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Author
Root, Allyson Barnett

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Essays in Health Economics

by

Allyson Barnett Root

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Committee in charge:

Associate Professor Michael L. Anderson, Co-chair
Associate Professor Benjamin R. Handel, Co-chair
Associate Professor Jonathan T. Kolstad
Associate Professor Aprajit Mahajan

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Abstract

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The first chapter, co-authored with Season Majors, Christopher Connolly, Mary Ann Friesen and Hassan Ahmed, studies how electronic blood glucose monitoring impacts physician and patient behavior. Recent technological development has led to increased availability of patient generated health data, which has the potential to influence medical treatment and health outcomes. However, it is not well understood how to most effectively integrate this new technology and data into large health systems. We conducted an experimental evaluation of multiple approaches to increase utilization of electronic blood glucose monitoring, among 7,052 patients with diabetes at 20 primary care practices. A physician education intervention successfully increased provider take-up of an online blood glucose monitoring tool by 64 percentage points relative to control, while a comparison of patient-focused reminder interventions revealed that emphasizing accountability to the provider was most successful at encouraging patients to actively track their blood glucose online. An assessment of downstream outcomes also revealed impacts of the interventions on prescribing behavior and A1c testing frequency. We interpret these results in the context of a conceptual framework in which patient generated data can affect patient behavior directly, and may also influence physician treatment decisions by acting as a complement or substitute for traditional health data sources.

In the second chapter, I study the effects of Medicaid and other means-tested benefits on immigrants' health outcomes, health care utilization, financial outcomes, and remittance behavior. The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996 bars most legal immigrants from receiving social benefits such as Medicaid and SNAP for their first five years of residency in the United States. I exploit this discontinuity in benefit eligibility to estimate the causal impact of means-tested benefits using regression discontinuity and difference-in-differences approaches. I find evidence for decreased savings and increased use of the emergency department as a result of gaining eligibility for Medicaid and SNAP.

In the third chapter, co-authored with Benjamin R. Handel, we conduct a randomized evaluation of strategies to facilitate advance directive (AD) completion among 4,850 patients aged 65 and over. Despite the significant economic and personal implications of end-of-life healthcare decisions, many fail to document their wishes or to select a representative who can make medical decisions on their behalf. We evaluate the effects of (i) an in-person drive to facilitate AD completion and (ii) electronic distribution of an informational video discussing advanced care planning. Among patients to whom communication was sent via email, we find no effect of in-person AD drives or of the informational video on AD upload rates. However, we estimate a 4.5 percentage point increase in AD uploads for patients who were contacted via letter about the AD drive, relative to patients who were sent a reminder letter only. This suggests that in-person drives may be impactful for increasing AD completion, but only if effectively advertised to patients. We also leverage surveys and granular data on patient health to understand how information frictions and hassle costs may influence advance care planning decisions.

Contents

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Chapter 1

Electronic Blood Glucose Monitoring: Impacts on Physician & Patient Behavior

1.1 Introduction

The percentage of the US population with diagnosed diabetes increased from 4% to over 7% from 1999 to 2014 (CDC, 2017), with nearly $1 in $5 of health care dollars spent caring for people with diabetes (American Diabetes Association, 2013). There is substantial evidence that improved average blood sugar control (as measured by A1c levels) is associated with significant decreases in the probability of complications from diabetes (American Diabetes Association, 2018). Commercially insured patients with type II diabetes who lower their A1c, blood pressure and lipid levels, experience significant reductions in total medical costs (Fitch et al., 2013). Recent research also suggests that reduction in blood glucose variability is associated with reduced risk of complications and mortality independently of average blood glucose and A1c (Cavalot et al., 2006; Sorkin et al., 2005).

For patients who are insulin-dependent, self-monitoring of blood glucose (SMBG) is a critical aspect of disease management and regulation of blood glucose levels and variability. The DCCT Research Group (1993) compared intensive insulin therapy guided by frequent blood glucose monitoring to conventional insulin treatment in a landmark randomized controlled trial. Intensive therapy with SMBG delayed the onset and slowed the progression of diabetic retinopathy, nephropathy and neuropathy in insulin-dependent patients.

However, for non-insulin dependent patients with type 2 diabetes, there has been some debate over the value of self-tracking. The ASIA randomized controlled trial of 689 patients over a period of 24 weeks found that patients assigned to perform 6 SMBG measurements per week had a statistically significant 0.3 reduction in A1c (intent-to-treat) after 6 months (Guerci et al., 2003). However, the DiGem randomized trial found that SMBG without additional training had no effect on A1c after 1 year (Simon et al., 2008). A meta-analysis

of 12 randomized controlled trials evaluating SMBG found a statistically significant mean reduction in A1c of 0.3 for studies with 6-month follow-up (effect sizes in individual studies ranged from .07 to .69), but no statistically significant change in A1c for studies with a 12 month follow up (Malanda et al., 2012). However, the review was criticized for including few studies with 12-month follow-up (two, one of which had only 22 subjects). A more recent meta-study was updated to include new randomized controlled trials, finding a somewhat larger statistically significant reduction in A1c at 6-month follow-up (-.36), and a statistically significant reduction in A1c at 12-month follow-up (-.28) (Zhu et al., 2016).

Two key factors in enhancing the effectiveness of SMBG seem to be patient adherence and physician involvement (Clark, 2007). In Polonsky et al. (2011), structured SMBG was compared to enhanced standard care for 483 patients with type two diabetes who were insulin-naive and had poor blood glucose control. The analysis revealed much larger effects for patients who adhered to the intervention (-0.5 A1c change). Additionally, patients in both the treatment and control group of this intervention were assigned to quarterly office visits, with structured SMBG assigned patients instructed to bring tracking data to consultations with their physician. Availability of this data encouraged primary care physicians to treat glycemia earlier, more frequently, and more effectively. Significantly more patients assigned to structured SMBG group received recommendations for a treatment change as compared with control subjects. These findings highlight the key role that physician engagement with SMBG data plays in its effectiveness.

Nearly all randomized controlled trials of SMBG have had patients monitor blood glucose using either pen and paper or storing on a monitoring device, both needing to be physically brought to an office visit for physician viewing. As emphasized above, physicians play an important role in interpreting blood glucose trends, but likely do not have access to this patient generated data between office visits. Though technology to electronically transmit blood glucose readings is available, it is not widely used as a standard practice of care. The TELEDIAB-1 study piloted the Diabeo system (a smartphone coupled to a website) which incorporated automated advice on insulin dosing and remote monitoring by tele-consultation. Use of the system improved A1c by 0.9% compared to control in patients with chronic, poorly controlled type 1 diabetes (Charpentier et al., 2011). Other small-scale controlled studies of remote blood glucose monitoring have found similar positive effects on patient A1c; researchers have posited increased patient-provider communication resulting in better treatment and adherence as a primary mechanism (Spearson and Mistry, 2016).

However, there are few examples of integration of such technologies into electronic medical record (EMR) systems in a manner that would allow for wide-scale use. One study demonstrated the feasibility of automatically sending data from continuous glucose monitors to EMR patient portals for physician viewing but did not test the impact of this on patient outcomes (Kumar et al., 2016). Other larger-scale observational studies have compared health outcomes of patients who use online patient portals with non-users, but these comparisons likely suffer from confounding un-observable factors that make it difficult to infer a causal relationship (Devkota et al., 2016; Ronda et al., 2015). To our knowledge, there is no randomized trial or prior research testing the causal impact of integrating data from

patient blood glucose monitoring into EMRs on a wide-scale. The first focus of this paper will be to report on a large randomized controlled trial of a practice level intervention to promote use of electronic blood glucose tracking functionality among physicians of patients with diabetes.

Many of the studies discussed above show a correlation between adherence and reduction in A1c, suggesting that SMBG is most effective when patients track regularly. The second goal of this study will be to estimate the effect of different reminder messaging approaches aimed at increasing patient use of electronic blood glucose flow sheets. Previous research has shown doctor patient communication is predictive of adherence (Friedman et al., 2008), so patients may feel more accountable if they anticipate their physicians will be looking at the results. One reminder message variation tested emphasizes physician engagement and monitoring of flow sheet entries. Past research in other contexts has also shown higher adherence to patient driven behaviors when compensation is provided (Roski et al., 2003). Another variation offered patients the chance to receive a gift card for every flow sheet entry completed. A total of four reminder treatment arms will be considered: (1) no reminder, (2) a generic reminder addressed from the medical group, (3) a physician accountability reminder addressed from the patient's physician and (4) a reminder addressed from the medical group with chance to win a gift card.

We combine and test these physician and patient-focused approaches to encouraging use of electronic blood glucose tracking, evaluating impacts on both utilization of the technology and downstream patient health and treatment outcomes. We find that the practice intervention was successful at increasing electronic flow sheet use, and that the version of reminder messaging focused on accountability to the provider was most successful at getting patients to track. Though we find no change in patient health as measured by A1c, we do observe significant downstream reductions in medication changes and prescription orders for both patients in treated practices (as compared to control practices), and patients assigned to the physician accountability reminder group associated with more intensive tracking (as compared to the no-reminder group). For patients in the physician reminder group, we also see slightly lower rates of A1c testing. We interpret these results in the context of a conceptual framework in which patient generated data can affect patient behavior directly, and may also influence physician treatment decisions by acting as a complement or substitute for traditional health data sources.

The remainder of this chapter is structured as follows: Section 1.2 outlines a conceptual framework. Section 1.3 describes the data and setting. Section 1.4 describes the study design and empirical approach. Section 1.5 reports the results of the evaluation and Section 1.6 concludes.

1.2 Conceptual Framework

Physicians traditionally use lab testing and other in-office evaluations to gather information about patient health and to determine optimal treatment plans. Patient generated health

data (PGHD) has the potential to act as both a complement and substitute to these established data sources. Wearable technology and other electronic tracking devices may directly replicate certain traditional medical tests and evaluations (e.g. EKGs (Coravos et al., 2019)). In other cases, PGHD captures different indicators and/or higher frequency data than lab testing (El-Amrawy and Nounou, 2015). In addition to acting as an additional data source for physicians, PGHD may also have a direct impact on patient health by improving patient lifestyle decisions. Tracking steps, sleep patterns, etc, gives patients direct, salient feedback about their adherence to lifestyle goals, which could theoretically motivate improvement.

Self-monitoring of blood glucose by patients with diabetes is potentially a unique case study of the impact of PGHD on patient health. In contrast to other types of patient generated data, it has already been established that tracking blood glucose has a positive (if modest) impact on patient health (Zhu et al., 2016). Additionally, it is known to be a substitute for a specific traditional lab test. A1c tests are ordered frequently for patients with diabetes, and indicate average blood glucose over the the last 1-2 months. For example, average blood glucose measurements of 190 would lead to an A1C of approximately 8.2 (Accu-Check, 2019). If a patient were tracking blood glucose consistently and reliably, and provided all of this information to his physician, there would be no reason to order an A1c test. If anything, the higher frequency data offers additional information on blood glucose variability and responds more quickly to new treatments and behaviors in comparison to A1c tests (Cavalot et al., 2006).

We consider three key categories of outcomes that may be influenced by the introduction of electronic blood glucose tracking: (1) A1c testing frequency, (2) patient treatment (medication changes and prescription orders), and (3) patient health (A1c test outcome). We assume that physicians seek to maximize patient health, and that holding health constant, prefer to forgo unnecessary treatments. Given this, predicting the effect of electronic monitoring on testing frequency is relatively straightforward; if the physician believes a specific patient is tracking reliably and that the data conveyed is a substitute for A1c test value, the expected impact on testing frequency is negative.

The question of how a patient's treatment (specifically his prescription medications) will be impacted is more complicated. If tracking data provides physicians with new information, this could update their assessment of optimal treatment, leading to changes or additions to medications. However, electronic tracking could also result in fewer medications and changes. As discussed, tracking blood glucose has been shown to have a positive impact on patient health, i.e. lower A1c test results. For patients with type II diabetes who don't take insulin, (more than 2/3 of patients with diabetes), tracking facilitates better management of blood sugar through diet and exercises.

Guidelines for treatment of patients with type II diabetes indicate that progression of treatment, including decisions to change or add medication, should be made primarily on the basis whether the patient's A1c target has been met (American Diabetes Association, 2018). There is some disagreement among recommendation bodies over whether the target should be for patients to achieve an A1c below 7, or whether an A1c between 7 and 8 is adequate (Qaseem et al., 2018). However, most practitioners agree that once an A1c cutoff

has been met, it is not beneficial to add additional pharmacological treatments in attempt to achieve even lower A1c. American College of Physician 2018 guidelines even recommend that pharmacologic therapy be de-intensified for patients with A1c levels less than 6.5 (Qaseem et al., 2018). This suggests that if engaging in SMBG decreases A1c as previous research has shown, it may act as a substitute for pharmacological treatments. However, patients can engage in SMBG non-electronically and see these benefits. Thus, we would only expect to observe this effect resulting from introduction of electronic tracking if the information sharing aspect increased the likelihood that patients track and/or improved lifestyle management in comparison to non-electronic SMBG.

The expected impact on patient health is also somewhat unclear– as discussed, electronic tracking could improve blood glucose control if it provides physicians with information which allows them to prescribe a better treatment plan, or if it's more effective at shifting patient behavior than non-electronic tracking. However, even if this is true, we might not expect to see a large sustained discrepancies in A1c test values between patients who use electronic tracking as compared to patients who do not, if the latter are more likely to be given additional pharmacological treatments as a substitute.

1.3 Data and Setting

This study was conducted at selected primary care offices part of a large health system in Northern Virginia. The health system's EMR patient portal, MyChart, has a feature through which patients can track their blood glucose electronically, allowing physicians to view entries in real time and be notified of out-of-range results. Within the year prior to the study initiation, functionality was also integrated to connect Apple's HealthKit to MyChart, so that patients with compatible glucometers can now link them to their smartphones, which can in turn be linked to MyChart to automatically transfer glucometer readings to the EMR. This update streamlines the tracking process for patients with compatible devices. Patients without a special glucometer can still easily enter readings through the MyChart app or website.

However, in order for patients to use these flow sheets, it is necessary for their primary care physician to place an order through the EMR system. At baseline, 0.1% of patients in the study sample had an open order allowing them to track their blood glucose using the flow sheets. Anecdotal evidence suggested that many physicians were not aware of the relatively new blood glucose tracking features in MyChart or how to set up tracking. This is in line with recent research concluding that information frictions are a key barrier to updating convention across medical practices (Chan, 2016).

Datasets were compiled monthly from EMR data collected as a normal part of care. No supplementary testing or surveys were conducted as a part of this study. Eight datasets described in Appendix Tables A.1 and A.2 were produced monthly from January through October 2018. Additionally, a dataset listing active medications associated with a patient's most recent encounter includes encounters going back to July 2017. A file corresponding

physician IDs to practices, treatment assignment, and practice size strata used for random assignment of practices was also merged into the data described above.

1.4 Study Design and Empirical Approach

The total study sample consisted of 7052 patients of 68 physicians at 20 selected primary care offices. Adult patients of physicians at these sites with a current diabetes mellitus diagnosis and active MyChart account at time of treatment administration were included in the study. All communications and data collection took place through MyChart, the electronic medical record patient portal. Physicians were instructed to exclude from the interventions any individual patients whom they identify as having contraindications for tracking of blood glucose (though in practice few patients were excluded).

Provider Assignment and Intervention

The provider-focused intervention was cluster randomized at the practice level at 1:1 allocation. Randomization stratified across practices by number of patients with diabetes (cluster size), dividing practices into five strata of four practices each. A random number generator from the statistical software R was used to assign two practices per strata (50%) to the treatment or control arm at the outset of the study. Table 1.1 conducts balance tests, running Specification 1 on pre-treatment baseline covariates (with no controls) and reporting coefficient β_1 which tests for difference across the assigned treatment and control practices. The patients in the treatment and control practices appear to be well balanced across these measures. The average patient age in the control group was 58.9 (SD=14.0), and mean baseline A1c was 7.2 (SD=1.6), which is slightly above the cutoff of 7 for optimal blood sugar control.

Balance on Pre-Treatment Covariates, Treatment and Control Practices					
Covariate	Control Mean	Control SD	Diff	SE	P
Age	58.863	(14.008)	-.049	(1.616)	.976
Male	.538	(.499)	.013	(.024)	.603
Ethnicity, White Non-Hispanic	.799	(.401)	.005	(.029)	.873
A1c, Baseline	7.218	(1.587)	-.011	(.055)	.849
Days Since Last A1c Test, Baseline	188.607	(190.291)	7.225	(11.26)	.539
Days Since Last Appointment, Baseline	147.858	(187.32)	7.728	(8.176)	.372
Completed Flow Sheet, Last 14 Weeks at Baseline	.001	(.033)	-.001	(.001)	.601
Number of Patient Message, Last 14 Weeks at Baseline	1.381	(2.819)	.117	(.144)	.438
Number of Phone Appts, Last 14 Weeks at Baseline	.896	(1.922)	.009	(.157)	.956
Number of In-Person Appts, Last 14 Weeks at Baseline	1.415	(3.068)	-.008	(.113)	.944
Medication List Changed, Last 14 Weeks at Baseline	.222	(.416)	-.015	(.018)	.417
Medication Removed, Last 14 Weeks at Baseline	.071	(.257)	-.009	(.008)	.281
Medication Added, Last 14 Weeks at Baseline	.217	(.412)	-.014	(.017)	.444
Number of RX Orders, Last 14 Weeks at Baseline	6.303	(20.279)	-.518	(.493)	.323
Number of New RX Orders, Last 14 Weeks at Baseline	6.298	(20.229)	-.518	(.493)	.323
Number of Diabetes RX Orders, Last 14 Weeks at Baseline	.86	(2.558)	-.053	(.074)	.49

Table 1.1: This table displays coefficients and standard errors for β_1 from equation 1.1 on pre-treatment covariates, which estimates the balance between treatment and control practices. Standard errors are in parentheses with p-values in the far right column ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

Primary care practices assigned to the control arm did not receive any intervention. Physicians at primary care practices selected for the treatment group were given information, encouragement, and assistance to batch order blood glucose flow sheets for all patients with diabetes with active MyChart accounts. The research team contacted physicians and practice managers with an explanation of the initiative and invited them to attend a virtual session to review instructions for completing batch orders and viewing entries through the system. These meetings were hosted by the research team and were scheduled according to physician availability within the first two weeks of the study. Following the virtual session, physicians had the option to request an in-person hands-on walk through of the order process. In this case, a member of the research team facilitated the batch order and showed the physician how to monitor patient entries. Providers were given the following template for a secure smart-text message to send to all patients receiving the flow sheets:

Dear [Patient],

You may have seen that there is a new electronic form available for you to enter your blood glucose results in the MyChart portal. Keeping track of your blood glucose can help you manage your diabetes and reduce your chances for developing complications like heart disease, kidney damage and eye damage. As part of a new [health system name] program to help patients with diabetes get healthy, I would like you to try entering the results of your home blood glucose tests on MyChart for the next 3 months. Please let me know if you have any questions.

Sincerely,

[Physician]

WHAT TO DO NEXT

1. Get help: If you need help learning how to measure your blood glucose or getting testing supplies, you can contact [Practice Contact].

2. Learn how: See [link to instructions/screenshots] to learn how to use the electronic blood glucose flow sheets on MyChart

3. Link your devices: You don't need to have special equipment to use the flow sheets, but if you have an Apple/iOS device and compatible glucometer, you can set up automatic data transfer. This will pull the results of your glucose tests into MyChart automatically, making it even easier to track. See [link to instructions] for help setting this up, and for a list of compatible devices.

4. Track your blood glucose: For the next 3 months, try to enter at least 6 measurements total per week, spread out over a few different days. However, don't be discouraged if you miss a day– even tracking occasionally could be beneficial to your health.

Note: If you've never tested your blood glucose before, get in touch with [Practice Contact] to make sure you have all the materials and instructions you need, and to confirm that testing is right for you. Patients who think they might be pregnant should also be sure to talk to a physician before starting any new tracking regime.

The batch flow sheet order also triggered two forms of alerts sent to providers, according to the default settings for electronic glucose flow sheets:

1. A bi-weekly report of values that the patient entered, or a notification that the patient did not enter data during the interval.

2. A notification when values are out of range (the patient would also receive a notification) according to the following rule:

Notification Setting	Current Default
Pre-Meal Range (entry outside range=alert)	60-180 mg/dl
Post-Meal Range	100-240 mg/dl
Nighttime Range	60-300 mg/dl

Patient Assignment and Intervention

After the initial two-week roll-out/administration period, follow-up emails were sent out by the research team to patients at treatment practices whose doctors placed flow sheet orders. Patients were assigned individually to one of four reminder treatment conditions

alphabetically by first letter of patient last name. Table 1.2 provides an overview of the sample and assignment structure.

Sample Description							
Practice Assignment	N Doc	N Doc Attend	N	L-Name Group	N	Reminder	N
Assigned Practice Treatment	34	23	3411	A-D	866	Gift Card	554
				E-K	888	Physician Accountability	573
				L-R	895	Basic	589
				S-Z	762	None	466
Assigned Practice Control	34	0	3641	A-D	926	N/A	0
				E-K	882	N/A	0
				L-R	1008	N/A	0
				S-Z	825	N/A	0

Table 1.2: This table describes the sample breakdown for the practice and reminder interventions.

Though it would have been ideal to assign patients to reminder groups randomly, it was logistically infeasible to send individual-level patient messaging without sorting on an existing field in the patient's EMR. There are some concerns that confounding factors such as ethnicity could correlate with assignment based on last name spelling, so this form of assignment is pseudo-random. Indeed, we test the balance of the pre-treatment covariates across reminder groups using Specification 2, and find that the "Basic" reminder group has a slightly lower baseline A1c and is slightly more likely to be white than the "No Reminder" group. All baseline covariates including both of these factors will be controlled for in reported analysis of reminder messaging. Causal interpretation of the results of this portion of the experiment will thus require the assumption that grouped last name spelling (controlling for pre-treatment covariates) is not independently related to likelihood of flow sheet adoption or any of the other outcomes. We also report placebo versions of all outcomes tested by re-running the analysis on these same last name letter designations but with patients in control practices, who were not sent reminders. Results for these falsifications tests can be found in Appendix Tables A.3-A.6.

Balance on Pre-Treatment Outcomes, Reminder Groups			
Covariate	TE, Basic	TE, GC	TE, Phys
Age	-.451 (.724) [.533]	-.128 (.729) [.861]	.876 (.725) [.227]
Male	.011 (.025) [.646]	.011 (.025) [.648]	.018 (.025) [.467]
Ethnicity, White Non-Hispanic	-.048 (.019) [.014**]	-.003 (.02) [.864]	.02 (.019) [.292]
A1c, Baseline	-.178 (.099) [.072*]	-.138 (.099) [.164]	-.16 (.099) [.105]
Days Since Last A1c Test, Baseline	11.884 (10.3) [.249]	-6.348 (10.346) [.54]	2.387 (10.278) [.816]
Days Since Last Appointment, Baseline	21.51 (9.887) [.03**]	7.569 (9.948) [.447]	9.774 (9.905) [.324]
Number of Patient Message, Last 14 Weeks at Baseline	-.201 (.165) [.223]	-.089 (.166) [.593]	.038 (.165) [.817]
Number of Phone Appts, Last 14 Weeks at Baseline	-.154 (.095) [.104]	.031 (.096) [.749]	-.085 (.095) [.372]
Number of In-Person Appts, Last 14 Weeks at Baseline	-.116 (.167) [.486]	-.18 (.168) [.286]	-.101 (.167) [.547]
Medication List Changed, Last 14 Weeks at Baseline	-.02 (.02) [.312]	.023 (.02) [.255]	.011 (.02) [.598]
Medication Removed, Last 14 Weeks at Baseline	-.011 (.012) [.371]	.022 (.012) [.065*]	.01 (.012) [.4]
Medication Added, Last 14 Weeks at Baseline	-.019 (.02) [.347]	.021 (.02) [.295]	.008 (.02) [.704]
Number of RX Orders, Last 14 Weeks at Baseline	-1.353 (.883) [.126]	-.285 (.889) [.748]	-1.384 (.884) [.118]
Number of New RX Orders, Last 14 Weeks at Baseline	-1.353 (.883) [.126]	-.285 (.889) [.748]	-1.384 (.884) [.118]
Number of Diabetes RX Orders, Last 14 Weeks at Baseline	-.049 (.092) [.593]	.083 (.092) [.371]	-.043 (.092) [.64]

Table 1.3: This table displays coefficients and standard errors for β_1, β_2, and β_3 from equation 1.2 on pre-treatment covariates, which estimates the balance between reminder message assignment groups. Standard errors are in parentheses with p-values in the far right column ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

Reminders were sent out every two weeks for the 12 weeks following the initial order period. The four reminder treatment conditions are described below:

1. Letters L-R

   Dear [Patient],

   Don't forget to track your blood glucose through MyChart! Keeping track of your blood glucose can help you manage your diabetes and reduce your chances for developing complications like heart disease, kidney damage and eye damage.

