MANAGEMENT AND BUDGET

Evaluation of ECHO Programs in Minnesota

Impact of Project ECHO on Opioid Use Disorder Treatment

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Table of contents

Executive summary

In recent years, Minnesota has invested in meeting the pressing need for services to prevent opioid use disorder (OUD) and to treat and assist in recovery for individuals who experience it. One investment was in Project ECHO, a tele-mentoring program that, in Minnesota, has focused on expanding capacity for treating OUD in primary care settings. Each ECHO program consists of a "hub" where specialists work in an interdisciplinary team and "spokes" (typically providers in rural or underserved areas, or primary care providers who do not have specialized training in treating a particular illness) who connect to the hub through regular videoconferences for didactic and case-based learning.

This study assesses the causal impact of the two largest and longest-persisting ECHOs in Minnesota, Hennepin Healthcare and Catholic Health Initiatives (CHI) – St. Gabriel's, on primary care provider prescribing behaviors and patient outcomes. The primary goal of these ECHOs was to teach providers to appropriately provide medications for opioid use disorder (MOUD), especially buprenorphine. MOUD are evidence-based treatments for substance use disorders that, when paired with counseling, are the most effective way to treat OUD and reduce overdoses (SAMHSA, 2021).

This novel analysis used Medicaid data to compare, over the course of 18 months, providers who participated in ECHO with like providers who did not participate. We also examined the outcomes of these providers' patients. Overall, the impact of the two Minnesota ECHO hubs on MOUD prescribing was very promising. We find:

- Providers who attended one or more ECHO sessions were more likely to provide buprenorphine to their patients with OUD than comparison providers at 6, 12, and 18 months after ECHO. For every 100 OUD patients that providers saw per month, ECHO providers prescribed buprenorphine for 6.5 more patients than comparison providers.
    - Providers who attended six or more ECHO sessions had the greatest growth in prescribing MOUD, suggesting that strong participation is important for seeing benefits from ECHO.
- Patients with a history of OUD who saw an ECHO-trained provider were more likely to receive buprenorphine prescriptions 6, 12 and 18 months after their initial visit with that provider (a 4.2 percentage point net increase at 18 months), relative to patients who saw a comparison provider.
- Both ECHO and comparison providers substantially decreased the amount of opioid analgesics they prescribed during the study period. The decreases were similar in scale for both groups.
- There was no difference in OUD patients' overall risk of receiving medical care for a nonfatal opioid overdose because of seeing an ECHO-trained provider.

The positive impacts on MOUD prescribing are meaningful because ECHOs are currently reliant on non-permanent federal and state funding. Given this evidence and other prior research, we believe ECHOs can be part of a robust continuum of care—prevention, early intervention, treatment, and recovery services —that mitigates the harm of opioid addiction.

Acknowledgments

We would like to thank our partners at the two ECHO hubs for their time, expertise, feedback, and assistance with data collection. At Hennepin Healthcare: Dr. Brian Grahan, Dr. Gavin Bart, and Beth Ryan; and at MEnD Recovery Services (formerly at CHI-St. Gabriel's): Dr. Heather Bell, Dr. Kurt Devine, and Katie Stangl. We also greatly appreciate our colleagues Ellie Garrett, Heather Petermann, Titi Adeniyi, and Monica Patrin at the Department of Human Services and Catherine Diamond and Dana Farley at the Department of Health for their support and comments on study design and data analysis. Many thanks to Dr. Yiliang Zhu and Jessica Reno at the University of New Mexico for their advice on all aspects of the study and their encouragement. Finally, our teammates at MMB, Dr. Pete Bernardy, Ian Williamson, and Cody Tuttle, have been instrumental in guiding this work.

About the team

MMB's Impact Evaluation unit is a team of data and social scientists that rigorously evaluates state investments and policies to find what works and what does not. The legislature established the team in 2019 to assess the impact of the state's response to the opioid epidemic and to study human services grants, broadly. We prioritize working with agencies and partners to identify and answer pressing questions, and creating evidence that is rigorous, relevant, and used by policymakers.

For more information or to learn about current and future areas of study, please visit https://osf.io/mzebh/ or contact ResultsFirstMN@state.mn.us.

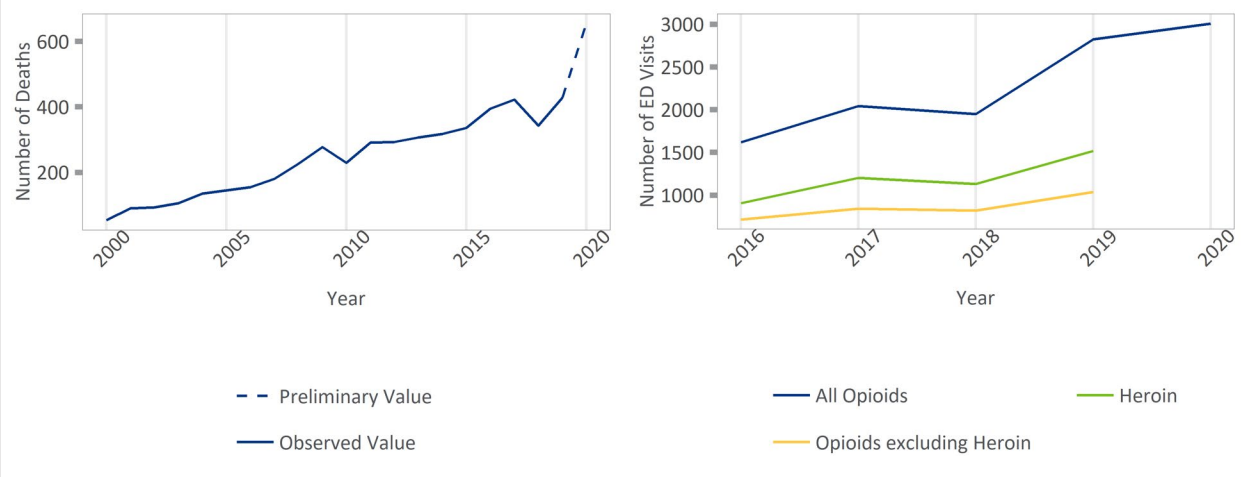
Introduction

The opioid epidemic in Minnesota

Over the past two decades, the opioid epidemic has rapidly emerged as one the most pressing public health emergencies in the United States. Since the late 1990s, opioid-related deaths have increased, first slowly and then precipitously. This increase in the rate of opioid-related deaths is associated with a shift from prescription opioid use to misuse of stronger opioids such as heroin and fentanyl (Planalp & Hest, 2020). By 2020, the United States hit an all-time high with 67,574 reported opioid-related overdose deaths, more than the deaths from car crashes or suicide (CDC, 2021).

Minnesota has mirrored this national trend. Fatal overdoses increased from 54 in 2000, to 229 in 2010, to 654 in 2020, in total taking 5,475 lives since the turn of the millennium. The harm has extended beyond deaths, with more than 11,000 visits to emergency departments in the state for nonfatal opioid overdoses between 2016 and 2020 (see Figure 1; Minnesota Department of Health, 2021). The aggregate growth in mortality and morbidity has been accompanied by troubling disparities across populations of color and indigenous communities; black Minnesotans are twice as likely to die from overdose as white Minnesotans, and American Indians are seven times more likely to die (DeLaquil, n.d.). In this crisis, the state has sought to invest in a mixture of promising and proven initiatives to blunt the impacts to our community.

Figure 1. Number of opioid overdose deaths and emergency department visits for nonfatal opioid overdoses in Minnesota



Note. Drug overdose death categories are non-exclusive. Data does not reflect nonfatal overdoses that occur but are not treated in an emergency department.

Treating opioid use disorder

To mitigate the harm of opioid use disorder (OUD), a range of prevention, treatment, and recovery resources are needed. One highly effective component on a continuum of care are medications for opioid use disorder (MOUD), offered in combination with counseling (SAMHSA, 2021). There are three

primary medications for OUD: methadone, naltrexone, and buprenorphine, each of which has its own barriers to access. Methadone can only be distributed by certified opioid treatment programs. There are only 16 programs in Minnesota, with four outside the Twin Cities metro area, making access a challenge.

