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The Effect of Medicaid Expansion on Diagnosis, Care, Treatment, and Health for Persons with Diabetes: Evidence from Medicaid Expansion in Wisconsin

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Abstract

The authors report time-series evidence on the effects of expansion of Medicaid coverage under the 2014 Affordable Care Act (ACA), on healthcare and outcomes for persons with diabetes, including medium and longer-term effects. They focus on diabetes, as a common, often-underdiagnosed condition for which effective treatments exist, and on childless adults, who were a particular target for ACA expansion. The authors use difference-in-differences (DiD) analysis applied to a longitudinal, uniquely detailed (relative to prior work) dataset which includes visit-level electronic healthcare records over 2011-2022 from Medical College of Wisconsin (a major Milwaukee-centered health system) linked to Medicaid enrollment records. They compare a treatment group of 1,679 newly-enrolled childless adults with diabetes to a propensity-score-balanced control group of 1,600 already-insured adults with diabetes (mostly parents with children at home). Gaining insurance leads to sharply higher utilization (outpatient, ED, and hospitalization); and large increases in new diagnoses of diabetes and other chronic conditions; related prescriptions; and testing rates. The authors find improvement in intermediate health outcomes (blood sugar, blood pressure, and cholesterol levels), and suggestive evidence of improved longer-term outcomes (macrovascular events; advanced kidney disease; amputations). They also compare persons with diabetes to other persons; DiD coefficients are generally larger for persons with diabetes, but percentage changes are often higher for other persons.

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I. Introduction

Health insurance is expensive, indeed uniquely so in the U.S. An important bottom-line policy question is, does the extra spending from expanding health insurance pay off in better population health. The existence of a link between expanding health insurance and improved health is not obvious, given that we already insure some vulnerable populations, including the elderly and the disabled; anyone needing emergency care must be treated regardless of ability to pay under the federal EMTALA law (Emergency Medical Treatment and Active Labor Act); and the existence of other elements of a healthcare safety net, including public hospitals in some areas and a national network of federally qualified health centers (FQHCs) that provide free or low-cost outpatient care to low-income persons. The effects of health insurance on health can only be studied at the margin, for persons not already covered by this important but incomplete health insurance and healthcare safety net. The effects of health insurance on health also turn out to be hard to find; many studies find mixed results. A recent book reports "widespread agreement among researchers that variation in medical care or in health insurance is not a major driver of variation in health." (Einav and Finkelstein, 2023, p.86).

We seek to make progress on the link between health insurance and health, focusing on persons with diabetes. If health insurance affects health, effects are more likely for persons with treatable chronic conditions, that might go undiagnosed or untreated without insurance. Diabetes is a good place to look for those effects. It is a common chronic condition, often underdiagnosed (especially for the uninsured), for which treatment can prevent or delay an array of long-term diabetes complications and reduce adverse outcomes. Gaining health insurance can lead to increased diagnosis of diabetes and related conditions, followed by treatment, which can potentially improve outcomes.

We study here a healthcare channel for the effect of health insurance on health.[1] We study childless adults in Wisconsin, who gained Medicaid insurance in 2014 under the Affordable Care Act (ACA). We extend a prior project using the same dataset (Farzana et al., 2025a). We also compare results for persons with versus without diabetes. We trace the causal channel from health insurance to healthcare to health. For persons with diabetes, gaining insurance translates rapidly into more healthcare, including visits, tests, new diagnoses of diabetes and related conditions, and prescriptions. We also find evidence that gaining insurance also translates, over a period of years, into better health.

[1] We do not study an income channel, in which persons who receive free or subsidized health insurance are wealthier, with wealth leading indirectly to improved health.

We rely on longitudinal electronic health records (EHR) from the Medical College of Wisconsin/Froedtert Health (MCW), a major, Milwaukee-based health system, over 2011-2022, to study the same patients, both before and after gaining Medicaid coverage. We link these records to Medicaid enrollment records. We conduct a difference-in-differences (DiD) analysis, and compare healthcare utilization and outcomes for a treatment group of newly enrolled childless adults to a control group of previously enrolled adults (mostly parents of minor children), using entropy balancing weights to balance the two groups. We study outpatient visits, emergency department (ED) visits, and hospitalizations; diagnoses for diabetes and diabetes-related chronic conditions; medication prescription rates; testing rates; and intermediate and longer-term health outcomes.

Relative to the prior literature on Medicaid expansion, our visit-level data offer unique advantages. We are not aware of other EHR-based research e spanning outpatient visits, ED visits, and hospitalizations, over a multiyear time period, other than our own similar study of Indiana (Owen et al., 2025). We uniquely study time dynamics for a multiyear period after Medicaid expansion.

The most comparable research comes from the Oregon Health Insurance experiment (OHIE) (Allen et al., 2010; Baicker et al., 2013; Finkelstein et al., 2012, 2016; Taubman et al., 2014; Inoue et al., 2024). We lack a randomized experiment, but our data have important strengths, relative to OHIE. First, we have data on outpatient visits, which are a large fraction of all visits, and the principal source for diagnoses, prescriptions, tests, and lab values. Second, OHIE had hospitalization data only for one year, and ED-visit data for about 18 months. We have a much larger effective sample over a multiyear period, with limited attrition.[2] The larger sample provides statistical power sufficient to study the subsample of persons with diabetes. Third, we can study pre-expansion trends, which provide evidence on who enrolls once eligible. We note that OHIE compared newly insured to uninsured persons; our control group is already insured persons.

In 2014, Wisconsin expanded Medicaid eligibility to include childless adults with incomes up to 100% of the federal poverty limit. Gaining Medicaid coverage led to a sharp increase in healthcare utilization, across outpatient visits, ED visits, and hospitalizations (Farzana et al., 2025). Here, we study persons with diabetes and find a more complex picture. Outpatient visits rise strongly after expansion to close to control group levels. ED visits and hospitalizations for treated persons are well below control group rates prior to expansion; jump at the time of expansion to above control group rates, suggesting pent-up healthcare demand; and then relax to control group rates over about a one-year period. The jump in ED visits is driven by visits leading to hospital admission.

[2] The OHIE initial sample is comparable in size to ours, but the effective sample is smaller, due to the modest fraction of compliers in their causal IV analysis, with rapid attrition from the treatment group.

Gaining Medicaid insurance predicts a sustained increase in new diagnoses of diabetes and related chronic conditions; prescription of related medications; and testing rates. We find improvement in several intermediate health outcomes, including blood pressure, blood sugar, and cholesterol. The post-expansion increase in diagnoses, prescriptions, testing, and improved intermediate health outcomes is larger in magnitude for persons with versus without diabetes, but percentage changes are often larger for persons without diabetes.

For longer-term adverse outcomes (we study acute myocardial infarction (AMI), stroke, advanced kidney disease, kidney failure, foot ulcer and amputation), event rates are low which limits statistical power, and any assessment is tentative. Consider heart attack and stroke rates. Over the first five post-expansion years (2014-2018), relative heart attack and stroke rates *rise.* We interpret this rise as reflecting greater willingness of insured persons to seek care for a possible heart attack or stroke, coupled with possibly worse pre-expansion health for the treatment group, rather than an actual increase in events for the newly insured. The similar rates after 2018 are suggestive of longer-term health gains from being insured.

Overall, we find evidence of substantial unmet healthcare demand among newly insured persons with diabetes. Some of that care is urgent, reflected in a post-expansion jump in ED-to-admission visits. Over time, as the treatment group obtains more outpatient care, their need for ED and hospital care recedes. This is consistent with greater access to outpatient care, over time, reducing need for ED and hospital care.

We note several limitations here, and provide details below:

Selection effects: We have a plausibly causal study of the effects of gaining insurance on a "complier" group of persons, newly eligible for Medicaid, who chose to enroll in Medicaid. The control group is already enrolled persons, who also chose to enroll.

Defining patients with diabetes. Diabetes is often underdiagnosed, with underdiagnosis especially likely for the uninsured. Thus, defining diabetes based on pre-expansion care will undercount diabetes in the treatment group.

Did treated patients obtain more outpatient care, or only more care at MCW? For outpatient care, some newly insured patients, who formerly obtained free or discounted care at FQHCs, may have moved to MCW, rather than obtaining more care**.** This concern should not affect ED or hospital care.

Retroactive coverage. Some treatment group members first received care and then obtained retroactive coverage, rather than enrolling first. We bound this effect at under 1% of the treatment group.

II. Background: The ACA and Prior Research

A. Health Insurance Expansion under the Affordable Care Act

The ACA provided federal support for expanding Medicaid coverage to all persons with incomes up to 138% of the Federal Poverty Line (FPL). Previously, Wisconsin provided coverage for low-income parents and caretakers with minor children at home ("parents"), but very limited coverage for adults without minor children at home ("childless adults"). The ACA also provided deeply subsidized private insurance,

through health insurance exchanges, for persons with incomes above the Medicaid threshold. Wisconsin expanded eligibility as of January 1, 2014, but only to 100% of FPL.

B. Overview of ACA Research

Farzana et al. (2025) provide an overview of ACA studies; see also Soni, Wherry, and Simon (2020) (review). We discuss here selected studies that address diabetes; see Appendix for additional studies. The Oregon Health Insurance Experiment (OHIE) is the prior work closest to our own. However, OHIE had a much smaller effective sample and only short-term data. It lacked power to study a subsample of persons with diabetes; and could not study time dynamics. OHIE compared Medicaid lottery winners to uninsured lottery losers. In contrast, we compare newly insured to already insured persons.

C. Studies Addressing Diabetes and Related Conditions

Diabetes is often underdiagnosed (e.g., Gwira, 2024). Earlier treatment predicts improved outcomes (e.g., Colagiuri et al., 2002; Holman et al. 2008). Medicaid expansion leads to more diabetes diagnoses (e.g., Baicker et al., 2013; Farzana et al., 2025; Owen et al., 2025). Huguet et al. (2018, 2023) report that HbA1c screening rates rose significantly in expansion states; higher visits by diabetic patients for acute blood glucose abnormalities, but no change in infection-related visits.

D. Gaps in the Medicaid Expansion Literature for Persons with Diabetes

For persons with diabetes, prior research on the effects of Medicaid expansion on health outcomes is limited to studies using survey data, rather than the EHR data we use. No prior research provides similar time-series evidence on the response of newly insured persons with diabetes to gaining Medicaid insurance, or compares the response of persons with versus without diabetes.

III. Data and Methods

A. Datasets

We construct the treatment and control groups by linking two datasets. The first is complete enrollment and claims records for all Medicaid enrollees in Wisconsin, over 2011-June 30, 2022, aged 35-85 (932,211 persons). Persons enter the Medicaid sample when they turn 35, and do not age out.

Wisconsin has separate eligibility categories for childless adults and parents of minor children. Prior to 2014, Wisconsin had very limited eligibility for childless adults, and covered parents with incomes up to 300% of FPL.[3] In 2014, Wisconsin made both groups Medicaid-eligible with an income limit of 100% of FPL, expecting that most previously insured parents with incomes above 100% of FPL would obtain coverage through the ACA health insurance exchanges. The vast majority of newly enrolled childless adults were previously uninsured (Dague, Burns, and Friedsam, 2022).

[3] Source: https://www.healthinsurance.org/medicaid/wisconsin/.

Our second dataset is EHR over 2011-June 30, 2022, for all persons aged 35-85 who made 1+ visits to an MCW facility during 2011-2020 (939,689 unique persons). The MCW records are reasonably complete for ED visits and hospitalizations only from the 4th quarter of 2012 on. We use the MCW data to measure healthcare utilization and outcomes, both before and after Medicaid expansion. This sample reflects the MCW catchment area and is primarily urban, from Southeast Wisconsin. MCW is one of three large health systems in this area, with a roughly one-third market share.