   Sincerely,

   [Health System Name]

2. Letters A-D

   Dear [Patient],

   You have been chosen for a special program to help you get started tracking your blood glucose through MyChart. For each day that you track your blood glucose on MyChart through August 2018, you will be entered to receive one of fifty $50 gift cards to Amazon.com. You'll be sent a secure message through MyChart in September 2018 if you've been selected to receive a gift card. Keeping track of your blood glucose can help you manage your diabetes and reduce your chances for developing complications like heart disease, kidney damage and eye damage.

   Sincerely,

   [Health System Name]

3. Letters E-K

   Dear [Patient],

   Don't forget to track your blood glucose through MyChart! Viewing your results helps me to respond if they are out of range, and improve your diabetes treatment to help you stay healthy. We will talk about your results at your next office visit. Keeping track of your blood glucose can help you manage your diabetes and reduce your chances for developing complications like heart disease, kidney damage and eye damage.

   Sincerely,

   [Primary Care Physician]

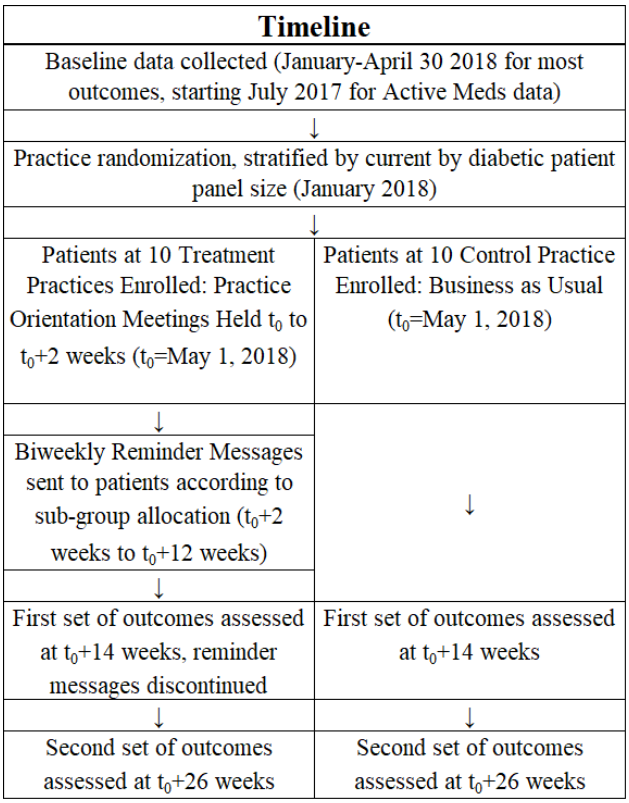
4. Letters S-Z: One initial reminder of type (1), no subsequent reminders[1]

Timeline

The trial consisted of a 14-week intervention phase with an additional 12-week follow-up phase. The total trial period was 26 weeks. Measurements were undertaken at three key

[1]The original study protocol indicated that no reminders would be sent to this group of patients; initial reminder was sent in error. This deviation may have resulted in slight underestimation of reminder effects.

time-points in each group: at baseline, directly after completing the 14-week intervention period, and at six-month follow-up (an additional 12 weeks after the intervention period). Baseline measurements were defined over the 14 weeks preceding first enrollment, except in the case of active medications, which were defined based on the most recent encounter within the 10 months prior to first enrollment. See the diagram below.



Empirical Approach

Specification 1

We evaluate the impact of the practice level intervention on flow sheet use and other outcomes Y by estimating and reporting β_1 from the equation below. T_i indicates whether the patient belonged to a treatment practice, and S_i is a strata fixed effect (5 strata of 4 practices each were determined by number of patients in each practice). In a second version of this specification, patient level controls X_i are incorporated with the goal of increasing precision of the estimates. Lin covariate adjustment is used, with CR2 standard errors clustered at practice level. Lin (2013) proposes this estimator, which centers pre-treatment covariates at mean zero and interacts them with the treatment indicator, as a strategy for mitigating

possible bias in the treatment effect estimate that could result from covariate adjustment (Freedman, 2008).

Covariates X include patient age (quadratic), sex (categorical), ethnicity (categorical), value of most recent baseline A1c test result (linear), days since most recent baseline A1c test result (linear), and days since most recent appointment at baseline (linear). Additionally, for outcomes referring to prescription medications, appointments, or secure messages, the 14-week baseline value of the outcome is included as an additional control variable. Missing values of continuous covariates are re-coded to a fixed value equal to the mean of that covariate and controlled for flexibly using a dummy variable indicating that the observation has a missing value for the covariate.

$$Y_i = \beta_0 + \beta_1 T_i + \alpha^\top S_i^C + \gamma^\top S_i^C T_i + \lambda^\top X_i^C + \omega^\top X_i^C T_i + \epsilon_i \tag{1.1}$$

Specification 2

To evaluate the impact of the reminder messages on outcomes, we will compare each reminder variation to the "No Reminder" group, reporting β_1, β_2, and β_3 from the estimating equation below. T_i indicates each patient's reminder message treatment assignment. Patient level controls X_i will also be incorporated, with the goal of increasing precision of the estimates and controlling for observable pre-intervention differences between reminder assignment groups. Lin covariate adjustment will be used, with HC2 standard errors, and the same list of covariates described above for the practice intervention evaluation.

$$Y_i = \beta_0 + \beta_1 T_{basic_i} + \beta_2 T_{gift_i} + \beta_3 T_{doc_i} + \lambda^\top X_i^C + \omega_1^\top X_i^C T_{basic_i} + \omega_2^\top X_i^C T_{gift_i} + \omega_3^\top X_i^C T_{doc_i} + \epsilon_i \tag{1.2}$$

A version of this specification will be estimated using the full sample of patients from treated practices, as well as one with a sample limited to patients who received flow sheet orders.

Specification 3

Finally, we will estimate the local average treatment effect of flow sheet use on down stream health and treatment outcomes:

$$Y_i = \pi_0 + \pi_1 FSuse_i + X_i \pi_2 + \nu_i \tag{1.3}$$

where FSuse is a measure of patient use of online glucose tracking (either on the extensive margin or log(1+entries)), and covariates X are defined above. We estimate equation 1.3 by two stage least squares (2SLS), using the following first stage equation:

$$FSuse_i = \delta_0 + \delta_1 T_{basic_i} + \delta_2 T_{gift_i} + \delta_3 T_{doc_i} + X_i \delta_4 + \mu_i \tag{1.4}$$

in which the excluded instruments are the reminder assignment group indicators. In other words, the 2SLS estimate of π_1 identifies the causal impact of electronic tracking among individuals induced to track by the reminder messaging (i.e. the compliers).

1.5 Results

Practice Intervention

Provider Participation and Flow Sheet Use

Of the 34 providers from ten practices assigned to the practice intervention arm, 23 attended a virtual orientation session and all but 5 of these requested an in-person session to facilitate the bulk orders. Reasons for not attending the orientation session or requesting an in-person meeting included scheduling/contact difficulties, staffing changes, or a preference not to participate in the study. Physicians who declined to participate cited concerns about negative impacts on provider workflow. Two physicians who initially placed the bulk flow sheet orders requested later that these orders be cancelled due to too many notifications. Table 1.4 below reports the impact of the practice intervention on flow sheet order rates. Patients at practices randomly selected for treatment were 63.6 (SE 13.5) percentage points more likely to receive an electronic flow sheet order, meaning that they had the option of tracking their blood glucose measurements through MyChart. They were 4.7 (SE 0.7) percentage points more likely to use the flow sheet at least once in the 14 weeks following implementation, and 2.3 (SE 0.4) percentage points more likely in the 15-26 weeks following implementation. See Appendix Tables A.9 and A.10 for summary statistics on flow sheet users. The average patient using the flow sheets made a total of 66 entries over the entire 26 week study period. 8.9% of patients made automated entries using Apple Health Kit, and among this sub-population, the entry rate was higher, averaging 121 entries over the study period. The average flow sheet user had a baseline A1c very close to the sample mean, indicating that this intervention did not appear to differential target particularly controlled or uncontrolled patients. Flow sheet users had more baseline interaction with the healthcare system (as evidenced by more recent A1c tests and appointments), and were more likely to be male.

Practice Intervention, Impact on Flow Sheet Use				
Outcome	Period	Control Mean (SD)	TE, No Covariates	TE, Covariates
Flow Sheet Orders	Weeks 1-14	.001 (.037)	.634 (.138) [.002***]	.636 (.135) [.001***]
	Weeks 1-26	.002 (.041)	.634 (.138) [.002***]	.636 (.135) [.001***]
Flowsheet Use (Extensive)	Weeks 1-14	.001 (.033)	.046 (.007) [0***]	.047 (.007) [0***]
	Weeks 15-26	.001 (.033)	.022 (.004) [.001***]	.023 (.004) [0***]
Flowsheet Use (Total)	Weeks 1-14	.011 (.439)	.528 (.086) [0***]	.537 (.087) [0***]
	Weeks 15-26	.009 (.386)	.348 (.113) [.015**]	.353 (.113) [.014**]

Table 1.4: This table displays coefficients and standard errors for β_1 from equations 1.1 (with and without covariates), which estimates the treatment effect of the practice intervention on patient flow sheet use, relative to business as usual. Standard errors are in parentheses with p-values in brackets ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

Patient Health and Treatment

Assignment to the practice intervention led to no effects on patient health, as measured through intent-to-treat estimates of effect on A1c. A clinically significant change in A1c of 0.3 is outside of the confidence interval for impacts at either 14 or 26 weeks, indicating a precisely estimated null effect. Alternate transformations of the A1c outcome including indicator variables for A1c below 7 and A1c improvement over baseline, also showed no effect. Appendix Table A.7 reports the results of quantile regressions, which estimate no statistically significant impact of the intervention at the 25th, 50th and 75th percentiles of the A1c distribution. The intervention also did not impact probability of receiving an A1c test. It is important to note however, that imperfect compliance with physician ordering and limited first stage impacts on flow sheet use mean we cannot interpret this as a direct failure of electronic tracking to impact downstream health. Rather, roll-out of electronic tracking in a system-wide setting with voluntary physician education and patient participation fails to produce measurable A1c changes.

Practice Intervention, Impact on Patient Health				
Outcome	Period	Control Mean (SD)	TE, No Covariates	TE, Covariates
A1c	At Week 14	7.196 (1.601)	-.002 (.052) [.968]	-.006 (.017) [.751]
	At Week 26	7.179 (1.54)	.043 (.051) [.422]	.035 (.025) [.202]
A1c Under 7	At Week 14	.551 (.497)	.008 (.016) [.618]	.007 (.011) [.508]
	At Week 26	.552 (.497)	.003 (.016) [.854]	.004 (.01) [.708]
A1c Improved Since Baseline	At Week 14	.2 (.4)	-.001 (.014) [.931]	.001 (.014) [.971]
	At Week 26	.297 (.457)	-.018 (.015) [.266]	-.014 (.016) [.392]
Days Since Last A1c Test	At Week 14	196.639 (201.15)	6.028 (11.827) [.624]	.315 (5.335) [.954]
	At Week 26	196.434 (207.394)	15.725 (9.56) [.138]	7.904 (5.302) [.174]
Had A1c Test in Period	At Week 14	.391 (.488)	.012 (.031) [.708]	.016 (.03) [.607]
	At Week 26	.626 (.484)	-.018 (.026) [.518]	-.01 (.026) [.704]

Table 1.5: This table displays coefficients and standard errors for β_1 from equations 1.1 (with and without covariates), which estimates the treatment effect of the practice intervention on patient health outcomes, relative to business as usual. Standard errors are in parentheses with p-values in brackets ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

Another outcome of interest is how electronic tracking impacts patient-physician interaction. Though it appears patients in the treatment group may have sent slightly more secure messages in the initial 14-week period, the increase of 0.067 (SE=.071) messages was not statistically significant. This corresponds to less than one additional message per week received per physician attributable to the intervention. There were also no statistically significant effects on phone or in-person appointments. This alleviates some concerns about indirect impacts of monitoring technology on provider-burden through increased patient communication and interaction. However, the messaging outcome reported does not include automated notifications generated as a result of the flow sheet orders. Data was not available on notifications, but anecdotally, some physicians expressed frustration with the volume of automated messages.

Practice Intervention, Impact on Patient-Physician Interaction				
Outcome	Period	Control Mean (SD)	TE, No Covariates	TE, Covariates
Appointment, In-Person	Weeks 1-14	1.275 (3.476)	-.059 (.099) [.563]	-.045 (.058) [.466]
	Weeks 1-26	2.326 (5.322)	-.14 (.131) [.317]	-.109 (.091) [.262]
Appointment, Phone	Weeks 1-14	.792 (1.651)	.024 (.114) [.835]	.029 (.058) [.632]
	Weeks 1-26	1.442 (2.547)	.02 (.17) [.911]	.033 (.091) [.727]
Number of Messages Sent by Patient	Weeks 1-14	1.385 (3.016)	.117 (.133) [.403]	.067 (.071) [.37]
	Weeks 15-26	1.128 (2.65)	.006 (.113) [.957]	-.022 (.063) [.731]

Table 1.6: This table displays coefficients and standard errors for β_1 from equations 1.1 (with and without covariates), which estimates the treatment effect of the practice intervention on patient-physician interaction, relative to business as usual. Standard errors are in parentheses with p-values in brackets ($^{*}p < .10,^{**} p < .05,^{***} p < .01$).

Table 1.7 reports the effect of the practice intervention on prescription orders and changes to active medications. We observe significantly lower rates of change to active medications, which is primarily driven by lower rates of addition. Patients in the treatment group were 4.8 percentage points less likely to see a change to their active medications within the 26 weeks following the start of the intervention. This result is somewhat surprising and seems to contradict the hypothesis that an increase in health data available to physicians would result in updates to a patient's treatment plan. One explanation is that physicians see SMBG as a substitute for additional prescription medications, or want to hold off on making changes to medications while the patient gathers tracking data. It is also possible that negative point estimates of the effect of the intervention on appointments could be driving this result. We estimate 4.6% fewer in-person appointments over this period in the treatment group, which is not statistically significant due to high variability in the outcome.

Practice Intervention, Impact on Patient Treatment				
Outcome	Period	Control Mean (SD)	TE, No Covariates	TE, Covariates
Any Medication List Change	Weeks 1-14	.204 (.403)	-.042 (.018) [.051*]	-.036 (.015) [.043**]
	Weeks 1-26	.311 (.463)	-.055 (.024) [.051*]	-.048 (.02) [.043**]
Medication Added	Weeks 1-14	.195 (.396)	-.037 (.018) [.075*]	-.032 (.015) [.063*]
	Weeks 1-26	.299 (.458)	-.052 (.023) [.052*]	-.046 (.019) [.041**]
Medication Removed	Weeks 1-14	.108 (.311)	-.021 (.012) [.108]	-.016 (.01) [.145]
	Weeks 1-26	.152 (.359)	-.026 (.017) [.158]	-.02 (.015) [.22]
Number of Prescription Orders	Weeks 1-14	5.111 (19.825)	-.289 (.502) [.581]	-.014 (.422) [.973]
	Weeks 1-26	9.296 (32.904)	-.708 (.904) [.456]	-.245 (.848) [.78]
Non-Refill	Weeks 1-14	5.111 (19.825)	-.289 (.502) [.581]	-.017 (.421) [.97]
	Weeks 1-26	9.296 (32.904)	-.708 (.904) [.456]	-.248 (.846) [.777]
Diabetes Related	Weeks 1-14	.678 (2.218)	-.033 (.091) [.724]	-.01 (.072) [.896]
	Weeks 1-26	1.28 (3.592)	-.142 (.173) [.435]	-.102 (.147) [.508]

Table 1.7: This table displays coefficients and standard errors for β_1 from equations 1.1 (no covariates) and 1.2 (covariates), which estimates the treatment effect of the practice intervention on patient treatment, relative to business as usual. Standard errors are in parentheses with p-values in brackets ($^{*}p < .10,^{**} p < .05,^{***} p < .01$).

We take two approaches to address false-discovery concerns from multiple hypothesis testing for healthcare use outcomes. First, we identify all 14-week outcomes that are related to measuring a patient's healthcare use. This includes phone appointments, in-person appointments, medication list changes (total only), number of prescription orders, number of diabetes-related prescription orders, and an indicator for whether the patient had an A1c test in the period. Then, we compute permutation adjusted p-values to control the family-wise Type I error rate (FWER), using the methodology outlined by Westfall et al. (1993), limiting

the sample to observations with non-missing values for all outcomes. The only statistically significant outcome, medication list changes, remains significant following this adjustment. Our second approach is to create a summary index based on these variables, following the procedure outlined in Anderson (2008). We test the effect of the practice intervention on this summary index, finding no significant impact. This suggests that while the practice intervention may have reduced patient medication changes specifically, it did not lead to an overall change in healthcare use. The results of this analysis are reported in Table 1.8 below.

Multiple Hypothesis Test for Physician Accountability Reminder 14-Week Overall Healthcare Use

Outcome	T-Stat	P-value	Adj. P-Value
Number of Phone Appointments	.721	[.479]	[.901]
Number of In-Person Appointments	-.568	[.572]	[.901]
Medication List Changed from Baseline	-4.236	[0***]	[.001***]
Number of RX Orders	-.059	[.951]	[.951]
Number of Diabetes RX Orders	-.306	[.765]	[.93]
Had A1c Test in Period	1.453	[.146]	[.503]
Healthcare Use Index	-.97	[.36]	

Table 1.8: This table displays test statistics, p-values, and adjusted p-values for β_1 from equation 1.1, which estimates the treatment effect of the practice intervention. Standard errors are not clustered by practice for this analysis. ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

Patient Reminder Intervention

Due to quasi-random assignment of patients to reminder treatment groups based on first letter of last name, causal interpretation of these results requires that grouped last name spelling is not independently related to these outcomes. In addition to controlling for pre-treatment patient characteristics and baseline outcome values, we also conduct a series of placebo test by re-running the analysis on these same last name letter designations but with patients in the Control practices, who did not receive flow sheet orders or reminders. Results of these falsifications tests can be found in Appendix Tables A.3-A.6. No significant differences between last name groupings were found for any of the outcomes.

Flow Sheet Use

Table 1.9 below shows how different reminder messaging impacts patient use of the flow sheets. Neither the gift card reminder nor the basic reminder resulted in use rates significantly different from the no-reminder group. However, we saw a 3.1 (SE 1.6) percentage point (52%) higher take-up rate among patients receiving the Physician Accountability reminder relative to the no-reminder group. In practice, every patient in the gift-card reminder group who made even one flow sheet entry ended up receiving a $50 gift-card to Amazon, though the high chance of winning may not have been evident to patients ex-ante. This result re-iterates the importance of intrinsic motivation and perception of provider engagement to

patient adherence, which has been documented in other healthcare contexts (Zolnierek and DiMatteo, 2009). The effect persisted even after reminders were no longer being sent out, in the period 15-26 weeks after implementation. Patients in the Physician reminder group had a 2.0 (SE 1.2) percentage point (63%) higher flow sheet use rate in the post-reminder period relative to the no-reminder group. Possible explanations for this persistence include long-term influence of this particular reminder on patients' evaluation of the value of tracking, and habit-formation.

Patient Reminder Intervention, Impact on Flow Sheet Use

		Full Sample				Patients with FS Orders			
Outcome	Period	Control Mean (SD)	TE, Basic	TE, GC	TE, Phys	Control Mean (SD)	TE, Basic	TE, GC	TE, Phys
Flow sheet Use (Extensive)	Weeks 1-14	.035 (.185)	.014 (.01) [.165]	.013 (.01) [.187]	.021 (.01) [.043**]	.058 (.234)	.016 (.015) [.289]	.016 (.015) [.294]	.031 (.016) [.052*]
	Weeks 15-26	.02 (.139)	0 (.007) [1]	.003 (.007) [.666]	.014 (.008) [.074*]	.032 (.177)	-.002 (.011) [.843]	.002 (.011) [.89]	.022 (.012) [.076*]
Flow sheet Use (Total)	Weeks 1-14	.501 (4.281)	-.147 (.175) [.401]	.162 (.229) [.479]	.191 (.215) [.373]	.82 (5.452)	-.278 (.279) [.319]	.227 (.367) [.537]	.308 (.349) [.377]
	Weeks 15-26	.353 (3.986)	-.13 (.171) [.447]	.057 (.205) [.779]	.142 (.211) [.501]	.577 (5.086)	-.231 (.276) [.402]	.073 (.332) [.826]	.238 (.347) [.492]

Table 1.9: This table displays coefficients and standard errors for β_1, β_2, and β_3 from equation 1.2, which estimates the treatment effect of different types of reminder messages received by patients in the treatment practices on patient flow sheet use, relative to no reminder. The left panel includes the full sample, while the right panel limits the sample to patients who received a flow sheet order. Standard errors are in parentheses with p-values in brackets ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

Patient Health and Treatment

The following three tables present results for how different reminder messages impact downstream outcomes including patient health, patient-physician interaction, and prescription medications. Because higher flow sheet use rates were observed for patients receiving the Physician Accountability reminder, we may expect to see differences in health and healthcare among the reminder groups that are attributable to differing flow sheet use rates. Though it is possible that patient reminders could directly affect these outcomes via changes to patient behavior un-related to flow sheet use, this seems a less likely explanation.

We find no discernible differences in patient A1c across the reminder groups. Though some estimates are marginally significant, these are not sustained through the 26 week outcome period. Appendix Table A.8 reports the results of quantile regressions, which estimate no statistically impact of the reminder messages at the 25th, 50th and 75th percentiles of the distribution of A1c. However, there does appear to be a statistically significant reduction in the probability of A1c test among the Physician Accountability reminder group (relative

to the no-reminder group), particularly during the initial 14 week period. Patients in this group are nearly 8 percentage points less likely to get an A1c test during this time frame. One explanation is that the information entered in the flow sheets is seen by physicians as a substitute for information gained from an A1c test. Note that any change in probability of A1c testing potentially confounds results evaluating A1c as an outcome.

Patient Reminder Intervention, Impact on Patient Health

		Full Sample				Patients with FS Orders			
Outcome	Period	Control Mean (SD)	TE, Basic	TE, GC	TE, Phys	Control Mean (SD)	TE, Basic	TE, GC	TE, Phys
A1c	Week 14	7.276 (2.823)	.013 (.039) [.741]	.055 (.044) [.205]	.067 (.04) [.092*]	7.352 (3.418)	.03 (.057) [.602]	.081 (.063) [.197]	.089 (.057) [.121]
	Week 26	7.327 (2.894)	-.057 (.051) [.269]	.022 (.056) [.692]	.043 (.054) [.431]	7.39 (3.493)	-.052 (.067) [.438]	.087 (.074) [.24]	.06 (.071) [.402]
A1c Under 7	Week 14	.556 (.497)	-.006 (.021) [.764]	-.012 (.021) [.592]	-.013 (.021) [.539]	.566 (.496)	-.017 (.027) [.535]	-.02 (.028) [.475]	-.042 (.028) [.129]
	Week 26	.545 (.498)	.016 (.022) [.462]	-.003 (.023) [.888]	-.009 (.023) [.701]	.562 (.497)	.01 (.029) [.728]	-.025 (.029) [.391]	-.041 (.029) [.16]
A1c Improved Since Baseline	Week 14	.224 (.417)	-.013 (.021) [.518]	-.014 (.021) [.497]	-.037 (.02) [.068*]	.233 (.423)	-.009 (.027) [.751]	-.027 (.027) [.315]	-.045 (.026) [.087*]
	Week 26	.291 (.455)	.019 (.023) [.426]	-.015 (.023) [.527]	-.013 (.023) [.573]	.305 (.461)	.012 (.03) [.702]	-.055 (.03) [.065*]	-.044 (.03) [.141]
Days Since Last A1c Test	Week 14	196.828 (205.93)	-3.48 (6.302) [.581]	.592 (6.571) [.928]	12.039 (5.936) [.043**]	185.445 (194.766)	-2.642 (8.184) [.747]	3.259 (8.397) [.698]	19.902 (7.469) [.008***]
	Week 26	208.781 (215.284)	-2.69 (7.859) [.732]	-1.232 (8.179) [.88]	5.889 (7.683) [.443]	202.751 (211.595)	-4.374 (10.117) [.666]	9.09 (10.347) [.38]	21.266 (9.485) [.025**]
Had A1c Test in Period	Week 14	.434 (.496)	-.026 (.025) [.311]	-.008 (.025) [.758]	-.054 (.025) [.028**]	.464 (.499)	-.029 (.032) [.372]	-.012 (.032) [.719]	-.08 (.031) [.011**]
	Week 26	.618 (.486)	-.016 (.026) [.544]	.009 (.026) [.742]	-.017 (.026) [.517]	.632 (.483)	.004 (.032) [.895]	-.001 (.032) [.973]	-.052 (.032) [.104]

Table 1.10: This table displays coefficients and standard errors for β_1, β_2, and β_3 from equation 1.2, which estimates the treatment effect of different types of reminder messages received by patients in the treatment practices on patient health outcomes, relative to no reminder. The left panel includes the full sample, while the right panel limits the sample to patients who received a flow sheet order. Standard errors are in parentheses with p-values in brackets ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

We observe no statistically significant differences in patient messaging or appointments among the reminder groups, with the exception of patient messaging at 14 weeks, for which we estimate a slightly higher rate in the basic message group relative to control. However,

this impact is no longer significant when considering only patients who received flow sheet orders, and is not sustained through weeks 14-26.