Naltrexone is more widely available because it can be prescribed by primary care providers. While effective for treating OUD and preventing the use of opiates (Syed & Keating, 2013), treatment with naltrexone carries several critical drawbacks. Patients must first go through detoxification before initiating naltrexone, which is a barrier for many people who suffer from severe addiction. The most effective form of naltrexone for OUD is through monthly injections, requiring patients to frequent a clinic that may be far from where they live (Morgan et al., 2018). Finally, while treatment with both methadone and buprenorphine has been shown to decrease mortality for people addicted to opioids (Sordo et al., 2017), naltrexone has not yet been shown to have the same mortality benefit (Larochelle et al., 2018).

Hennepin Health's ECHO hub

Hennepin Healthcare is based in Minneapolis. It is a large, integrated health care system that includes a trauma center, a hospital, a large psychiatric program, and a network of primary care clinics.

Hennepin's ECHO team includes two physicians in the Addiction Medicine department, a psychiatrist, and a psychologist. During the study period, the hub offered weekly one-hour sessions every Thursday that were open to anyone who wanted to attend.

Hennepin's ECHO program was designed to help providers integrate appropriate buprenorphine prescribing practices into their primary care settings.

Like naltrexone, buprenorphine is effective for treating OUD (Washington State Institute of Public Policy, 2016) and can be prescribed by primary care providers, making it more accessible than methadone. Unlike injectable naltrexone, buprenorphine can be taken orally at home as part of a daily regimen, mitigating the need for patients to travel to a clinic for injections. Given challenges with naltrexone induction and non-adherence (Binswanger & Glanz, 2018; Lee et al., 2018) and scarcity of programs that can dispense methadone, buprenorphine is often the preferred medication for MOUD.

Despite this preference, provider capacity is limited; only about 4.6 percent of prescribers in Minnesota were able to prescribe buprenorphine as of June 2020. This limited use of buprenorphine arose in part because providers were, until recently, required to complete a Drug Abuse Treatment Act waiver training (DATA-waiver) to prescribe buprenorphine.[1] To receive a waiver, physicians had to complete 8 hours of training and other qualified practitioners (e.g., physician assistants) had to complete 24 hours of training. In addition, providers were limited in the number of patients they could

[1] In April 2021, the DEA removed restrictions, allowing providers to prescribe buprenorphine without DATA-waiver training, if they prescribed to no more than 30 patients concurrently. For more, see 86 Federal Register 22439.

prescribe to concurrently. Even after receiving a DATA-waiver, many providers report not actively prescribing buprenorphine because of low reimbursement rates, lack of time for additional patients, concerns about diversion, and lack of training (Huhn & Dunn, 2017; Molfenter et al., 2019).

The DATA-waiver training requirements were recently relaxed by the federal government, removing one barrier for some providers who treat up to 30 patients with buprenorphine. However, the training is still mandatory for providers who wish to treat more than 30 patients. Further, training and support may still be needed for primary care providers to successfully prescribe buprenorphine. There is, therefore, considerable interest in interventions to increase access and capacity for providing MOUD.

CHI-St. Gabriel's ECHO hub

CHI-St. Gabriel's, located in Little Falls, Minnesota, is a multi-facility health care organization that includes a hospital, clinic sites, a home health and hospice agency, and a senior housing complex.

The CHI-St. Gabriel's ECHO team includes two physicians who are board certified in addiction medicine. During the study period, the hub offered weekly one-hour sessions every Wednesday that anyone could attend.

CHI-St. Gabriel's program focused on educating community providers on opioid and controlled substance topics, pain management, and establishing buprenorphine clinics in primary care settings.

Project ECHO

Project ECHO (Extension for Community Health Outcomes) is a tele-mentoring program that focuses on expanding capacity for treating specific health conditions. Developed in 2003 to train providers in treating Hepatitis C, ECHO has since expanded to a variety of other chronic conditions, including substance use disorders like OUD. Each ECHO program consists of a "hub" where specialists work in an interdisciplinary team and primary care teams, or "spokes," who connect to the hub through regular videoconferences for didactic and case-based learning. This arrangement allows for learning and guided practice, especially for providers working in rural, unserved, or remote area, and primary care providers who do not have specialized training in treating a particular illness (Komaromy et al., 2016).

ECHO programs in Minnesota

Beginning in 2017, Minnesota's Department of Human Services began funding three OUD-focused ECHO hubs as part of the state's opioid epidemic response: Hennepin Healthcare, CHI-St. Gabriel's[2], and Wayside Recovery Center. A fourth hub called Midwest Tribal was added in 2020 and a fifth, Stratis Health, was added in 2021.

[2] In early 2021, the ECHO hub team members transitioned to MEnD Recovery Services, which is where the ECHO is currently housed. The ECHO program is no longer associated with CHI-St. Gabriel's Health.

Each hub has a different approach to addressing opioid misuse and addiction. Wayside Recovery Center's ECHO emphasizes using peer recovery to treat OUD in women and mothers. Midwest Tribal focuses on culturally responsive treatment for substance use disorders, specifically for American Indian populations. Hennepin Healthcare and CHI-St. Gabriel's primary aim is to increase MOUD capacity among primary care providers. They also provide education on safe opioid analgesic prescribing practices, tapering high-dose opioid prescriptions, transitioning patients who are misusing prescription opioids to MOUD, and other substances that can lead to overdose and death.

Evaluating the impacts of ECHO programs in Minnesota

A small number of studies have examined the effect of ECHO or ECHO-like programs for treating substance use disorder. However, in our literature review, we found that few of these studies used comparison groups to plausibly isolate causal effects. Instead, most studies use a pre-post design without a comparison group. The studies that were designed to test causal effects do provide some evidence that ECHO may increase providers' clinical knowledge (Anderson et al., 2017), reduce opioid prescribing (Katzman et al., 2019), and increase buprenorphine prescribing (Gadomski et al., 2020), compared to non-ECHO trained providers. To our knowledge, there are no causal studies that investigate both prescriber and patient outcomes, and no studies that are focused on Minnesota's population.

This study assesses the causal impact of Hennepin Healthcare and CHI-St. Gabriel's ECHO programs on increasing access to MOUD and reducing opioid prescriptions and nonfatal overdoses. We selected these two hubs for the evaluation because they are the largest and longest-running ECHOs, their approaches are similar, and they emphasize increasing MOUD capacity. During the study period, both hubs offered weekly one-hour virtual sessions that included a didactic presentation from an expert in the field and an opportunity for providers to share a specific patient case (see the sidebars for additional details).

To evaluate the impacts of these two ECHO programs, we conducted an observational study that sought to answer two key research questions:

1. Among Medicaid enrolled primary care providers who are eligible to obtain a DATA-waiver to prescribe buprenorphine, does attending one or more ECHO sessions (compared to well-matched providers who do not attend any ECHO sessions) change the likelihood that the provider:
    a. obtains a DATA-waiver,
    b. prescribes MOUD, or
    c. prescribes opioids or high-dose opioids (>= 90 morphine milligram equivalents/day)?
2. Among adult patients with a history of OUD who are enrolled in Medicaid, does being treated by a provider who attended at least one ECHO session (compared to patients who are treated by a provider who did not attend any ECHO sessions) change the likelihood that the patient:
    a. is prescribed buprenorphine or receives any MOUD, or
    b. receives medical care for a nonfatal opioid overdose?

We also examined two exploratory questions: a) Does the number of ECHO sessions a provider attends affect their outcomes? and b) Do patients who see providers less than three months after they started ECHO have different outcomes compared to patients who see providers three or more months after they started ECHO?