The treatment group is all persons aged 36-64 as of January 1, 2014, who gain coverage as childless adults between April 1, 2014 (start of new enrollment) and March 31, 2015, with at least a 6-month gap since prior coverage. We use a starting age of 36 to exclude persons who were Medicaid-eligible in 2013 (which we would not otherwise observe), and an ending age of 64 years to exclude people who are or will soon be Medicare-eligible. We otherwise keep persons in the sample after they age into Medicare eligibility. Of 73,793 childless adults in the treatment group statewide, the usable group is 10,684 persons who also have MCW data. The control group is all persons aged 36-64 who were Medicaid-insured at any time during 2013 and either did not lose coverage in the first half of 2014 when Wisconsin reduced the income threshold for coverage, or regained coverage by the end of 2014. Of 73,255 persons in the control group statewide, the usable group is 11,568 persons with MCW data. We use the Medicaid data only to define the treatment and control groups.

B. Sample Balance and Entropy Balance Reweighting

The control group is younger and more heavily female than the treatment group (Table 1). We address this imbalance by applying ATT (average treatment effect on the treated) weights to the control group, using entropy balancing (eBalance) weights (Hainmueller, 2012), which creates near-exact covariate balance between the treated and control groups. These weights can be viewed as constrained inverse propensity weights (Zhao and Percival, 2017; Zhao, 2019). We use separate weights for the 2014-diabetic, 2018-diabetic, and non-diabetic samples. We balance only on the mean, because most of the variables we balance on are binary.

The covariates we balance on are: age in years at January 1, 2014, sex, race/ethnicity (the available categories are Black, Hispanic (non-Black), Asian (not Black or Hispanic), White (not Hispanic), and other/missing) and quintiles of area-level socioeconomic status measured at ZIP code level in 2014 (area-SES), using the Graham Social Deprivation Index (Butler et al., 2013). The treatment group is heavily White and Black, and, as expected, mostly lower SES. Table-1 shows summary sample statistics before and after balancing.

We do not balance on pre-treatment health, because observed health depends on healthcare, which depends on insurance status. The control group received more healthcare, and therefore more disease diagnoses, and will appear to be in worse health than the treatment group. After expansion, chronic disease

diagnosis rates for the treatment group gradually catch up to the control group, suggesting similar health at the time of expansion.

C. Defining Diabetes, and the Subsample of Persons with Diabetes

We use a diabetes definition adapted from SupremeDM (Nichols et al., 2012); see Appendix for details. We construct a narrow 2014-diabetic subsample (diagnosed with diabetes by year-end 2014), a broad, 2018-diabetic subsample (diagnosed by year-end 2018), and a non-diabetic subsample (those not in the 2018-diabetic subsample). We prefer the 2018-diabetic subsample; the logic supporting this definition is that underdiagnosis of diabetes is especially likely for the treatment group prior to expansion. Farzana et al. (2025a) find a sharp post-expansion increase in diabetes diagnoses for the treatment group; the "catchup" diagnoses largely occur by 2018 (Appendix Figure App-11). The 2018 definition reflects input from diabetes experts; it also provides a larger sample and thus greater statistical power (Table 1).

Yet it is uncomfortable to use post-expansion diagnoses to define which persons have diabetes. We therefore also study the 2014-diabetic subsample. We report results for both groups in regression tables, but provide graphs for, and principally discuss, the 2018-diabetic sample. See Appendix for full results for the 2014-diabetic and non-diabetic subsamples.

D. Medicaid Enrollment Over Time

We consider the treatment and control groups as fixed; regardless of subsequent changes in enrollment. Some decline over time in Medicaid enrollment is expected, for several reasons, due to persons earning more than 100% of FPL; aging into Medicare eligibility; moving out of state; or dying. Figure App-1, Panel A (left graph), shows the monthly percentage of treated and reweighted control persons who were Medicaid eligible over the sample period. Eligibility of both groups declines only gradually over the sample period; with reasonably parallel trends for treated and control persons.

Figure App-2, Panel B, provides *daily* new enrollments for the 2018-diabetic subsample. Almost all enrollments are on the first day of a calendar month, for persons found eligible during that month. Expansion began on April 1, 2014 (957 persons; 60% of treated persons in this subsample), with smaller spikes in each subsequent month. We therefore treat April 1, 2014, as the start of the treatment period.

Because we keep the treatment and control groups fixed, once formed, and some people who lose Medicaid insurance gain other coverage, our study can be seen as estimating the effects of gaining *health insurance*, initially Medicaid, but with some migration to commercial insurance or aging into Medicare.

E. Regression Methods

We use a classic DiD model with patient and calendar quarter fixed effects, applied separately to the 2014-diabetic, 2018-diabetic, and non-diabetic subsamples:

$$Y_{i,t} = \beta_0 + \alpha_i + \gamma_t + \Theta(\text{Post} \cdot \text{w}_\text{i}) + \epsilon_{it} \quad (1)$$

Here $Y_{i,t}$ is the outcome, e.g., outpatient visits in quarter t by individual i. The α_i and γ_t are individual and year*calendar quarter fixed effects (FE), w_i is a treatment group indicator, Post is a treatment period indicator (=1 beginning in 2Q 2014). Θ is the treatment effect. Post and w_i, if included in the regression, would be absorbed by the FE. We do not include health-related covariates because these are affected by insurance status. The DiD model with eBalance weights is formally doubly robust (Zhao and Percival, 2017). Standard errors (s.e.'s) are clustered on patient.

We also modify eqn. (1) to provide separate treatment effect estimates for each time period, before and after treatment, which we present in leads-and-lags graphs. Let time 0 be the first treatment period quarter (2Q 2014), and k index time relative to 2Q 2014. We define a base quarter b as ($b = -5$ quarter -5 (1Q 2013)). The model is:

$$Y_{i,t} = \beta_0 + \alpha_i + \gamma_t + \sum_{k=-m,k\neq b}^{q} B_t^k w_i \lambda_k + \varepsilon_{it} \quad (2)$$

Here B_t^k is an indicator which equals 1 in period k, 0 otherwise; other variables are defined above. We rely in the text on univariate ratio graphs, and present the corresponding leads-and-lags graphs using eqn. (2) in the Appendix. Results are similar with both approaches.

Many figures report cumulative treatment/control ratios of rates, using bootstrapped 95% confidence intervals (CIs); see Appendix for details. For some outcomes, any treatment effect should emerge with a lag. Any lag will bias downward DiD estimates, which reflect an average over the treatment period.

Many DiD designs compare newly treated to never-treated units. Our control group, in contrast, is already insured persons. The implicit assumption is that the controls have been insured for long enough so that their response to gaining insurance has stabilized. This appears reasonable based on the visit ratio graphs in Figure 1.

IV. From Health Insurance to Healthcare Utilization

We assess in this part the evidence for the effects of gaining health insurance on healthcare use by persons with diabetes.

A. Outpatient Visits

Table 2 provides DiD regression results, following eqn. (1), for all three visit types. It provides separate coefficients for the 2014-diabetic, 2018-diabetic, and non-diabetic subsamples. In Table 2, col. (1), the DiD coefficient for the 2018-diabetic subsample is 0.500, which is an 89% increase relative to the pre-expansion mean of 0.559 (measured over 1Q 2013-1Q 2014). Both statistical and clinical significance are strong.

A concern with the outpatient-visit estimates is that some treatment group members may have previously obtained outpatient care from FQHCs or perhaps other providers, and switched to MCW after becoming insured. We address this concern below.

Figure 1, Panel A reports univariate outpatient visit patterns for treated and control persons in the 2018-diabetic subsample, by calendar quarter over 2011 through June 30, 2022.[4] The dip in 1H 2020 in this and other figures reflects the COVID-19 pandemic. In each panel of Figure 1, the left-hand graph reports univariate rates for each group; the right-hand graph shows the treated-to-control visit rate ratio. Visit rates for both groups increase during the pre-treatment period. This reflects MCW expansion during 2Q-2012 through 3Q-2013, with a more stable MCW footprint after that. The outpatient treated/control ratio shows reasonably parallel relative trends, with a treated/control ratio around 40-50%. This suggests that the parallel-trends assumption underlying DiD analysis is plausible during the treatment period, when MCW's footprint was fairly stable.

Treated patients show a sharp jump in outpatient visits beginning in 2Q 2014 (the first post-expansion quarter), rising further over the next several quarters, to slightly less than control group rates. Visit rates for the two groups then move closely together for the rest of the sample period.

One possible explanation for the visit rate ratio approaching but not reaching 1.0 is that treatment group members, while uninsured, had become accustomed to receiving less health care. We provide evidence below that the two groups have similar hospitalization and chronic disease rates, suggesting similar health.

B. ED Visits in General

Table 2, col. (2) provides DiD regression results for all ED visits. Figure 1 Panel B presents the corresponding univariate and ratio graphs. In Table 2, for the 2018-diabetic subsample, there is a modest, statistically insignificant increase in overall ED visits by newly insured persons of 0.015 visits per quarter (14% increase over the treatment group pre-treatment mean). However, this modest coefficient masks important time dynamics.

In Figure 1, Panel B, ED visits increase during the pre-expansion period for both groups, reflecting MCW expansion, but the treated/control ratio in the right-hand graph shows no overall trend. This supports the parallel trends assumption during the treatment period.

At the start of expansion, in 2Q 2014, treatment group visit rates jump, from around 80% to around 120% of control group rates (a roughly 50% rise). Elevated treatment group rates last for about a year, followed by similar rates during 2015-2018, thus reflecting a roughly 25% rise in treatment group rates. Treatment group rates then fall to below control group rates beginning in 2019. Figure 1 confirms the importance of time dynamics in ED visit use after gaining insurance. The reasons for the relative decline in

[4] See Appendix Figures App-6 to App-9 for leads-and-lags graphs and for similar graphs for the 2014-diabetic and non-diabetic subsample.

treatment group visits after 2018 are unclear, but one possible reason is medium-term improvements in health, leading to reduced need for ED care.

The jump in both outpatient and ED visits in 2014 suggests unmet demand for healthcare among newly insured persons with diabetes. The partial drop in treatment-group ED visits in 2015 could reflect a combination of: (i) that need having been met; and (ii) people who initially used the ED for non-urgent care finding a source of regular care.

C. ED Visit Time Dynamics: Admitted versus Discharged

A headline OHIE result was a 40% jump in ED visits. This increase was concentrated among ED visits leading to discharge, with no significant change in more-serious visits leading to hospital admission. The implication from OHIE was that for serious conditions, warranting hospital admissions, most low-income uninsured people were already going to the ED, perhaps relying on the EMTALA requirement that they receive treatment regardless of ability to pay.

We provide evidence for a very different pattern for diabetic patients. In Table 2, columns (4)-(5), we study separately ED-to-discharge and ED-to-admission visits. The DiD coefficient for ED-to-discharge visits for 2018-diabetic patients is near-zero, indeed slightly negative. In contrast, the ED-to-admission coefficient is +0.018 and strongly significant. (a 52% rise in ED-to-admission visits).

In Figure 1, Panels C and D, we separate ED visits into ED-to-discharge and ED-to-admission visits. First, consider ED-to-discharge visits. The pre-expansion mean for the treated group is only slightly less (88%) of the control group mean (Table 2). ED-to-discharge visits jump modestly at the onset of Medicaid expansion, but only to control group rates. Treated and control group rates are similar for several years, but treated group rates fall after about mid-2018, with a treated/control ratio generally 0.8 or lower; indeed often lower than during the pre-treatment period. These low rates present a puzzle. One might hope that over time, newly insured persons obtain regular outpatient care, leading to some replacement of ED-to-discharge visits with outpatient visits. Gradual improvement in health could also lead to reduced need for ED care. But these possibilities cannot readily explain a long-term treated/control ratio well below 1, nor a ratio that is below the similar ratio for outpatient visits. We again speculate that treatment group members, while uninsured, became accustomed to receiving less health care, including less ED care.

The time dynamics and treated/control ratios are quite different for ED-to-admission visits. For these visits, the pre-expansion treated/control ratio averages 71% (Table 2). Treated group ED-to-admission visits spike in 2014 to well above control group rates; then relax to somewhat above control group rates over 2015-2020, and perhaps somewhat below control group rates in 2021 and 2022 (Figure 1, Panel D). This pattern provides strong evidence that prior to gaining insurance, some uninsured patients with diabetes were not coming to the ED, even for conditions serious enough to warrant hospitalization. As we discuss below, apparent ED-avoidance includes heart attack and stroke (Figure 3, Panel B).