Patient Reminder Intervention, Impact on Patient Health									
		Full Sample				Patients with FS Orders			
Outcome	Period	Control Mean (SD)	TE, Basic	TE, GC	TE, Phys	Control Mean (SD)	TE, Basic	TE, GC	TE, Phys
Appointments, In-Person	Weeks 1-14	1.314 (3.914)	.007 (.163) [.966]	-.113 (.16) [.48]	-.084 (.161) [.601]	1.421 (4.517)	-.045 (.247) [.856]	-.257 (.234) [.273]	-.249 (.239) [.297]
	Weeks 1-26	2.358 (5.4)	-.058 (.224) [.796]	-.127 (.233) [.585]	-.085 (.229) [.711]	2.408 (5.845)	-.104 (.318) [.743]	-.227 (.319) [.478]	-.225 (.321) [.484]
Appointments, Phone	Weeks 1-14	.827 (1.64)	.049 (.075) [.512]	.028 (.072) [.697]	-.021 (.067) [.75]	.785 (1.619)	.049 (.093) [.596]	.014 (.09) [.873]	-.072 (.085) [.399]
	Weeks 1-26	1.457 (2.417)	.081 (.113) [.472]	.03 (.106) [.774]	.039 (.1) [.693]	1.41 (2.376)	.049 (.141) [.73]	-.003 (.132) [.98]	-.041 (.128) [.75]
No. Messages Sent by Patient	Weeks 1-14	1.407 (2.872)	.268 (.131) [.041**]	.079 (.12) [.512]	.16 (.123) [.194]	1.715 (3.291)	.159 (.178) [.373]	-.074 (.166) [.657]	-.035 (.166) [.832]
	Weeks 15-26	1.226 (2.803)	-.044 (.122) [.722]	-.155 (.117) [.187]	-.092 (.116) [.425]	1.356 (3.109)	-.104 (.16) [.513]	-.223 (.154) [.147]	-.239 (.15) [.11]

Table 1.11: This table displays coefficients and standard errors for β_1, β_2, and β_3 from equation 1.2, which estimates the treatment effect of different types of reminder messages received by patients in the treatment practices on communication outcomes, relative to no reminder. The left panel includes the full sample, while the right panel limits the sample to patients who received a flow sheet order. Standard errors are in parentheses with p-values in brackets ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

We observe significant reductions in prescriptions for the Physician Accountability reminder group. Patient in this reminder group who received a flow sheet order saw 31% fewer prescription orders during the initial 14-week outcome period than patients assigned to the group that did not receive reminders. At least some of the change appears to be driven by a reduction in orders for diabetes related prescriptions, which were 28% lower relative to the no-reminder group. Because patients receiving this reminder were induced to use electronic flow sheets more often than patients in other groups, this could represent a downstream outcome of SMBG and resulting lifestyle management acting as a substitute for prescription medications. This is similar to the posited explanation for the previously reported negative impact of the practice level intervention on medication changes. Again, negative but non-significant effects of the reminder on both phone and in-person appointment rates could have also influenced this result.

Patient Reminder Intervention, Impact on Patient Health									
		Full Sample				Patients with FS Orders			
Outcome	Period	Con. Mean (SD)	TE, Basic	TE, GC	TE, Phys	Control Mean (SD)	TE, Basic	TE, GC	TE, Phys
Any Medication List Change	Weeks 1-14	.177 (.382)	-.018 (.018) [.333]	-.02 (.018) [.267]	-.022 (.018) [.232]	.189 (.392)	-.04 (.023) [.082*]	-.035 (.023) [.122]	-.021 (.023) [.37]
	Weeks 1-26	.26 (.439)	-.024 (.021) [.254]	-.006 (.021) [.77]	.02 (.021) [.336]	.277 (.448)	-.04 (.026) [.125]	-.031 (.026) [.238]	.02 (.027) [.462]
Medication Added	Weeks 1-14	.169 (.375)	-.012 (.018) [.494]	-.016 (.018) [.365]	-.016 (.018) [.356]	.18 (.385)	-.031 (.023) [.17]	-.031 (.022) [.162]	-.015 (.023) [.525]
	Weeks 1-26	.247 (.431)	-.024 (.02) [.237]	-.002 (.021) [.906]	.029 (.021) [.158]	.264 (.441)	-.04 (.026) [.123]	-.021 (.026) [.418]	.029 (.027) [.28]
Medication Removed	Weeks 1-14	.093 (.291)	-.003 (.014) [.814]	-.014 (.014) [.312]	-.01 (.013) [.453]	.107 (.31)	-.032 (.018) [.068*]	-.028 (.017) [.111]	-.025 (.018) [.159]
	Weeks 1-26	.126 (.332)	-.01 (.016) [.522]	.002 (.016) [.894]	.006 (.016) [.684]	.144 (.351)	-.038 (.02) [.058*]	-.024 (.02) [.24]	-.006 (.02) [.764]
Number of Prescription Orders	Weeks 1-14	5.912 (19.041)	-.727 (.7) [.299]	-.907 (.727) [.212]	-1.009 (.699) [.149]	5.83 (18.405)	-.079 (.877) [.928]	-.599 (.919) [.515]	-1.824 (.701) [.009***]
	Weeks 1-26	10.554 (33.799)	-1.211 (1.247) [.332]	-1.854 (1.278) [.147]	-1.65 (1.171) [.159]	9.987 (32.454)	-.152 (1.53) [.921]	-.683 (1.631) [.676]	-2.104 (1.33) [.114]
Non-Refill	Weeks 1-14	5.912 (19.041)	-.727 (.7) [.299]	-.907 (.727) [.212]	-1.009 (.699) [.149]	5.83 (18.405)	-.079 (.877) [.928]	-.599 (.919) [.515]	-1.824 (.701) [.009***]
	Weeks 1-26	10.554 (33.799)	-1.211 (1.247) [.332]	-1.854 (1.278) [.147]	-1.65 (1.171) [.159]	9.987 (32.454)	-.152 (1.53) [.921]	-.683 (1.631) [.676]	-2.104 (1.33) [.114]
Diabetes Related	Weeks 1-14	.718 (1.686)	-.099 (.078) [.201]	-.043 (.078) [.58]	-.138 (.07) [.049**]	.77 (1.697)	-.044 (.104) [.671]	-.043 (.102) [.678]	-.212 (.086) [.014**]
	Weeks 1-26	1.318 (2.938)	-.23 (.121) [.057*]	-.185 (.128) [.148]	-.246 (.115) [.033**]	1.35 (2.817)	-.157 (.155) [.309]	-.14 (.164) [.393]	-.286 (.14) [.042**]

Table 1.12: This table displays coefficients and standard errors for β_1, β_2, and β_3 from equation 1.2, which estimates the treatment effect of different types of reminder messages received by patients in the treatment practices on patient treatment, relative to no reminder. The left panel includes the full sample, while the right panel limits the sample to patients who received a flow sheet order. Standard errors are in parentheses with p-values in brackets ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

Again, we seek to address false-discovery concerns from multiple hypothesis testing for the impact of the Physician Accountability reminder on healthcare use outcomes. We take the same approaches outlined above in the practice treatment section, with the same list of 14-week healthcare use outcomes. The sample is limited to patients in treated practices who received flow sheet orders, who have non-missing values for all outcomes included.

Though the un-adjusted p-values show significant impacts of the Physician Accountability reminder on A1c test frequency and diabetes prescription orders in comparison to the no-reminder group (as in previously reported analysis), the adjusted estimates are no longer statistically significant. This suggests caution in interpreting the effect of this reminder on specific healthcare use outcomes. However, the result of the summary index test shows a negative statistically significant impact of the reminder on healthcare use overall. Again, the mechanism of this effect is posited to be the increased electronic blood glucose tracking seen in this reminder group. The results of this analysis are reported in Table 1.13 below. We do not repeat this analysis for the Basic and Gift Card reminders due to the lack of significant effects on tracking and downstream outcomes reported in previous analysis.

Multiple Hypothesis Test for Physician Accountability Reminder 14-Week Overall Healthcare Use

Outcome	T-Stat	P-value	Adj. P-Value
Number of Phone Appointments	-.364	[.72]	[.891]
Number of In-Person Appointments	-.43	[.677]	[.891]
Medication List Changed from Baseline	-1.231	[.219]	[.517]
Number of RX Orders	-1.481	[.137]	[.428]
Number of Diabetes RX Orders	-1.81	[.066*]	[.264]
Had A1c Test in Period	-2.16	[.034**]	[.171]
Healthcare Use Index	-2.789	[.005***]	

Table 1.13: This table displays test statistics, p-values, and adjusted p-values for β_2 from equation 1.2, which estimates the treatment effect of different types of reminder messages received by patients in the treatment practices on patient treatment, relative to no reminder. The sample is limited to patients who receive a flow sheet order. ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

Flow Sheet Local Average Treatment Effect

Finally, we use overall variation in electronic tracking rates induced by reminder messaging to estimate the direct impact of flow sheet use on downstream healthcare use outcomes. We estimate equation 1.3, instrumenting for flow sheet use with reminder group assignment, and report results in Table 1.14 below. Though point estimates are very large and of the same sign as intent-to-treat (ITT) effects previously reported of the practice and reminder interventions, none are statistically significant. Narrow variation in the first stage across reminder groups most likely limited statistical power in this case.

Electronic Tracking LATE Estimates on Downstream Outcomes at 14 Weeks		
	Tracking Measure	
Outcome	Extensive Margin Use	Log(1+Entries)
Number of Phone Appointments	-2.936	-1.61
	(3.154)	(1.283)
	[.352]	[.21]
Number of In-Person Appointments	-8.329	-4.059
	(8.618)	(3.216)
	[.334]	[.207]
Medication List Changed from Baseline	-.686	.034
	(.825)	(.284)
	[.406]	[.906]
Number of RX Orders	-61.664	-26.047
	(40.229)	(15.59)
	[.125]	[.095*]
Number of Diabetes RX Orders	-6.969	-2.56
	(4.803)	(1.719)
	[.147]	[.137]
Had A1c Test in Period	-2.256	-.803
	(1.509)	(.585)
	[.135]	[.17]
Healthcare Use Index	-2.557	-.894
	(1.62)	(.602)
	[.115]	[.138]

Table 1.14: This table displays coefficients and standard errors for the 2SLS estimate of π_1 from equation 1.3, which estimates the treatment effect of electronic monitoring. Standard errors are in parentheses with p-values in brackets ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

1.6 Conclusion

This study evaluates multiple strategies for encouraging utilization of electronic blood glucose tracking, and measures the downstream impact of a system-wide, practical implementation of this tool. We find that a provider training and support intervention was successful at increasing utilization of the flow sheets, and that patient reminder messaging focused on accountability to the provider was most successful at getting patients to track. Though no change in patient health resulted, we observed significant downstream reductions in medication changes and prescription orders for both patients in treated practices (as compared to control practices), and patients assigned to the reminder group associated with more intensive tracking. We also documented slightly lower rates of A1c testing for patients in this reminder group, suggesting a possibly substitute-ability of patient generated health data for formal lab testing. One limitation of this study is that it focused only on patients who had already activated online patient portals. Mook et al. (2018) show that patients who are non-English speaking, non-white, older, or living in less affluent areas are less likely to be portal users. Future research could focus on how to encourage use of patient generated data and remote monitoring for these harder to reach populations.

Though technologies for patients to collect and share their health data in real time are

increasingly available, use of these technologies in a clinical setting remains limited. Prior to this study, just 0.1% of patients in our sample were tracking blood glucose data through their electronic medical records, though the feature had been available for more than a year. While many patients at treatment practices eventually stopped tracking, about 1/3 of those who tracked continued to do so well after reminders were no longer sent out. We interpret this as evidence that low utilization of remote monitoring technology is at least in some cases attributable to lack of awareness rather than low value assessment. Thus, it is important to understand how health systems can practically promote take-up and awareness of new technologies. This study makes an important contribution on this dimension, showing that provider training and support is a critical first step, and that utilization can be further promoted among patients by emphasizing engagement and accountability to healthcare providers. Future research could explore other low-cost strategies for enhancing patients' intrinsic motivation, such as automated dynamic responses to entered data.

The intent-to-treat effects reported for downstream health and healthcare outcomes show the impact of system-level promotion of new monitoring technology, where participation is voluntary for patients and physicians. Understanding how the introduction of remote electronic monitoring and patient generated health data impacts care in a practical clinical setting is vital as these technologies become more widely available. This paper evaluates the causal impact of integrating glucose monitoring data into EMRs on a wide-scale, a unique and novel contribution to the literature. Future work should address how systems can be designed to be more user-friendly for both physicians and patients, which may further increase use and benefit of these technologies.

Chapter 2

Immigrant Enrollment in Medicaid and Means-Tested Benefits After PRWORA

2.1 Introduction

After the passage of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA), most legal immigrants were barred from receiving social benefits such as Medicaid and SNAP for their first five years of residency in the United States (US Department of Health and Human Services, 2009). Though some states introduced state-funded benefits to reach those who were otherwise eligible during this waiting period, most have no access to a formal social safety net during the five-year gap. This discontinuity in eligibility rules for legal immigrants provides an opportunity to study how enrollment in benefits changes after restrictions are removed and how access to social services affects immigrants' health outcomes, health care utilization, financial outcomes, and remittance behavior.

Though several studies have compared the outcomes of insured and uninsured populations (Institute of Medicine of the National Academies, 2003), inferring the causal impact of a program like Medicaid is challenging. Those receiving and not receiving the program are different in ways that are difficult to adequately control for and are likely correlated with outcomes. Some studies have used quasi-experimental variation to assess the effects of Medicaid (Engelhardt and Gruber, 2011) or SNAP (Hoynes and Schanzenbach, 2010), but most of these focus on very specific populations where variation in eligibility occurs, such as children or the elderly. A randomized experiment was used to evaluate the impact of receiving Medicaid in the recent Oregon Health Study, but the population for this study was comprised of mostly white US citizens, all of whom live in Oregon (Finkelstein et al., 2012).

I use data collected on new Legal Permanent Residents in the New Immigrant Survey (NIS) to study the impact of gaining eligibility to apply for means-tested benefits on immigrant outcomes. To my knowledge, this is the first study exploiting this discrete change

in eligibility after five years of residency to study the causal effects of these benefits on immigrants specifically. The NIS is a nationally representative survey with data on outcomes unique to immigrants, including remittances. I analyze the effect of gaining eligibility to apply for means-tested benefits using a regression discontinuity design that compares outcomes before and after the five-year cutoff. I also employ a difference-in-differences (DID) approach to compare changes in outcomes over time among immigrants living in states with no supplementary benefits during the five-year gap period, to immigrants who have access to state-funded benefits to bridge the gap in federal benefits during their first five years of residency.

I find limited evidence for decreased savings and increased use of the emergency department as a result of gaining eligibility for Medicaid and SNAP, but am not able to obtain precise estimates of the impact on remittances. The link between Medicaid and increased usage of medical services is supported by previous research; in particular the Oregon Health Study also finds that Medicaid enrollment increases usage of the emergency department (Finkelstein et al., 2012). I find no evidence for positive mental health effects of Medicaid or increases in financial well-being which are reported in prior literature. However, this is likely attributable to issues with self-reported data and statistical power rather than a precisely estimated null effect.

The rest of the chapter is structured as follows: Section 2.2 provides background on the state-by-state eligibility rules for Medicaid and food assistance. Section 2.3 describes the data and construction of outcome variables. Section 2.4 presents the empirical framework. Section 2.5 presents main results. Section 2.6 concludes and Section 2.7 lists tables and figures.

2.2 Immigration Status and Eligibility for Means-Tested Benefits

For the purposes of this paper, I will discuss only the law as it applies to adult "qualified immigrants" as all immigrants in this sample are adult Legal Permanent Residents (LPRs), and thus fall into this category. Under PRWORA, qualified immigrants who become LPRs after August 22, 1996 are not eligible to receive federally funded means-tested benefits prior to having been residents of the United States for at least five years. Refugees and asylees are exempt from this ban, and may be automatically eligible to receive these benefits upon entry. Federal means-tested benefit programs available to adults include Medicaid, the Supplemental Nutrition Assistance Program (SNAP, formerly Food Stamps), and cash assistance programs (TANF, SSI, SSGB) (US Department of Health and Human Services, 2009). In this paper, I focus on SNAP and Medicaid, as a negligible portion of the population receives benefits from cash assistance programs (only 12 people in the NIS sample receive these benefits).

A number of factors allow adult LPRs to receive food stamps and Medicaid prior to the

five-year waiting period. Under the Children's Health Insurance Program Re-authorization Act of 2009, the federal government permitted states to use federal funding to provide Medicaid to pregnant women and child immigrants (US Department of Health and Human Services, 2009). As pregnant women make up a negligible portion of the immigrants interviewed for this sample, most of whom were interviewed prior to 2009, this is not particularly relevant. However, it is important to note that PRWORA only prevents states from using *federal* funds to provide Medicaid and SNAP to LPRs during the five-year waiting period. Since Medicaid is partially state funded (Kaiser Family Foundation, 2014), states can choose to allocate their own funding to the coverage of LPRs during this five-year federal ban (California Pan-Ethnic Health Network, 2014). Furthermore, some states have their own self-funded food stamp programs (in addition to SNAP), the eligibility rules of which are not subject to PRWORA. See Tables 2.4 and 2.5 to see a list of which states use state funding to provide health coverage (National Immigration Law Center, 2015) and food assistance (National Immigration Law Center, 2007) to immigrants during the five-year federal ban.

However, among these states, there is a great deal of heterogeneity in the portion of LPRs to whom benefits are extended. In most states, only select groups, such as pregnant women or abused immigrants are eligible for state funded Medicaid prior to becoming eligible for federally funded Medicaid after five years of residency. Tables 2.4 and 2.5 include a breakdown of the eligibility rules for states which offer state-only health coverage and food assistance. Note that these tables outline restrictions to otherwise eligible immigrants. That is, states each have their own eligibility requirements for food assistance and medical coverage that apply to the entire population (such as being under a certain percentage of the Federal Poverty Line), but then have additional restrictions on eligibility for qualified immigrants. States not listed in the tables provide no state-funded coverage to immigrants prior to the five-year federal ban.

Thus, one should expect to see a significant jump in the number of those enrolled in Medicaid between the fifth and sixth years of residency in all states except California and New York, where all immigrants are Medicaid-eligible with no waiting period. One should expect a similar jump in food stamp enrollment for all states except for California, Connecticut, Maine, Nebraska, Washington and Wisconsin, where all immigrants are eligible for food stamps with no waiting period. I will exploit this differential change in eligibility across states to look at the effects of these two programs on immigrant health status, health care utilization, financial outcomes, and transfer behavior.

In addition to changes in benefit eligibility, LPRs also become eligible to apply for citizenship after five years. NIS data does not indicate whether survey respondents have become citizens at the point they are interviewing. The primary changes that occur if one obtains citizenship are the right to vote, higher priority to bring family members to the United States and becoming eligible for Federal jobs. It is plausible that these changes could affect some of the outcomes studied in this paper, though it is impossible to know the prevalence of citizenship in the sample. The reported regression discontinuity analysis will require the assumption that benefit eligibility is the only relevant change impacting outcomes at the five-year cutoff. However, the difference-in-differences strategy will assume only that other

factors do not differentially affect areas with state-funded Medicaid and food stamps for immigrants. This will be discussed in more detail in Section 2.4.

2.3 Data

The New Immigrant Survey

The data used for analysis in this paper is from the New Immigrant Survey (Jasso et al., 2014). The NIS is a panel study of new legal immigrants to the United States. A stratified random sample of 12,500 adults was selected from the population of immigrants who became LPRs between May and November of 2003. Contact information and data on immigration status for individuals included in the sampling frame was obtained through electronic administrative records compiled for new immigrants by the US government. The sampling was stratified across spouses of US citizens, employment principals, diversity principals, and all other immigrants, meaning that the sample includes both new-arrival and adjustee immigrants who were already residing in the US illegally or with a temporary non-immigrant visa. The sampling frame was limited to the top 85 Metropolitan Statistical Areas (which includes approximately 89% of immigrants), top 38 counties, and a random sample of 10 MSAs and 15 counties from those remaining (Jasso et al., 2005).

Initial interviews were conducted as soon as possible after the immigrant received his or her green card, and in the respondent's preferred language. The NIS is the first survey of its kind, in that it sampled individuals from administrative data and sought a high response rate by intensively seeking out and accommodating respondents. From the original sample, 8,573 respondents were successfully contacted and interviewed in the first round, which took place between June 2003 and June 2004. The baseline survey instrument obtained extensive information on several topics of interest including health, schooling, marriage and family, skills, languages and English language skills, labor force participation, earnings, use of government services, networks, transfers, travel, and religion. The study design calls for respondents to be re-interviewed every three to five years. The first follow-up survey was conducted between June 2007 and November 2009, and successfully contacted and re-interviewed 4,363 respondents. Data from this initial follow-up survey is the focus of the analysis in this paper.

80% of respondents successfully contacted for the second survey round were interviewed over the phone between June 2007 and April 2008, while the 20% remaining "difficult-to-locate" adults received follow-up for in-person interviews between March and November of 2009. This heterogeneity in time of survey will be critical for this study's identification strategy, in that the design relies on a change in government program eligibility after five years of residency, and respondents in these two cohorts of survey timing fall on different sides of this cutoff. One concern with the data in this respect is that respondents who were interviewed in the second group might be systematically different from those in the first group in ways that would impact program eligibility or utilization, outside of their

tenure as LPRs. Additionally, different survey mediums (phone vs. in-person interviews) could impact responses to certain questions. Table 2.3 compares those in the first wave of surveys to respondents in the second wave. Notably, the late surveyed group is younger, less likely to have moved, and slightly less likely to report being in good health at the baseline survey. These attributes could be correlated with the outcomes studied here, and observable differences could also signal that further un-observable differences exist between the two groups. Strategies for addressing this issue will be discussed in further detail in Section 2.4.

Sample Definition

The sample includes NIS main respondents and their married spouses if the spouse is also an LPR. All analysis is limited to non-refugees who are under age 65. As mentioned in Section 2.2, many refugees and asylees are automatically enrolled in Medicaid upon arrival and are not subject to the five-year waiting period for benefit eligibility. Seniors are not considered in order to limit issues with dual eligibility for Medicare, which is not subject to the five-year waiting period. Further restrictions are made to limit the sample to those who were interviewed in specified bandwidths around the five-year cut-off, and to likely "otherwise eligible" individuals as determined by income relative to the federal poverty-line.

The sample used for the regression discontinuity analysis is also restricted to states in which discontinuities in program eligibility exist, that is, all states but California and New York. Note that there are several other states in which state-funded food stamp programs extend eligibility to immigrants in the five-year gap. However, the publicly available NIS data-set groups those in smaller states by region (due to identification concerns), and these states are spread out among different regions. Since each state (Connecticut, Maine, Nebraska, Washington and Wisconsin) comprises a small portion of the population within each region (see Table 2.2 for the distribution of the sample across regions), a discontinuity in eligibility for benefits still exists for the majority of that population. Thus, I consider all regions/states except California and New York to be "treatment states" and include them in the sample.

Key Variable Construction and Descriptive Statistics

Key variables analyzed for the purposes of this paper include enrollment status in the two studied programs (Medicaid and food stamps), health insurance status, state of residency, months since date of legal permanent residency, income as a percentage of the Federal Poverty Line, presence of co-resident children, and four sets of outcomes. See Table 2.1 for summary statistics on mean values of these variables in the sample population. Enrollment, health insurance status, and state of residency are constructed directly from individual questions in the survey in the Health and Life Insurance and Demographics modules, respectively. I will discuss the construction of other key variables below.

