (0.004) | (0.001) |
| County FE | ✓ | ✓ |
| Time FE | ✓ | ✓ |
| Clustered SE | | ✓ |
| Num. obs. | 240 | 240 |
| $R^2$ (full model) | 0.96 | 0.96 |
| $R^2$ (proj model) | | |
| Adj. $R^2$ (full model) | 0.96 | 0.96 |
| Adj. $R^2$ (proj model) | | |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Standard errors are clustered at unemployment rate, as this variable was used to stratify the sampling of counties

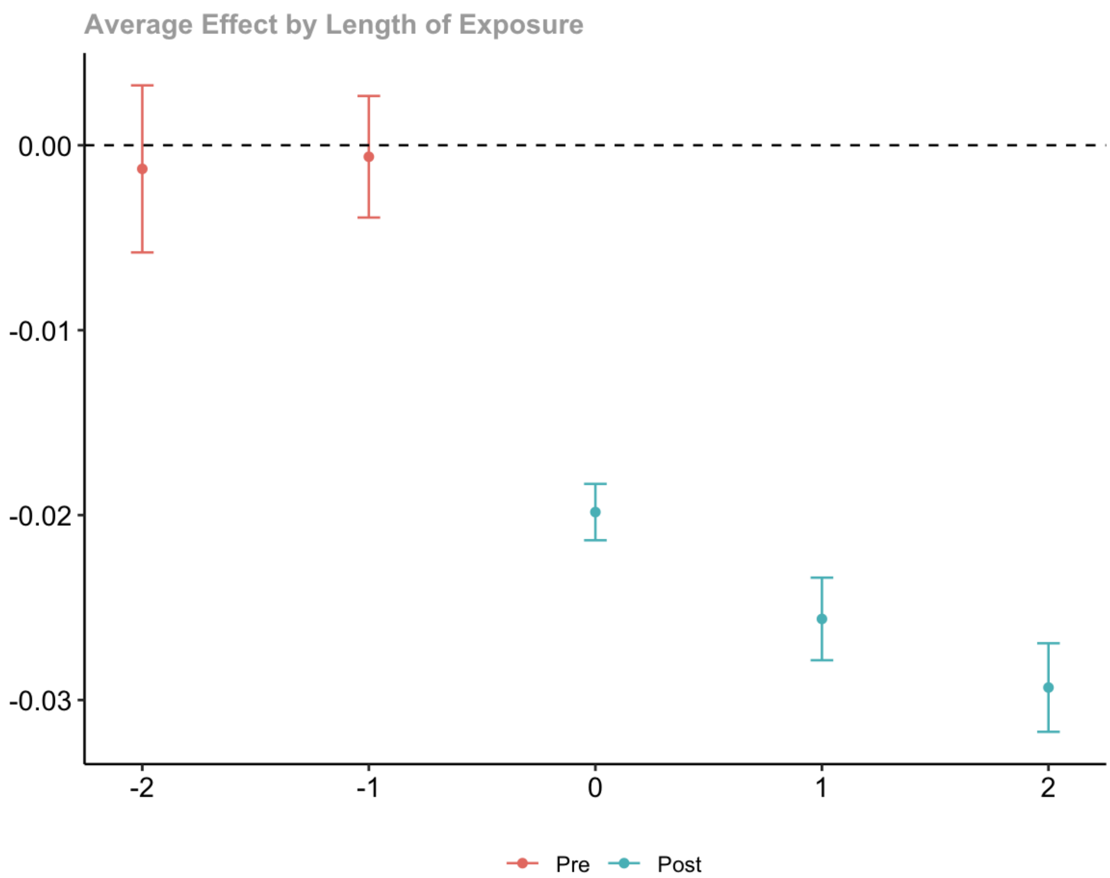
$D_{1ct}$ denotes the treated at $(t_0 + 2)$, and $\tau$ is a dummy variable for the year 2022. A summary of regression results are reported in Table 5, including a second model that clusters standard errors around unemployment rate and prevention quality. We see that the $\delta$-coefficient is highly significant ($p < 0.001$), although the magnitude of the coefficient is smaller than our model-free results. This could be the time and county-level fixed effects absorbing any unobserved differences in the treated and control counties. Using the same function of hospital costs as above, a 2.22% reduction in preventable hospitalizations would result in a $\$779M$ decline in hospital costs per year across the United States.