In Appendix Figure App-4, we report the ED admission rate – the fraction of ED visits that lead to admission. The treatment group rate is below the control group rate prior to expansion, but rises to generally above the control group rate after expansion. Compare Janke et al. (2020) (post-expansion increase in ED admission rates in 5 expansion states).

One explanation for why the initial spike in ED-to-admission visits subsides after about 18 months is that initial pent-up need was largely satisfied. Also, some visits by newly insured people could involve primary-care-treatable events; these persons later found a regular primary care provider, and received care sufficient to avoid the need for future ED-to-admission visits.

D. Hospitalizations

In Figure 1, Panels E and F, we report rates for total hospitalizations and direct hospitalizations (not from ED).[5] Results for hospitalization from the ED are captured in the ED-to-admission graphs in Panel D. Note that the y-axis scale is expanded for direct hospitalization, because most hospitalizations come from the ED. There is no evidence of non-parallel pre-treatment trends, either in Figure 1 or in the leads-and-lags graphs in Figure App-6.

For all hospitalizations, the treatment group pre-treatment mean is around 65% of the control group mean (Table 2). The treated/control ratio jumps to above 1 at the onset of expansion, remains mostly above 1.0 through 2017, and is roughly 1.0 after that. The DiD estimate is a 63% rise in total hospitalizations.

For direct hospitalizations, the treatment group pre-treatment mean is only 47% of the control group mean (Table 2). Treatment group rates rise in 2014 to roughly control group rates; remain similar to control group rates through 2019, but then rise to well above control group rates during the pandemic, for unclear reasons. The DiD estimate is a 108% rise in direct hospitalizations for the treated group.

E. Receipt of Regular Outpatient Care

One potential benefit of having health insurance is greater likelihood of receiving regular outpatient care. In Table 2, col. (7) and Figure 1, Panel G, we report results for a measure of regular care, defined as two outpatient visits within the prior 365 days.[6] Regular care for treatment group members increases substantially (coeff. = 0.100); this is a 76% increase versus the pre-expansion treatment group mean. However, the treatment group regular care rate remains modest and below the control group rate. For both groups, the guideline recommendation for patients with diabetes of twice-yearly visits is more the exception than the rule.

[5] The direct hospitalizations include transfers from a non-MCW facility; we cannot separate these hospitalizations from other direct hospitalizations with our data.

[6] Given the 365-day lookback, we limit the sample period to start in 1Q 2012. We exclude visits within two weeks after a prior visit, to exclude multiple closely spaced visits for the same condition.

F. Change in Outpatient Visit Frequency vs. Change in Visit Location

Our EHR data comes from MCW, which has a roughly one-third market share in the Milwaukee area. This raises a threat to inference for outpatient visits: Some uninsured patients may have obtained free or low-cost outpatient care from FQHCs or perhaps other providers, and switched to MCW once insured rather than obtaining more care. This concern arises only for outpatient care. The Milwaukee area does not include a public hospital. Thus, for hospital-based care, there was no source of cheaper care, and no reason why gaining insurance would lead patients to switch to MCW from another facility. We confirmed informally that all three major Milwaukee-area health systems follow similar practices on billing the uninsured; they do not pursue collection for persons with incomes under 250% of FPL.

We cannot directly assess the extent to which newly insured persons move their outpatient visits to MCW with our data, but believe the likely extent is small. First, in our parallel project in Indiana, we use data from the Indiana Health Insurance Exchange (IHIE), and study the effects on visit rates of gaining Medicaid coverage in a setting with stronger coverage of outpatient visits (informally estimated at 60-80% coverage) (Owen et al., 2025). The Indiana treatment effects are similar to those for MCW, with treated/control visit rate ratios around 0.5 pre- expansion, which rise after expansion to roughly 1.0.

Second, two separate studies of FQHCs found no evidence of significant attrition following Medicaid expansion. Cole et al. (2017) found parallel trends in 2014 in visit rates to FQHCs in expansion and non-expansion states. Huguet et al. (2020) study attrition for a multistate FQHC network, for a pre-expansion 2012 cohort (first seen in 2012), a pre-expansion 2013 cohort (first seen in 2013), and a post-expansion 2014 cohort (first seen in 2014), and found similar attrition rates for all three cohorts. They conclude:

> Contrary to expectation, our results did not support the assumption that patients who gained coverage following the ACA, sought care outside of community health centers as the attrition rates and the probability of changing providers remained constant over the ACA implementation period.

The Cole and Huguet results are consistent with no meaningful migration of outpatient visits from FQHCs to other providers. The Cole confidence intervals rule out effects larger than around 15-20%.[7]

We used simulation to bound the extent of potential bias. We proceed as follows. In each quarter from 2011Q1 through 2014Q1, we randomly added pseudo-visits to treatment group members in the pre-expansion period. We added 10%, 20%, 30%, 40%, and 50% additional pseudo visits, re-estimated the treated/control outpatient visit ratio and report results in Appendix Table App-2 and Figure App-8. With 20% pseudo-visits (the Cole upper bound), the DiD coefficient for outpatient visits by 2018-diabetic patients in Table 2 becomes 0.41 versus the 0.50 in Table 2.

[7] These are our informal estimates, based on the numerical values in Cole et al. appendix, § E.

In Figure App-8, as we increase the percentage of pseudo pre-treatment visits assigned to the treatment group, the treated/control ratio rises, but even at 50% additional visits (far above the Cole upper bound), there is a substantial jump in this ratio at the time of expansion. The DiD coefficients shrink, but remain clinically important and statistically strong (Table App-2).

G. Healthcare Utilization by Persons With versus Without Diabetes

We also compared the response to Medicaid expansion of persons with versus without diabetes. We hypothesized that patients with diabetes would respond more strongly to gaining insurance, due to greater healthcare needs. In Table 2, for outpatient visits and hospitalizations, DiD coefficients are positive and strongly statistically significant for both groups, and are larger in magnitude for diabetes patients. ED coefficients are an exception, these are larger for non-diabetic patients; driven by ED-to-discharge patients.

Even when coefficients are larger for diabetic patients, percentage increases relative to the pre-treatment mean are larger for non-diabetic persons. This pattern of increases that are larger in magnitude for persons with diabetes, but similar or larger percentage changes for non-diabetic persons, will recur across many outcomes in later tables.

H. Assessing Parallel Trends

A valid DiD design must satisfy the parallel trends assumption, that time trends would have been parallel during the treatment period for the treated and control groups, but for the treatment. This assumption is not testable, but an important marker for plausibility is whether there are parallel trends in the pre-treatment period.

Assessing parallel pre-treatment trends is complicated for our setting, in which MCW expanded its market share through acquisitions during the pre-treatment period, but had a largely stable footprint after expansion. Measured by visit counts, outpatient visits are not parallel during the pre-treatment period (Figure 1). However, the ratio of treated/control visits is reasonably parallel. This provides reasonable support for the parallel trends assumption during the treatment period for both visit *ratios* and for visit *counts*.

I. Selection Effects for Who Enrolls in Medicaid

Our DiD analysis relies on an exogenous change to Medicaid *eligibility* in 2014. Yet signing up for Medicaid reflects enrollee choice. We should expect eligible persons with unmet healthcare needs to be more likely to sign up, especially persons with known near-term healthcare needs. In full-sample results (Farzana et al., 2025a), ED visit rates and hospitalization rates rise for 1-2 quarters before expansion, consistent with persons with urgent healthcare needs (proxied by recent prior ED visits or hospitalizations) being more likely to enroll. We found more limited evidence for a pre-expansion rise in ED visits or hospitalizations for persons with diabetes: we found only a modest increase in ED-to-admission visits in the three weeks prior to enrollment.

Different health insurance expansions will affect different complier populations, who may have different healthcare needs (Kowalski, 2023). For our study, one can see the 2014-diabetic, 2018-diabetic, and non-diabetic subsamples as including different complier populations, with different treatment effect magnitudes.

J. The Role of Retroactive Coverage

Wisconsin Medicaid rules allow applicants to seek retroactive coverage, starting up to 3 months before the application month. This raises the question of which comes first for the newly insured, healthcare use or Medicaid coverage. This turns out to be only a small concern for our sample. In Farzana et al. (2025a), we estimate an upper bound on the extent to which new enrollees obtain care first, and are enrolled later, at 70 persons out of 10,634 (0.66%).

V. Link from Utilization to Diagnosis and Treatment

We seek here to trace as much as we can of the causal chain running from health insurance to healthcare and then to health. We continue to trace that causal chain in this part. We study diagnosis of diabetes and related conditions, care for those conditions, and several intermediate health outcomes. Type 2 (adult-onset) diabetes, if diagnosed, responds to diabetes and cardiovascular medications, which can lower blood sugar, blood pressure, and cholesterol, and reduce long-term complications. We hypothesized that gaining Medicaid insurance would increase new diabetes and related diagnoses, which would lead to increased prescriptions, increased testing, and perhaps measurably lower blood sugar, blood pressure, and cholesterol levels. b

Farzana et al. (2025a) studied incident diabetes diagnoses for the full sample and found a sharp jump in diabetes diagnoses for the treatment group after expansion, leading to a gradual rise in the cumulative treated/control diagnosis rate ratio, from 0.6 in 1Q 2014 to around 0.90 by 2018. We reproduce those results in Appendix Figure App-11 and Table App-3. Here, we study incident diagnoses for other chronic conditions, related to underlying diabetes. We start the sample period in 3Q 2011, due to erratic results for some outcomes in 1H 2011.

A. New Diagnoses for Diabetes-Related Conditions

Table 3 col. (1)-(5) reports DiD coefficients for a number of chronic conditions that are associated with diabetes: microvascular disease (retinopathy or neuropathy); peripheral vascular disease; kidney disease; proteinuria, and ischemic heart disease. Figure 2 reports related graphs for the 2018-diabetic subsample. See Appendix for diagnosis codes.

For diabetes-related diagnoses, there can be two offsetting effects from gaining health insurance. First, insurance can lead to increased diagnosis of existing conditions, much as for diabetes. Second, in the longer run, better care could lead to lower true incidence of diabetes complications. Over the time frame of this study, we expect increased diagnoses to dominate.

All DiD coefficients are positive; most are statistically significant, with large percentage increases relative to the pre-expansion treatment group mean. For the 2018-diabetic subsample, these range from 41% for proteinuria to 78% for kidney disease.

Figure 2 shows the cumulative treated/control diagnosis ratio.[8] Across conditions, the cumulative diagnosis ratio rises and then levels off, reflecting diagnoses for the treatment group catching up to those for the control group. The long-term ratios are around 1.0 after expansion (somewhat higher for peripheral vascular disease). Catchup in diagnosis rates is faster for conditions that are more readily evident to patients (e.g., peripheral vascular disease) and more gradual for less-evident conditions.

B. Medication Prescriptions for Diabetes and Related Diagnoses and Treatment

Once diabetes is diagnosed, a cascade of events should happen. These include increased prescriptions of diabetes medications; regular outpatient checkups, including HbA1c testing; regular foot and eye exams, and checks for common diabetes complications (American Diabetes Association, 2024; Navaneethan et al., 2023). We study new medication prescriptions in Table 3, columns (6)-(8). We have data on prescriptions, not fills or use, but prescription fills for Medicaid enrollees are usually free, so cost should not be a barrier. The MCW data should capture initial prescriptions, but may not consistently capture refills.

We examine first-time prescriptions for three common medications for persons with diabetes: for diabetes itself, high cholesterol (statins), and high blood pressure. We study prescription of statins and blood pressure medications both as evidence of access to medication, and as proxies for elevated cholesterol and hypertension, which many persons with diabetes also have. All DiD coefficients are clinically large and statistically significant. Percentage increases relative to the treatment group pre-expansion mean are 38% for blood pressure medications, 51% for diabetes medications, and 63% for statins.

Figure 2 shows cumulative ratio graphs for these prescriptions.[9] The cumulative treatment/control medication ratio rises gradually in the pre-treatment period, but is only 60-70% just before expansion. This ratio rises over the first few post-expansion years to around 1.0.