Running Variable: Months Since Date of Residency

For all main respondents, the NIS contains the exact commencement date of legal permanent residence along with exact date of survey. This permits direct construction of a running variable for number of residency months. However, for spouses, the NIS lists only the year of commencement of legal permanent residence. If spouses became LPRs in the same year as their partners (for whom exact dates of residency are listed) I assume that they immigrated in the same month. This is the case for over half of the spouses. Otherwise, I assume the spouse immigrated mid-year. As many of these spouses' interview dates fall outside the 12 and 18 month sample bandwidths, this does not have substantive impact on the results. See Figure 2.1 for the distribution of surveys around the cutoff.

Eligibility: Income and Co-Resident Minor Children

Income is used to restrict the sample population to likely-eligible individuals. Prior to the implementation of the Affordable Care Act, states had widely differing methods of determining eligibility for Medicaid. In addition to having different income cutoffs, they also measured income differently, and may or may not have included asset tests in determination of eligibility. After the passage of the Affordable Care Act, rules for measuring income were standardized, regardless of whether states chose to expand Medicaid. As I am unable to identify the exact state of residency for many immigrants in the sample, I use the current standardized rules for measuring income, which are fairly representative of those used in many states prior to the change. The following items are included in Modified Adjusted Gross Income (MAGI): wages, tips, self-employment income, unemployment compensation, Social Security, Social Security Disability Income (SSDI), pension/retirement income, alimony, capital gains, investment income, rent and royalty income, and un-taxed foreign income. The following are not included in MAGI: Supplemental Security Income (SSI), child support, veterans' disability payments, and workers compensation (US Department of Health and Human Services, 2016).

The NIS includes questions related to all of these items in the income and assets modules of the survey, which are answered by either the spouse or main respondent, depending on who is more knowledgeable about household finances. Though answers to some of these questions were given in currencies other than USD, the NIS provides a supplementary data file converting these denominations to USD using PPP and market exchange rates. I construct a household-level MAGI income variable using the responses from this section converted to USD (if applicable) using market exchange rates. I then use reported household size to determine MAGI as a percentage of the Federal Poverty Line in 2008 (US Department of Health and Human Services, 2008).

The income cutoffs for Medicaid also vary widely. In 2009 income cutoffs for adults ranged from 11% of the Federal Poverty Line in Alabama to 215% of the FPL in Minnesota for full Medicaid and 300% of the FPL for premium assistance in Maine (Kaiser Family Foundation, 2009). Most states also require that individuals have co-resident children in

order to to be eligible to receive Medicaid. Income cutoffs for SNAP are standardized at 130% of the FPL (US Department of Agriculture, 2016).

For certain analyses, I restrict the sample to "likely otherwise-eligible" immigrants, who would meet eligibility requirements for means-tested benefits if they were not banned from applying until after five years of residency. These include a more generous restriction to 300% of the FPL with co-resident children and a stricter restriction to 130% of the FPL with co-resident children. Further discussion of this issue will follow in Section 2.4, but given the imprecision of measuring self-reported income, using the 300% FPL cutoff to limit the sample may be more successful in capturing all truly-eligible immigrants.

Outcomes: Health and Health Care Use

All seven outcomes related to health can be linked directly to survey questions in the Health or Health Care Utilization modules of the NIS, and were answered separately by both spouses and main respondents. These include whether the respondent reported being in "Good" "Very Good" or "Excellent" health, and whether the respondent reported being sad or depressed for two weeks or more in a row over the last year. Also included are whether the individual has received any preventative health care (including mammogram, pap smear, cholesterol check, flu shot or prostate check) since 2003 (date when first immigrated), whether the individual has been hospitalized in the last 12 months, number of doctor's visits in the last 12 months, whether the respondent has a place that he or she usually goes for medical care, and whether the place he or she usually goes for medical care is the emergency room (as opposed to a clinic or primary care physician).

Outcomes: Financial

Six of the reported financial outcomes are linked to survey questions in the Assets, Transfers and Health Care Utilization modules of the NIS. These include credit card debt, savings, and other debt (not including home, car or credit card debt, but including medical debt). All of these outcomes are based on questions answered at the *household* level. This outcome grouping also includes total out-of-pocket (OOP) medical costs for hospitalizations or stays at long term care facilities, doctor visits, dental care, outpatient surgeries and prescription drugs. Out-of-pocket costs were assessed at the individual level. Finally, two additional outcomes are sourced from the Transfers module of the NIS, which was administered at the household level. This module asks about money given to or received from family or friends living outside the household, both within and outside the United States. As in the income section, some amounts were reported in foreign currencies, and these were converted to USD using market exchange rates. The outcomes studied are total receipts (also any receipts) and total (any) transfers. Funds remitted to an immigrant's home country are captured in the transfer variables. In Appendix Section B.1, I develop a simple model to show that the expected impact of benefit eligibility on remittances is theoretically ambiguous.

2.4 Empirical Framework

Reduced Form Regression Discontinuity

Effect on Enrollment

I estimate the reduced form effect of passing the five-year cut-off on enrollment in means-tested benefits with the following equation:

$$y_{ie} = \beta_0 + \beta_1 POST_i + \epsilon_{ihe} \tag{2.1}$$

where i denotes an individual, h denotes a household and e denotes the benefit. $POST$ is an indicator variable for whether or not immigrant i was interviewed after he or she had been a legal permanent resident for at least five years. The coefficient on $POST$ (β_1) is the main coefficient of interest, and gives the average difference in mean enrollment level between the treatment group (those interviewed after the five-year cut-off) and control group (those interviewed before the five-year cutoff). Determination of eligibility for Medicaid can take up to 45 days, so I use those in their 62nd or greater month of residency as the treatment group. The bandwidth for primary specifications is 18 months on either side of the cutoff, though I also report results limiting this to 12 months. Because there are few surveys very near to the 62 month cutoff (see Figure 2.1), decreasing the bandwidth further than 12 months does not allow for adequate sample size. I also report results limiting the sample to the likely otherwise eligible population. All samples for this analysis are limited to respondents not living in New York or California. Standard errors are robust and clustered at the household level.

Effect on Outcomes

I estimate the reduced form effect of passing the five-year cut-off on health and financial outcomes with the following equation:

$$y_{io} = \beta_0 + \beta_1 POST_i + \beta_2 POST_i * M_i + \beta_3 M_i + \epsilon_{ihe} \tag{2.2}$$

where M is number of months since residency, centered at $POST$. Including linear time trends is intended to account for the expectation that some of the outcomes may change over time (in particular transfers and receipts). I report the coefficient on $POST$ (β_1), which estimates the level change in outcomes between the treatment and control groups. I also report β_2 which estimates the change in slope of outcomes over time between treatment and control group. This change in slope could be important for outcomes such as health care utilization, which may be expected to increase with exposure to Medicaid. The sample for this analysis is limited to respondents not living in New York or California, within 18 months of the cutoff for benefit eligibility (using 62 months), who are in the "likely otherwise eligible" i.e. under 300% of the FPL with co-resident children. Standard errors are robust and clustered at the household level. I also report Bonferroni corrected p-values, which adjust for the number of outcomes in each table.

IV "Fuzzy" Regression Discontinuity

This design can also be exploited to estimate the direct effect of Medicaid and/or food stamps on the health and financial outcomes. For this specification, I estimate the first stage as

$$BENEFIT_i = \gamma_0 + \gamma_1 POST_i + \gamma_3 M_i + \varepsilon_{ihe} \tag{2.3}$$

and the second stage as

$$y_{io} = \theta_0 + \theta_1 \gamma_1 BENEFIT_i + \theta_3 M_i + \xi_{ihe} \tag{2.4}$$

$BENEFIT_i$ is a dummy for whether the individual is receiving Medicaid or food stamps. I do not include the interaction term in this specification due to issues with weak instruments. Thus, I assume that outcomes change linearly but that only the level of this change is affected by benefit enrollment. The sample for this analysis is limited to respondents not living in New York or California, within 18 months of the cutoff for benefit eligibility (using 62 months), who are in the "likely otherwise eligible" i.e. under 300% of the FPL with co-resident children. Standard errors are robust and clustered at the household level. I also report Bonferroni corrected p-values, which adjust for the number of outcomes in each table. I report Kleibergen-Paap F-statistics to test for weak instruments. Generally, an F-statistic below 10 indicates a potential problem, and the results of this test indicate that the instrument is borderline weak. IV estimates should thus be interpreted with caution.

Difference-in-Differences

One concern with the survey data is that those surveyed later are categorically different from those surveyed earlier in ways other than benefit eligibility. In particular, respondents in the "treatment group" for the RD approach were those who were difficult to contact in the first round of the survey, and were thus interviewed much later, and in-person rather than over the phone. It is likely that they are different from the earlier-surveyed group in ways other than benefit eligibility.

One method of dealing with this issue is to use immigrants living in the states of California and New York as a "control group" in a difference-in-differences design. These states provide state-funded Medicaid to immigrants during the five-year federal ban, so there is no discontinuity in eligibility. This is unlike standard DID designs in that the "control" group is actually being treated during the entire period. However, as long as the parallel trends assumption is satisfied in the pre-period and one can assume that that other factors do not differentially affect areas with state-funded benefits over time, a convergence of outcomes between California, New York, and other states from pre- to post-period would provide evidence of the effects of the programs. The required assumptions are supported by graphical evidence in Figures 2.2-2.6. Additionally, a lack of change in trends between pre- and post-periods for California (which has no discontinuity for food stamp or Medicaid eligibility) gives confidence that selection along these outcomes between interview groups is not a substantial concern.

In additional to graphical analysis provided in Figures 2.2-2.6, I estimate the following model:

$$y_{io} = \beta_0 + \beta_1 POST_i + \beta_2 POST_i * TREAT_i + \beta_3 TREAT_i + \epsilon_{ihe} \tag{2.5}$$

where $TREAT_i$ is a dummy for whether the respondent is not living in New York or California. The coefficient β_2 denotes the effect of gaining eligibility for Medicaid on the stated outcome. I also use two-stage least squares to estimate the direct effect of Medicaid on the outcomes, in the spirit of Duflo (2001), where the first stage is:

$$MEDICAID_i = \gamma_0 + \gamma_1 POST_i + \gamma_2 POST_i * TREAT_i + \gamma_3 TREAT_i + \varepsilon_{ihe} \tag{2.6}$$

and the second stage is:

$$y_{io} = \theta_0 + \theta_1 POST_i + \gamma_2\theta_2 MEDICAID_i + \theta_3 TREAT_i + \xi_{ihe} \tag{2.7}$$

The sample for this analysis is limited to respondents within 18 months of the cutoff for benefit eligibility (using 62 months), who are "likely otherwise eligible" i.e. under 300% of the FPL with co-resident children. Standard errors are robust and clustered at the household level. I also report Bonferroni corrected p-values, which adjust for the number of outcomes in each table. I report Kleibergen-Paap F-statistics to test for weak instruments. Generally, an F-statistic below 10 indicates a potential problem, and it is clear that this is a significant issue for this strand of analysis. IV coefficients should thus be interpreted with caution.

One concern with the above analysis is the appropriateness of California and New York as counterfactuals for the rest of the country. Though the parallel trends assumption appears to be broadly satisfied, limiting the "treatment" group to a state or states that more closely resemble the "control" will further address this issue. Furthermore, focusing on California, which offers immigrants constant access to both Medicaid and food stamps (unlike New York, which only funds Medicaid), may offer a more precise estimate of the benefit eligibility change. Thus, I report additional estimates of equations 2.5-2.7, further limiting the sample to immigrants from only California and New Jersey. New Jersey has Medicaid and food stamp enrollment rates that are closest to those of California for legal residents of greater than five years. California's enrollment rates are both around 11% and New Jersey's are 13% and 10% respectively, while most other states are either much higher or lower. New Jersey does not offer Medicaid or food stamps to immigrants during the federal waiting period.

2.5 Results

Enrollment in Benefits

Figure 2.2 present graphical evidence of the effect of the discontinuity in eligibility on enrollment in means-tested benefits. Mean values of each outcome variable are binned by two

month periods, and only bins with at least ten individuals appear on the graphs. Values for individuals in states that experience the discontinuity for both programs are plotted separately from the values of California and New York. Graphically, it is clear that for both Medicaid and food stamps, a jump in enrollment occurs after the cutoff date. Furthermore, this increase does not seem to occur in New York (for Medicaid) or California (for food stamps and Medicaid).

Results from the RD analysis in Table 2.6 confirm these findings. Enrollment in Medicaid increases by 5.5% after the cutoff in the full sample, and 10.6% in the sample restricted to individuals under 300% of the FPL with co-resident children. Enrollment increases slightly more in the sample restricted to individuals under 130% of the FPL, but the more generous cutoff provides a better balance of both identifying likely eligible individuals and retaining sample size. For analysis of outcomes, I use the <300% FPL sample.

Results from the difference-in-differences analysis in Table 2.7 provide additional confirmation of this increase. With this specification, enrollment in Medicaid increases by 12.5% in the sample under 300% of the FPL.

Health and Health Care Use

Figures 2.3-2.4 provide graphical evidence of the effects of gaining benefit eligibility after the five-year cutoff on health outcomes. In Figure 2.3, there is a slight increase in preventative care, though this pattern is also present in states having consistent access to Medicaid. In Figure 2.4, there appears to be a jump in hospitalizations at the five year point, and the portion of people who go to the ER as their primary source of medical care seems to be increasing in time after this cutoff.

Table 2.8 reports parametric RD estimates for the effect of gaining eligibility on this set of health outcomes. None of the relationships are strong enough to have significant p-values. Table 2.10 reports the DID estimates for health outcomes. Again, we see very little in terms of strong parametric evidence, though it does support the graphical observation that using the ER as a primary source of medical care increases with access to Medicaid. The reduced form estimates indicate a 4% increase in this outcome as a result of the change in Medicaid eligibility status, while the IV estimates report a 30% increase resulting from being actually enrolled in Medicaid. This finding is not unexpected– similar results were reported in the Oregon Health Study (Finkelstein et al., 2012). Medicaid lowers the cost of visiting the emergency room, and for those who might have barriers to finding and/or developing a relationship with a primary care physician, it is a viable option for regular care.

Financial Outcomes and Transfers

Figures 2.5-2.6 provide graphical evidence of the effect of change in eligibility for benefits on financial outcomes, including money given to and received from family and friends outside the household. In Figure 2.5, there is evidence for an *increase* in out-of-pocket medical costs that does not occur in California and New York. Though this is unexpected, it is

certainly possibly that lower prices for medical care due to Medicaid enrollment would lead beneficiaries to pay more out-of-pocket, due to an increased volume of care. However, this graphical observation does not bear out in parametric estimation.

The RD results presented in Table 2.9 show decreased savings levels after the eligibility cutoff by a level amount of about $6700. In Table 2.11, the DID results show a similarly significant and large coefficient. This effect seems implausibly large, so it may be driven at least partially by outliers. However, decreased savings is a probable outcome of benefit eligibility. If immigrants were previously self-insuring against health shocks through saving, this could explain the decrease. Alternatively, in 2009 some states required asset tests in order to prove Medicaid eligibility, and SNAP currently requires that countable resources be less than $2,250 in determination of eligibility for food assistance. Thus, it is possible that respondents spent or moved savings to less liquid forms in order to qualify for benefits.

There is no conclusive evidence for impact of benefit eligibility on any of the other outcomes, including remittances.

2.6 Conclusion

The question of how immigrants respond to discontinuities in eligibility for social welfare benefits is an important one. Even with passage of the Affordable Care Act and expansion of Medicaid in many states, the five-year ban on immigrant receipt of federally funded benefits will remain in effect. Roughly one million immigrants become legal permanent residents each year (US Department of Homeland Security, 2013), indicating that hundreds of thousands of low-income, otherwise eligible individuals are denied access to health care and food assistance. It is clear from the results of this study that many apply and receive benefits immediately after passing the five-year marker.

Unfortunately, limited data and sample size precluded precise estimation of effects of this eligibility change on many outcomes. However, there is limited evidence for a decrease in formal savings that is a result of gaining access to Medicaid and food stamps, as well as increased use of the emergency department as a primary source of medical care. Further research is needed to develop a more complete understanding of these dynamics.

2.7 Tables and Figures

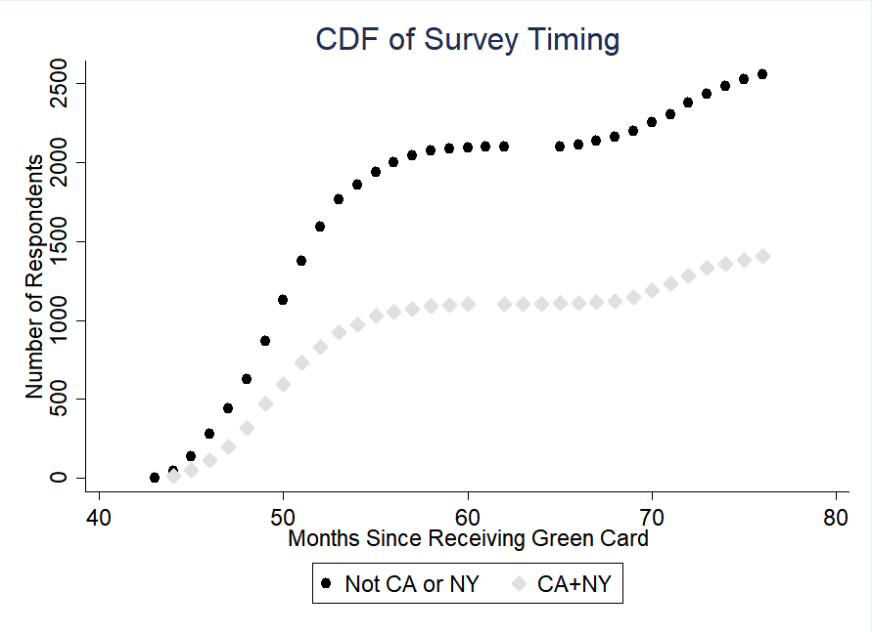


Figure 2.1

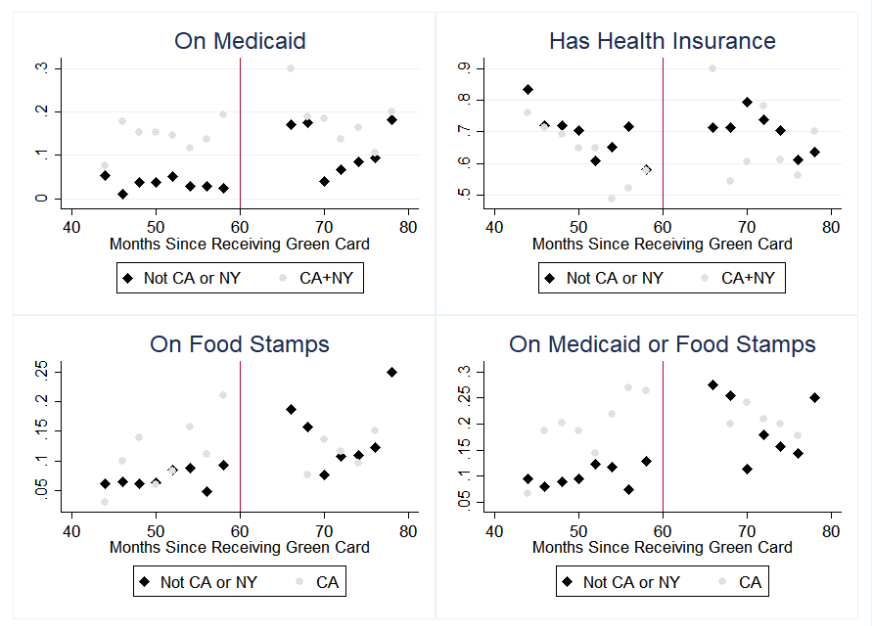


Figure 2.2

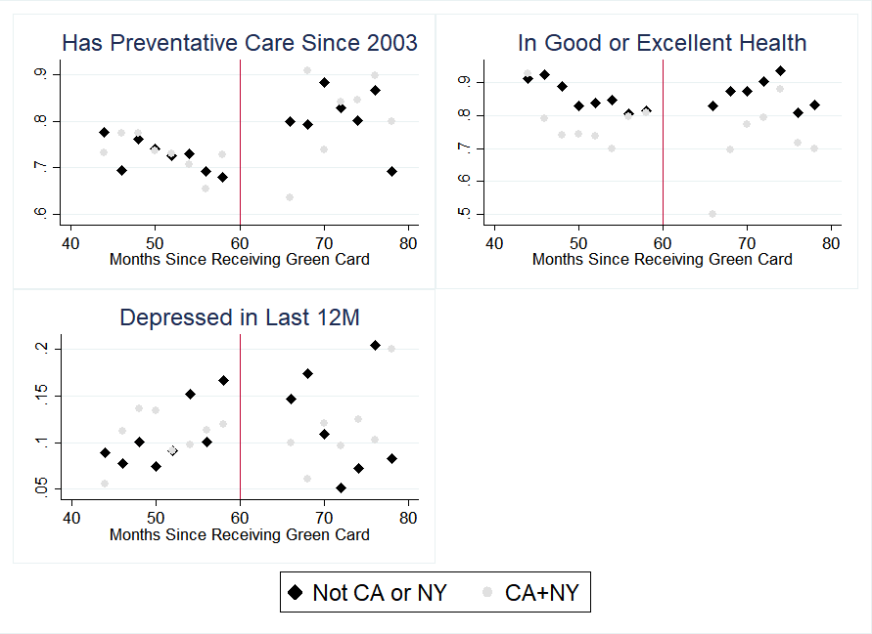


Figure 2.3

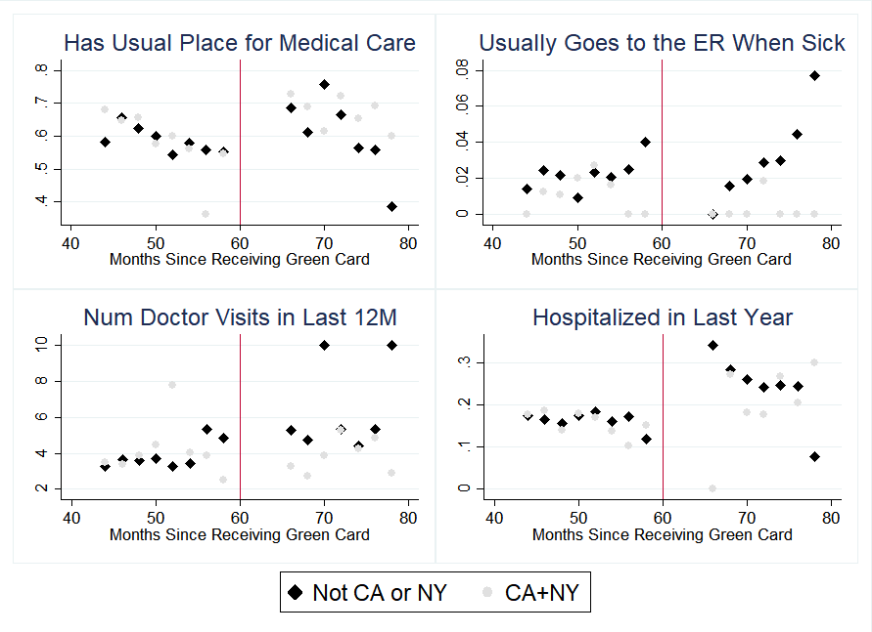


Figure 2.4

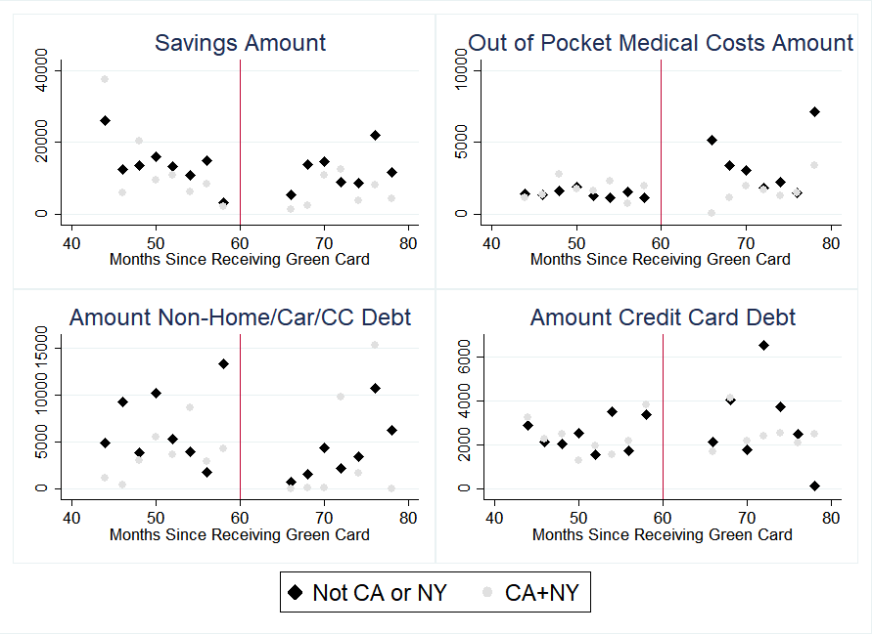


Figure 2.5

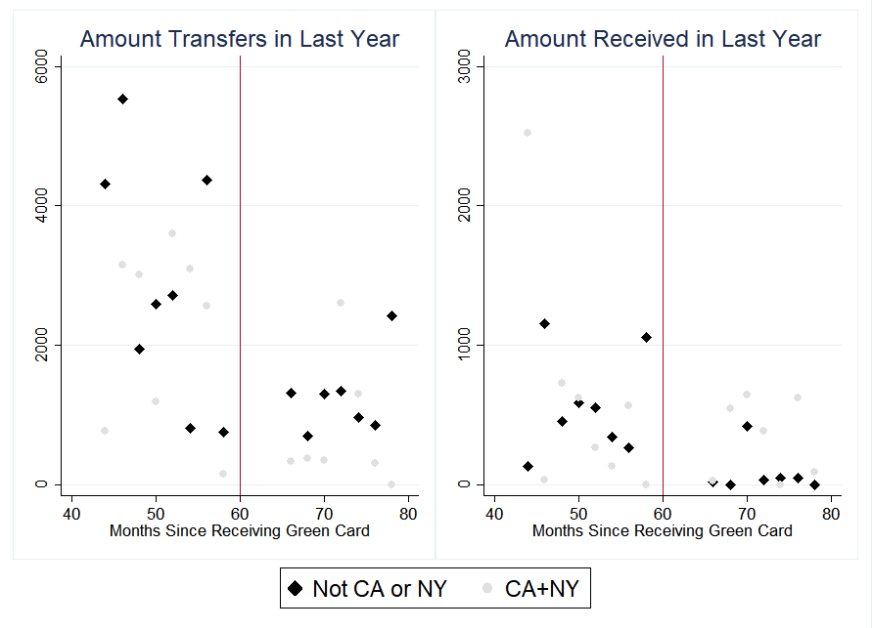


Figure 2.6

Summary Statistics			
Outcome	Mean	SD	N
MAGI	52634.25	76845.41	3877
On Medicaid	0.083		4014
Has Health Insurance	0.683		4073
On Food Stamps	0.087		3812
On Medicaid or Food Stamps	0.148		3570
Has Had Preventative Care Since 2003	0.753		4375
Has Usual Place of Care	0.609		4357
Usually Goes to ER when Sick	0.018		4381
Number of Visits to Doctor in Past Year	4.285	17.425	3749
Hospitalized in Past Year	0.179		4381
In Good Health	0.828		4108
Depressed in Last Year	0.103		4082
Amount Savings	13091.13	45880.11	3856
Amount Other Debt	5327.838	36415.54	3788
Amount Credit Card Debt	2426.668	6876.901	3773
Amount OOP Medical Costs	1783.236	5502.697	4381
Amount Transfers to Family/Friends	2454.68	11492	3723
Amount Received from Family/Friends	503.129	4304.653	3718