Data and methods

Study design

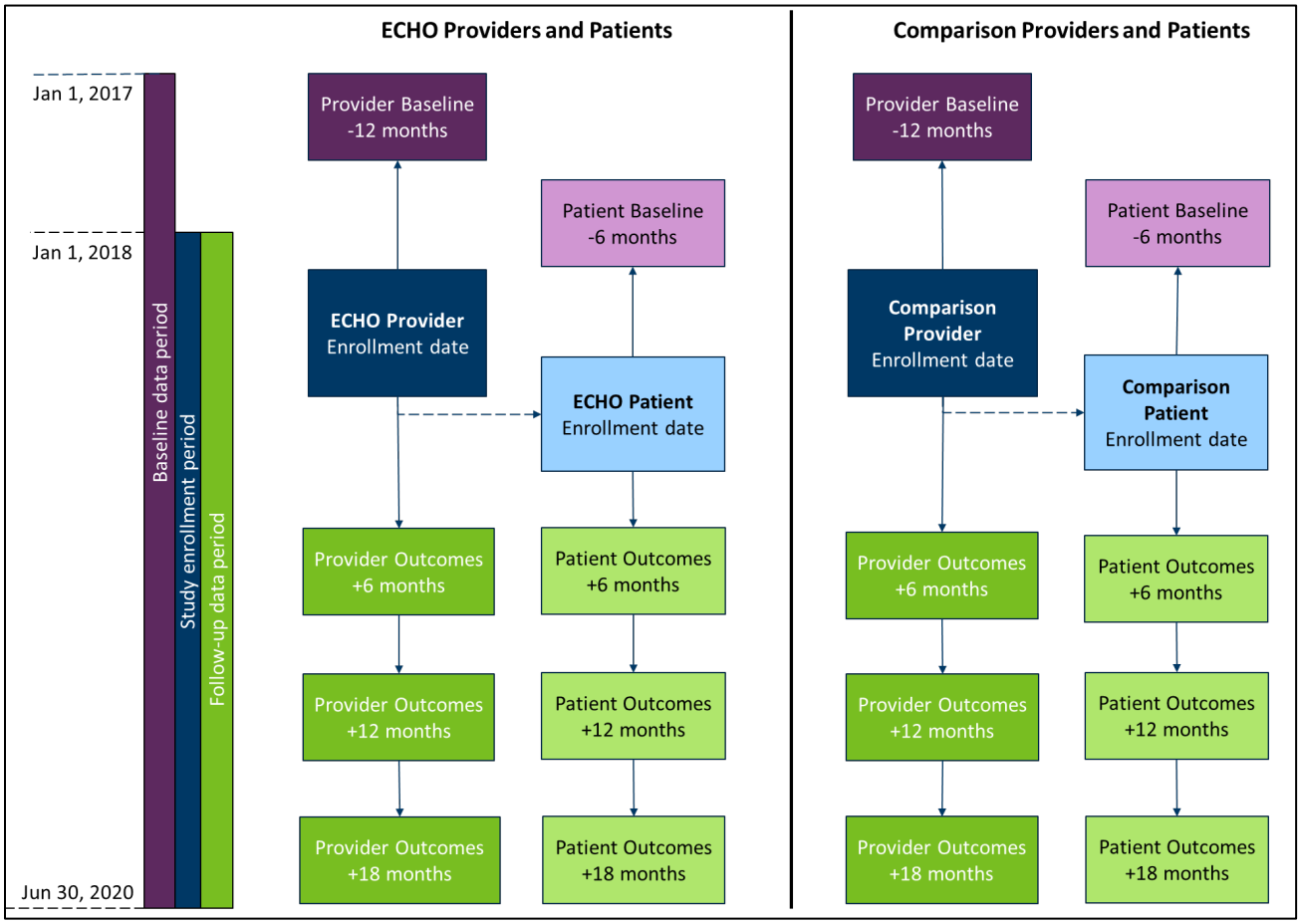
This evaluation was an observational cohort study comparing outcomes of providers who attended at least one ECHO session (we call this group the "treatment group" or "ECHO providers") and their patients, to providers who did not attend ECHO (we call this group "comparison providers") and their patients. We used an intention-to-treat approach for our analysis, meaning that providers were assigned to the treatment group if they attended one or more ECHO sessions during the study period. We identified comparison providers as individuals who did not attend any ECHO sessions and were most similar to treatment providers on relevant characteristics, like age, geographic region, practice specialty, and prior prescribing behavior. We statistically balanced the treatment and comparison groups to account for potential biases that could have arisen from baseline differences in these groups (see Appendix A).

After identifying the final sample of providers, we identified their eligible patients (see Participants section below for eligibility criteria). Patients were assigned to treatment and comparison groups based on which provider (ECHO or comparison) they saw first during the study period. We similarly adjusted treatment and comparison patients to account for baseline differences (again, see Appendix A).

The study period was 2.5 years, from January 1, 2018 to June 30, 2020. We collected follow-up data on outcomes for each provider and patient for up to 18 months following study enrollment. The study enrollment date for ECHO providers is the date they first attended an ECHO session. Providers and patients who did not meet the defined eligibility criteria were excluded from the study.

Baseline data was collected for the 12-month period immediately preceding provider enrollment, and the six-month period immediately preceding patient enrollment. For each provider and patient, we assessed outcomes in the baseline period, and at six months, 12 months, and 18 months after study enrollment. Participants who lacked complete data for a follow-up period were removed from that outcome period. See Figure 2 for an overview of the study design, enrollment flow, and timeline.

Figure 2. Study design, enrollment, and timeline



Note. Provider and patient baseline and follow-up periods may not be aligned. For example, if a provider was enrolled on January 1, 2018 and the patient first interacted with that provider on June 1, 2019, the provider's six-month outcome period would be from January 1 – June 1, 2018, but the patient's six-month outcome period would be from June 1 – December 1, 2019.

Participants

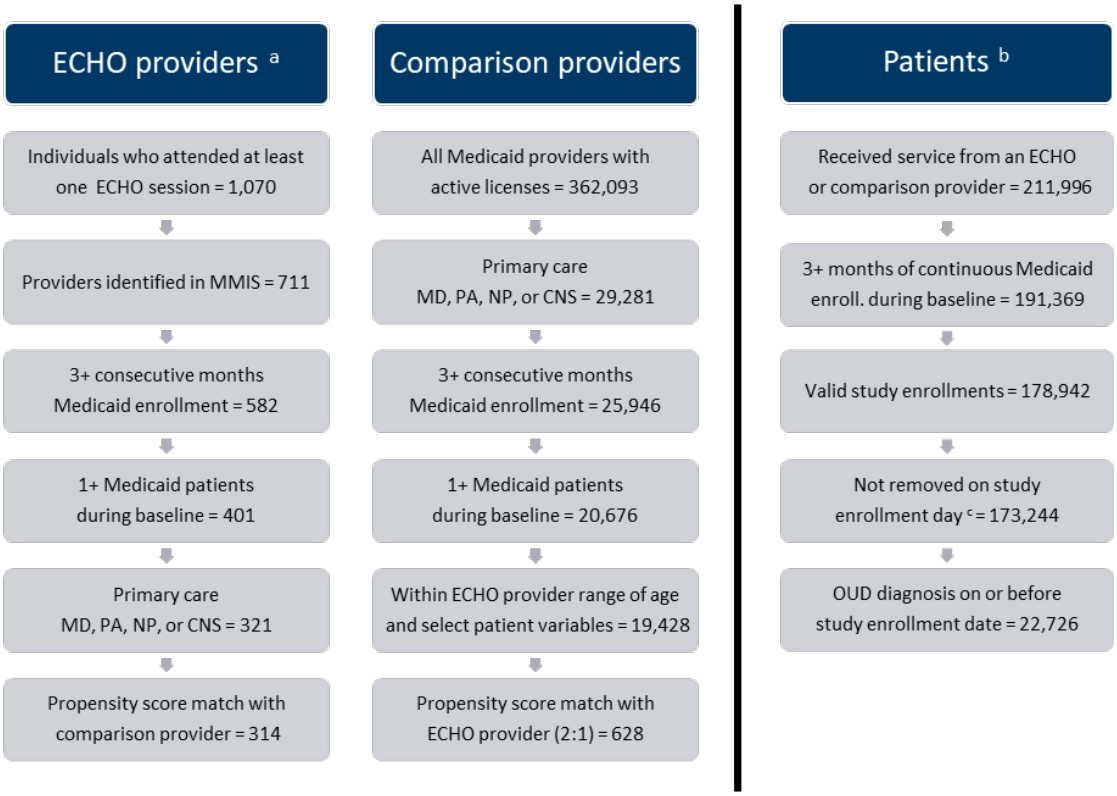
We limited the provider sample based on provider location[3], being a Medicaid provider, being a primary care provider[4], and having certain credential types.[5] We focused on primary care providers because a goal for both ECHO hubs was to expand MOUD access to office-based settings. Providers were not required to treat any OUD patients to be included in the sample.