**4.3.3. Robustness Checks** Note, however, that in Figure 5, we see a steep decline in hospitalizations immediately following the initial treatment period, with diminishing, monotonically-decreasing effects in the subsequent periods. This suggests that the effect initially significantly reduces preventable hospitalizations with a leveling-off effect for periods $t + 1$ and $t + 2$. Angrist and Pischke (2008) suggest that the following approach when estimating the causal effect within a diff-in-diff design that has multiple time periods is preferable,

$$H_{ict} = \alpha_c + \varsigma_c + \lambda_t + \sum_{\tau=0}^{m} \delta_{-\tau} D_{c,t-\tau} + \sum_{\tau=1}^{q} \delta_{+\tau} D_{c,t+\tau} + \epsilon_{ict} \tag{12}$$

where the first sum allows for $m$ lags and the second allows for $q$ leads in time-periods. This model specification will allow us to measure whether the causal effect fades over time. The results outlined in Table **??** show that there is a sharp reduction of preventable hospitalizations immediately following the introduction of treatment ($\hat{\delta} = -0.020$), which is similar to our estimate for the treatment effect in the reduced model, and in subsequent periods the ATT monotonically-decreased at a lesser rate, until we reach the causal effect produced in our non-parametric model. These effects are visualized in Figure 7.

**Figure 7** **Lagged ATT on Preventable Hospitalizations**

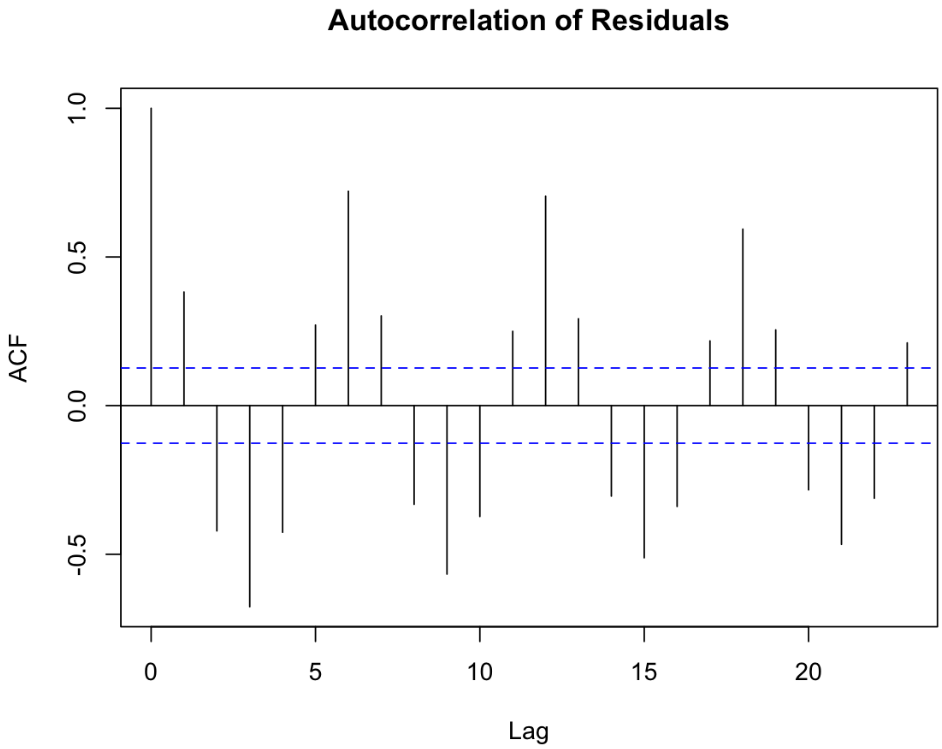



Bertrand, Duflo, and Mullainathan (2004) suggest that standard errors are inconsistent for difference-in-difference estimators when there are multiple periods, as the outcome and predictors may be serially correlated between periods. One solution is outlined above, where we aggregate the time-series panel data to county-year levels. We see in Table 7 that standard errors are inflated compared to our base OLS specification. Before we check the other solutions, let's test for serial correlation of our residuals using Wooldridge's method,

$$H_{ict} - H_{ic,t-1} = \alpha + \varsigma_c + \lambda_t + \delta(X_{ict} - X_{ic,t-1}) + \epsilon_{ict} - \epsilon_{ic,t-1} \tag{13}$$

The test rejects the null-hypothesis, $H_0$, of the absence of serial correlation (F-Stat: 860.92, $p < .001$), and we can visualize the autocorrelation of residuals in the figure below.

**Figure 8**




*Note.* Autocorrelation of residuals in base OLS specification.

One potential solution is correction using $AR(1)$, but this has been found to be ineffective for few periods (Bertrand et al. 2004). Another option would be adjusting standard errors using the empirical variance covariance matrix, but this only works under the assumption that the cross-panel data is homoskedastic, which we violate. Instead, we will generate an arbitrary variance-covariance matrix with random effects, given by,

$$W = (V'V)^{-1}\Big(\sum_{j=1}^{N} u_j' u_j\Big)(V'V)^{-1} \tag{14}$$

$$u_j = \sum_{t=1}^{T} e_{jt} v_{jt}, \tag{15}$$

where N is the total number of counties, $V$ is the matrix of independent variables, and $u_j$ is the estimated residual. This specification produces inflated standard errors relative to the base specification.