Table 2.1

NIS Sample by Region	
Region	Fraction Sample
Moved Regions Between Rounds 1 and 2	0.1351
California	0.2599
Florida	0.0749
Illinois	0.055
New Jersey	0.0631
New York	0.1096
Texas	0.0821
New England (CT,MA,ME,NH,RI,VT)	0.0687
Middle Atlantic (DE,DC,MD,PA)	0.0592
South Atlantic (GA,NC,SC,VA,WV)	0.0724
East South Central (AL,KY,TN,MS)	0.0104
East North Central (IN,MI,OH,WI)	0.0384
West North Central (IA,MN,MO,ND,SD,NE,KS)	0.0224
West South Central (LA,OK,AR)	0.0046
Mountain (AZ,CO,ID,NM, NV,UT,WY,MT)	0.0451
Pacific (AK,HI,OR,WA,FM,AP)	0.0331
US Territories (PR,VI,GU)	0.0005

Table 2.2

Survey Group Comparison			
Covariate	Wave 1 Mean	Wave 2 Diff	SE
Household Size	3.365	-0.065	0.05
Year of Birth	1965.793	1.663	0.322
Married	0.817	-0.011	0.013
Moved	0.151	-0.041	0.012
Employed	0.777	0	0.015
MAGI	54615.77	-2699.862	2896.355
Baseline in Good Health	0.937	-0.017	0.008
Baseline Often In Pain	0.083	0.006	0.009

Table 2.3

Medicaid Eligibility for Immigrants	
State	Who among otherwise eligible adult Legal Permanent Residents is eligible for state funded Medicaid (or other health coverage if indicated) prior to five years of residency? (as of 2010)
Alaska	Persons with terminal illnesses or chronic conditions
California	All
Delaware	Pregnant women
District of Columbia	All through DC Health Care Alliance (not Medicaid)
Hawaii	Pregnant women, seniors, persons with disabilities receive Medicaid; all others receive state premium assistance only if income below 100% FPL
Illinois	Pregnant women, abused immigrants
Massachusetts	Seniors, persons with disabilities if income below 100% of FPL, pregnant women
Minnesota	Pregnant women, abused immigrants receive full state funded Medicaid coverage, all others eligible to receive limited state-funded Medicaid coverage
Nebraska	Pregnant women
New Jersey	Pregnant women, others starting in 2010 (not relevant for this sample)
New Mexico	Pregnant women, abused immigrants
New York	All
Pennsylvania	All eligible for state-funded Medicaid, but more restrictive eligibility requirements than federally-funded Medicaid
Virginia	Pregnant women
Washington	Seniors, persons with disabilities eligible for limited coverage, pregnant women for full coverage

Table 2.4

Food Assistance Eligibility for Immigrants	
State	Who among otherwise eligible adult Legal Permanent Residents is eligible for state funded food assistance prior to five years of residency? (as of 2010)
California	All
Connecticut	All who have been residents for 6 months
Maine	All
Minnesota	Only those older than 50 or receiving TANF
Nebraska	All
Washington	All
Wisconsin	All

Table 2.5

Enrollment in Means-Tested Benefits After 5-year Ban (Regression Discontinuity)				
Outcome	Dependent Variable: Post-5 Years Residency)			
	Bandwidth 18 months	*Bandwidth 12 months*	*HH w/Co-Resident Children <300% FPL (BW±18m)*	*HH w/Co-Resident Children <130% FPL (BW±18m)*
On Medicaid	0.055	0.042	0.106	0.115
	(0.015)	(.017)	(.035)	(.043)
	[0]	[.012]	[.003]	[.008]
	N=2581	N=1616	N=934	N=617
Has Health Insurance	0.028	0.064	0.066	0.035
	(.024)	(.028)	(.047)	(.058)
	[.244]	[.024]	[.163]	[.546]
	N=2619	N=1639	N=945	N=624
On Food Stamps	0.05	0.048	0.116	0.08
	(.018)	(.022)	(.042)	(.048)
	[.007]	[.027]	[.006]	[.096]
	N=2449	N=1523	N=932	N=611
On Medicaid or Food Stamps	0.078	0.074	0.155	0.133
	(.022)	(.026)	(.046)	(.053)
	[0]	[.004]	[.001]	[.012]
	N=2301	N=1426	N=918	N=603

Table 2.6: This table reports estimates of β_1 from equation 2.1, the effect of passing the five-year residency cut-off on immigrant enrollment in means-tested benefits. Sample includes all non-refugee, under 65 y.o. spouses and main respondent from the New Immigrant Survey, living in all states except California and New York. Robust standard errors are clustered at the household level and are reported in parentheses. P-values reported in square brackets.

Enrollment in Means-Tested Benefits After 5-year Ban (Difference-in-Differences)					
Outcome	Dependent Variable: Post-5 Years Residency)				
	Bandwidth 18 months	*Bandwidth 12 months*	*HH w/Co-Resident Children <300% FPL (BW±18m)*	*HH w/Co-Resident Children <130% FPL (BW±18m)*	*HH w/Co-Resident Children <300% FPL (BW±18m) CA+NJ*
On Medicaid	0.045	0.018	0.125	0.122	0.29
	(.029)	(.034)	(.055)	(.068)	(.154)
	[.121]	[.609]	[.024]	[.076]	[.061]
	N=4013	N=2505	N=1578	N=1099	N=584
Has Health	0.016	-0.011	0.084	0.081	0.031
Insurance	(.04)	(.047)	(.068)	(.082)	(.168)
	[.684]	[.813]	[.219]	[.321]	[.855]
	N=4072	N=2542	N=1598	N=1114	N=587

Table 2.7: This table reports estimates of γ_2 from equation 2.6, the effect of passing the five-year residency cut-off in a state with a cut-off on immigrant enrollment in Medicaid. Sample includes all non-refugee, under 65 y.o. spouses and main respondent from the New Immigrant Survey. Robust standard errors are clustered at the household level and are reported in parentheses. P-values reported in square brackets.

Effect of Benefit Eligibility on Medical Outcomes (Regression Discontinuity)			
Outcome	Reduced Form		IV
	Post (Change in Level)	*Months Post (Change in Slope)*	*Enrolled in Benefits (Instrumented with Post)*
Any Preventative Care Since 2003	-0.152	0.012	-0.259
	(.116)	(.01)	(.315)
	[.189]	[.247]	[.411]
	[1]	[1]	[1]
			F=8.702
	N=948	N=948	N=918
Has Usual Place For Medical Care	0.109	0.005	0.342
	(.144)	(.014)	(.393)
	[.45]	[.716]	[.384]
	[1]	[1]	[1]
			F=8.413
	N=943	N=943	N=913
Usually Goes to ER When Sick	-0.052	0.006	-0.025
	(.057)	(.006)	(.103)
	[.369]	[.353]	[.81]
	[1]	[1]	[1]
			F=8.702
	N=950	N=950	N=918
Number of Visits to Doctor in Last Year	-3.811	0.159	-5.595
	(3.927)	(.375)	(7.88)
	[.332]	[.672]	[.478]
	[1]	[1]	[1]
			F=10.005
	N=870	N=870	N=843
Hospitalized in Last Year	0.113	-0.014	0.042
	(.122)	(.012)	(.313)
	[.358]	[.23]	[.893]
	[1]	[1]	[1]
			F=8.702
	N=950	N=950	N=918
In Good or Excellent Health	0.136	0.015	0.768
	(.132)	(.013)	(.446)
	[.303]	[.241]	[.085]
	[1]	[1]	[.595]
			F=8.495
	N=942	N=942	N=912
Depressed in Last Year	-0.001	-0.014	-0.313
	(.096)	(.009)	(.299)
	[.989]	[.116]	[.296]
	[1]	[.812]	[1]
			F=8.574
	N=944	N=944	N=914

Table 2.8: Column 1 reports estimated coefficient β_1 from equation 2.2. Column 2 reports estimated coefficient β_2 from equation 2.2. Column 3 reports estimated coefficient θ_1 from equation 2.4. Sample includes all non-refugee, under 65 y.o. spouses and main respondent from the New Immigrant Survey who have co-resident children and are under 300% of the federal poverty line, living in all states except California and New York. Robust standard errors are clustered at the household level and are reported in parentheses. P-values reported in square brackets: 2nd is corrected for multiple inferences using the Bonferroni method. Kleibergen-Paap F-statistics are reported for weak identification testing.

Effect of Benefit Eligibility on Financial Outcomes (Regression Discontinuity)			
Outcome	Reduced Form		IV
	Post (Change in Level)	*Months Post (Change in Slope)*	*Enrolled in Benefits (Instrumented with Post)*
Savings Amount	-6717.626	-404.908	-31160.93
	(3545.595)	(275.961)	(20226.61)
	[.059]	[.143]	[.124]
	[.354]	[.858]	[.744]
			F=8.702
	N=942	N=942	N=918
Other Debt Amount	777.954	-490.27	-10894.23
(Includes Medical)[a]	(6758.587)	(469.996)	(19224.41)
	[.908]	[.297]	[.571]
	[1]	[1]	[1]
			F=8.552
	N=923	N=923	N=906
Credit Card Debt Amount	606.182	-231.934	-2967.352
	(2703.977)	(214.933)	(6247.606)
	[.823]	[.281]	[.635]
	[1]	[1]	[1]
			F=8.556
	N=916	N=916	N=899
Out of Pocket Medical	-694.502	208.388	3398.393
Costs Amount	(3141.276)	(347.606)	(3827.617)
	[.825]	[.549]	[.375]
	[1]	[1]	[1]
			F=8.702
	N=950	N=950	N=918
Total Transfers to Friends	-1828.504	86.948	-3915.787
and Family outside HH	(2794.734)	(235.388)	(14204.27)
in Last 12m	[.513]	[.712]	[.783]
	[1]	[1]	[1]
			F=8.336
	N=896	N=896	N=874
Total Receipts from Friends	-130.75	23.651	8.356
and Family Outside HH	(351.055)	(35.253)	(1854.608)
in Last 12m	[.71]	[.503]	[.996]
	[1]	[1]	[1]
			F=8.429
	N=892	N=892	N=871

Table 2.9: [a]Includes all debt except home, car and credit card. Column 1 reports estimated coefficient β_1 from equation 2.2. Column 2 reports estimated coefficient β_2 from equation 2.2. Column 3 reports estimated coefficient θ_1 from equation 2.4. Sample includes all non-refugee, under 65 y.o. spouses and main respondent from the New Immigrant Survey who have co-resident children and are under 300% of the federal poverty line, living in all states except California and New York. Robust standard errors are clustered at the household level and are reported in parentheses. P-values reported in square brackets: 2nd is corrected for multiple inferences using the Bonferroni method. Kleibergen-Paap F-statistics are reported for weak identification testing.

Effect of Benefit Eligibility on Medical Outcomes (Difference-in-Differences)				
Outcome	Full Sample		CA and NJ Only	
	Reduced Form	*IV*	*Reduced Form*	*IV*
Any Preventative Care Since 2003	0.069	0.518	0.166	0.567
	(.047)	(.436)	(.105)	(.444)
	[.143]	[.234]	[.114]	[.202]
	[1]	[1]	[.798]	[1]
		F=5.091		F=3.532
	N=1606	N=1578	N=591	N=584
Has Usual Place For Medical Care	-0.03	-0.254	0.215	0.767
	(.061)	(.522)	(.136)	(.538)
	[.628]	[.627]	[.114]	[.155]
	[1]	[1]	[.798]	[1]
		F=4.961		F=3.464
	N=1599	N=1571	N=589	N=582
Usually Goes to ER When Sick	0.037	0.302	0.096	0.331
	(.016)	(.183)	(.082)	(.355)
	[.019]	[.099]	[.245]	[.352]
	[.133]	[.693]	[1]	[1]
		F=5.091		F=3.532
	N=1609	N=1578	N=592	N=584
Number of Visits to Doctor in Last Year	4.147	30.79	7.677	21.514
	(2.714)	(23.209)	(3.981)	(13.566)
	[.127]	[.185]	[.054]	[.114]
	[.889]	[1]	[.378]	[.798]
		F=5.777		F=5.051
	N=1466	N=1442	N=536	N=530
Hospitalized in Last Year	0.041	0.359	0.101	0.331
	(.052)	(.438)	(.129)	(.544)
	[.436]	[.412]	[.432]	[.543]
	[1]	[1]	[1]	[1]
		F=5.091		F=3.532
	N=1609	N=1578	N=592	N=584
In Good or Excellent Health	0.015	0.141	-0.066	-0.311
	(.055)	(.475)	(.134)	(.653)
	[.788]	[.767]	[.623]	[.635]
	[1]	[1]	[1]	[1]
		F=4.712		F=2.218
	N=1599	N=1571	N=588	N=581
Depressed in Last Year	0.004	0.073	0.143	0.48
	(.04)	(.32)	(.107)	(.521)
	[.917]	[.82]	[.181]	[.358]
	[1]	[1]	[1]	[1]
		F=5.069		F=3.503
	N=1602	N=1574	N=590	N=583

Table 2.10: Columns 1 and 3 report estimated coefficient β_2 from equation 2.5. Columns 2 and 4 report estimated coefficient θ_2 from equation 2.7. Sample in first two columns includes all non-refugee, under 65 y.o. spouses and main respondent from the New Immigrant Survey who have co-resident children and are under 300% of the federal poverty line. The sample in columns 3 and 4 is further restricted to a comparison of California and New Jersey. Robust standard errors are clustered at the household level and are reported in parentheses. P-values reported in square brackets: 2nd is corrected for multiple inferences using the Bonferroni method. Kleibergen-Paap F-statistics are reported for weak identification testing.

Effect of Benefit Eligibility on Financial Outcomes (Difference-in-Differences)				
Outcome	Full Sample		CA and NJ Only	
	Reduced Form	*IV*	*Reduced Form*	*IV*
Savings Amount	-5706.721	-45373.46	-9778.171	-32153.72
	(2276.22)	(25780.95)	(3485.996)	(20597.91)
	[.012]	[.079]	[.005]	[.119]
	[.072]	[.474]	[.03]	[.714]
		F=5.22		F=3.544
	N=1600	N=1569	N=591	N=583
Other Debt Amount	-4252.243	-31722.04	-241.577	-1138.563
(Includes Medical)[a]	(5721.488)	(45244.28)	(7722.86)	(22005)
	[.457]	[.483]	[.975]	[.959]
	[1]	[1]	[1]	[1]
		F=5.751		F=4.591
	N=1575	N=1546	N=582	N=574
Credit Card Debt Amount	236.276	1905.015	-1286.993	-3805.729
	(987.39)	(7765.578)	(1133.727)	(3504.176)
	[.811]	[.806]	[.257]	[.278]
	[1]	[1]	[1]	[1]
		F=5.246		F=4.44
	N=1563	N=1534	N=574	N=566
Out of Pocket Medical	860.335	6840.359	1283.772	4363.52
Costs Amount	(804.094)	(7401.108)	(2026.676)	(7895.91)
	[.285]	[.356]	[.527]	[.581]
	[1]	[1]	[1]	[1]
		F=5.091		F=3.532
	N=1609	N=1578	N=592	N=584
Total Transfers to Friends	-601.173	-4218.862	-789.561	-2631.893
and Family outside HH	(642.797)	(4795.314)	(877.204)	(3293.797)
in Last 12 m	[.35]	[.379]	[.369]	[.425]
	[1]	[1]	[1]	[1]
		F=6.335		F=3.414
	N=1530	N=1501	N=567	N=560
Total Receipts from Friends	-510.878	-3677.753	-404.726	-1393.29
and Family Outside HH	(417.619)	(3296.202)	(436.125)	(1624.189)
in Last 12m	[.221]	[.265]	[.354]	[.391]
	[1]	[1]	[1]	[1]
		F=6.053		F=3.329
	N=1525	N=1496	N=566	N=559

Table 2.11: [a]Includes all debt except home, car and credit card. Sample in first two columns includes all non-refugee, under 65 y.o. spouses and main respondent from the New Immigrant Survey who have co-resident children and are under 300% of the federal poverty line. The sample in columns 3 and 4 is further restricted to a comparison of California and New Jersey. Columns 1 and 3 report estimated coefficient β_2 from equation 2.5. Columns 2 and 4 report estimated coefficient θ_2 from equation 2.7. Robust standard errors are clustered at the household level and are reported in parentheses. P-values reported in square brackets: 2nd is corrected for multiple inferences using the Bonferroni method. Kleibergen-Paap F-statistics are reported for weak identification testing.

Chapter 3

Deferring Agency at End-of-Life: The Role of Information and Advance Directives

3.1 Introduction

Policies on medical care and decision making at end-of-life are or will become personally relevant to most people in the United States. A 2013 Pew Research survey found that 47% of adults have had experience with a terminal illness of a close friend or family member within the past five years, of which the issue of withholding life sustaining treatment came up in roughly half of cases (Pew, 2013). Nearly half of all adults over the age of 65 report giving a great deal of thought to their own wishes for end-of-life medical treatment. In a large sample of all individuals dying between 2000 and 2006, around 43% required decision making, most of whom were no longer able to make those decisions for themselves (Silveira et al., 2010).

End-of-life care is also of significant economic importance. Care for patients in the last year of life accounts for around a quarter of yearly Medicare spending, or nearly 4% of the entire federal budget (Hogan et al., 2001). Average out-of-pocket medical expenses in the last year of life approached $12000 in 2006, with the 90th percentile paying roughly $29000 (Marshall et al., 2011). Despite these large outlays, care often appears not to align with the wishes of patients and their families. Over 80% of individuals state that they want to avoid hospitalization and intensive care at the end of life, yet 73% of people over the age of 65 die in a hospital (Dartmouth IHPCP, 2017; Hall et al., 2013). One survey of family members of hospitalized patients who ultimately died found that 30% reported dissatisfaction with communication and decision making (Baker et al., 2000). Furthermore, multiple studies find no evidence that higher spending leads to improved life expectancy, and even that patients who forgo aggressive care in favor of palliative care live longer (Skinner and Wennberg, 1998; Dartmouth IHPCP, 2017; Temel et al., 2010).

Improved advance care planning (ACP) and completion of advance directives (ADs), which allow patients to appoint a medical decision maker and identify their wishes for care in different end-of-life situations, may help to reduce the discrepancy between the care that patients want and the care that they ultimately receive. Observational evidence suggests that ADs influence decisions made at the end of life toward alignment with patients' preferences. Subjects who indicate limited treatment or comfort care (over 90% of individuals with ADs) are more likely to receive it than those who do not complete an AD, as are patients who request all life-prolonging treatments possible (Silveira et al., 2010). Patients who do not report having end-of life discussions on average report higher rates of aggressive care, which is in turn associated with worse patient quality of life (Wright et al., 2008). Though there is still limited experimental evidence on the effects of ADs and ACP, one recent randomized controlled trial found that end-of-life wishes were much more likely to be known and followed for patients receiving facilitated ACP than for those who received usual care (Detering et al., 2010). Laws enhancing incentives for compliance with ADs reduce the probability of dying in an acute care hospital (Kessler and McClellan, 2004). Currently available results are mixed, but several studies have found evidence that ADs and ACP can reduce spending at end-of-life, especially for patients with dementia and those living in geographical areas of high end-of-life spending (Nicholas et al., 2011; Dixon et al., 2015; Klingler et al., 2016). Recent innovation in electronic storage of ADs is also expected to solve some of the challenges in implementation of ADs, increasing their effectiveness and cost-saving potential (Institute of Medicine, 2014).

Policymakers and providers have recognized the value of ACP and ADs and have recently increased resources to support this facet of care. In 2016, Medicare began reimbursement of physicians who consult with their patients on ACP (Zeitoun, 2015). Additionally, the fraction of patients who have an AD on file has more than doubled in the last 20 years. However, many individuals have yet to take this critical step in planning for their end-of-life medical care. Nationally, 63.3% of adults do not have an AD on file, as well as 32.4% of patients who ultimately require surrogate decision making (Yadav et al., 2017; Silveira et al., 2010). For certain populations, completion rates are even lower. Black Americans are roughly half as likely as white Americans to complete an AD (Johnson et al., 2008). Additionally, advance directive completion is strongly associated with income: those with annual income greater than $75k are nearly twice as likely to complete an advance directive as those with annual income less than $25k, controlling for other factors (Rao et al., 2014). This suggests that low-income populations may be especially vulnerable to receiving dis-preferred end-of-life care.

It is not well understood why many patients fail to engage in advance care planning and ultimately face care that appears to be misaligned with stated preferences. We explore patients' decisions of whether and how to complete an AD, leveraging detailed electronic medical records (EMR) and survey data from a pilot evaluation of two ACP programs. These programs were evaluated for feasibility and impact on a sample of 4850 elderly patients at two Providence St. Joseph Health primary care clinics in Oregon.

Our first contribution will be to report the effects of two interventions (and their inter-

action) on AD completion and decisions, each aimed at addressing a separate theoretical barrier to planning for end-of-life. The first intervention facilitated AD completion through the hosting of a clinic Advance Directive Drive, which aimed to address the time and logistical difficulty of AD completion by providing patients with resources to ensure legally validation of their document and successful uploading to the EMR. Completing a legally valid AD and integrating it with the patient's medical records can be complex. Many states require ADs to be notarized or signed by multiple witnesses, who are not permitted to be the patient's family or medical care providers (California Department of Justice, 2017). People may be deterred by these hassle costs and avoid planning for end-of-life care until it's too late (Baicker et al., 2015). In this case, an intervention that lowers the immediate costs of completing ADs and encourages patients not to procrastinate could be successful in increasing completion rates. We compare AD upload rates across one clinic that held a series of drives, and another that did not. We find no significant difference in AD upload rates between the two clinics during the outcome period among patients to whom communication was sent via email. However, we find significant increases (4.5 percentage points) in upload rates for patients without email who were contacted via letter about the drive relative to patients who were sent a reminder letter only.