The patient sample included individuals who were 18 years or older when enrolled in the study, were enrolled in Medicaid for at least three consecutive months and were eligible in the month before

[3] Providers must be based in Minnesota, South Dakota, North Dakota, Wisconsin, or Iowa. Comparison providers were drawn from the same geographic regions as ECHO providers.
[4] Defined by NPI provider codes indicating family/pediatric medicine, internal medicine, OBGYN, preventive, psychiatry/neurology, general practice, physician assistant, nurse practitioner, or certified nurse specialist.
[5] This only includes providers eligible for the DATA-waiver: physicians, physician assistants, nurse practitioners, and certified nurse specialists.

enrollment, and who had ever been diagnosed with an OUD (based on Medicaid records). Figure 3 shows the flow of participants through eligibility criteria and how we arrived at the final sample.

Figure 3. Provider and patient inclusion criteria and flow



[a] We excluded from our sample eight eligible providers identified as ECHO trainers/facilitators. [b] Numbers for patients refer to "enrollments" in the study, not to unique individuals. [c] Data from 5,698 valid enrollments was removed because 1) the patient saw both an ECHO and a comparison provider on their study enrollment day, 2) the patient died on their study enrollment day, or 3) the patient lost Medicaid eligibility on their study enrollment day.

Data sources

This study uses administrative data.[6] See Appendix B for details on linking across data sources.

- ECHO attendance: All Project ECHO hubs collect attendance data through a web-based program management software called iECHO. iECHO tracks information about ECHO attendees, including participants' name, email, credentials, clinic address, and date(s) of attendance.
- DATA-waiver status: Administrative records from the Drug Enforcement Agency's Controlled Substances Act Registry database were used to determine whether each provider had a DATA-waiver to prescribe, dispense, or administer buprenorphine; the quarters when the waiver was

[6]This study was approved by Minnesota's Department of Human Services Institutional Review Board (IRB #383). A data sharing agreement between DHS and MMB allows secure sharing between the two agencies.

active; and the patient limit for the waiver (30, 100, or 275 patients). The dataset was missing some quarters of the study period; it included 2015/Q1-Q4, 2018/Q1, 2019/Q1-Q4, and 2020/Q1.

- Medicaid claims: Data on buprenorphine prescriptions, MOUD, opioid prescriptions, and receiving medical care for nonfatal opioid overdoses was obtained through Minnesota's Medicaid prescription pharmacy and outpatient claims/encounter records (the Medicaid Management Information System, or MMIS). MMIS also includes information on provider and patient characteristics.

Outcome measures

Outcomes were aggregated into six-month follow-up periods after study enrollment (i.e., 1-6, 7-12, and 13-18 months) by identifying if the outcome occurred at all (binary outcomes) or averaging the outcomes across the months of the follow-up period (continuous outcomes). There were different outcome measures for providers and patients (see Tables 1 and 2).

Table 1. Provider outcome measures

Outcome	Coding values
DATA-waiver status	1 = provider has a DATA-waiver 0 = provider does not have a DATA-waiver
Active use of DATA-waiver	1 = provider wrote at least 1 buprenorphine prescription 0 = provider did not write any buprenorphine prescriptions
Buprenorphine prescriptions[7]	% of OUD patients per month for whom the provider wrote a buprenorphine prescription
MOUD provision (buprenorphine, naltrexone, or methadone)[8]	% of OUD patients per month for whom the provider provided MOUD
Opioid prescriptions	Number of opioid MMEs prescribed per patient per month (all patients, not just those with a history of OUD)
High-dose opioid prescriptions	Number of high-dose (>= 90 MMEs/day) opioid prescriptions per 1,000 patients per month (all patients, not just those with a history of OUD)

[7] We identified buprenorphine prescriptions in MMIS by the buprenorphine NDC codes from the HEDIS list for medication treatment for opioid abuse or dependency (National Committee for Quality Assurance, 2019)
[8] Nearly all MOUD prescriptions written by providers were either buprenorphine or naltrexone.

Table 2. Patient outcome measures

Outcome	Coding values
Buprenorphine prescription	1 = patient was prescribed buprenorphine 0 = patient was not prescribed buprenorphine
MOUD provision (buprenorphine, naltrexone, or methadone)[9]	1 = patient was provided MOUD 0 = patient was not provided MOUD
Nonfatal opioid overdose[10]	1 = patient received medical care for a nonfatal opioid poisoning 0 = patient did not receive medical care for a nonfatal opioid poisoning

Statistical models

To address our research questions, we used weighted generalized linear mixed-effects (GLMM) regression models for provider analyses and weighted generalized estimating equation (GEE) models for patient analyses. These models estimate the average effects of ECHO on outcomes during the 1-6, 7-12, and 13-18-month follow-up periods, following a generalized difference-in-differences methodology. Further details on the statistical models are available in Appendix C.

Results

Participants

Providers

The final provider sample included 942 providers (314 in the ECHO group and 628 in the comparison group). Follow-up data was available at six months for 273 ECHO (87 percent) and 541 comparison (86 percent) providers, at 12 months for 232 ECHO (74 percent) and 475 comparison (76 percent) providers, and at 18 months for 199 ECHO (63 percent) and 407 comparison (65 percent) providers.

There was a great deal of variation in the number of ECHO sessions that providers attended, ranging from one to 92 sessions across the study period. The average number of sessions attended was eight (SD = 15), and median was two sessions. Thirty-eight percent of providers attended only one ECHO session (Figure 4). Figure 5 shows the distribution of the final sample of ECHO providers across Minnesota; a small number of providers (three percent of ECHO and four percent of comparison providers) were from states that border Minnesota.

There were no statistically significant differences between matched ECHO and comparison providers on any observed demographic or other baseline measures including number of patients with an OUD or SUD diagnosis, DATA-waiver status, buprenorphine prescribing, or opioid prescribing (see Table 3, Appendix D). Importantly, this indicates that the treatment and comparison provider groups were similar on all measured characteristics at baseline (there may, however, be important characteristics that are not included in the available datasets).