The increases in blood pressure and cholesterol medications compare to the null result found by OHIE for cholesterol medications (Baicker et al., 2013). They underscore the value of using EHR versus the survey data that OHIE used to study prescription rates. Myerson et al. (2018) found higher diabetes medication prescriptions in expansion states. Ghosh, Simon and Sommers (2019) report increased

[8] See Appendix Figure App-12 for univariate and leads-and-lags graphs, and for graphs for the 2014-diabetic and non-diabetic subsamples.

[9] See Appendix Figure App-13 for univariate and leads-and-lags graphs and for similar graphs for the 2014-diabetic and non-diabetic subsample.

Medicaid-paid prescription fills in expansion states in 2014 and early 2015 across major drug classes, not offset by fewer commercial-pay or self-pay fills.[10]

C. Testing Rates

More visits should lead to more tests. Some tests will underlie the increase in diagnoses; guideline treatment for diabetes calls for regular testing, including regular HbA1c tests (American Diabetes Association, 2024). We study an array of tests and report results in Appendix Table App-5 and Figure App-10. Testing rates for the treatment group jump, promptly after expansion, to levels similar to the controls.

D. Comparison of Persons with versus without Diabetes

Table 3 also reports post-expansion diagnosis and prescription changes for patients without diabetes. Coefficients are smaller for non-diabetics, but percentage increases relative to the pre-treatment mean are consistently larger.

VI. Intermediate Health Outcomes

We continue in this Part to trace the causal chain from health insurance to health. Improved diagnosis and treatment can hopefully lead to improved health outcomes. In particular, more prescriptions for blood pressure medications, statins, and diabetes medications, assuming reasonable adherence, should lead to reductions in blood pressure, cholesterol levels, blood glucose and HbA1c levels. We limit the sample to patients with at least one pre-expansion and one post-expansion test of the indicated type (the only patients for whom a DiD estimate with patient FE is meaningful).

Table 4 reports DiD results. Panel 1 reports full sample results. However, we expect a treatment effect primarily for patients who are diagnosed and then receive corresponding medications. Panel 2 therefore limits the treatment group to persons who received a corresponding medication during the treatment period, and excludes treatment-period observations prior to the first treatment-period prescription. Below, we discuss the Panel 2 results. We caution that this approach does not provide a causal estimand for all treated persons. Figure 3 provides univariate graphs for selected intermediate outcomes.[11]

Sample sizes, reported in Table 4, are much smaller than the number of treated and control patients. The sample restrictions could produce selection bias, but we see no good alternative for DiD estimation.

A. Blood Pressure

For the 2018-diabetic subsample, we find an insignificant 0.72 point estimated drop in systolic blood pressure (0.35 points for diastolic). However, we find a statistically significant 1.68 point drop for the 2014 diabetic subsample (0.90 for diastolic). These drops, although small, are an appreciable percentage

[10] Zhang et al. (2024, review) found that insurance expansions generally predict both more prescriptions and lower blood pressure levels.

[11] See Appendix Figure App-14 for full graphs (univariate, leads-and-lags, and ratio graphs) for all studied outcomes.

of the feasible reductions. Blood pressure medications can lower systolic/diastolic pressure by 15/10 points, on average (Wu et al., 2005). But as of 1Q 2014 (last quarter of pre-treatment period), the 2014-diabetic treatment group had 77% of the medication rate of the controls. If their post-expansion medication rate equaled the controls, the predicted reduction would be (1-77%) * (15/10) = 3.45/2.30 points. The estimates in Panel 2 reflect 49%/39% of this potential reduction.

The univariate graphs in Figure 3 show that the treatment group has higher average systolic blood pressure than the controls in the pre-expansion period. Treatment group levels gradually converge to control group levels after expansion, consistent with gradual improvement in health.

Marino et al. (2020) found greater reductions in systolic and diastolic blood pressure for newly Medicaid-insured, FQHC patients with diabetes, relative to continuously insured patients, during the first two post-expansion years, averaging 1.8/1.0 points for systolic/diastolic pressure. Gotanda et al. (2021) found a statistically significant 3.0-point reduction in systolic blood pressure but oddly, an insignificant 1.1 point *increase* in diastolic pressure, for low-income persons in expansion vs. non-expansion states in 2015-2016, using the NHANES survey.[12]

B. Cholesterol Levels

For the 2018-diabetic subsample, we find a statistically strong 10.5 point drop in cholesterol levels. In the pre-treatment period, the treatment group statin prescription rate was 56% of that for the controls. Full catchup in prescription rates implies a reduction of 10.5/(1 - 56%) = 23.9 points for each newly treated person, an amount consistent with the medical literature on the expected cholesterol reduction from statins. In Figure 3, the treatment group has higher average total cholesterol than the controls in the pre-expansion period; the average drops during the treatment period to control group levels.

Marino et al. (2020) found a 3.3-point relative reduction in low-density lipoprotein (LDL) cholesterol for newly-Medicaid insured, FQHC patients with diabetes, relative to continuously insured patients, during the first two post-expansion years. Gotanda et al. (2021) found a statistically insignificant 6.8-point drop in LDL for low-income persons in expansion vs. non-expansion states, using the NHANES survey.

C. Blood Glucose and HbA1c Levels for Persons with Diabetes

In Panel 2, we find a statistically significant 8.3-point reduction in blood glucose levels and a marginally significant 0.23 drop in HbA1c for the 2018-diabetic subsample.

[12] OHIE found a null result for blood pressure levels (Baicker et al., 2013). A followup study using causal forest analysis (Inoue et al., 2024) reported a reduction of 4.96 points (systolic) and 3.91 (diastolic) for a subsample predicted to have high blood pressure benefit. We distrust these results because the mean blood pressure effect, estimable from their Figure 1, is much larger than the mean effect reported in Baicker et al. (2013).

Glucose levels fluctuate, so the correspondence is rough for any individual test but should be reasonable on average. Studies comparing glucose levels from continuous monitoring to HbA1c levels estimate: HbA1c = $\frac{glucose\ level + 46.7}{28.7}$.[13] Thus, the observed decline in glucose levels implies a (8.3/28.7 = 0.29) point drop in HbA1c levels.

This estimate can be compared to the feasible level. First-line diabetes medications such as metformin reduce HbA1c by about 1 point, on average (Hirst et al., 2012). Pre-expansion, the treatment group had 61% of the control-group medication rate. Full catchup in medication rates implies a feasible HbA1c reduction of around 0.4. The observed HbA1c decline (0.29 inferred from glucose tests; 0.22 direct estimate) is 73%/55% of this feasible reduction.

The univariate graphs for blood glucose (Figure 3) show that the treatment group has higher average blood glucose than the controls in the pre-expansion period. Treatment group levels gradually drop toward control group levels during the treatment period, but do not achieve full convergence. HbA1c levels show a similar pattern (Appendix Figure App-14).

Marino et al. (2020) found a relative HbA1c reduction of 0.24% for newly Medicaid-insured FQHC patients with diabetes. Gotanda et al. (2021) found a statistically significant 0.14 point drop in HbA1c levels for low-income persons in expansion vs. non-expansion states, using the NHANES survey.

D. Summary for Intermediate Health Outcomes

Our DiD point estimates for reduction in blood pressure, cholesterol, blood glucose, and HbA1c levels show benefit from new medications for these intermediate outcomes. The point estimates imply meaningful percentages of the feasible reductions with full catchup in prescription rates and full medication adherence.

VII. Longer-Term Health Outcomes

In Table 5, we report evidence on a variety of longer-term health outcomes: macrovascular events (heart attack (acute myocardial infarction, AMI) and stroke);[14] advanced kidney disease; kidney failure;

[13] See https://professional.diabetes.org/glucose_calc (American Diabetes Association site, containing the conversion formula); Nathan et al. (2008).

[14] We did not treat transient ischemic attack as a stroke. To exclude incidental and rule-out diagnoses, we considered only incident hospitalizations, with AMI or stroke as the primary discharge diagnosis. Results were similar if we excluded from the treatment group patients with AMI or stroke events in the Medicaid enrollment month, which could reflect reverse causation, with the hospitalization leading to retroactive Medicaid coverage. . In Appendix Table App-6, we separate AMI into STEMI (ST-segment elevation MI, due to complete blockage of a coronary artery) and nSTEMI (non-ST-segment elevation MI, usually due to partial artery blockage), and separate stroke into hemorrhagic and ischemic.

diabetic foot ulcer and amputation. Figure 3, Panel B, reports selected, corresponding univariate graphs.[15] The figures show semiannual instead of quarterly rates due to the small number of events.

A. Macrovascular Events

Consider first AMI and stroke hospitalizations. We stress that we only observe events that lead to care. AMI and stroke symptoms are not always obvious to patients, and the uninsured may be less likely to seek care, especially for less severe events. Compare Pines et al. (2021), who found a decline in ED visits for AMI and stroke early in the COVID-19 pandemic period.

For AMI, the DiD point estimates are insignificant. The univariate graphs show higher AMI rates for the treatment group over 2014-2018, and similar rates after that. This could reflect two offsetting effects: worse cardiovascular risk factors in the pre-treatment period, continuing into the initial treatment period (suggested by higher blood pressure, cholesterol, and blood glucose,(see Panel A), leading to higher event rates, and a longer-term drop in actual events, reflecting insurance gradually leading to improved health.

For stroke, DiD estimates are positive and significant. The univariate graphs show lower pre-expansion treatment group rates, higher rates over 2014-2019, and similar rates after that. Yet more actual events after expansion are implausible. A more likely story: insured patients were at higher stroke risk pre-expansion; once insured, they were more likely to seek care for a stroke event; and by 2020-2022, their actual risk converged to control group risk.

Considering AMI and stroke together, the time-series evidence in Figure 3 supports the value of insurance. Higher event rates once insured are implausible. Our results suggest instead that the uninsured sometimes avoid care even for severe conditions, and are more likely to seek care once insured. Receiving care can reduce harm from the incident event; improved care can reduce repeat event risk and, over time, can reduce incident event risk. We find suggestive evidence of lower long-run risk. Higher incident event rates for the treatment group rates over roughly 2014-2018, and similar rates thereafter, are consistent with initial worse health for the uninsured and gradual catchup. One would need a larger sample to assess this story more rigorously.

Note that our sample period largely predates the use for patients with diabetes of SGLT-2 and GLP-1 (glucagon-like peptide-1) medications, which are known to reduce macrovascular risk, and the more recent use of GLP-1 medications for obese patients more generally. This suggests that the effects of health insurance, in improved access to medications and thus lower AMI and stroke risk, could be higher today than during our study period.

[15] See Appendix Figure App-15 for the remaining graphs and for leads-and-lags and ratio graphs for all outcomes.

B. Severe Kidney Disease

In Table 5, we find positive and significant coefficients for both advanced kidney disease and kidney failure. The positive coefficients are likely to reflect earlier diagnosis and treatment of kidney disease once the treatment group is insured. Here too, higher underlying disease rates are implausible. For advanced kidney disease, the treatment group rate is generally below the control group rate in the pre-expansion period, jumps to above the control group rate after expansion, and remains elevated through around 2018 (Figure 3). This is consistent with catchup in diagnosis after expansion.[16]

C. Foot Ulcers and Amputations

We study incident foot ulcer diagnoses (any treatment setting), and amputations (in hospital). For foot ulcers, the coefficient in Table 5 is insignificant and close to zero. The univariate graphs in Figure 3 do not suggest strong time trends. In Table 5, the coefficient for any amputation is positive but insignificant, consistent with the null results for foot ulcers, of which only a small fraction lead to amputation.

IX. Limitations

A. Limitations

We study poor childless adults, with income < 100% FPL; the response of other persons to gaining insurance may be different. We study persons who chose to enroll in Medicaid. Medicaid expansion is exogenous, but the decision to enroll is not, and reflects patients' perceived need for health care. Results would likely be different for other eligible persons if they had been, hypothetically, enrolled automatically. We lack information on how many eligible persons did not enroll.