Another possible barrier to AD completion is information frictions around ACP. For example, patients may not know their likelihood of requiring proxy decision making, what care they would receive in absence of an AD, or their preferences over this care. Halpern et al. (2013) found that seriously ill patients are influenced in their decision between comfort and life extension oriented care by which option is the default on the AD form. A proposed explanation for this behavior is that patients don't have strong preferences about their end-of-life care, but a lack of information could also explain these results. There is some evidence that providing easy-to-read ADs and information about related decisions increases completion rates (Sudore et al., 2017). As such, we evaluate a second intervention, which electronically distributed informational videos and materials to patients. This intervention was randomly assigned and distributed to half of eligible patient in each of the two sample clinics. We find no significant effect of the electronic information intervention on AD completion, which we attribute to very low view rates.

The evaluation of these novel approaches to increasing AD completion rates has the potential to provide insight into the validity of some of the proposed theoretical explanations for failure to complete an AD. However, it is important to note that the decision not to complete an AD is potentially consistent with a fully informed, time-consistent decision maker. Logistical, psychological and interpersonal costs to completing an AD may be high relative to perceived benefits, and thus difficult to overcome with a simple policy change. Additionally, certain patients may approve of the default bias toward life-extension in absence of an AD. Becker et al. (2010) argue that very high spending near end-of-life is rationalizable if existing estimates of the value of a life year do not apply to the end of life (due to factors such as low opportunity cost of medical spending near ones death). To further investigate the interaction between preferences, beliefs, information and time inconsistency, we develop a simple model of the decision to complete an AD. We also conduct descriptive analysis to

assess the relationship between information frictions, hassle costs, health status, and actual AD decisions.

The remainder of this chapter is structured as follows: Section 3.2 outlines a preliminary economic model of the decision to complete an AD, which motivates our research question and design. Section 3.3 describes the data and context. Section 3.4 describes the interventions and empirical approach. Section 3.5 presents the results and Section 3.6 concludes.

3.2 Preliminary Model

Setup

Assume that every year, there is a probability of reaching end-of-life $D(a, h)$ which is a function of age and current health status (increasing in a, decreasing in h). Upon reaching end-of-life, there is a probability p that an agent will be unable to make decisions about end-of-life care. The agent is able to make his preferred care decision at end-of-life with probability $1 - p$. For simplicity, assume that if the agent reaches death, there are two possible outcomes: comfort and life extension. The agent's utility under comfort is 0, and his utility under life extension is w_l.

If the agent is able to make his own decisions at end-of-life, his wishes are implemented with certainty and his resulting utility is $max(0, w_l)$. If the agent incurs the cost c_a and completes an advanced directive, he can implement either comfort or life extension with certainty in the case that he reaches end-of-life and is not able to make his own decisions. If the agent has not completed an advanced directive and reaches end-of-life with inability to make his own decisions, life extension is implemented with probability q.

Assume that in the present period, the agent does not know the state of the world $\omega \in \Omega$ which will be realized when the agent reaches end-of-life, and $w_l(\omega)$ depends on the state of the world. The agent has a prior belief $\mu_0(w_l) \in int(\Delta(\Omega))$ about the state of the world and distribution of w_l. Under this model, the agent will choose to complete an advanced directive in the present period if:

$$\sum_{t=0}^{\infty} \delta^t D(h, a+t)(max\{0, E[w_l]\} - qE[w_l]) \geq \frac{c_a}{p} \tag{3.1}$$

Let the agent's utility in this model be given by $V(h, a, q, p, w_l, c_a)$, and expected values above be over the agent's prior on the distribution of w_l.

This setup implies that agents will be more likely to fill out advanced directives if they are older or sicker. The probability of reaching a situation where an advanced directive is needed is also relevant to the decision. If most agents are able to make their own decisions and the AD doesn't end up being used, then there isn't much value to completing it. Finally, if decisions made in absence of an AD will align with the patients' preferences, he is less likely to fill out an advanced directive.

Advance Directive Takeup with Procrastination

Now suppose that the agent's discount factor is quasi-hyperbolic, so his discount factor in period 0 is equal to 1 and his discount factor in period t is equal to $\beta\delta^t$. Consider the case when a time consistent agent with $\beta = 1$ chooses to complete an advance directive. That is,

$$\sum_{t=0}^{\infty} \delta^t D(h, a+t)(max\{0, E[w_l]\} - qE[w_l]) \geq \frac{c_a}{p} \tag{3.2}$$

Holding the other parameter values constant, suppose the agent is time inconsistent ($\beta < 1$) and naive. In this case, the agent compares the utility of completing the directive today with the utility of doing so tomorrow. He completes the directive today if

$$D(h, a)(max\{0, E[w_l]\} - qE[w_l]) \geq \frac{c_a(1-\beta\delta)}{p} \tag{3.3}$$

Otherwise, he will procrastinate until the probability of reaching end-of-life $D(h, a)$ in the current period is high enough to motivate him to complete the directive. Thus, an agent who is time inconsistent and naive will be strictly less likely than an otherwise identical time consistent agent to complete a directive. If we consider hassle and psychological costs of filling out advanced directives, c_a might potentially be quite high and deter AD completion. This will be exacerbated if agents are time inconsistent, and even small c_a may prevent agents from completing ADs. Lowering costs through the drives (reduced hassle) may increase rate of AD completion.

Advance Directive Takeup with Information

Now, suppose that the agent can choose to incur cost c_i and receive a signal $s(\omega)$ which provides information on the state of the world and distribution of w_l, leading to a posterior belief $\mu_s \in \Delta(\Omega)$. Suppose that signal reveals the state of the world with certainty, so the value of w_l becomes known if the agent incurs c_i.

We can think of this problem in two stages: first, the agent decides whether or not to pay c_i and learn w_l. Once w_l is revealed (or not), the agent chooses whether to pay cost c_a and complete the advanced directive. The agent pays c_i if

$$-c_a\big[P|w_l| > w_l^*\big] + \sum_{t=0}^{\infty} p\delta^t D(h, a+t)(qE[w_l \mid |w_l| < w_l^*] + E[w_l \mid w_l > w_l^*]) - c_i > V(h, a, q, p, w_l, c_a) \tag{3.4}$$

Expected values are over the agent's prior on the distribution of w_l, and w_l^* is such that

$$\sum_{t=0}^{\infty} \delta^t D(h, a+t)(max\{0, w_l^*\} - qw_l^*) = \frac{c_a}{p} \tag{3.5}$$

In the second stage, the agent will complete an advanced directive and incur additional cost c_a if he has learned that $|w_l| > w_l^*$, or if he has not paid c_i and

$$\sum_{t=0}^{\infty} \delta^t D(h, a+t)(max\{0, E[w_l]\} - qE[w_l]) \geq \frac{c_a}{p}. \tag{3.6}$$

When information is not available or is very high cost, agents will not fill out an advanced directive if their expectation is that they will not have a strong preference for life extension or comfort. However, if they are relatively unsure of this belief (prior distribution of preference for life extension is spread out), they may be induced to pay a reasonable cost for information and fill out an AD. Lowering the cost of information may thus increase the rate of AD completion. On the other hand, without information, agents who expect that they will have strong preferences for life extension or comfort will fill out an AD. However, if they are relatively unsure of this belief and information is low cost, they may pay for information and learn that they do not have strong preferences for life extension or comfort. As a result, it is also possible that lowering the cost of information would reduce AD completion.

3.3 Data and Setting

Our study partner, Providence St. Joseph Health (PSJH), is one of the largest nonprofit health systems in the United States. With 51 hospitals and 829 clinics operating in Washington, Oregon, California, Montana, New Mexico, Texas and Alaska, PSJH treats over 5 million patients each year. Medical group leaders across PSJH recently prioritized ACP for primary care patients over the age of 65, with the aim of increasing the proportion of patients with a completed AD saved in the EMR. PSJH routinely tracks and monitors clinical performance related to ACP documentation and other standards of care. In our sample, around 14% of patients had an AD at the start of the study.

As part of reaching the ACP targets described above, PSJH had previously used videos from ACP Decisions, which provide evidence-based explanations of key topics relevant to ACP and the completion of ADs, available in multiple languages (El-Jawahri et al., 2015). Clinicians and patients have welcomed these videos to support meaningful conversations, and they are associated with changes in informed choices patients make regarding treatments such as CPR and mechanical ventilation. ACP Decisions videos can be used in medical facilities or at home, and can be distributed to patients electronically.

Electronic Medical Record Data

For all patients in the sample, we had access to detailed EMR data including healthcare use (office visits, hospital stays, medications), health status (diagnoses, depression screening score), insurance status and demographics. See Table 3.1, which lists the sample mean and standard deviation for EMR indicators and variables such as the Charlson co-morbidity

index which were calculated from EMR data. The table also profiles survey respondents, a subset of the full sample (to be discussed in more detail later).

Demographic, Health Status, and Healthcare Use Covariates from EMR

	Full Sample			Survey Respondents		
Covariate	Mean	(SD)	N	Mean	(SD)	N
Visits (PSJH) in Last 2 Years	12.14	(12.51)	4850	12.1	(10.35)	338
Number of Medications	9.81	(8.26)	4850	9.53	(8.27)	338
Number of Inpatient Days (at PSJH), Last 2 Years	.95	(3.78)	4850	.74	(2.72)	338
Had Surgury (at PSJH), Last 2 Years	0	(.06)	4850	.01	(.11)	338
Age	73.99	(7.68)	4850	72	(6.13)	338
Height	65.75	(4.02)	4848	66.16	(3.81)	338
Weight	175.65	(44.55)	4848	179.44	(44.49)	338
BMI	28.44	(6.31)	4848	28.68	(6.16)	338
Depression Screening Score	1.38	(3.46)	3332	1.07	(2.8)	262
Number of Diagnoses	14.75	(9.12)	4827	14.41	(9.29)	337
Charlson Comorbidity Index	.91	(1.33)	4827	.78	(1.36)	337
PCP Specialty: Internal Medicine	.13		4850	.11		338
PCP Specialty: Family Practice	.73		4850	.75		338
Male	.37		4850	.36		338
English is Primary Language	.97		4850	1		338
White, Non-Hispanic	.89		4850	.95		338
Not Religious	.28		4850	.3		338
MyChart Activated	.71		4850	.96		338
Married	.6		4850	.64		338
Public Insurance	.89		4850	.91		338

Table 3.1: This table lists health and demographic information for the full sample, and separately for the sample that completed the optional survey.

Surveys

We collected data from participants who responded to an email survey sent prior to the beginning of intervention activities. Responses were recorded using the REDCap (Research Electronic Data Capture) electronic data capture tool hosted by PSJH. The survey was distributed via email to a randomly selected 80% of patients meeting the inclusion criteria in the two selected clinics. The survey was open to participation for two weeks and was distributed directly prior to the implementation of the interventions. An initial email message was sent to all eligible patients and then three follow-up messages were sent at twice weekly intervals to patients who had not yet responded. See Table 3.1 for a comparison of survey respondents to the full sample of eligible patients. On average, survey respondents were younger, healthier, and more likely to be white than patients in the full sample.

Table 3.2 lists a summary of key survey responses. Questions addressed patient's knowledge and beliefs about aspects of end-of-life care, as well as factors that might influence the value of completing an AD, such as whether the patient wished to designate someone other

than the legal default as their medical decision maker. Appendix Section C.4 includes the full survey questions and response options.

Survey Responses			
Response	Mean	(SD)	N
Background: Close Friend/Relative had Terminal Illness, Last 5 Years	.625		309
Background: Currently Working	.205		322
Background: Ever Worked as a Healthcare Provider	.207		323
Background: Has Bachelor's Degree or Above	.609		322
Background: Income <$50k	.254		338
Background: Income >$100k	.24		338
Background: Income >$50k <$100k	.325		338
Background: Legal Default Surrogate w/in Driving Distance	.895		306
Background: Legal Default Surrogate- Child	.251		331
Background: Legal Default Surrogate- Other	.063		331
Background: Legal Default Surrogate- Parent	.015		331
Background: Legal Default Surrogate- Spouse	.622		331
Background: Legal Default Surrogate- Sibling	.048		331
Background: Number of Legal Default Surrogates	1.34	(.883)	329
Background: Religion is Moderately or Very Important	.581		322
Background: Spends 4+ Weeks/Year at Vacation Home	.115		338
Belief: ACP is a Hassle	.273		304
Belief: Default for HC Providers is Comfort Care	.145		311
Belief: Default for HC Providers is Life Extension	.254		311
Belief: Hours it Would Take to Complete AD	2.706	(6.881)	236
Belief: Probability will Need AD Ever	67.733	(26.048)	255
Belief: Probability will Need AD w/in 5 Years	26.274	(23.519)	266
Discounting: 1 Year Discount Factor	.682	(.288)	278
Discounting: Procrastinates on ACP	.368		304
Health: In Very Good or Excellent Health	.52		323
Health: Number of Bad Health Days in Last Month	1.914	(5.547)	292
Information: Correct Response Regarding Feeding Tube Effectiveness	.228		311
Preference: Ambivalence for Life Prolonging Treatments at End of Life	.344		311
Preference: Prefers Life Prolonging Treatments at End of Life	.141		311
Preference: Prefers to Die at Home	.615		314
Preference: Prefers to Stop Treatment for Permanently Unconsious	.828		309
Self Reported ACP: Chosen or Desired Proxy is the Legal Default	.502		297
Self Reported ACP: Has Had Detailed ACP Discussions w/ Family	.364		308
Self Reported ACP: Has a HC Proxy	.768		306
Self Reported ACP: Has an AD	.752		306
Self Reported ACP: Intends to Appoint HC Proxy w/in Next 6m	.098		306
Self Reported ACP: Intends to Complete an AD w/in 6m	.118		306

Table 3.2: This table summarizes responses to the optional survey.

3.4 Intervention Design and Empirical Approach

In July 2018, one of the two pilot clinics was selected to host an in-person AD Drive, where patients had the opportunity to ask questions about ACP and ADs, receive access to appropriate witnesses to validate an AD, and receive assistance in uploading an AD to the EMR. The drives were held in an open-house style with no appointment needed, for several

hours on three different days. One week prior to the drives, email communications were sent to the target population at both clinics. This target population was defined by patients who were 65 or older, had no AD on file with PSJH, and had an email address listed in their patient records.

In addition to receiving a generic reminder about completing an AD, patients received either (1) a notification of the in-person AD Drives happening in their clinic the following week, (2) a link to an 11-minute video and informational brochures on ADs and planning for end-of-life,[1] (3) both the AD Drive notification and video link, or (4) a generic reminder only. Full text of these email communications can be found in Appendix Section C.2. Patients were randomly assigned to receive the video link, while the Drive notification was assigned based on clinic. Thus, patients in the No-Drive clinic received either communication (2) or (4), while patients at the Drive clinic received either (1) or (3). For patients selected to receive the video link, we observed what portion of the video was viewed.

Patients without an email on file who otherwise met sample criteria were sent paper letter versions of the communications. Those at the AD Drive clinic received notification of the drive with a reminder about completing an AD, while those at the No-Drive clinic received a generic reminder only. Letter proofs can be found in Appendix Section C.3. Table 3.3 shows the breakdown of the interventions and the number of individuals assigned to each treatment. It also shows the number of patients who watched the ACP decisions videos and the number of patients who uploaded an AD after the interventions in each treatment group.

This study was pre-registered in the AEA RCT Registry. See Appendix Section C.1 for further detail.

Sample and Intervention Participation

Sample	N	N (No AD at baseline)	N (Uploaded AD post baseline)	N (Watched Video)
Clinic with Drive	2337	1994	54	8
Patient No Email (Drive Invitation Letter)	432	376	28	0
Patient Has Email	1905	1618	26	8
Sent Video Link + Drive Invitation	819	809	11	8
Drive Invitation	1086	809	15	0
Clinic Without Drive	2513	2177	35	11
Patient No Email (Reminder Letter)	988	847	19	0
Patient Has Email	1525	1330	16	11
Sent Video Link	670	658	5	11
Reminder Only	855	672	11	0

Table 3.3: This table describes the sample and intervention participation.

Empirical Approach

We estimate equation 3.7 below, reporting $\beta_1 - \beta_3$ to determine the impact of the drive and video interventions as well as their interaction. X_i is a list of controls from EMR data, listed

[1] created by ACP Decisions

in Table 3.1; I[Drive Only] is an indicator for individuals at the clinic hosting the drives who did not receive a video link; I[Video Only] is an indicator for individuals at the clinic not hosting the drives who received a video link; and I[Video & Drive Clinic] is an indicator for individuals at the clinic hosting the drives who also received a video link.

$$Y_i = \beta_0 + \beta_1 I[DriveOnly] + \beta_2 I[VideoOnly] + \beta_3 I[Video\&DriveClinic] + X_i + \epsilon_i \quad (3.7)$$

Though both clinics volunteered to host the drives and one was chosen arbitrarily, selection of patients into each of these clinics is not likely to be random. Thus, to make a causal interpretation, we must assume that the behavior of patients between the two clinics with respect to uploading of ADs is not systematically different as a result of factors other than the AD Drive. Because the outcome is narrowly defined as uploads occurring directly after the communications and drive, we may be more confident in attributing estimated effects to the interventions described. We also take a pooled approach and estimate the following:

$$Y_i = \gamma_0 + \gamma_1 I[DriveClinic] + X_i + \epsilon_i \quad (3.8)$$

$$Y_i = \alpha_0 + \alpha_1 I[Video] + X_i + \epsilon_i \quad (3.9)$$

Equation 3.8 estimates the pooled effect of the drive for patients in both video assignment groups, while equation 3.9 estimates the pooled effect of receiving the video across both clinics.

3.5 Results

Table 3.4 presents the results of the estimation of equation 3.7 above. Here, we limit the sample to only patients who had an email on file. Communication for all four arms in this group was via email. We further limit the sample to only patients who did not have an AD at baseline. Results are reported for a specification with and without covariates. We estimate lower rates of upload for all treatment groups in comparison to the reminder only group. The comparison is statistically significant at the 0.10 level for the group that received video link only, who were 1.2 percentage points (SE 0.6) less likely to upload an AD than those in the reminder only group. There is also a significant reduction in probability of uploading an AD with dis-preference for life prolonging treatments for this group, but this is likely driven by the lower overall upload rate.

Treatment Effect, Patients with Email							
		No Covariates			EMR Covariates		
Post-Intervention AD Upload	Control Mean (SD)	Drive+ Video	Drive Only	Video Only	Drive+ Video	Drive Only	Video Only
Any	.016 (.127)	-.006 (.006) .371	-.003 (.006) .64	-.012 (.006) .076*	-.006 (.007) .346	-.004 (.007) .529	-.012 (.006) .062*
Has HC Proxy	.015 (.121)	-.007 (.006) .267	-.003 (.006) .65	-.01 (.006) .103	-.007 (.006) .24	-.004 (.006) .509	-.011 (.006) .087*
Has HC Proxy (Validated)	.009 (.094)	-.003 (.005) .53	-.001 (.005) .915	-.007 (.005) .167	-.004 (.005) .407	-.002 (.005) .697	-.007 (.005) .153
Has End-of-Life Instruction	.012 (.109)	-.005 (.006) .383	-.001 (.006) .837	-.009 (.006) .159	-.007 (.006) .283	-.003 (.006) .598	-.009 (.006) .135
Has End-of-Life Instruction (Validated)	.009 (.094)	-.003 (.005) .545	.001 (.005) .889	-.006 (.005) .292	-.005 (.006) .359	-.001 (.006) .787	-.006 (.005) .265
Indicates Life Prolonging Treatment Dispreference	.012 (.109)	-.005 (.006) .361	-.003 (.006) .646	-.013 (.006) .032**	-.007 (.006) .294	-.004 (.006) .504	-.013 (.006) .031**
Indicates Feeding Tube Dispreference	.012 (.109)	-.005 (.006) .361	-.003 (.006) .646	-.013 (.006) .032**	-.007 (.006) .294	-.004 (.006) .504	-.013 (.006) .031**

Table 3.4: This table displays coefficients and standard errors for β_1, β_2, & β_3 from equation 3.7, which estimates the treatment effect of the AD Drive and ACP Video, relative to receiving only a reminder email in the clinic with no drive. Standard errors are in parentheses with p-values below ($^{*}p < .10$,$^{**}p < .05$,$^{***}p < .01$).

Table 3.5 shows the pooled analysis from estimation of equations 3.8 and 3.9. The takeaways are similar to the analysis above. The lower upload rate among treatment groups seems surprising, especially as few patients assigned to the video group actually watched the video, as shown in Table 3.3. Of the 1486 patients who received a link to the video and materials, only 19 clicked through to the video, and of these, only 13 watched more than half. Of the 19 that clicked through, just two uploaded an AD in the post-intervention period.

One possible contributing factor is that communications sent out to each group were of different length. The simplest email was sent to the reminder only group (due to less required detail), which also had the highest upload rate. It could be the case that patients receiving longer emails were less likely to read them, resulting in fewer uploads for these groups. Also possible is that the video link and information led patients to perceive the task of completing an AD to be more challenging, and were deterred by this.

Pooled Treatment Effect, Patients with Email						
	Drive			Video		
Post-Intervention AD Upload	Control Mean (SD)	TE No Co-variates	TE Co-variates	Control Mean (SD)	TE No Co-variates	TE Co-variates
Any	.013 (.115)	.001 (.004) .74	.001 (.005) .88	.018 (.131)	-.007 (.004) .128	-.007 (.004) .129
Has HC Proxy	.013 (.112)	0 (.004) .935	-.001 (.005) .903	.016 (.126)	-.007 (.004) .109	-.007 (.004) .114
Has HC Proxy (Validated)	.008 (.09)	.002 (.004) .626	0 (.004) .914	.011 (.107)	-.005 (.004) .184	-.005 (.004) .184
Has End-of-Life Instruction	.012 (.109)	.001 (.004) .792	0 (.005) .918	.016 (.124)	-.006 (.004) .144	-.006 (.004) .144
Has End-of-Life Instruction (Validated)	.009 (.094)	.002 (.004) .663	0 (.004) .943	.012 (.11)	-.005 (.004) .2	-.005 (.004) .197
Indicates Life Prolonging Treatment Dispreference	.011 (.105)	.002 (.004) .551	.001 (.005) .811	.016 (.126)	-.007 (.004) .073*	-.007 (.004) .077*
Indicates Feeding Tube Dispreference	.011 (.105)	.002 (.004) .551	.001 (.005) .811	.016 (.126)	-.007 (.004) .073*	-.007 (.004) .077*

Table 3.5: This table displays coefficients and standard errors for γ_1 & α_1 from equations 3.8 & 3.9, which estimate the pooled treatment effect of the AD Drive relative to no drive, and ACP Video relative to no video, for patients with email only. Standard errors are in parentheses with p-values below ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

Patients at the AD Drive clinic without an email address on file who otherwise met sample criteria were sent a paper notification that the drive would be taking place with reminder to complete an AD. Patients in the No-Drive clinic were sent a paper reminder to complete an AD. Text for these letters was in line with what was received by the email groups. Proofs can be found in Appendix Section C.3.

Table 3.6 reports the estimated impact of the drive for the no-email group and the full sample including patients with and without email. The table reports estimates of γ_1 from equation 3.8. For the no-email group, we see a statistically significant 4.5 percentage point (SE 1.3) increase in the upload rate at the Drive clinic compared to the No-Drive clinic. This corresponds to a 1.1 percentage point (SE 0.4) higher upload rate for the full sample of patients. Though this comparison is non-experimental, this potentially indicates that the letter was a better method of encouraging patients to attend the drives than the email. Furthermore, it was not the effect of having received a reminder letter alone that explains this result, as patients at the clinic without the drive still received a reminder letter.