[9] In the patient models, the patient could have received MOUD from any Medicaid provider, including those who were not in our sample (e.g., methadone at an opioid treatment program).
[10] ICD-10 codes T40.0, T40.1, T40.2, T40.3, T40.4, or T40.6

Figure 4. Number of ECHO sessions that providers attended

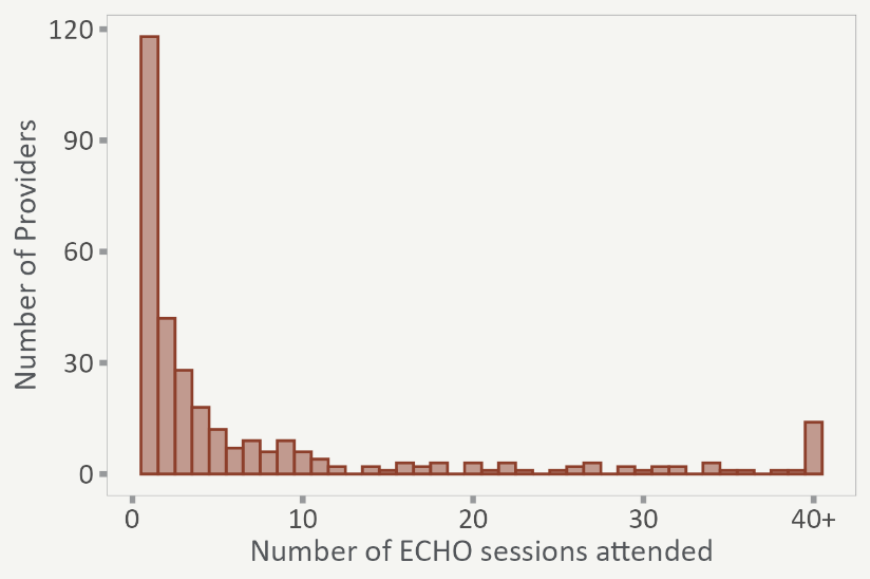
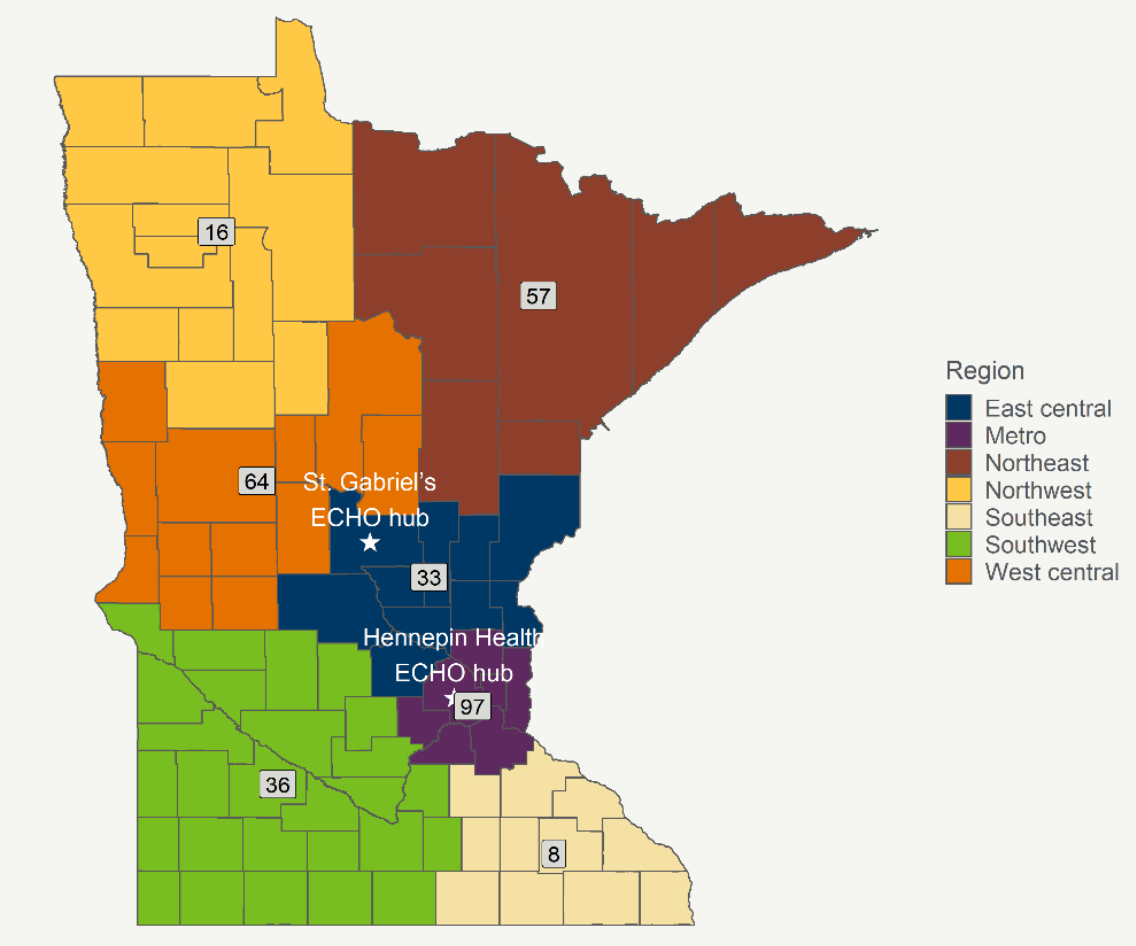


Figure 5. Number of ECHO providers in the final sample, by Prevention Region



Patients

The final sample of adult patients with a history of OUD included 9,327 patients of ECHO providers and 13,399 patients of comparison providers, for a total sample of 22,726. ECHO and comparison providers saw an additional 150,518 adult patients who were otherwise eligible but did not have a history of OUD and were not included in our analysis.

Follow-up data was available at six months for 7,319 ECHO (79 percent) and 10,040 comparison (75 percent) patients, at 12 months for 4,960 ECHO (53 percent) and 6,554 comparison (49 percent) patients, and at 18 months for 2,608 ECHO (28 percent) and 3,470 comparison (26 percent) patients. There were several baseline differences between OUD patients who saw ECHO and comparison providers (see Table 4, Appendix D). After applying the inverse probability of treatment weights (see Appendix A), measured baseline characteristics were balanced between groups.

Outcomes

Provider outcomes

The results for the main provider models are available in Table 5, Appendix D. Providers who attended ECHO were more likely than comparison providers to obtain a DATA-waiver for buprenorphine prescribing up to 18 months following initiation of ECHO training. At baseline, 16 percent of ECHO providers and 16 percent of matched comparison providers already had DATA-waivers. Among ECHO providers, this grew over time to 35 percent within 18 months, compared to just 18 percent of comparison providers (see Figure 6, Panel A). We estimate that ECHO training increased DATA-waivers by nearly 17 percentage points over 18 months, compared with the number of waivers that would have been obtained without ECHO training.

We found that ECHO training also increased the active use of DATA-waivers for buprenorphine prescribing. As shown in Figure 6, Panel B, at baseline, 17 percent of ECHO providers had written at least one buprenorphine prescription in the previous year before starting ECHO, compared to 16 percent of comparison providers. Those numbers diverged over 18 months, with 27 percent of ECHO providers prescribing buprenorphine at least once and comparison providers falling to just 12 percent prescribing between 12 and 18 months after baseline. We estimate that 14 percent more ECHO providers prescribed buprenorphine at 12 to 18 months than would have if they had not received ECHO training.

ECHO providers also prescribed buprenorphine and provided MOUD (which included buprenorphine, naltrexone, or methadone) to an increasing share of patients who had previously been diagnosed with OUD. Prior to beginning ECHO, ECHO providers prescribed buprenorphine for five percent and provided any MOUD for five percent of their OUD patients each month; those numbers increased to 12 percent (for buprenorphine or any MOUD) at the 18-month follow-up. Over the same period, comparison providers' monthly prescriptions of buprenorphine grew from three percent of monthly OUD patients to four percent (see Figure 6, Panel C), while any MOUD provision held steady at four percent (see Figure 6, Panel D). We estimate that ECHO training led providers to prescribe buprenorphine for 6.5 more OUD patients per 100 per month and provide any MOUD to 6.3 more OUD patients per 100 per month, than they would have without ECHO training.