We faced a challenge in defining the treatment group, because diabetes is often underdiagnosed and underdiagnosis is especially likely for the uninsured. We responded by studying 2014-diabetic and 2018-diabetic subsamples separately.

We had a limited pre-treatment period, generally 3 years for outpatient visits but only 18 months for ED visits and hospitalizations, in which to assess pre-treatment trends. However, ratios of treated/control visit rates appear reasonably parallel.

We can balance the treated and control groups on age, sex, race/ethnicity, and area-SES, but cannot balance on health status, because observed status depends on receiving healthcare, and thus on treatment assignment. We also cannot balance on family status (children at home).

We study only Wisconsin. Garthwaite et al. (2019) report interstate heterogeneity in the effects of Medicaid expansion on ED visit and hospitalization rates. Zhao and Nianogo (2024) find heterogeneity in

[16] We see a similar catchup for ESRD. See Appendix Figure App-15. This implies that insured persons initiate dialysis sooner than uninsured persons, despite similar demographics. This could explain the finding in Swaminathan et al. (2018) of reduced one-year mortality for persons initiating dialysis after ACA expansion.

the effect of expansion on diabetes-related hospitalizations. However, the Wisconsin results presented here are consistent with our parallel study of Indiana (Owen et al., 2025).

Within Wisconsin, we rely on data from MCW, which operates in Milwaukee and Southeastern Wisconsin, a mostly urban/suburban area. Effects could be different in rural areas.

We lack data on healthcare received outside MCW. Some of the post-expansion rise in outpatient care could reflect newly insured persons switching to MCW from FQHCs or other providers. This should be a smaller concern for ED visits and hospitalizations. We offer reasons to believe this effect is small, but cannot directly measure it.

We can measure new diagnoses and medication prescriptions only within MCW; some patients may have been previously diagnosed or received prescriptions elsewhere.

Some Medicaid enrollment could start with a visit to the ED or the hospital, after which the patient receives retroactive coverage. We provide evidence, however, that retroactive coverage can explain only a fraction of the post-enrollment jump in healthcare use.

We follow defined treatment and control groups over a long time period, during which some persons may move in and out of Medicaid coverage or other insurance. However, we observe parallel trends for the fractions of treated and control persons who are Medicaid-enrolled during the treatment period. We do not know, during periods when persons were Medicaid ineligible, how many had private insurance, from employment or the health insurance exchanges.

For macrovascular events, we can only measure events for which patients seek care.

Our sample period includes the COVID-19 pandemic period. Especially in 2020, the pandemic strongly affected visit rates for both treated and control patients, but we find no evidence of important relative changes between the two groups.

X. Conclusion

Newly Medicaid-insured childless adults with diabetes used substantially more healthcare almost across the board: for outpatient visits; ED-to-admission visits; and hospitalizations, but not ED-to-discharge visits. After expansion, there was a near-term surge in ED-to-admission visits and all hospitalizations to well above control group levels. This surge lasted roughly 18 months; after which treatment and control group rates were similar. Newly insured persons with diabetes received substantially more chronic disease diagnoses; related medications; and tests of all types. We find evidence of health benefit for several intermediate outcomes (blood pressure, cholesterol levels, blood glucose), and suggestive evidence of improvement for longer-term outcomes (AMI, stroke).

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Table-1. Summary Statistics for Treatment and Control Groups

Table shows visit counts during sample period and sample characteristics (age as of January 1, 2014; SES observed in 2020) for the treatment and control groups, shown separately for 2014-diabetic (diagnosed by year-end 2014), 2018-diabetic (diagnosed by year-end 2018) and non-diabetic (not diagnosed by year-end 2018) subsamples. Statistics for control group are shown before and after reweighting using eBalance weights. Reweighting is done separately for 2014-diabetic, 2018-diabetic and non-diabetic subsamples. *t*-statistic is for two-sample difference in means. 2014-Diabetic Patients: $N_{Treated} = 794$; $N_{Control} = 868$. 2018-Diabetic Patients: $N_{Treated} = 1{,}679$; $N_{Control} = 1{,}600$. Non-diabetic Patients: $N_{Treated} = 8{,}969$; $N_{Control} = 9{,}968$.

Patients	**2014-Diabetic Subsample**				**2018-Diabetic Subsample**				**Non-diabetic Subsmple**			
	Treated	**Control**	**t-stat (pre-reweight)**	**Control reweighted**	**Treated**	**Control**	**t-stat (pre-reweight)**	**Control reweighted**	**Treated**	**Control**	**t-stat (pre-reweight)**	**Control reweighted**
Patient and visit counts												
Total patients	794	868	---	---	1,679	1,600	---	---	8,969	9,968	---	---
Total visits	54,627	72,969	---	68,513	99,952	113,411	---	122,021	189,000	237,838	---	218,160
Outpatient	46,499	63,008	---	60,482	84,954	96,925	---	106,479	159,789	205,237	---	190,660
ED	5,524	6,994	---	5,471	9,961	11,653	---	10,570	22,652	26,754	---	21,655
Hospitalization	2,604	2,967	---	2,560	5,037	4,833	---	4,972	6,559	5,847	---	5,809
Patient demographics												
Mean age	51.5256	47.0637	12.82	51.5248	51.4351	46.8645	18.54	51.4161	49.1686	44.2794	49.45	49.1577
Sex: % Male	0.5516	0.3629	7.86	0.5516	0.5605	0.3425	12.84	0.5596	0.5987	0.2750	47.56	0.5978
Race: % White	0.5038	0.4044	4.09	0.5038	0.5027	0.3781	7.23	0.5022	0.4927	0.4751	2.42	0.4925
Race: % Black	0.4194	0.4608	-1.70	0.4194	0.4127	0.4856	-4.20	0.4128	0.4319	0.4052	3.74	0.4318
Race % Hispanic	0.0290	0.0438	-3.45	0.0290	0.0560	0.0881	-3.57	0.0562	0.0187	0.0390	-6.51	0.0189
Race % Other	0.0479	0.0910	-1.60	0.0479	0.0286	0.0481	-2.92	0.0287	0.0566	0.0807	-8.32	0.0568
SDI quintile 1	0.1171	0.0922	1.66	0.1171	0.1126	0.0900	2.14	0.1125	0.1039	0.1067	-0.63	0.1039
SDI quintile 2	0.1612	0.1429	1.04	0.1612	0.1519	0.1175	2.88	0.1518	0.1254	0.1508	-5.04	0.1255
SDI quintile 3	0.1134	0.1094	0.25	0.1134	0.1340	0.1175	1.42	0.1340	0.1330	0.1392	-1.25	0.1330
SDI quintile 4	0.1499	0.1567	-0.38	0.1499	0.1531	0.1631	-0.79	0.1531	0.1480	0.1475	0.09	0.1480
SDI quintile 5	0.4484	0.4827	-1.40	0.4484	0.4401	0.4994	-3.40	0.4402	0.4803	0.4449	4.88	0.4802

Table 2. Quarterly Visit Rates

Columns (1)-(6) show coefficients on Post*Treated from DiD regressions, with patient and calendar quarter fixed effects, of indicated visit types on treatment group dummy interacted with Post (=1 during treatment period) and constant term. Column (7) is similar but outcome is regular care (2+ outpatient visits in last 4 quarters (current quarter plus three previous quarters). Sample is quarterly data over 1Q 2011 (outpatient visits), 1Q 2012 (regular care), or 3Q 2012 (ED visits, hospitalizations) through 2Q 2022. Table rows show results from separate regressions using 2014-diabetic subsample (**Panel A**); 2018-diabetic subsample (**Panel B**); and non-diabetic subsample (**Panel C**). Control group uses eBalance weights, computed separately for each subsample. Post-treatment increase (%) = (DiD coefficient *100/pre-treatment mean for indicated treatment group). Coefficients on constant term are suppressed. Standard errors clustered on patient in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% levels, respectively. Significant results, at 5% level or better, in **boldface**. 2018-Diabetic Subsample: $N_{Treated} = 1{,}679$; $N_{Control} = 1{,}600$. 2014-Diabetic Subsample: $N_{Treated} = 794$; $N_{Control} = 868$. Non-diabetic Subsample: $N_{Treated} = 8{,}969$; $N_{Control} = 9{,}968$.

		Visit Type						
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample		**Outpatient**	**ED**	**Hospital**	**ED-to-Discharge**	**ED-to-Admission**	**Direct Hospitalization**	**Regular Care**
A. Post*Treated for 2014-diabetic patients		**0.56225*** (0.11899)**	0.01808 (0.01786)	**0.03082*** (0.01159)**	-0.00017 (0.01223)	**0.01824** (0.00924)**	**0.01257** (0.00492)**	**0.13110** (0.01850)**
B. Post*Treated for 2018-diabetic patients		**0.49955*** (0.07790)**	0.01534 (0.01162)	**0.02762*** (0.00727)**	-0.00266 (0.00797)	**0.01800*** (0.00583)**	**0.00962*** (0.00305)**	**0.09997** (0.01254)**
C. Post*Treated for Non-diabetic patients		**0.21618*** (0.01791)**	**0.02314*** (0.00343)**	**0.01087*** (0.00150)**	**0.01614*** (0.00273)**	**0.00700*** (0.00124)**	**0.00387*** (0.00066)**	**0.05022*** (0.00414)**
Post-expansion increase (%)	2014-diabetic	59.7	10.2	37.6	-0.2	28.1	74.5	62.9
	2018-diabetic	89.3	14.3	63.0	-3.7	51.6	107.7	76.4
	Non-diabetic	97.2	46.6	101.3	39.3	82.0	177.1	84.6
2013Q1-2014Q1 means								
Treated (Control) patients, Group A		0.942 (2.068)	0.178 (0.191)	0.082 (0.108)	0.113 (0.114)	0.065 (0.077)	0.017 (0.031)	0.208 (0.400)
Treated (Control) patients, Group B		0.559 (1.462)	0.107 (0.131)	0.044 (0.068)	0.072 (0.082)	0.035 (0.049)	0.009 (0.019)	0.131 (0.289)
Treated (Control) patients, Group C		0.222 (0.494)	0.050 (0.066)	0.011 (0.017)	0.041 (0.054)	0.009 (0.012)	0.002 (0.005)	0.059 (0.132)
Patient-quarter observations								
Group A		76,184	64,813	64,813	64,813	64,813	64,813	69,784
Group B		150,330	127,871	127,871	127,871	127,871	127,871	137,678
Group C		865,462	738,435	738,435	738,435	738,435	738,435	794,922
R^2 (within)								
Group A		0.353	0.227	0.137	0.204	0.142	0.064	0.416
Group B		0.329	0.237	0.119	0.240	0.127	0.058	0.391
Group C		0.252	0.133	0.069	0.131	0.071	0.042	0.352

Table 3. Quarterly New Diagnosis and Medication Rates

Sample and regressions are same as Table 2, except for outcomes. Sample is quarterly data for indicated groups over 3Q2011-2Q2022. Columns (1)-(5) show results from DiD regressions of first (incident) diagnosis for indicated conditions and columns (6)-(8) are for first prescription of medication of indicated type. Table rows show results from separate regressions using 2014-diabetic subsample (**Panel A**); 2018-diabetic subsample (**Panel B**); and non-diabetic subsample (**Panel C)** Control group use eBalance weights, computed separately for each subsample. Post-treatment increase (%) = (DiD coefficient *100/ pre-treatment mean for indicated treatment group). Prevalence ratio is treated/control ratio for cumulative incidence of first diagnosis through 1Q 2014. Coefficients on constant term are suppressed. Standard errors clustered on patient in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% level, respectively. Significant results, at 5% level or better, in **boldface**.