As a final robustness check, we bootstrap the aggregated lagged OLS regression with 10,000 iterations and perform doubly robust estimation. These produce the least inflated standard error estimates (0.001). Coefficients for each model are reported in Table 6, the boostrapped sample is not reported.

**Table 6 Robustness Checks**

| | (1) OLS | (2) Arbitrary Variance Covariance |
|---|---|---|
| Intercept | $6.380^{***}$ | $6.398^{***}$ |
| | (.0039) | (.0075) |
| time | 0.0016 | .0018 |
| | (.0011) | (.0028) |
| Treated*Time | $-0.0207^{***}$ | $-0.0251^{***}$ |
| | (.0015) | (.0031) |
| Num. obs. | 240 | 240 |
| $R^2$ (full model) | 0.96 | 0.014 |
| $R^2$ (proj model) | | |
| Adj. $R^2$ (full model) | 0.96 | 0.014 |
| Adj. $R^2$ (proj model) | | |

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

## 5. Policy Implications.

Health access in rural geographies, historically and presently, represents a huge strain on rural health institutions and residents. Disparities in healthcare are affected by a complex set of institutional controls, including lack of funding, greater distances to local primary care physicians, and psychological barriers at both the physician and patient level. Policy-makers and researchers must address these disparities using a variety of techniques, including thinking outside of traditional policy and medical-related measures.

If health utilization in rural communities were moderated by the perception of access and spatial distance, policy-makers could use marketing and economic tools to improve health outcomes at a low-cost. The initial findings in this paper suggest that, if we were to loosen the external validity assumption, modifying the choice architecture of rural residents would result in a $989.02 million reduction in preventable costs on hospitals at virtually no-cost to government institutions. In addition, we provide evidence that these nudges improve health outcomes in terms of preventative care.

Additional studies should be implemented to assess the efficacy of this study across sub-populations of both rural and urban Americans, as well as measure the impact on a variety of health outcomes.

## 6. Limitations.

This study is limited in that we are only concerned with the effect of nudges on primary care utilization rates in rural American communities. The external validity of the results can only be reasonably applied to a small sub-population of American individuals, and it would be difficult to extend the findings to urban and suburban populations. The author suggests that follow-up studies are required to measure the effect on a more representative sample, in order to gather precise estimates for the total population and to measure the financial impact on health institutions per dollar spent. Additionally, we are only concerned with the causal effect of nudges, with observable covariates. Additional studies should be implemented to consider the psychological and geographical differences between urban and rural residents in terms of health access. For example, Coombs et al. (2022) suggests that rural residents are distrusting of primary care, holding beliefs that physicians will judge their ways of life or that they will be seen as "lesser" individuals if they engage in utilization. We make no claims to address all determinants with respect to health access in these communities.

Additionally, our sample is relatively limited to a small sub-population of rural residents in America. Future studies should scale the sample to cover a more representative sample of rural residents across states. This should easily be achievable using the 3$^{rd}$ party providers customer-base, where traditional A/B testing designs are used, rather than a survey-design.

## 7. Conclusion.

In this paper, we find that there are significant increases in utilization rates of local primary care physicians in rural communities by prompting residents with proximity in terms of travel time, as well as providing them with default-choice and availability prompts. We find that potential patients are 1.7 times as likely to schedule appointments for primary care physicians if they are exposed to travel-time metrics at time of scheduling. Respondents were 1.46 times as likely to schedule compared to the time-only condition when they were provided with a default-choice and asked to fill out an information form with their availability.

A difference-in-difference design was used to measure the causal effect of these modifications to the 3$^{rd}$ party providers website on treated counties in Montana, with respect to preventable hospitalizations. The findings suggest that there were a significant reduction in hospitalizations after the first year of intervention, with diminishing effects in subsequent periods. Additional studies should be implemented to measure the effect in future years, as the effect may be limited to an to the three-year span before leveling-off.

The study aimed to address the perception of health-access in rural communities, and whether it was a true disparity of access, or the result of information-asymmetry between residents and providers. Again, the author makes no claims to definitively answer this research question, and recognizes that there are indeed many externalities that affect healthcare in these geographies. However, we as researchers should explore economic and psychological alternatives to boost healthcare utilization in regions that historically are under-served and having high-rates of negative health outcomes. The goal of this paper was to provide one such panacea.

## Acknowledgments

I'd like to thank my professors, Anna Tuchman and Eric Anderson, for assigning this project. It is an enlightening process to start the research process with simulated data, and it has inspired me to collect real data to see this project through to completion. I would also like to thank my medical colleagues for humoring my research questions and helping with assessing the validity of my ideas.

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