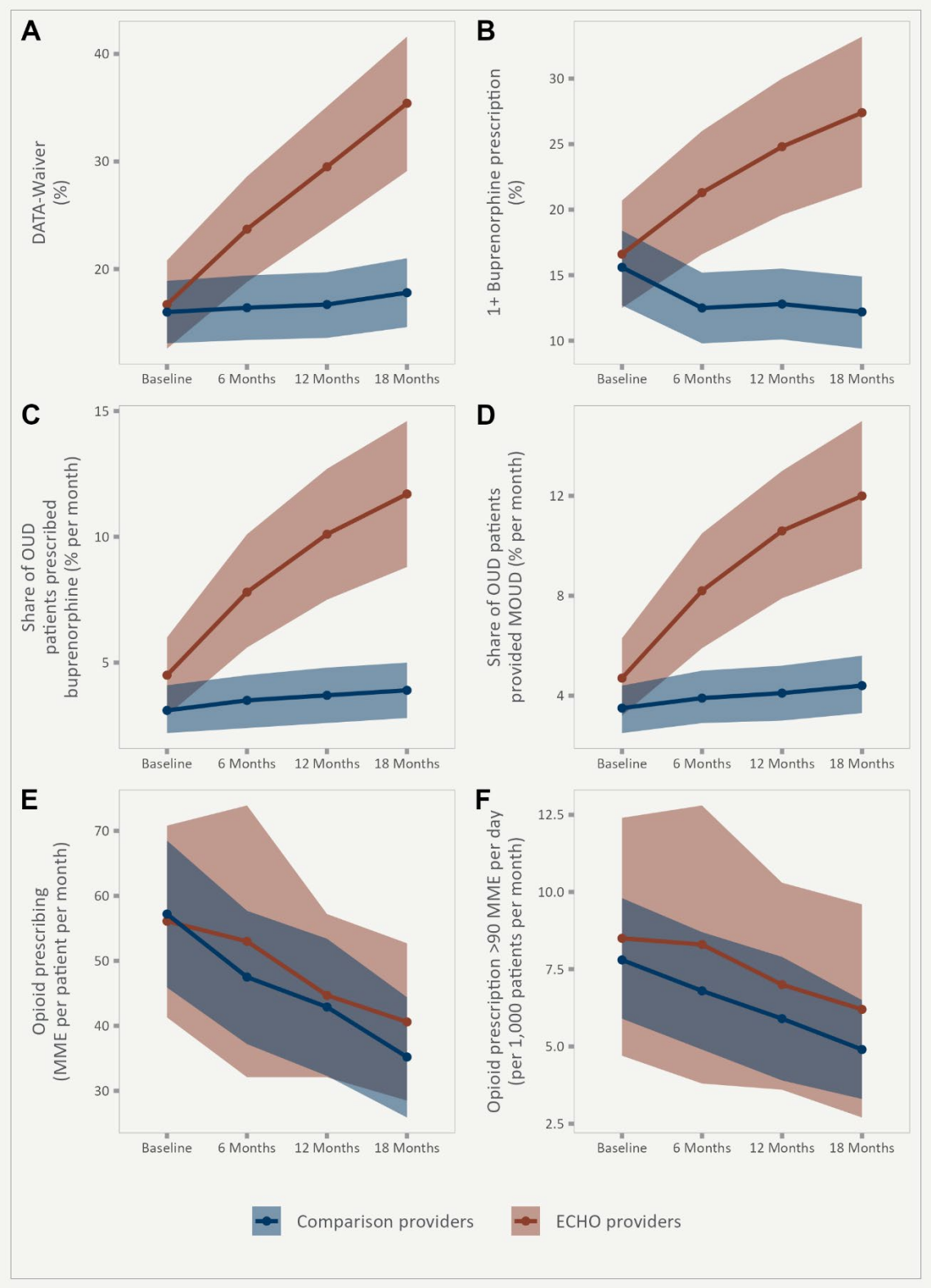
We did not find an effect of ECHO training on the overall quantity of opioid analgesic prescribing, as measured by average MMEs per patient or the number of high-dose prescriptions (over 90 MME per day). The measures of MMEs and high-dose prescriptions were for providers' full patient panels; they were not limited to patients with a history of OUD. Both measure of opioid analgesics declined steadily over time for all providers, regardless of which treatment group they were in. There were no significant differences between ECHO and comparison providers in the rate of decline over time or at any follow-up period. Figure 6 shows the results for opioid MMEs (Panel E) and high-dose prescriptions (Panel F).

Exploratory and sensitivity analyses

We conducted an exploratory analysis on the association between the number of ECHO session that providers attended and their outcomes. We found that providers who attended more ECHO sessions were more likely to obtain DATA-waivers and prescribe MOUD, compared to providers who attended fewer sessions. Unlike our main analysis, however, this was not a causal analysis; we cannot conclude that attending more ECHO sessions *caused* providers to obtain DATA-waivers or prescribe MOUD. This is because providers chose how many ECHO sessions to attend, and that choice is likely correlated with other unmeasured (or measured) factors, such as motivation to treat OUD or clinic culture. This confounding of sorting into lower and higher ECHO attendance means that we cannot say that providers with low attendance would have had the same improvements as providers with high attendance, if only they had attended more ECHO sessions. The detailed results of this analysis are available in Appendix E.

To confirm the robustness of our main findings for providers, we conducted three sensitivity analyses that tested for differential effects due to a) the COVID-19 pandemic, b) which hub the providers attended (Hennepin or CHI-St. Gabriel's), or c) additional OUD-focused funding that some clinics received from the state through a grant program called Tackling Opioid Use With Networks (TOWN). In summary, none of these factors changed our results or conclusions; the sensitivity analyses are further described in Appendix F.

Figure 6. ECHO and comparison provider outcomes over 18 months. Panels A, B, E, and F include all providers. Panels C and D include providers who treated patients with a history of OUD in a given follow-up period



Patient outcomes

Patients with a history of OUD who saw ECHO-trained providers were more likely than patients who saw comparison providers to receive buprenorphine up to 18 months following their first visit with the provider (see Table 7, Appendix D for full results). In the baseline period, 16 percent of OUD patients who saw either an ECHO provider or a comparison provider had filled a buprenorphine prescription. Within six months of seeing their provider, 22 percent of ECHO patients and 18 percent of comparison patients filled a buprenorphine prescription. Buprenorphine prescriptions decreased slightly over the next year to 19 percent of ECHO patients and 15 percent of comparison patients at 18 months follow-up (see Figure 7, Panel A). We estimate that OUD patients who saw an ECHO-trained provider were 4.2 percentage points more likely at 18 months to have a buprenorphine prescription, compared with the number of patients who would have had buprenorphine prescriptions absent ECHO.