		New Diagnoses				New Medications		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Neuropathy or Retinopathy	**Peripheral Vascular**	**CKD or Nephropathy**	**Proteinuria**	**Ischemic Heart Disease**	**Diabetes**	**Blood Pressure**	**Any Statin**
A. Post*Treated with Diabetes, 2014 group	**0.00881*** (0.00252)**	0.00075 (0.00130)	**0.00768*** (0.00272)**	0.00114 (0.00105)	0.00137 (0.00188)	**0.01224*** (0.00300)**	**0.00778*** (0.00295)**	**0.00694*** (0.00252)**
B. Post*Treated with Diabetes, 2018 group	**0.00695*** (0.00157)**	**0.00218** (0.00087)**	**0.00615*** (0.00173)**	0.00073 (0.00062)	0.00233* (0.00119)	**0.01096*** (0.00195)**	**0.00897*** (0.00200)**	**0.00638*** (0.00160)**
C. Post*Treated without Diabetes	**0.00245*** (0.00038)**	**0.00054** (0.00022)**	**0.00143*** (0.00038)**	**0.00031** (0.00014)**	**0.00102*** (0.00030)**	n.m.	**0.00400*** (0.00057)**	**0.00177*** (0.00038)**
Post-expansion increase (%)								
2014-Diabetic	50.7	12.9	53.5	30.2	14.7	27.5	19.5	35.8
2018-Diabetic	73.9	73.2	78.2	40.9	47.7	51.4	37.5	63.0
Non-diabetic	124.9	173.0	86.7	231.7	138.6	n.m.	48.9	101.8
2013Q1-2014Q1 means								
Treated (Control) patients, Group A	0.0174 (0.0243)	0.0058 (0.0043)	0.0144 (0.0220)	0.0038 (0.0022)	0.0093 (0.0092)	0.045 (0.054)	0.040 (0.050)	0.019 (0.029)
Treated (Control) patients, Group B	0.0094 (0.0153)	0.0030 (0.0031)	0.0079 (0.0133)	0.0018 (0.0014)	0.0049 (0.0063)	0.021 (0.031)	0.024 (0.033)	0.010 (0.019)
Treated (Control) patients, Group C	0.0020 (0.0046)	0.0003 (0.0007)	0.0017 (0.0022)	0.0001 (0.0002)	0.0007 (0.0014)	n.m.	0.008 (0.010)	0.002 (0.003)
Prevalence ratios (1Q 2014)								
With Diabetes, Group A	0.638	0.904	0.653	0.797	0.875	0.725	0.770	0.671
With Diabetes, Group B	0.548	0.668	0.554	0.660	0.728	0.610	0.679	0.558
Without Diabetes (Group C)	0.380	0.384	0.535	0.295	0.445	n.m.	0.582	0.414
Patient-quarter observations						72,984	72,984	72,984
Group A	72,984	72,984	72,984	72,984	72,984	144,004	144,004	144,004
Group B	144,004	144,004	144,004	144,004	144,004	n.m.	830,192	830,192
Group C	830,192	830,192	830,192	830,192	830,192	0.031	0.029	0.020
R^2 (within)						0.013	0.013	0.014
Group A	0.021	0.019	0.019	0.020	0.019	n.m.	0.016	0.020
Group B	0.019	0.019	0.019	0.021	0.018	0.725	0.770	0.671
Group C	0.022	0.022	0.024	0.023	0.021	0.610	0.679	0.558

Table 4. Intermediate Health Outcomes

Sample is limited to patients with 1+ tests of indicated type both before and after expansion. Regressions are similar to Table 2. Sample period measuring outcomes is 3Q2011-2Q2022. **Panel 1**. Full sample. **Panel 2**. treatment group is limited to persons with first blood pressure medication prescription (cols. (1)-(2)), statin prescription (col. (3)), or diabetes medication prescription (cols. (4)-(7)) during treatment period, and for persons with first prescription in treatment period, excludes treatment period observations prior to quarter with first medication. **Both panels**. Control group means use eBalance weights, , computed separately for each of the three subsamples. Sample size is indicated in the table. Standard errors clustered on patient in parentheses. Regressions include constant term, coefficient is suppressed. *, **, *** indicates significance at the 10%, 5%, and 1% level, respectively. Significant results, at 5% level or better, in **boldface**.

	(1)	(2)	(3)	(4)	(5)
Outcome	**Systolic Blood Pressure**	**Diastolic Blood Pressure**	**Total Cholesterol Levels**	**Glucose Levels**	**HbA1c levels**
Panel 1. Full Sample					
A. Post*Treated w Diabetes 2014	**-1.44836**** **(0.72750)**	-0.69367* (0.41580)	-5.14558* (2.93270)	**-9.04657**** **(3.68842)**	**-0.26114**** **(0.12594)**
B. Post*Treated w Diabetes 2018	-0.51298 (0.64392)	-0.16875 (0.36955)	**-5.39110**** **(2.72455)**	**-7.21259**** **(3.08899)**	-0.23022* (0.12061)
C. Post*Treated without Diabetes	-0.48390 (0.44028)	**-0.66408***** **(0.25503)**	-2.58496 (2.36188)	0.47306 (0.84056)	n.m.
Panel 2: Limited Treatment Group					
A. Post*Treated w Diabetes 2014	**-1.67595**** **(0.79818)**	**-0.89874**** **(0.44462)**	**-9.91760***** **(3.29335)**	**-10.34483**** **(4.08545)**	-0.25136* (0.13241)
B. Post*Treated w Diabetes 2018	-0.72163 (0.70175)	-0.34930 (0.39580)	**-10.49386***** **(3.11746)**	**-8.33012**** **(3.50776)**	-0.22049* (0.12759)
C. Post*Treated without Diabetes	**-1.58496***** **(0.52945)**	**-1.14760***** **(0.30362)**	**-13.47026***** **(3.38220)**	n.m.	n.m.
Post-expansion change (%) (Panel 2) Diabetic-2014; Diabetic 2018 || Non-Diabetic	-1.2 ; -0.5 || -1.2	-1.1 ; -0.4 || -1.4	-5.3 ; -5.5 || -6.6	-5.8; -5.0 || n.m.	-3.1 ; -2.7 || n.m.
2013Q1-2014Q1 means (Panel 2)					
Diabetic-2014: Treated (Control)	134.87 (130.30)	79.83 (77.52)	188.51 (175.24)	177.91 (155.63)	8.14 (7.43)
Diabetic-2018: Treated (Control)	134.70 (131.12)	80.05 (78.23)	190.64 (177.85)	166.40 (148.33)	8.03 (7.35)
Non-Diabetic: Treated (Control)	131.54 (127.78)	80.24 (78.30)	205.24 (196.23)	n.m.	n.m.
Diabetic-2014: Treatment (Control) 1A || 2A	550 (682) || 431 (682)	550 (682) || 431 (682)	256 (323) || 197 (323)	499 (627) || 404 (627)	307 (393) || 271 (393)
Diabetic-2018: Treatment (Control) 1B || 2B	775 (926) || 620 (926)	775 (926) || 620 (926)	306 (404) || 236 (404)	660 (802) || 533 (802)	349 (439) || 305 (439)
Patient-quarter obs.:					
Diabetic 2014: Panel 1 || 2	23,120 || 21,531	23,120 || 21,531	4,345 || 3,921	16,364 || 15,126	8,698 || 8,297
Diabetic 2018: Panel 1 || 2	31,892 || 29,527	31,892 || 29,527	5,244 || 4,692	20,902 || 18,864	9,470 || 8,970
Non-Diabetic: Panel 1 || 2	75,860 || 63,832	75,860 || 63,832	6,127 || 4,863	26,648 || n.m.	n.m. || n.m.
R^2 (within)					
Panel 1 : A || B || C	0.43 || 0.42 || 0.48	0.42 || 0.41 || 0.45	0.60 || 0.62 || 0.64	0.46 || 0.49 || 0.45	0.56 || 0.57 || n.m.
Panel 2 : A || B || C	0.43 || 0.42 || 0.47	0.42 || 0.41 || 0.45	0.61 || 0.62 || 0.64	0.45 || 0.48 || n.m.	0.55 || 0.56 || n.m.

Table 5. Longer Term Outcomes for Diabetic Patients

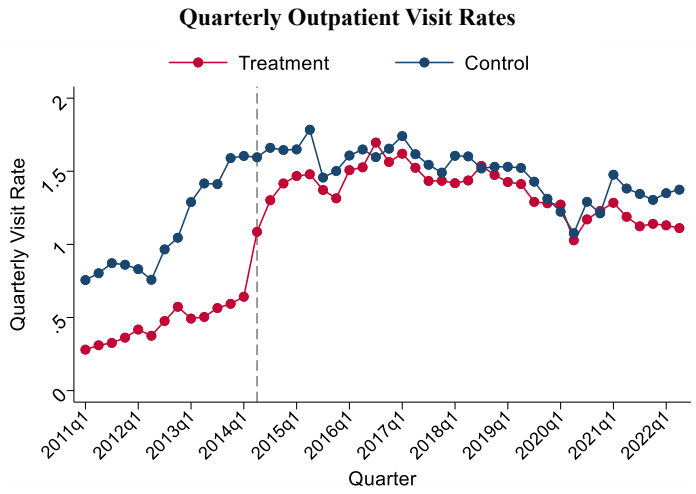
Sample is same as Table 2, and regressions are similar, but use semiannual instead of quarterly data. Advanced kidney disease is incident diagnosis of chronic kidney disease (CKD) stage 4 or 5. Diabetic Foot Ulcer, Advanced kidney disease and kidney failure (end stage renal disease, ESRD) are incident diagnoses (in any treatment setting). Sample period for columns 1 through 3 is April 2011-March 2022. AMI and stroke are limited to incident (first-time) hospitalizations, with AMI or stroke as primary discharge diagnosis. Amputation is limited to incident events. Sample period for hospitalizations begins 4Q 2012 due to largely missing MCW data on hospitalizations before then. **All columns**: Control group means use eBalance weights. Coefficient on constant term is suppressed. Standard errors clustered on patient in parentheses. *, **, *** indicates significance at the 10%, 5%, and 1% level, respectively. Significant results, at 5% level or better, in **boldface** (omitted for constant term). n.m. = not meaningful.