Drive Treatment Effect, Full Sample and Patients Without Email						
	Full Sample			No Email Sample		
Post-Intervention AD Upload	Control Mean (SD)	TE No Covariates	TE Covariates	Control Mean (SD)	TE No Covariates	TE Covariates
Any	.016 (.126)	.011 (.004) .014**	.011 (.005) .04**	.022 (.148)	.052 (.012) 0***	.045 (.013) .001***
Has HC Proxy	.015 (.122)	.01 (.004) .022**	.009 (.005) .066*	.021 (.144)	.051 (.012) 0***	.043 (.013) .001***
Has HC Proxy (Validated)	.011 (.102)	.008 (.004) .03**	.007 (.004) .09*	.017 (.128)	.034 (.01) .001***	.027 (.011) .014**
Has End-of-Life Instruction	.014 (.117)	.011 (.004) .011**	.01 (.005) .032**	.02 (.14)	.046 (.011) 0***	.043 (.012) .001***
Has End-of-Life Instruction (Validated)	.011 (.104)	.009 (.004) .024**	.008 (.004) .071*	.017 (.128)	.037 (.01) 0***	.032 (.011) .005***
Indicates Life Prolonging Treatment Dispreference	.013 (.115)	.01 (.004) .018**	.009 (.005) .044**	.021 (.144)	.032 (.011) .003***	.028 (.012) .017**
Indicates Feeding Tube Dispreference	.013 (.115)	.01 (.004) .013**	.01 (.005) .038**	.021 (.144)	.035 (.011) .001***	.031 (.012) .011**

Table 3.6: This table displays coefficients and standard errors for γ_1 from equation 3.8, which estimate the pooled treatment effect of the AD Drive relative to no drive. Standard errors are in parentheses with p-values below ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

One alternative explanation is that differences in patient characteristics between those who have email addresses and those who do not are driving the differential response to the outreach, rather than the mode of outreach (paper vs. email) itself. To address this question, we use coarsened exact matching to re-weight the data for balance along observable characteristics between the email and no-email populations. Results are reported in Table 3.7. Re-weighting by patient demographics (age, gender, ethnicity, language, and co-morbidity index) leads to little change in the relative treatment effects for patients with and without email, indicating that selection on these factors is not driving the observed difference. Adding an indicator for whether the patient has an active MyChart account to the re-weighting scheme actually leads to a larger estimated treatment effect for patients without email. This is likely because patients without an active MyChart account are less likely to respond to the drive intervention, and are also less likely to have an email address on file. While it is still possible that unobservables could explain the difference in treatment effect from the paper letter vs email outreach, it does not appear to be driven by observable characteristics.

Drive Treatment Effect: Re-weighting Patients With and Without Email on Observables

Weighting On	Control Mean Upload (SD)	TE, No Email Patients	Difference, Email Patients
Un-weighted	.016 (.126)	.052 (.014) [0***]	-.048 (.015) [.001***]
Clinic, Demographics	.016 (.126)	.046 (.016) [.004***]	-.04 (.017) [.017**]
Clinic, Demographics, Mychart Activation	.016 (.126)	.073 (.026) [.005***]	-.064 (.027) [.016**]

Table 3.7: This table displays coefficients and standard errors for γ_1 from equation 3.8, using different re-weighting schemes. Standard errors are in parentheses with p-values below ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

Test for Priming

Three weeks prior to the AD drives, a survey was sent by email to 80% of the entire 65+ population at the two clinics who had an email on file. To test for any potential influence of the survey on probability of AD upload, we regress post-intervention upload on an indicator for receipt of survey, and report the results in Table 3.8 below. We find a positive but not statistically significant impact of receiving the survey on AD uploads.

Test for Survey Effect, Patients with Email			
Post-Intervention AD Upload	No Survey Mean (SD)	TE No Covariates	TE Covariates
Any	.009 (.093)	.007 (.006) .209	.007 (.006) .228
Has HC Proxy	.007 (.083)	.007 (.005) .159	.007 (.005) .183
Has HC Proxy (Validated)	.007 (.083)	.003 (.004) .534	.003 (.004) .566
Has End-of-Life Instruction	.007 (.083)	.007 (.005) .178	.006 (.005) .212
Has End-of-Life Instruction (Validated)	.007 (.083)	.004 (.005) .433	.003 (.005) .487
Indicates Life Prolonging Treatment Dispreference	.007 (.083)	.007 (.005) .178	.007 (.005) .187
Indicates Feeding Tube Dispreference	.007 (.083)	.007 (.005) .178	.007 (.005) .187

Table 3.8: This table displays estimates testing for a priming effect of having received an email survey on ACP prior to the experiment. Standard errors are in parentheses with p-values below ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

AD Uploads

In addition to piloting interventions aimed at increasing rates of AD upload, we implemented a chart review to extract data on the content of ADs that were uploaded during and prior to the study. We conducted a descriptive analysis of AD content, and related this to findings from the survey and patient health records. In Table 3.9, we compare AD uploads made before baseline to those made during the outcome period, and test whether AD characteristics are different between these two groups. We find slight differences: ADs uploaded during the treatment period were 9.7 percentage points (SE 5.7) more likely than previous uploads to have legally validated healthcare proxy selections.

Differences Between ADs Uploaded During Intervention Period and Previously Uploaded ADs					
AD Upload Characteristic	N (Pre)	N (Post)	Pre-Experiment Mean (SD)	TE No Covariates	TE Covariates
Has HC Proxy	558	89	.889 (.315)	.044 (.035) .213	.045 (.036) .215
Has HC Proxy (Validated)	558	89	.581 (.494)	.094 (.056) .096*	.097 (.057) .09*
Has End-of-Life Instruction	558	89	.801 (.4)	.075 (.045) .092*	.073 (.045) .108
Has End-of-Life Instruction (Validated)	558	89	.624 (.485)	.084 (.055) .126	.067 (.056) .228
Indicates Life Prolonging Treatment Dispreference	470	83	.866 (.341)	.014 (.04) .737	.004 (.042) .915
Indicates Feeding Tube Dispreference	470	83	.868 (.339)	.023 (.04) .556	.013 (.041) .761

Table 3.9: This table displays estimates testing for differences between ADs uploaded prior to the experiment and those uploaded during the post-intervention period. Standard errors are in parentheses with p-values below ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

Focusing just on uploads that were made before the pilot, we find that several factors in patients' health records and in survey responses are predictive of whether a patient had uploaded an AD prior to the pilot interventions. The tables below report the estimation of two Lasso models. In Table 3.10, we regress all EMR covariates from Table 3.1 on the indicator for pre-intervention upload, and report remaining non-zero coefficients. The full sample is used in this regression. The covariates indicating patient age, public insurance, English as a primary language, MyChart activation, and internal medicine as PCP specialty predict relatively large increases in patient probability of upload. Covariates of a recent surgery or high depression screening score have a negative association with AD upload rate. However, the R^2 for this model is quite small, indicating only about 6% of the variation in upload rates is explained by EMR covariates.

In Table 3.11, we regress all EMR and survey outcomes from tables 3.1 and 3.2 on an indicator for pre-intervention AD upload, and report estimated non-zero lasso coefficients. The sample for this regression is only survey respondents. Here, the R^2 is a bit larger–the model explains about 22% of the variation in AD outcomes, but it also includes responses to questions which directly ask about the respondents' advance care planning. AD completion was positively associated with self-reported health and negatively associated with self-reported procrastination. Low (<\$50k) and high (>\$100k) incomes were also negatively associated with AD completion in comparison to incomes between these two ranges.

Predictors of Pre-Intervention Upload	
N	4727
R Squared	.0608
EMR Covariate	*Lasso Coef*
Age	.0035
BMI	-.0007
English is Primary Language	.0664
Public Insurance	.0347
Married	-.0036
Number of Medications	.0036
MyChart Activated	.0418
PCP Specialty: Family Practice	.0101
PCP Specialty: Internal Medicine	.0473
Had Surgery, Last 2 Years	-.0476
Visits in Last 2 Years	.0025
Weight	-.0003
White, Non-Hispanic	.0108
Depression Screening Score	-.0031
Missing Depression Screening Score	-.0218
Missing Number of Diagnoses	-.0009

Table 3.10: This table lists non-zero lasso coefficients for EMR covariates that predict pre-intervention AD upload.

Predictors of Pre-Intervention Upload, Survey Sample		
N	328	
R Squared	.2194	
Source	*Covariate*	*Lasso Coef*
EMR	Age	.0005
EMR	BMI	-.0023
EMR	Number of Medications	.0047
EMR	MyChart Activated	-.0468
EMR	PCP Specialty: Family Practice	.0581
EMR	Visits in Last 2 Years	.0019
EMR	Weight	-.0003
EMR	Number of Diagnoses	.001
Survey	Background: Has Bachelor's Degree or Above	.0038
Survey	Belief: Default for HC Providers is Comfort Care	.0738
Survey	Belief: Default for HC Providers is Life Extension	.077
Survey	Discounting: 1 Year Discount Factor	-.0345
Survey	Discounting: Missing Discount Factor	-.0251
Survey	Self Reported ACP: Has an AD	.1226
Survey	Self Reported ACP: Has a HC Proxy	.0689
Survey	Background: Ever Worked as a Healthcare Provider	-.07
Survey	Background: Income >$100k	-.0263
Survey	Background: Income <$50k	-.0309
Survey	Self Reported ACP: Has Had Detailed ACP Discussions w/ Family	.0276
Survey	Missing Belief: Probability will Need AD w/in 5 Years	-.03
Survey	Discounting: Procrastinates on ACP	-.0833
Survey	Health: In Very Good or Excellent Health	.0512

Table 3.11: This table lists non-zero lasso coefficients for EMR and survey covariates that predict pre-intervention AD upload for the population taking the survey.

3.6 Conclusion and Planned Future Work

This study evaluates two interventions aimed at increasing AD completion and EMR integration. Among patients to whom communication was sent via email, we find no effect of in-person AD drives or of electronic distribution of an informational video on AD upload rates. However, we estimate a 4.5 percentage point increase in AD uploads for patients who were contacted via letter about the AD drive, relative to patients who were sent a reminder letter only. This suggests that in-person drives may be impactful for increasing AD completion, but only if effectively advertised to patients. Low click-through rates to the videos also suggests that electronic communications may not be an effective way to reach this population. The pilot also resulted in significant logistical feedback that will be incorporated into the design of future work. Attendance at the drives was fairly low, with around 30 patients total in attendance. Though the drive was intended to focus more on facilitating completion of the AD document, anecdotally, many of the attendants had little knowledge or information about ADs and came in looking for basic information.

Future planned piloting will take several steps to address these issues. First, outreach will be tied to patients who have an upcoming appointment. Patients will thus have a built-in opportunity to drop off their AD for upload to the EMR, and may be more likely to review materials in preparation for their appointment. Additionally, this iteration will add paper letter outreach to several intervention arms, which may be a more suitable method of communication for this patient population. A pure control of no outreach will be compared to electronic outreach, and four paper letter arms which also include AD forms and informational brochures. Finally, all electronic outreach will be deployed through the patient secure messaging system, rather than email as in our previous pilot. Other pilot programs at PSJH have shown higher open rates of around 65% using this system.

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Appendix A

Electronic Blood Glucose Monitoring: Impacts on Physician & Patient Behavior

A.1 Trial Registry and Analysis Plan

This study was pre-registered on ClinicalTrials.gov. Design and planned analysis can be publicly accessed on ClinicalTrials.gov under study identifier NCT03542487. This manuscript is a comprehensive report of all planned analysis. The list below notes any departures from pre-specified analysis (PSA), necessary clarifications, or additional analysis included in this report which was not pre-specified.

- PSA identified the baseline period for baseline covariates as "length of the outcome period". Outcome period vary and baseline data availability was limited to 4 months prior to study start date, so a constant baseline period of 14 weeks was used for all outcomes (matching primary outcome period), running Jan 23-Apr 30.
- PSA did not include the reported outcomes "Days Since Last A1c Test" and "Had A1c Test in Period"
- PSA included the outcomes "Total Messages Sent By Physicians" and "Total Messages Received By Physicians". These outcomes were not reported due to data quality concerns.
- Physician fixed effects are replaced with strata fixed effects for all practice treatment effect analysis. Treatment is constant within physician, meaning that physician fixed effects are not appropriate in this context.
- Physician fixed effects are omitted from the reminder treatment analysis due to practical difficulties with estimation and standard error calculation using the Lin covariate adjustment, related to constant outcome values within physician reminder group interactions.

- Non-covariate adjusted analysis is not reported for the reminder treatment effect estimation, due to differences across reminder assignment groups in baseline covariates.
- PSA did not include local average treatment effect estimation using practice and reminder treatment assignment as instruments for flow sheet use
- PSA did not include summary index tests and computation of permutation adjusted p-values for step-down multiple testing procedures

A.2 Data

Available Data Description	
Dataset and Variable Name	Variable Description
(1) ACTIVE MEDS	**Medication Level**
pat_ID	*Patient Id*
Most_Recent_Contact_Date	*Most Recent Appointment Date*
PAT_ENC_CSN_ID	*Encounter Id Of Appointment*
CURRENT_MED_ID	*Current Medication List At Time Of Appointment*
IS_ACTIVE_YN	*Whether Medication Is Active*
description	*Description Of Medication*
(2) OVERALL REGISTRY REPORT	**Patient Level**
PAT_ID	*Patient Id*
Last Initial	*Last Initial Of Patient Name*
Provider ID	*Primary Care Provider Id*
birth date	*Patient Birth date*
sex	*Patient Sex*
ethnicity	*Patient Ethnicity*
HBA1C_LAST	*Value Of Most Recent A1C Test*
HBA1C_LAST_DT	*Date Of Last A1C*
last office visit	*Date Of Last Office Visit*
OFF_VIS_PROV_ID	*Id Of Last Office Visit*
Activation date	*Date MyChart Activated*
(3) PRESCRIPTION ORDERS	**Order Level**
ORDER_MED_ID	*Order Id*
PAT_ID	*Patient Id*
Description	*Description Of Medication*
dose	*Dose Amount*
measurement	*Measurement Of Dose*
QUANTITY	*Quantity Of Doses*
FREQ_NAME	*Frequency Medication Prescribed*
Ordering Date	*Date Medication Ordered*
(4) FLOW SHEET ORDERS	**Order Level**
PAT_ID	*Patient Id*
Description	*Description Of Order Type*
Ordering Date	*Ordering Date*
Authrzing_PROV_ID	*Provider Authorizing Order*

Table A.1: Variable and data descriptions, datasets 1-4.

Available Data Description (Cont.)	
Dataset and Variable Name	Variable Description
(5) FLOW SHEET READINGS	**Flow sheet Entry Level**
PAT_ID	*Patient Id*
entry date	*Date Of Glucose Entry*
entry time	*Time Of Glucose Entry*
MEAS_VALUE	*Value Of Glucose Entry*
FLO_MEAS_NAME	*Category Of Glucose Entry*
(6) MYCHART MESSAGES TO PATIENT	**Message Level**
MESSAGE_ID	*Message Id*
recipient ID	*Patient Id*
senderID	*Sender Id*
message date	*Message Date*
message time	*Message Time*
Read/Unread	*Whether Message Has Been Read At Time Of Data Pull*
(7) MYCHART MESSAGES FROM PATIENT	**Message Level**
MESSAGE_ID	*Message Id*
recipient ID	*Recipient Id*
senderID	*Patient Id*
message date	*Message Date*
message time	*Message Time*
Read/Unread	*Whether Message Has Been Read At Time Of Data Pull*
(8) ENCOUNTERS	**Encounter Level**
PAT_ID	*Patient Id*
VISIT_PROV_ID	*Visit Provider Id*
visit date	*Date Of Encounter*
PAT_ENC_CSN_ID	*Encounter Id Of Appointment*
NAME	*In Person Vs Telephone Encounter*

Table A.2: Variable and data descriptions, datasets 5-8.

A.3 Placebo Tests

Placebo: Reminder Impact on Flow Sheet Use					
Outcome	Period	Control (SD)	TE, Basic	TE, GC	TE, Phys
Flowsheet Use (Extensive)	Weeks 1-14	.004 (.06)	-.003 (.002) [.208]	-.004 (.002) [.086*]	-.004 (.002) [.086*]
	Weeks 15-26	.004 (.06)	-.003 (.002) [.208]	-.004 (.002) [.086*]	-.004 (.002) [.086*]
Flowsheet Use (Total)	Weeks 1-14	.042 (.905)	-.045 (.039) [.245]	-.05 (.039) [.201]	-.05 (.039) [.201]
	Weeks 15-26	.039 (.81)	-.044 (.034) [.2]	-.045 (.034) [.192]	-.045 (.034) [.192]

Table A.3: Displays coefficients and standard errors for β_1, β_2, and β_3 from equation 1.2, which estimates the placebo treatment effect of reminder message assignment groups on flow sheet use outcomes in the control practices, at which patients received no reminders. Standard errors are in parentheses with p-values in brackets ($^{*}p < .10,^{**} p < .05,^{***} p < .01$).

Placebo: Reminder Impact on Patient Health					
Outcome	Period	Control (SD)	TE, Basic	TE, GC	TE, Phys
A1c	Week 14	7.209 (1.593)	-.018 (.041) [.654]	-.046 (.045) [.299]	-.034 (.042) [.426]
	Week 26	7.203 (1.562)	-.033 (.046) [.482]	-.077 (.049) [.118]	-.018 (.049) [.716]
A1c Under 7	Week 14	.538 (.499)	.022 (.019) [.235]	.013 (.019) [.509]	.016 (.019) [.397]
	Week 26	.551 (.498)	.004 (.02) [.858]	.004 (.02) [.843]	.003 (.02) [.887]
A1c Improved Since Baseline	Week 14	.194 (.395)	0 (.018) [.98]	.013 (.019) [.506]	.022 (.02) [.257]
	Week 26	.297 (.457)	-.009 (.021) [.667]	.005 (.022) [.816]	.018 (.023) [.426]
Days Since Last A1c Test	Week 14	190.728 (200.919)	4.036 (5.937) [.497]	-.081 (6.177) [.989]	1.391 (6.251) [.824]
	Week 26	190.695 (208.378)	9.908 (7.116) [.164]	3.119 (7.368) [.672]	-2.121 (7.577) [.78]
Had A1c Test in Period	Week 14	.399 (.49)	-.005 (.023) [.84]	.016 (.024) [.498]	-.014 (.024) [.57]
	Week 26	.642 (.48)	-.028 (.024) [.253]	-.012 (.025) [.637]	-.016 (.025) [.53]

Table A.4: Displays coefficients and standard errors for β_1, β_2, and β_3 from equation 1.2, which estimates the placebo treatment effect of reminder message assignment groups on patient health outcomes in the control practices, at which patients received no reminders. Standard errors are in parentheses with p-values in brackets ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

Placebo: Reminder Impact on Patient-Physician Interaction					
Outcome	Period	Control (SD)	TE, Basic	TE, GC	TE, Phys
Appointment, In-Person	Weeks 1-14	1.307 (3.572)	-.143 (.154) [.353]	-.047 (.157) [.763]	-.109 (.158) [.489]
	Weeks 1-26	2.438 (5.885)	-.41 (.231) [.076*]	-.02 (.261) [.941]	-.22 (.246) [.372]
Appointment, Phone	Weeks 1-14	.801 (1.647)	-.073 (.065) [.259]	.055 (.071) [.441]	-.032 (.069) [.647]
	Weeks 1-26	1.429 (2.471)	-.104 (.1) [.3]	.171 (.112) [.128]	.002 (.104) [.987]
Number of Messages Sent by Patient	Weeks 1-14	1.387 (3.001)	.084 (.12) [.482]	.097 (.136) [.475]	-.122 (.117) [.299]
	Weeks 15-26	1.199 (2.927)	-.024 (.119) [.838]	.037 (.13) [.775]	-.203 (.112) [.07*]

Table A.5: Displays coefficients and standard errors for β_1, β_2, and β_3 from equation 1.2, which estimates the placebo treatment effect of reminder message assignment groups on patient-provider interaction outcomes in the control practices, at which patients received no reminders. Standard errors are in parentheses with p-values in brackets ($^{*}p < .10, ^{**}p < .05, ^{***}p < .01$).

Placebo: Reminder Impact on Patient Treatment					
Outcome	Period	Control (SD)	TE, Basic	TE, GC	TE, Phys
Any Medication List Change	Weeks 1-14	.221 (.415)	-.002 (.018) [.91]	-.021 (.019) [.264]	-.016 (.019) [.385]
	Weeks 1-26	.331 (.471)	-.009 (.021) [.677]	-.022 (.021) [.306]	-.015 (.021) [.486]
Medication Added	Weeks 1-14	.213 (.41)	-.005 (.018) [.77]	-.025 (.018) [.167]	-.019 (.019) [.305]
	Weeks 1-26	.319 (.466)	-.01 (.021) [.619]	-.024 (.021) [.26]	-.016 (.021) [.444]
Medication Removed	Weeks 1-14	.115 (.319)	.01 (.014) [.474]	-.006 (.014) [.668]	-.007 (.014) [.599]
	Weeks 1-26	.162 (.369)	.006 (.016) [.694]	-.005 (.016) [.75]	-.013 (.016) [.409]
Number of Prescription Orders	Weeks 1-14	4.71 (20.057)	-.702 (.855) [.411]	.383 (.879) [.663]	.209 (.898) [.816]
	Weeks 1-26	8.49 (31.747)	-1.19 (1.344) [.376]	1.031 (1.492) [.489]	.785 (1.526) [.607]
Non-Refill	Weeks 1-14	4.71 (20.057)	-.701 (.854) [.412]	.383 (.878) [.663]	.218 (.897) [.808]
	Weeks 1-26	8.49 (31.747)	-1.187 (1.342) [.377]	1.031 (1.49) [.489]	.799 (1.525) [.6]
Diabetes Related	Weeks 1-14	.648 (1.806)	-.024 (.088) [.782]	.064 (.076) [.397]	.096 (.107) [.371]
	Weeks 1-26	1.239 (2.715)	-.056 (.124) [.654]	.077 (.126) [.538]	.204 (.185) [.272]

Table A.6: Displays coefficients and standard errors for β_1, β_2, and β_3 from equation 1.2, which estimates the placebo treatment effect of reminder message assignment groups on patient treatment outcomes in the control practices, at which patients received no reminders. Standard errors are in parentheses with p-values in brackets ($^{*}p < .10$,$^{**}p < .05$,$^{***}p < .01$).

A.4 A1c Quantile Regressions

Clinic Intervention, Impact on Patient A1c Quantiles				
Period	Control Mean	TE Q25	TE Q50	TE Q75
At Week 14	7.196 (1.601)	0 (.03) [1]	0 (.034) [1]	-.1 (.064) [.121]
At Week 26	7.179 (1.54)	0 (.024) [1]	0 (.037) [1]	0 (.059) [1]

Table A.7: This table displays coefficients and standard errors for the treatment effect of the clinic intervention on patient health outcomes at different A1c quantiles, relative to business as usual. No covariates are included in this regression. Standard errors are in parentheses with p-values in brackets ($^{*}p < .10,^{**} p < .05,^{***} p < .01$).

Placebo: Reminder Impact on Patient Health					
Quantile	Period	Control (SD)	TE, Basic	TE, GC	TE, Phys
Q25	Week 14	7.276 (2.823)	.1 (.073) [.171]	0 (.063) [1]	0 (.073) [1]
	Week 26	7.327 (2.894)	0 (.063) [1]	-.1 (.074) [.176]	-.1 (.073) [.171]
Q50	Week 14	7.276 (2.823)	0 (.078) [1]	0 (.078) [1]	0 (.077) [1]
	Week 26	7.327 (2.894)	0 (.07) [1]	0 (.079) [1]	0 (.078) [1]
Q75	Week 14	7.276 (2.823)	-.1 (.134) [.455]	-.1 (.111) [.369]	-.1 (.145) [.491]
	Week 26	7.327 (2.894)	-.2 (.136) [.142]	-.1 (.127) [.431]	-.1 (.146) [.494]

Table A.8: This table displays coefficients and standard errors for the treatment effect of the reminder intervention on patient health outcomes at different A1c quantiles, relative to business as usual. No covariates are included in this regression. Standard errors are in parentheses with p-values in brackets ($^{*}p < .10,^{**} p < .05,^{***} p < .01$).

A.5 Summary Statistics on Patients Using Flow Sheets

Demographic and Baseline Covariates				
Covariate	Mean, Patients Not Completing FS	Difference	SE	P
Age	58.934	-.976	(1.12)	.384
Male	.543	.064	(.039)	.098*
Ethnicity, Hispanic	.059	.018	(.018)	.319
Ethnicity, White Non-Hispanic	.803	.025	(.031)	.427
Ethnicity, Other	.138	-.043	(.027)	.109
A1c, Baseline	7.21	.025	(.136)	.855
Missing A1c, Baseline	.049	-.037	(.017)	.027**
Days Since Last A1c Test, Baseline	193.254	-71.058	(15.101)	0***
Days Since Last Appointment, Baseline	152.809	-71.809	(14.964)	0***
Missing Days Since Last Appointment, Baseline	.019	-.019	(.01)	.074*
Number of Patient Message, Last 14 Weeks at Baseline	1.391	1.663	(.24)	0***
Number of Phone Appts, Last 14 Weeks at Baseline	.892	.096	(.15)	.522
Number of In-Person Appts, Last 14 Weeks at Baseline	1.404	.036	(.252)	.886
Medication Removed, Last 14 Weeks at Baseline	.067	-.007	(.019)	.708
Medication Added, Last 14 Weeks at Baseline	.21	.028	(.032)	.373
Medication List Changed, Last 14 Weeks at Baseline	.214	.024	(.032)	.452
Number of RX Orders, Last 14 Weeks at Baseline	6.047	.144	(1.497)	.924
Number of New RX Orders, Last 14 Weeks at Baseline	6.044	.146	(1.495)	.922
Number of Diabetes RX Orders, Last 14 Weeks at Baseline	.825	.419	(.176)	.017**
Completed Flow Sheet, Last 14 Weeks at Baseline	0	.024	(.002)	0***

Table A.9: This table present comparisons of patient demographics and baseline covariates for those who used the blood glucose flow sheet post-intervention, relative to those who did not.