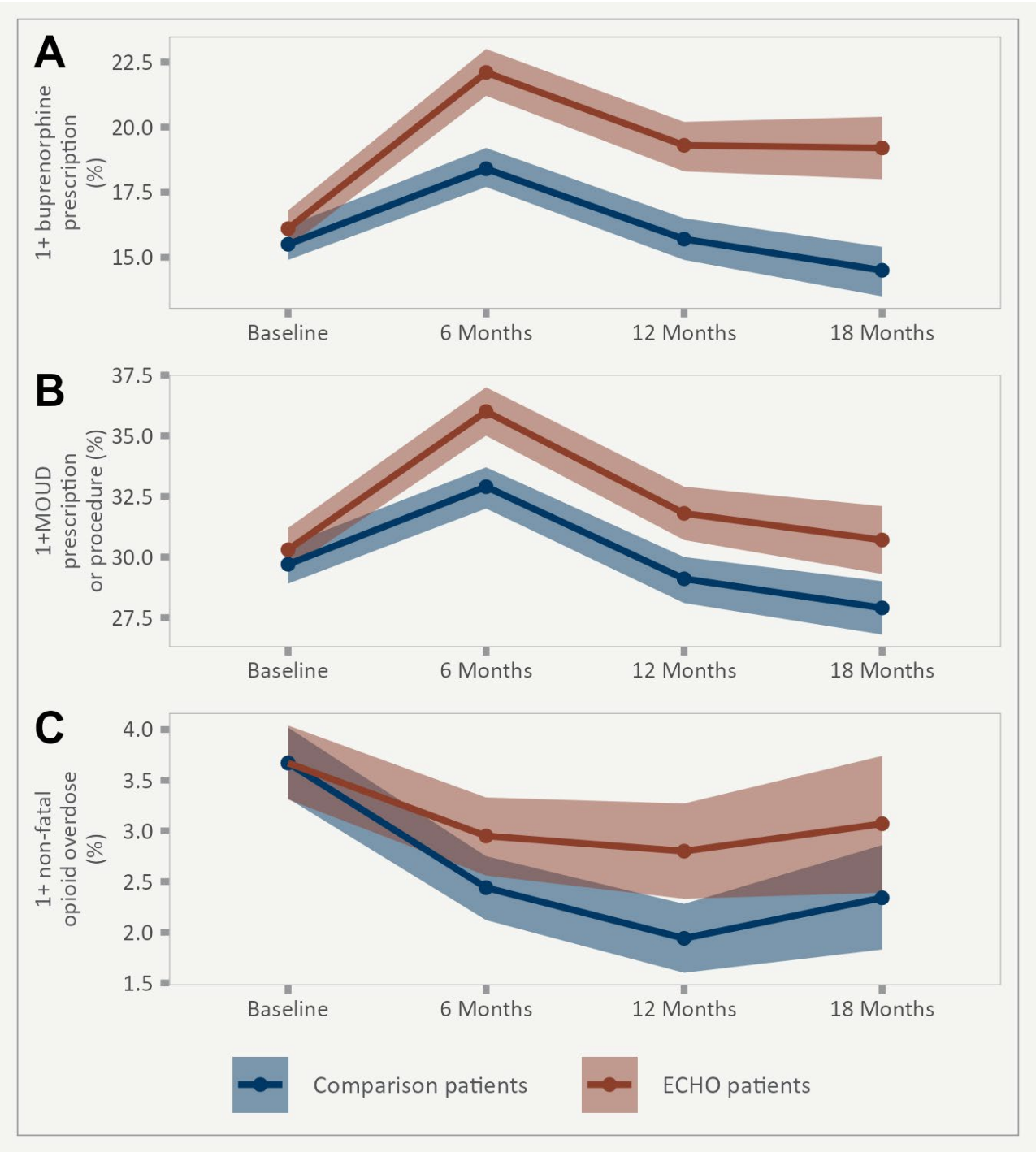
Similarly, we estimate that OUD patients who saw ECHO-trained providers were 2.5 percentage points more likely to have any kind of MOUD (buprenorphine, naltrexone, or methadone) in the six months following their initial visit, and 2.2 percentage points more likely to have MOUD from 12 to 18 months later, than if they had seen a comparison provider (see Figure 7, Panel B). The change in the proportion of patients who received medical care for nonfatal overdose in the period after seeing a study provider was not statistically significantly different for ECHO patients than for comparison patients (see Figure 7, Panel C).

Exploratory analysis

We conducted an exploratory analysis to test whether the impacts were different for patients who saw ECHO providers shortly after the providers attended their first ECHO session (within the first three months) compared to patients who had their first interaction with an ECHO provider three months or more following the provider's first ECHO session. We found evidence that patients who first interacted with a provider three months or more after the provider's first ECHO session were more likely to receive a prescription for MOUD. This finding is consistent with conversations with ECHO program administrators and other experts suggesting a delay in buprenorphine prescribing due to the time to obtain a DATA-waiver and implement changes to prescribing practices. Full discussion of the results of this analysis are available in Appendix E.

However, these findings should be taken as suggestive and not as causal analysis. This is because patients who see medical providers frequently (and therefore are more likely to have first seen a provider shortly after the provider's study enrollment) are systematically different from patients who see medical providers only infrequently. For example, OUD patients who saw their provider three months or more after study enrollment were less likely to enter the study with a prior buprenorphine prescription, and more likely to enter with a prior nonfatal overdose, than patients who saw their provider within the first three months. We cannot conclude that, had a patient who saw their provider earlier simply held off and seen their provider later instead, they would have experienced the same improvements in outcomes.

Figure 7. ECHO and comparison patient outcomes over 18 months



Discussion and conclusions

The opioid epidemic has taken the lives of thousands of Minnesotans and caused individuals and families an unfathomable amount of pain and grief. Federal, state, and local entities have sought to increase access to effective treatments for opioid addiction. This research shows Minnesota's investments in Project ECHO have been an effective way to expand capacity for prescribing evidence-based OUD treatments.

In this study, we tracked treatment and comparison providers and observed how their patterns of MOUD prescribing, opioid prescribing, and nonfatal opioid overdoses diverged over time. To do this, we carefully constructed a group of comparison providers that were similar to the ECHO providers at baseline. This design provides confidence that any differences were likely the result of participating in ECHO, rather than inherent differences in the providers and patients themselves. To our knowledge, this is the first study that used a causal design to examine both provider and patient outcomes and focused on Minnesota's population.

Our assumption of a causal relationship is drawn from three observations. First, the providers and patients in the ECHO and comparison groups had similar characteristics and similar outcomes over the 12 months (for providers) or six months (for patients) before encountering the ECHO program. Second, we observed a clear dose-response relationship between the number of ECHO sessions a provider attended and the change in their clinical practice (see Appendix E). Third, patients who saw ECHO providers immediately after their training began saw little change in their clinical treatment compared with the comparison patients, while patients who saw ECHO providers several months after their training began had a greater probability of receiving a buprenorphine prescription, even though they were being treated by the same set of providers (see Appendix E).

Our findings suggest that ECHO was highly effective at increasing capacity for providers to treat patients using MOUD. Of the providers who attended one or more ECHO sessions, 35 percent obtained a DATA-waiver to prescribe buprenorphine within 18 months after beginning ECHO, compared to just 18 percent of providers who did not attend ECHO. We know that simply being waivered is not enough – past studies have shown that many providers become waivered but never prescribe buprenorphine (Huhn & Dunn, 2017; Molfenter et al., 2019). We found, however, that ECHO providers not only got waivered, but they also actively used their waivers to prescribe buprenorphine at a much higher rate (27 percent) than comparison providers (12 percent) at 18 months.

Along the same lines, ECHO-trained providers treated a higher percentage of their OUD patients using MOUD than the comparison group. Our measures of MOUD included buprenorphine, naltrexone, or methadone, but increases in buprenorphine accounted for nearly all the increase in providing MOUD. This suggests that waivered providers did not simply shift prescribing from one medication to another, but rather increased the amount of MOUD available to their patients. This is echoed in the patient results, which showed patients of ECHO providers were more likely to receive buprenorphine specifically, and MOUD generally, than patients of comparison providers in all follow-up periods.

We did not find evidence that ECHO caused providers to reduce their opioid analgesic prescribing, as measured by average MMEs per patient or high-dose prescriptions. It is possible that effects were

masked by a well-documented trend in Minnesota (and across the U.S.) towards a reduction in opioid prescribing over time for all providers, due at least in part to recent regulations and policies that require or encourage them to reduce their opioid prescribing. Given the variety of initiatives to discourage problematic opioid analgesic prescribing, it may be difficult to detect the impact of a single program like ECHO, especially because safe opioid prescribing was a secondary goal for the two ECHO hubs in this study. Further, our measure of opioid prescribing did not capture nuanced shifts in provider behavior that ECHO trained on, such as the correct pace and timing for tapering opioids and transitioning patients to buprenorphine. We were not able to assess these practices through our administrative records. Qualitative interviews or provider surveys could provide a more complete picture of the quality of providers' approaches to tapering opioid prescriptions.

In addition, although prescription opioids continue to contribute to the opioid epidemic, the driving factor behind increasing opioid overdose deaths has shifted to illicit opioids, like heroin and fentanyl (Planalp & Hest, 2020), suggesting that reducing the availability of all opioids, not just prescriptions, is critical.

We also did not find that ECHO participation affected receiving medical care for nonfatal opioid overdoses among patients. This may be due to patient, community, or treatment environment characteristics that we were not able to account for or were not measured. For example, if the patients who saw ECHO providers had more severe OUD before being enrolled in the study, they may have been more likely to experience subsequent nonfatal overdoses for reasons that were not related to encountering ECHO providers. It is also possible that our measure of MOUD (whether a patient received them or not) was not sufficient to be linked to preventing overdoses. A more stringent measure, such as whether a patient consistently received MOUD, received MOUD plus counseling, or whether providers used the correct buprenorphine dosage and tapering procedures, may have been more closely tied to nonfatal overdoses.