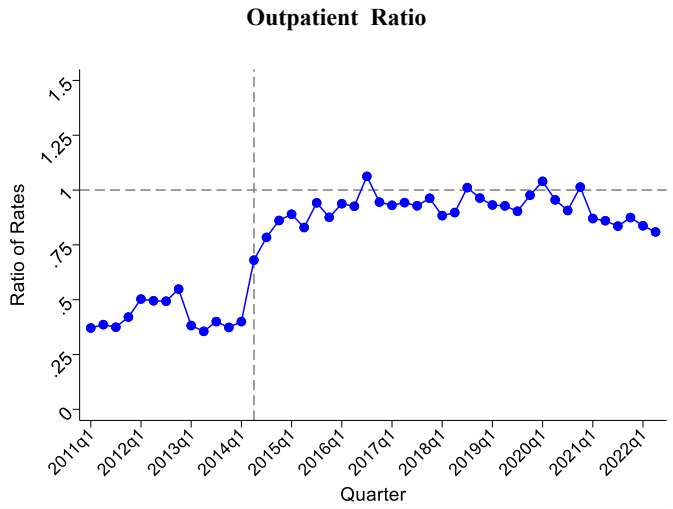
	(5)	(6)	(1)	(2)	(3)	(4)
Outcome	**AMI**	**Stroke**	**Advanced CKD**	**ESRD**	**Diabetic Foot Ulcer**	**Any amputation**
A. Post*Treated w Diabetes 2014	-0.00309 (0.00195)	**0.00572**** **(0.00269)**	**0.00298**** **(0.00150)**	**0.00528***** **(0.00127)**	-0.00050 (0.00154)	0.00124 (0.00109)
B. Post*Treated w Diabetes 2018	-0.00039 (0.00106)	**0.00368**** **(0.00149)**	**0.00254**** **(0.00112)**	**0.00403***** **(0.00098)**	0.00006 (0.00092)	0.00064 (0.00062)
C. Post*Treated without Diabetes	-0.00013 (0.00017)	**0.00074**** **(0.00037)**	**0.00053**** **(0.00021)**	**0.00060***** **(0.00020)**	n.m.	n.m.
Post-expansion change (%): 2014-Diabetic; 2018-Diabetic	-40.9 ; -10.9	113.5; 154.5	39.4 ; 53.3	139.7 ; 169.2	-9.9; 2.5	98.5 ; 107.5
Post-expansion change (%): Non-Diabetic	-116.6	83.0	237.7	538.1	n.m.	n.m.
Pre-expansion total events						
Diabetic 2014: Treatment || Control ; Control Reweighted	12 || 7; 5.7	8 || 18; 17.8	12 || 34; 20.5	6 || 32; 19.6	19 || 14; 13.4	2 || 4; 3.5
Diabetic 2018: Treatment || Control ; Control Reweighted	12 || 7; 6.96	9 || 19; 20.9	16 || 43; 35.5	7 || 39; 32.9	21 || 14; 15.6	2 || 4; 3.9
Non-Diabetic: Treatment || Control ; Control Reweighted	7 || 4; 2.3	16 ||21; 27.2	7 || 28 || 31.9	3 || 27; 32.1	n.m.	n.m.
Post-expansion total events						
Diabetic 2014: Treatment || Control ; Control Reweighted	31 ||37; 36.7	53 || 32; 32.7	72 || 64; 57.1	63 || 46; 32.4	52 || 41; 43.1	21 || 15; 13.4
Diabetic 2018: Treatment || Control ; Control Reweighted	81 ||60; 64.6	121||79;85.4	147||121; 131.4	122 || 87; 83.2	104 || 69; 87.7	42 || 31; 34.9
Non-Diabetic: Treatment || Control ; Control Reweighted	75 ||38; 68.7	147 || 86;101	104 || 80; 94.7	85 || 67; 77.3	n.m.	n.m.
Pre-expansion means (2013Q4-2014Q1)						
Diabetic 2014: Treatment	0.00756	0.00504	0.00756	0.00378	0.00504	0.00126
Diabetic 2014: Control	0.00557	0.00932	0.00355	0.00239	0.00037	0.00158
Diabetic 2018: Treatment	0.00357	0.00238	0.00476	0.00238	0.00238	0.00060
Diabetic 2018: Control	0.00331	0.00521	0.00296	0.00165	0.00019	0.00077
Non-Diabetic: Treatment	0.00011	0.00089	0.00022	0.00011	n.m.	0.00011
Non-Diabetic: Control	0.00006	0.00024	0.00072	0.00071	n.m.	0.00010
Patient-half year observations						
Panel A	31,578	31,578	36,492	36,492	36,492	31,578
Panel B	62,301	62,301	72,002	72,002	72,002	62,301
Panel C	359,803	359,803	415,096	415,096	415,096	359,803
R^2 (within)						
Panel A	0.051	0.052	0.042	0.043	0.043	0.052
Panel B	0.051	0.050	0.042	0.043	0.043	0.052
Panel C	0.052	0.052	0.045	0.045	n.m.	0.053

Figure 1. Univariate Visit Rates

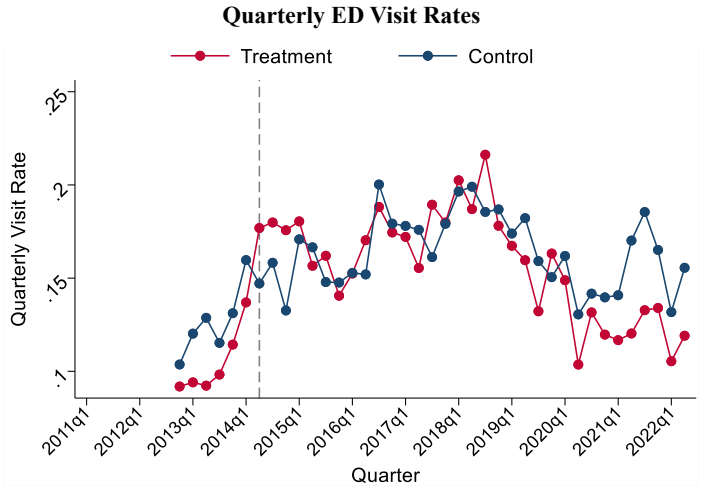
Figure shows visit rates by calendar quarter for the 2018-diabetic subsample. Sample is quarterly data over 1Q 2011 (outpatient visits), or 3Q 2012 (ED visits, hospitalizations) through 2Q 2022. **Panel A**. Outpatient visits. **Panel B**. ED visits. **Panel C**. ED-to-discharge visits. **Panel D**. ED-to-admission visits. **Panel E**. All hospitalizations. **Panel F**. Direct hospitalizations (not from ED). **Panel G.** Regular care. **All panels**. Left-hand graphs show univariate rates for treatment and control groups. Right-hand graphs show treatment/control ratio of univariate rates. Control group uses eBalance weights. Vertical line indicates start of expansion period.