Descriptive Statistics, Flow Sheet Entries							
	Week 1-2	Week 3-6	Week 7-10	Week 11-14	Week 15-18	Week 19-22	Week 23-26
Entries for all Patients							
Total Patients	40	122	113	85	87	60	53
Entries/Patient	11	23.352	32.142	22.788	28.805	26.767	28.679
10th Percentile	107.662	99.669	101.485	100.01	100.306	96.935	96.72
25th Percentile	110.667	111.131	113.364	114.455	114.19	109.38	110.69
50th Percentile	124.938	128.516	130.469	133.5	129	129.889	124.431
75th Percentile	141.012	151.661	147.5	147	147.597	146.337	146.402
90th Percentile	188.601	179.828	178.062	167.921	166.411	172.35	179.037
Entries for Patients with Consistent Use							
Total Patients	10	32	32	32	32	32	32
Entries/Patient	21.3	39.688	51.25	31.5	36.531	36.844	31.844
10th Percentile	90.957	102.405	94.92	95.272	92.914	94.662	95.981
25th Percentile	109.962	108.258	112.148	107.179	107.46	109.145	110.659
50th Percentile	118.458	130.907	131.87	136.225	134.158	131.135	129.214
75th Percentile	133.675	150.375	148.893	147.433	147.147	148.373	146.551
90th Percentile	152.445	171.523	169.68	167.163	170.338	172.706	168.348

Table A.10: This table presents the distribution of flow sheet entry values.

Appendix B

Immigrant Enrollment in Medicaid and Means-Tested Benefits After PRWORA

B.1 Model for the Effect of Benefits on Remittances

The effect of Medicaid and SNAP on health and financial outcomes has been extensively studied in previous work, albeit with different populations (Finkelstein et al., 2012). Thus, I will not attempt to model the impact of Medicaid and food stamps on these outcomes. However, one unique factor about the sample of recent immigrants considered in this chapter is that many are financially supporting family members and friends, some of whom live outside of the United States. Eligibility for and receipt of means-tested benefits might affect an immigrant's choice of whether and how much to remit. In the following, I will present a simple model adapted from Amuedo-Dorantes and Pozo (2006) to illustrate that the effect of gaining eligibility for means-tested benefits on remittance behavior is potentially ambiguous.

In this model, migrants coming from non-correlated labor market enter a coinsurance agreement with the household left behind. The migrant sends funds home when the household experiences negative income shocks, and has a partially altruistic motivation for remitting. However, the source household also supports the migrant by paying for migration costs and/or sending funds when the migrant is out of work. For this reason, the migrant is also partially motivated to remit by a desire for reciprocity. In a two-period model, the immigrants' utility can be specified as

$$U = \sigma ln C_1 + (1 - \sigma) ln(a + x) + \delta ln C_2 \tag{B.1}$$

Here, C denote migrant consumption in each period, x is remittances, $a + x$ is total home income, δ is the discount factor between periods 1 and 2, and the parameter σ denotes the relative weighting of consumption to altruism derived from family consumption. In the second period, 2 states can be realized with probability of the bad state equal to π. In the

good state, the migrant receives income Y_H, while in the bad state, he receives income Y_L. If the bad state is realized, the migrant also receives a transfer from the source household, $g(x)$ which is an increasing, concave function of remittances sent in the first period, and does not fully account for the difference between Y_H and Y_L. If the migrant is eligible, the migrant can also receive W in social welfare benefits in the bad state. Consumption in period 1 is constrained by $C_1 \leq Y_1 - x - s$, where $Y_1$is income in period 1 and s is savings across periods. Consumption in period 2 is

$$C_2 \leq \pi(Y + s(1+r) + W + g(x)) + (1-\pi)(Y_H + s(1+r)) \tag{B.2}$$

Substituting the budget constraints into utility and optimizing, a first order condition is:

$$FOC_x = \frac{\partial U}{\partial x} = \frac{-\sigma}{C_1} + \frac{1-\sigma}{a+x} + \frac{\delta\pi g'(x)}{C_2} = 0 \tag{B.3}$$

$$\Rightarrow -\sigma C_2(a+x) + C_1C_2(1-\sigma) + C_1\delta\pi g'(x)(a+x) = 0 \tag{B.4}$$

Now, to find the effect of W on remittances, we find:

$$\frac{\partial x}{\partial W} = -\frac{\frac{\partial FOC_x}{\partial W}}{\frac{\partial FOC_x}{\partial x}} = \frac{\sigma\pi\delta(a+x) - C_1\pi\delta(1-\sigma)}{(\sigma+1)\pi\delta g'(x)(a+x) + C_2 - C_1\pi\delta(g'(x)(1-\sigma) + g''(x)(a+x))} \tag{B.5}$$

The sign of $\frac{\partial x}{\partial W}$ is ambiguous. Remittances may increase or decrease when the migrant gains eligibility for means tested programs, depending on the immigrant's relative value of the home household's consumption and his desire to insure against his own shocks. As a form of social insurance, means-tested benefits may be a substitute for the informal insurance the migrant receives from the household to whom he or she remits, decreasing the need for this arrangement. However, these programs can also be thought of as positive income shocks, and may increase the migrant's ability to behave altruistically and to respond to negative income shocks in the home-country household. Thus, under this model, the effect of gaining eligibility for means-tested benefits has an ambiguous effect on remittances.

Appendix C

Deferring Agency at End-of-Life: The Role of Information and Advance Directives

C.1 Trial Registry and Analysis Plan

This study is registered in the AEA RCT Registry and the unique identifying number is: AEARCTR-0003038. The list below notes any departures from pre-specified analysis (PSA), necessary clarifications, or additional analysis included in this report which was not pre-specified.

- PSA described a synthetic control analysis to evaluate the impact of the AD Drive on AD completion rates. Multi-clinic data required to conduct this analysis was not yet available at the time of this report, but this will be added in future versions. A simple comparison between upload rates at the two sample clinics is performed in place of this analysis.
- PSA did not include descriptive analysis of factors predicting pre-intervention upload.
- PSA proposed heterogeneous effects analysis performed using the causal tree method. Because no significant treatment effect was estimated for most patients, this was not deemed necessary. PSA also proposed model estimation using survey data, which was precluded by limited sample size.

C.2 Email Communication Texts

Communication 2: AD drive invite and a reminder to complete AD

All patients at Providence are encouraged to engage in Advance Care Planning and to have an Advance Directive completed and stored in their medical record. You are being contacted because you do not have an Advance Directive stored in your medical record.

Advance Care Planning focuses on the care you want if you are unable to speak for yourself because of an injury or illness. Your plan is based on your values, goals, and the type of care you want to receive. Your decisions about your Advance Care Planning should be written down in an Advance Directive.

An Advance Directive is a legal form that tells your family and doctors about the care you would like to receive. It is important to know that an Advance Directive is different from having a Physician Orders for Life-Sustaining Treatment, commonly known as a POLST.

Your physician, [Dr. PCP Name] invites you to attend an upcoming Advance Directive Drive held at [Clinic Name] to get in-person help with filling out an Advance Directive. There will be staff members who can answer your questions, help you fill out the form, ensure it is legally valid, and add the form to your medical record. You can come to the clinic during any of the following times:

- [time] and [time] on [dates].
- [time] and [time] on [dates].
- [time] and [time] on [dates].

If you're not able to come to the Advance Directive Drive, your care team can answer any questions about having an Advance Directive at your next appointment. In addition, you can find Providence resources on Advance Care Planning by clicking here.

If you're not able to come to the Drive and want to fill out an Advance Directive on your own, just follow these steps:

1. Click here to download and print the Advance Directive form.
2. Talk to you loved ones about the care you want to receive.
3. Complete the form.
4. Sign the form in front of 2 witnesses or a notary public.
5. Have the 2 witnesses or notary public sign the form.
6. Upload the form to MyChart, or bring it to your next appointment.

We look forward to seeing you soon.

To your health,

[PCP] and your Providence care team

Communication 1: AD drive invite with ACP Decisions videos and a reminder to complete AD

All patients at Providence are encouraged to engage in Advance Care Planning and to have an Advance Directive completed and stored in their medical record. You are being contacted because you do not have an Advance Directive stored in your medical record.

Advance Care Planning focuses on the care you want if you are unable to speak for yourself because of an injury or illness. Your plan is based on your values, goals, and the type of care you want to receive. Your decisions about your Advance Care Planning should be written down in an Advance Directive.

An Advance Directive is a legal form that tells your family and doctors about the care you would like to receive. It is important to know that an Advance Directive is different from having a Physician Orders for Life-Sustaining Treatment, commonly known as a POLST.

Your physician, [Dr. PCP Name] invites you to attend an upcoming Advance Directive Drive held at [Clinic Name] to get in-person help with filling out an Advance Directive. There will be staff members who can answer your questions, help you fill out the form, ensure it is legally valid, and add the form to your medical record. You can come to the clinic during any of the following times:

- [time] and [time] on [dates].
- [time] and [time] on [dates].
- [time] and [time] on [dates].

Before you attend the Drive, or even if you're not able to, [Dr. PCP Name] invites you to watch a 10 minute video about Advance Care Planning and read a few pages of information about some of the decisions you need to make when filling out an Advance Directive.

[Link to video & Information Sheets]

If you're not able to come to the Advance Directive Drive, your care team can answer questions about an Advance Directive at your next appointment. In addition, you can find Providence resources on Advance Care Planning by clicking here.

If you're not able to come to the Drive and want to fill out an Advance Directive on your own, follow these steps:

1. Click here to download and print the Advance Directive form.
2. Talk to you loved ones about the care you want to receive.
3. Complete the form.
4. Sign the form in front of 2 witnesses or a notary public.
5. Have the 2 witnesses or notary public sign the form.
6. Upload the form to MyChart or bring it to your next appointment.

We look forward to seeing you soon.

To your health,

[PCP] and your Providence care team

Communication 3: A distribution of the ACP Decisions video and a reminder to complete AD

All patients at Providence are encouraged to engage in Advance Care Planning and to have an Advance Directive completed and stored in their medical record. You are being contacted because you do not have an Advance Directive stored in your medical record.

Advance Care Planning focuses on the care you want if you are unable to speak for yourself because of an injury or illness. Your plan is based on your values, goals, and the type of care you want to receive. Your decisions about your Advance Care Planning should be written down in an Advance Directive.

An Advance Directive is a legal form that tells your family and doctors about the care you would like to receive. It is important to know that an Advance Directive is different from having a Physician Orders for Life-Sustaining Treatment, commonly known as a POLST.

[Dr. PCP Name] would like you to watch a 10-minute video about Advance Care Planning and read through a few pages of information about some of the decisions you need to make when filling out an Advance Directive.

[Link to video & Information Sheets]

Your care team can answer any questions about having an Advance Directive at your next appointment. In addition, you can find Providence resources on Advance Care Planning by clicking here.

If you want to fill out an Advance Directive now, just follow these steps:

1. Click here to download and print the Advance Directive form.
2. Talk to you loved ones about the care you want to receive.
3. Complete the form.
4. Sign the form in front of 2 witnesses or a notary public.
5. Have the 2 witnesses or notary public sign the form.
6. Upload the form to MyChart, or bring it to your next appointment.

We look forward to seeing you soon.

To your health,

[PCP] and your Providence care team

Communication 4: A reminder to complete AD

All patients at Providence are encouraged to engage in Advance Care Planning and to have an Advance Directive completed and stored in their medical records. You are being contacted because you have not completed and/or provided an Advance Directive to be stored in your medical record.

Advance Care Planning focuses on the care you want if you are unable to speak for yourself because of an injury or illness. Your plan is based on your values, goals and the type of care you want to receive. Your decisions about your Advance Care Planning need to be written down in an Advance Directive.

An Advance Directive is a legal form that tells your family and doctors about the care you would like to receive. It is important to know that an Advance Directive is different from having a Physician Orders for Life-Sustaining Treatment, commonly known as a POLST.

Your care team can answer any questions about having an Advance Directive at your next appointment. In addition, you can find Providence resources on Advance Care Planning by clicking here.

If you want to fill out an Advance Directive now, just follow these steps:

1. Click here to download and print the Advance Directive form.
2. Talk to you loved ones about the care you want to receive.
3. Complete the form.
4. Sign the form in front of 2 witnesses or a notary public.
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6. Upload the form to MyChart, or bring it to your next appointment.

We look forward to seeing you soon.

To your health,

[PCP] and your Providence care team

C.3 Paper Letter Communication Proofs

Sent to Patients at Drive Clinic

PROVIDENCE
Health & Services

Providence Medical Group - Mercantile
4015 Mercantile Drive #200
Lake Oswego, OR 97035

First Name, Last Name
1234 Cascade Pl.
Portland, OR 12345-6789

Dear [PATIENT NAME],

All patients at Providence are encouraged to engage in Advance Care Planning and to have an Advance Directive completed and stored in their medical record. You are being contacted because you do not have an Advance Directive stored in your medical record.

Advance Care Planning focuses on the care you want if you are unable to speak for yourself because of an injury or illness. Your plan is based on your values, goals, and the type of care you want to receive. Your decisions about your Advance Care Planning should be written down in an Advance Directive.

An Advance Directive is a legal form that tells your family and doctors about the care you would like to receive. It is important to know that an Advance Directive is different from having a Physician Orders for Life-Sustaining Treatment, commonly known as a POLST.

Your physician, [Dr. PCP Name] invites you to attend an upcoming Advance Directive Drive held at [Clinic Name] to get in-person help with filling out an Advance Directive. There will be staff members who can answer your questions, help you fill out the form, ensure it is legally valid, and add the form to your medical record. You can come to the clinic during any of the following times:

- July 16, 2018 8am-12pm and 1pm-5pm
- July 18, 2018 8am-12pm and 1pm-5pm
- July 20, 2018 8am-12pm and 1pm-5pm

If you're not able to come to the Advance Directive Drive, your care team can answer any questions about having an Advance Directive at your next appointment. In addition, you can find Providence resources on Advance Care Planning by visiting www.providence.org/institute-for-human-caring.

If you're not able to come to the Drive and want to fill out an Advance Directive on your own, just follow these steps:

1. Visit www.providence.org/institute-for-human-caring
2. Click on Patient and Family Resources → Advance Directives
3. Click on Oregon to download and print the Advance Directive form.
4. Talk to you loved ones about the care you want to receive.
5. Complete the form.
6. Sign the form in front of 2 witnesses.
7. Have the 2 witnesses sign the form.
8. Upload the form to MyChart, or bring it to your next appointment.

We look forward to seeing you soon.

To your health,

[PCP] and your Providence care team

Sent to Patients at No-Drive Clinic

PROVIDENCE
Health & Services

Providence Medical Group - Gresham
440 NW Division Street
Gresham, OR 97030

First Name, Last Name
1234 Cascade Pl.
Portland, OR 12345-6789

Dear [PATIENT NAME],

All patients at Providence are encouraged to engage in Advance Care Planning and to have an Advance Directive completed and stored in their medical records. You are being contacted because you have not completed and/or provided an Advance Directive to be stored in your medical record.

Advance Care Planning focuses on the care you want if you are unable to speak for yourself because of an injury or illness. Your plan is based on your values, goals and the type of care you want to receive. Your decisions about your Advance Care Planning need to be written down in an Advance Directive.

An Advance Directive is a legal form that tells your family and doctors about the care you would like to receive. It is important to know that an Advance Directive is different from having a Physician Orders for Life-Sustaining Treatment, commonly known as a POLST.

Your care team can answer any questions about having an Advance Directive at your next appointment. In addition, you can find Providence resources on Advance Care Planning by visiting www.providence.org/institute-for-human-caring.

If you want to fill out an Advance Directive now, just follow these steps:

1. Visit www.providence.org/institute-for-human-caring
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8. Upload the form to MyChart, or bring it to your next appointment.

We look forward to seeing you soon.

To your health,

[PCP] and your Providence care team

C.4 Pilot Questionnaire

Category: Personal/Demographic Questions

In this section, we'll ask you some basic questions about your personal details and health. You can choose not to answer any of the individual questions.

1. What is the highest level of education you have completed or the highest degree you have received?

    a) Less than a high school degree
    b) High school degree or equivalent (e.g., GED)
    c) Some college but no degree
    d) Associate's degree
    e) Bachelor's degree
    f) Graduate and/or professional degree (MA, PhD, JD, etc.)
    g) Prefer not to answer

2. Are you now employed full-time, part-time or not employed?

    a) Full-time
    b) Part-time
    c) Not employed
    d) Retired
    e) Prefer not to answer

3. Have you ever worked as a health care provider?

    a) Yes, currently
    b) Yes, but not currently
    c) No
    d) Prefer not to answer

4. What was your gross household income (before taxes and deductions are taken out) for last year (2017)? Your best estimate is fine.

    a) $0-$25,000
    b) $25,000-$50,000
    c) $50,000-$100,000
    d) above $100,000

e) Prefer not to answer

5. How big of a role do personal religious beliefs play in your life?

   a) Very big role
   b) Moderate role
   c) Small role
   d) No role at all
   e) Prefer not to answer

6. In general, would you say your health is

   a) Excellent
   b) Very Good
   c) Good
   d) Fair
   e) Poor
   f) Prefer not to answer

7. During the past 30 days, for about how many days did poor physical or mental health keep you from doing your usual activities, such as self-care, work, or recreation?

   a) Total number of days (0-30): _
   b) Prefer not to answer

8. Indicate whether you have any the following living relations (randomize order)

   a) Spouse
   b) Adult Children (Indicate Number _)
   c) Parents (Indicate Number _)
   d) Syblings (Indicate Number _)
   e) Prefer not to answer

9. Indicate which of these relations live within one hour driving distance of your home address.

   a) Spouse
   b) Adult Children (Indicate Number _)
   c) Parents (Indicate Number _)
   d) Syblings (Indicate Number _)

e) Prefer not to answer

10. Indicate how often you communicate with these relatives, whether through phone, email or in person (option for each relative stated above)

    a) Every week or nearly every week
    b) Fewer than every week but at least once per month
    c) Fewer than every month but at least once per year
    d) Fewer than once per year
    e) Prefer not to answer

11. Do you have a secondary home residence where you spend more than 4 consecutive weeks out of the year away from your primary residence?

    a) Yes
    b) No
    c) Prefer not to answer

Category: Beliefs About End of Life

In this section, we'll ask you some questions about your values and beliefs. You can choose not to answer any of the individual questions, or select don't know if you're not sure.

12. At the end of life, some people are willing to live through a lot for a chance of living longer. Other people believe that certain things would be very hard on their quality of life. Life support treatment can be things like CPR, a breathing machine, feeding tubes, dialysis, or transfusions. Which of these statements comes closest to your point of view, even if neither is exactly right?

    a) Life supporting medical treatments are worth the costs because they allow people to live longer and better quality lives
    b) Life supporting medical treatments often create as many problems as they solve.
    c) Neither/Both equally
    d) Don't know
    e) Prefer not to answer

13. One type of life supporting medical treatment is a feeding tube, which can be used to give food to people who are not able to drink or eat on their own. For patients with an advanced disease like advanced cancer or dementia who might receive a feeding tube, which of the following do you think is true?

a) Feeding tubes improve quality of life but do not help these patients live longer

b) Feeding tubes help these patients live longer but do not improve quality of life

c) Feeding tubes improve quality of life and help these patients live longer

d) Feeding tubes do not help these patients live longer and do not improve quality of life

e) Don't know

f) Prefer not to answer

14. At the end of life, some people with serious illnesses would prefer to die at home, as opposed to a hospital or other medical facility. Which of these statements comes closest to your point of view, even if neither is exactly right?

a) It is preferable to die at home

b) It is preferable to die in a hospital or medical facility

c) Neither/Both equally

d) Don't know

e) Prefer not to answer

15. Which comes closer to your view? If a patient is unconscious and is very unlikely that he or she will become conscious again, doctors and nurses should continue treatment and do everything possible to keep the patient alive. Or, doctors and nurses should stop treatment and just keep the patient comfortable.

a) Continue treatment

b) Stop treatment

c) Don't know

d) Prefer not to answer

16. What do you think would happen if a patient didn't leave any written instructions for his or her healthcare, and then became permanently unconscious? Would treatment to keep the patient alive be continued?

a) Treatment would always be continued

b) Treatment would sometimes be continued and sometimes stopped

c) Treatment would be stopped

d) Don't know

e) Prefer not to answer

Category: Personal Advance Care Planning

In this section, we'll ask you some questions about your personal exoerience with advance care planning. You can choose not to answer any of the individual questions.

17. Have you had any personal experience in the last five years with a relative or close friend suffering from a terminal illness or in a coma?

    a) Yes
    b) No
    c) Don't Know
    d) Prefer not to answer

18. Before today, how much had you discussed your wishes for medical treatment with close family or friends?

    a) A great deal of discussion
    b) Some discussion
    c) Not very much discussion
    d) No discussion at all
    e) Prefer not to answer

19. Have you appointed a health care representative to direct your healthcare if you cannot do so?

    a) Yes
    b) No, but intend to within the next six months
    c) No, but intend to more than 6 months from now
    d) No, I do not intend to appoint a healthcare representative
    e) Don't Know
    f) Prefer not to answer

20. If YES Who have you appointed as your health care representative? If NO Who would you want to make medical decisions for you if you were not able to make them for yourself? (randomize order)

    a) Spouse
    b) Adult child
    c) Parent
    d) Sibling

e) Adult relative or close friend

f) Other

g) Don't Know

h) Prefer not to answer

21. Have you documented instructions for health care providers to follow if you can no longer direct your own healthcare?

a) Yes

b) No, but intend to within the next six months

c) No, but intend to more than 6 months from now

d) No, I do not intend to appoint a healthcare representative

e) Don't Know

f) Prefer not to answer

22. Rate how much you agree with the following statement: I procrastinate in taking steps to prepare for a time when I may not be able to direct my own healthcare.

a) Agree Completely

b) Agree Somewhat

c) Disagree Somewhat

d) Disagree Completely

e) Don't Know

f) Prefer not to answer

23. Which of these statements comes closest to your point of view, even if neither is exactly right?

a) Taking steps to plan for a time when I may not be able to direct my own healthcare makes me feel good because I am prepared.

b) Taking steps to plan for a time when I may not be able to direct my own healthcare makes me feel bad because I do not like to think about or discuss these issues.

c) Both equally

d) Neither

e) Don't know

f) Prefer not to answer

24. In the state of Oregon, there is a form you can fill out and sign called an advance directive. On this form, you can appoint a healthcare representative who will direct your healthcare if you can no longer do so, AND/OR document instructions for health care providers to follow if you can no longer direct your own healthcare. How much time do you think it would take you to learn about the choices on this form, decide on your preferences, and complete a legally valid advance directive? Total number of hours: _

25. Rate how much you agree with the following statement: The amount of time you would need to spend to completing an advance directive is a big inconvenience, and might lead you to delay doing so.

    a) Agree Completely
    b) Agree Somewhat
    c) Disagree Somewhat
    d) Disagree Completely
    e) Don't Know
    f) Prefer not to answer

26. Suppose that you did NOT fill out a form to appoint a health care representative or document any instructions for your healthcare, and you became unable to make your own healthcare decisions. By law, who do you think would be responsible for making your healthcare decisions? (display only relevant answers)

    a) Spouse
    b) One of my children
    c) All of my children
    d) Parent
    e) One of my siblings
    f) All of my siblings
    g) Another relative
    h) A friend
    i) Doctor
    j) Other
    k) Don't Know
    l) Prefer not to answer

27. How likely do you think it is that you will need someone else to make medical decisions for you in the next 5 years?

a) _%chance

b) Don't Know

c) Prefer not to answer

28. How likely do you think it is that you will need someone else to make medical decisions for you at some point in your life?

    a) _%chance

    b) Don't Know

    c) Prefer not to answer

Category: Discounting

In this section, we'll ask you a series of hypothetical questions for research purposes. Answer the questions as best you can, even if you're not completely sure of your response. You can also choose not to answer any of the individual questions.

29. Would you prefer to receive $100 today, or $105 one year from today? (note this format of question will be repeated until patients state they'd prefer to take the money in one year

    a) $100 today [repeat question with higher value for in one year]

    b) $105 in one year [end question sequence]

    c) Prefer not to answer