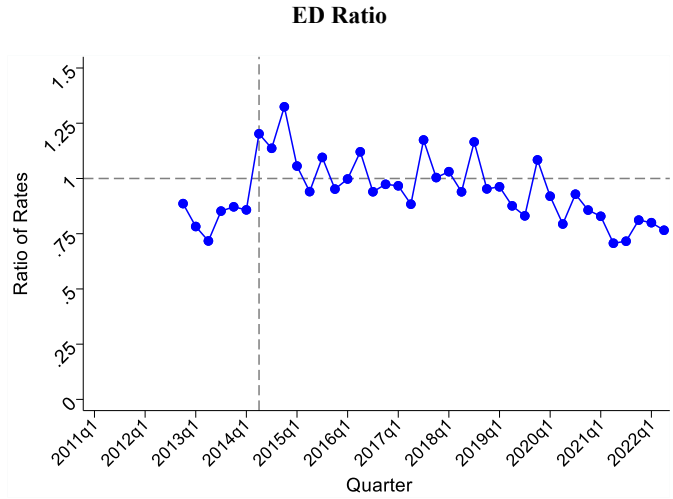
Panel A: Outpatient Visits



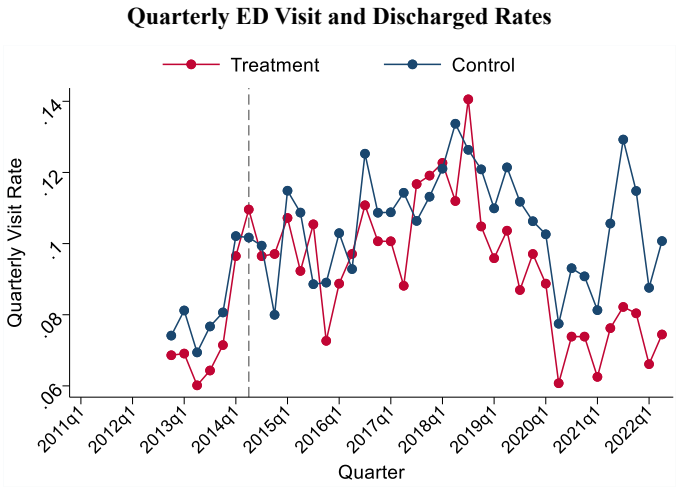


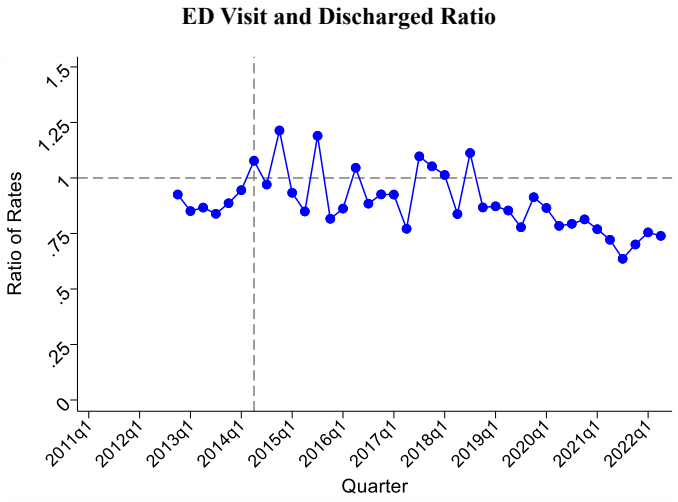
Panel B: All ED Visits



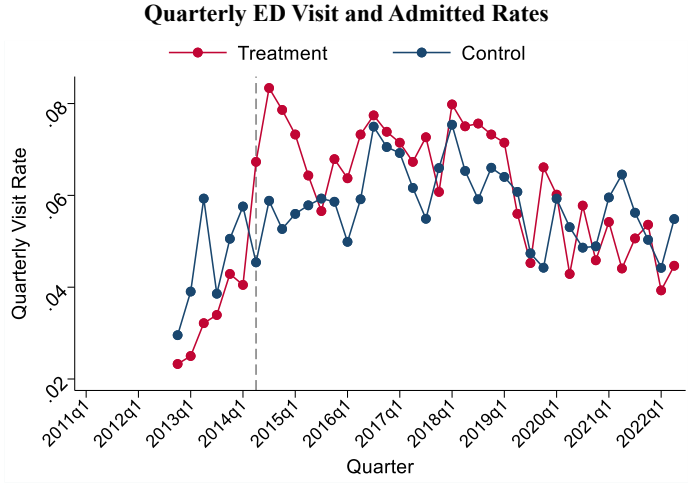


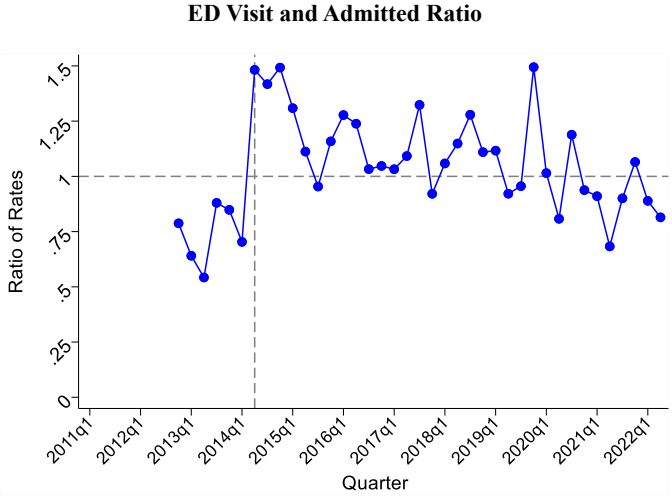
Panel C: ED-to-Discharge Visits



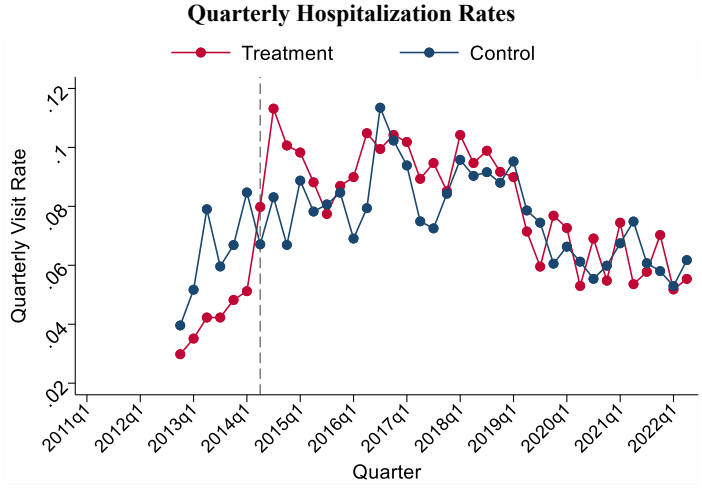


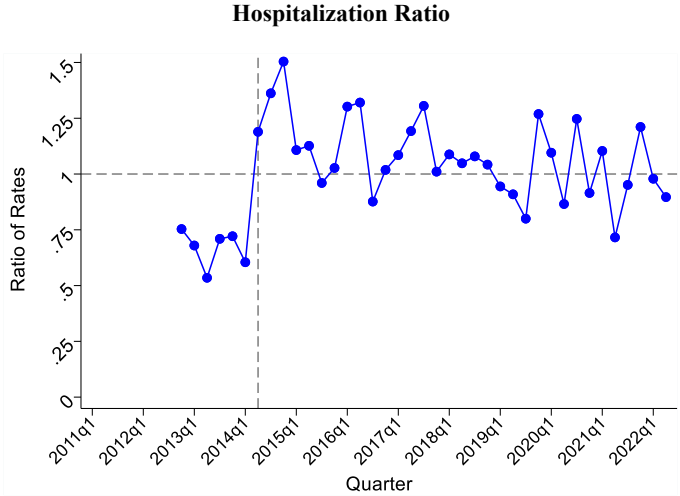
Panel D: ED-to-Admission Visits



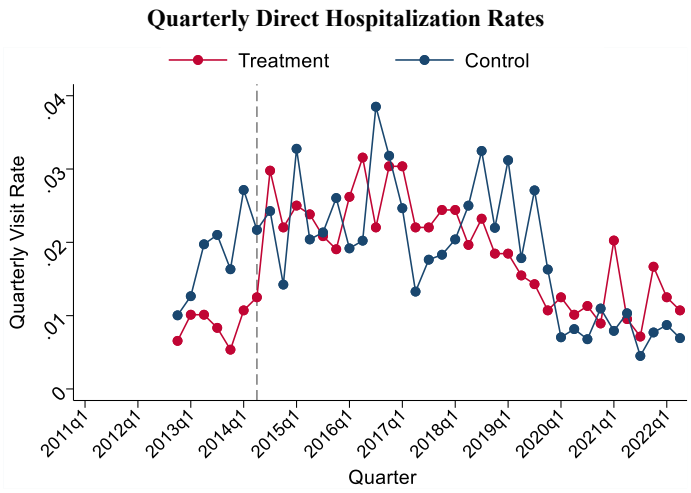


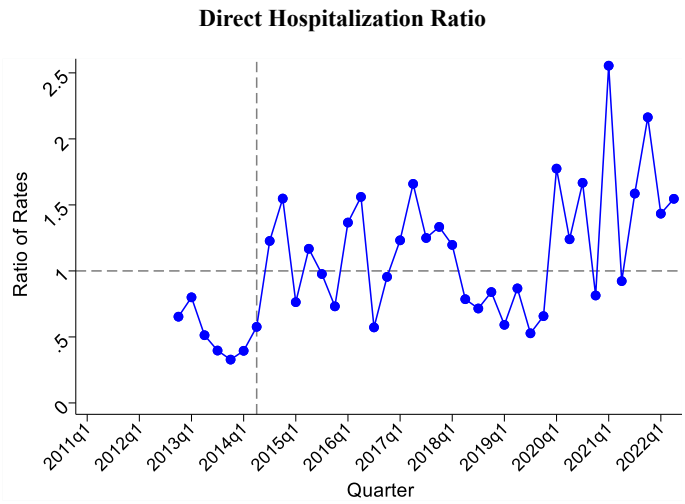
Panel E: All Hospitalizations



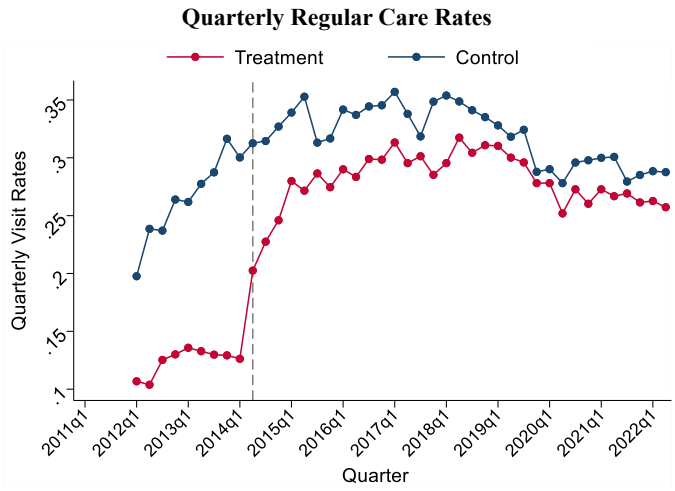


Panel F: Direct Hospitalizations, not from ED





Panel G: Regular Care



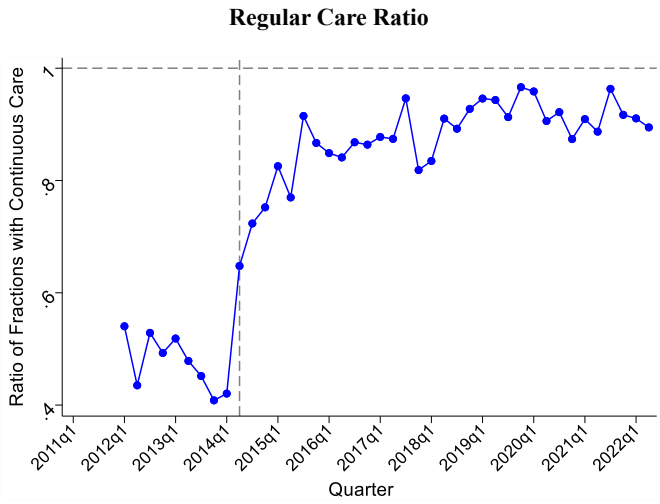
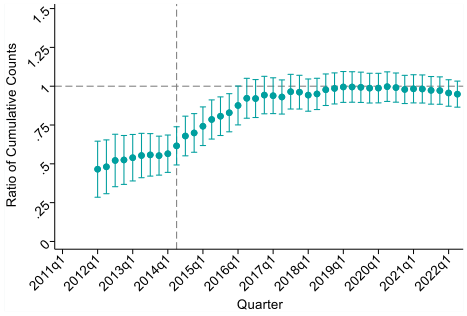


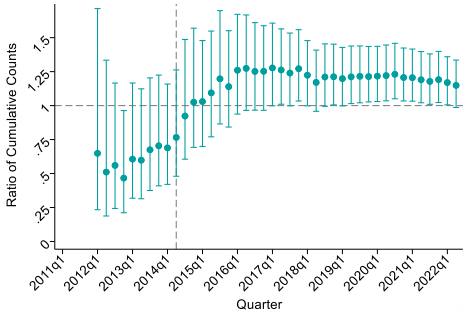
Figure 2. New Diagnoses and Prescriptions, Cumulative Ratio

Figure shows cumulative ratio of proportion of treated patients to proportion of control patients, for 2018-diabetic subsample, with indicated diagnoses or prescriptions and 95% CIs. **Ratio=** $\frac{\textit{treatment group cumulative counts}}{\textit{reweighted control group cumulative counts}}$. . Vertical line indicates start of expansion period. Control group uses eBalance weights.

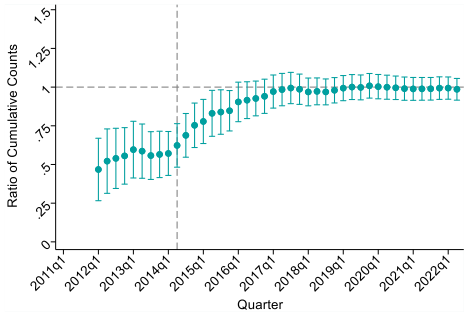
Microvascular Disease



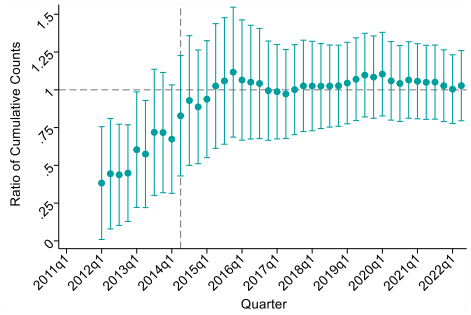
Peripheral Vascular Disease



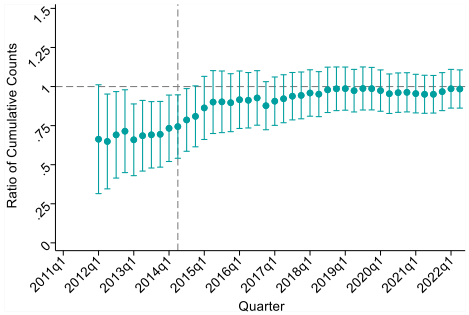
Kidney Disease (CKD any stage, or nephropathy)



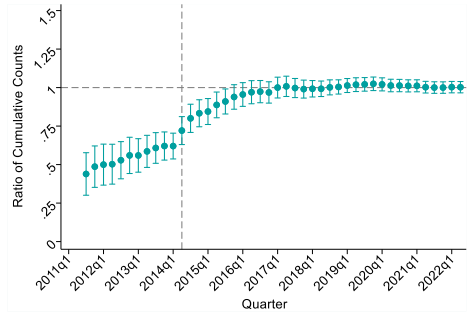
Proteinuria



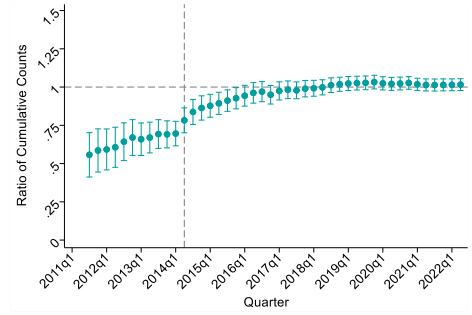
Ischemic Heart Disease



Diabetes Medication Prescription



Blood Pressure Medication Prescription



Statin Prescription

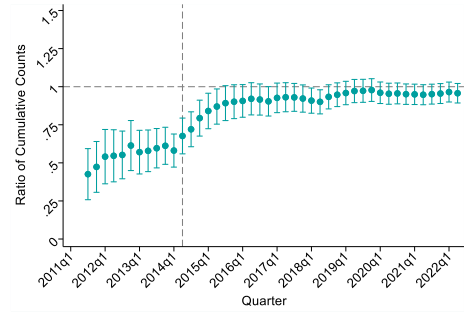
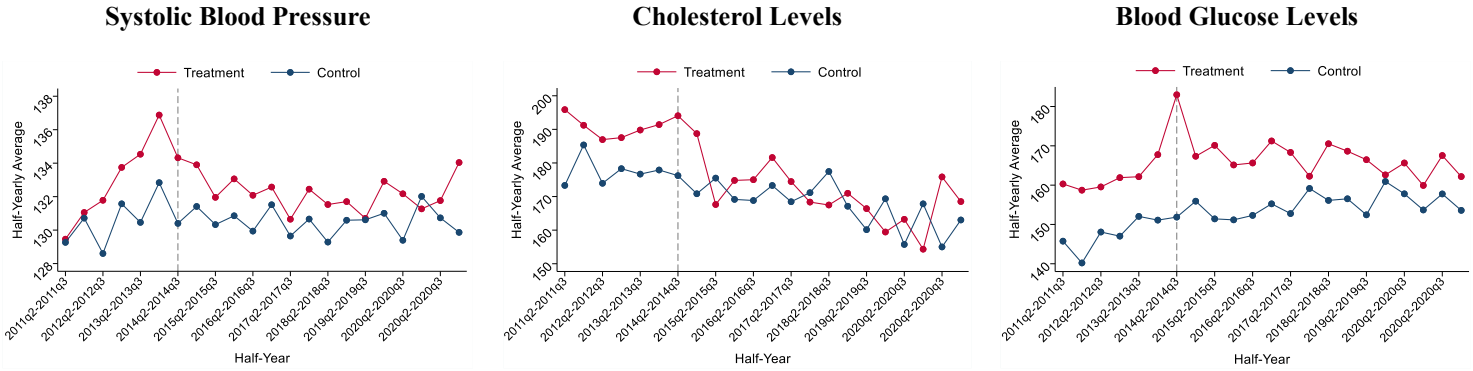


Figure 3. Health Outcomes

Figure provides univariate rates of selected indicated intermediate (Panel A) and longer-term (Panel B) health outcomes for 2018-diabetic subsample over 2Q 2011-1Q 2022 (for hospital-based outcomes, 4Q 2012-1Q 2022), using semiannual data. Vertical line indicates start of expansion period. Control group uses eBalance weights. **Panel A**. Treatment group is limited to persons with 1+ prescriptions during treatment period for blood pressure medication (blood pressure graph) statin (cholesterol graph), or diabetes medication (blood glucose graph). For treated persons, we exclude treatment period observations prior to quarter with first medication.

Panel A. Intermediate Health Outcomes



Panel B. Longer-Term Health Outcomes