Importantly, our exploratory analyses suggest that providers may benefit most from regularly attending ECHO. We found no differences between providers who attended one ECHO session and the comparison providers, but large differences emerged between providers who attended six or more ECHO sessions and the comparison providers. This suggests retaining providers in ECHO training is vital to the success of future ECHOs; ECHO providers and policymakers should test ways to increase retention. We also found greater effects when patients saw a provider three or more months after the beginning of ECHO training, indicating that effects of ECHO are likely to materialize only after training has had time to affect prescriber behavior.

One limitation of this study is that the population of providers and patients who were affected by ECHO is different than the general population of providers and patients. When looking at the full population of Medicaid providers, the providers included in our study (both ECHO and comparison), were more likely to already have a DATA-waiver and prescribe buprenorphine, more likely to work with patients with OUD or other substance use disorders, and more likely to have prescribed opioid pain medications. While we are confident that the comparison providers we selected for this study were a good match for ECHO providers, our findings may not be generalizable to providers who do not currently see OUD patients or prescribe opioid pain medication.

Our access to medical claims data was limited to what was available in MMIS, which means we focused specifically on Medicaid providers and Medicaid patients. Our findings are relevant for that group but may not generalize to other populations. A portion of ECHO attendees did not serve Medicaid patients and were excluded from our analysis; future work may be able to include providers and patients outside Medicaid to get a fuller picture of ECHO's impacts.

A limitation of any observational study is the possibility that unmeasured baseline differences existed between treatment and comparison groups, which weakens our ability to conclude that ECHO caused an increase in MOUD capacity. A commonly cited unmeasured factor is motivation – some providers are simply more motivated to attend ECHO or treat OUD patients, and that motivation might lead them to increase their capacity to offer MOUD. The best way to avoid this kind of selection bias is to randomly assign providers to attend or not attend ECHO; that was not feasible in this study but is a direction for future research.

Even considering these limitations, this rigorous study supports that Project ECHO is an effective intervention for increasing access and capacity for MOUD. Continued and expanded use of Project ECHO can be one important tool—in a robust continuum of care—to mitigate the harm of opioids on our communities.

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Appendix A: Matching and weighting procedures to create like comparisons

A critical step in our analysis was to identify comparison providers who were as similar as possible to the ECHO providers on relevant characteristics and clinical practices at baseline. Doing so increases our confidence that differences in outcomes between these groups result from attending ECHO, and not from some other factor, such as provider groups having different numbers of OUD patients in their patient panel.

To create the comparison group of non-ECHO providers, we performed a two-stage process of propensity score matching followed by adjustment via inverse probability of treatment weights (IPTWs) using data from the baseline period. For each comparison provider, we randomly selected a study enrollment date from the distribution of ECHO provider enrollment dates.

For propensity score matching, we used the following variables to identify like comparison providers who were similar to the ECHO providers:

- Provider type (physician, Nurse Practitioner, Physician Assistant, Clinical Nurse Specialist)
- Geographic region (as defined by Minnesota's seven regional prevention coordination areas, and a separate code for "out of state")
- Age at study enrollment (continuous)
- Study enrollment date (continuous)
- Baseline (0-12 months before study enrollment) characteristics
    - Average number of Medicaid patients per month (continuous)
    - Percent of patients per month diagnosed with any SUD (continuous)
    - Percent of patients per month diagnosed with OUD (continuous)
    - Ever prescribed buprenorphine (yes, no)
    - Number of opioid MMEs prescribed per patient per month (continuous)

Matching was performed by first identifying the so-called "propensity-to-treat" for a given ECHO provider, based on the above variables measured during the baseline period. This propensity was expressed as a logit to linearize around the probability. Next, we identified all providers from the universe of eligible comparison providers whose propensity score (when calculated using the same method) was within 0.2 standard deviations of the ECHO provider's own logit propensity score (Austin, 2011; Murphy et al., 2017). Finally, we selected the two closest matching comparison providers from this propensity-matched pool for our final analysis. Enrolling comparison providers at a 2:1 ratio helps to increase sensitivity for low-frequency outcomes and captures a degree of the variance across baseline measures within the comparison population that could lead to the same propensity score.

Once our treatment and comparison providers were defined by the matching process, we weighted individual provider contributions to our statistical models by the inverse of that provider's propensity to receive treatment (i.e., providers whose characteristics made them unlikely to receive their treatment assignment are treated as more informative by the model). The use of IPTW[11] in observational studies is

[11] Here we use stabilized IPTW, following recommendations from Robins et al. (2000).

crucial for reducing selection bias in treatment effects (Robins et al., 2000). Further, combining propensity score matching with IPTW has been shown to reduce bias in the estimation of treatment effects for healthcare-related evaluations (Linden, 2014).

Patients were assigned to treatment (ECHO) and comparison (non-ECHO) groups based on the providers they saw during the study period, rather than as the result of propensity score matching. Thus, the population of ECHO patients was defined as those patients who saw an ECHO provider during the study period, while comparison patients were defined as those patients who saw a provider assigned to the comparison group (as defined above).

Some patients saw both ECHO and comparison providers. If patients saw an ECHO provider, they were assigned to the ECHO group until the end of the study, loss of Medicaid eligibility, or death. If patients saw a comparison provider, they were assigned to the comparison group until the end of the study, seeing an ECHO provider (and being re-enrolled as an ECHO patient[12]), loss of Medicaid eligibility, or death.

We weighted patient models using stabilized IPTW based on the following baseline variables:

- Demographics (age at study enrollment [banded], sex [male, female], county, language [English, non-English, unknown], race [white, non-white, unknown])
- Buprenorphine prescription (yes, no) and/or any MOUD prescription (yes, no) in 1-6 months prior to study enrollment
- Received medical care for nonfatal opioid overdose (yes, no) in 1-6 months and 7-12 months prior to study enrollment
- Study enrollment date (continuous)
- Medicaid eligibility and plan characteristics in the month before study enrollment
    - Plan type (Managed care, fee for service)
    - Eligibility status (Medicaid only, dual Medicaid/Medicare)
    - Eligibility code (income-based, medical/health-based, unknown)

[12] Many patients saw both treatment and comparison providers during the study period. Patients who first saw a comparison provider, then later saw an ECHO provider, were censored from the comparison group on the day before their ECHO provider visit (N = 2,257). These patients were then re-enrolled in the study in the ECHO group with a new baseline period (which included outcomes that occurred while they were in the comparison group). Thus, these patients contributed data to both the comparison group and the treatment group estimates. This was done under the assumption that seeing a comparison provider (versus seeing a provider who was not selected for this study, but also was not participating in ECHO) would not change the estimated effect of seeing an ECHO provider, while the reverse would not be true. The decision to censor ongoing follow-up time from the comparison group was made to limit cross-contamination between the two groups. An alternate analysis, in which patients were censored without re-enrollment and an inverse probability of attrition weight was used to adjust the uncensored patient sample to represent the full uncensored population, produced estimates consistent with the results presented in this report.

Appendix B: Linking individuals across datasets

Data was collected from multiple sources with no common identifier to link individuals across the different datasets. We therefore used several approaches to identify matches between the providers who participated in ECHO, their information in the Medicaid dataset (MMIS), and the DATA-waiver dataset.

Of the 1,070 unique individuals who attended ECHO, we were able to identify 711 (66 percent) in MMIS (see Table 3, below). We began by creating prospective matches between the providers who participated in ECHO (from the iECHO data) and their information in MMIS using name, credentials, and clinic addresses. Fifty-two percent of ECHO providers were linked through this method. For ECHO providers who did not match on these fields in MMIS, we looked up their National Provider Identifier (NPI) number in the National Plan & Provider Enumeration System (NPPES), a publicly available dataset, using name, credentials, and clinic address. We then matched those NPIs to the NPIs available in MMIS. An additional 11 percent of ECHO providers were linked through this method. For the remaining unlinked people in iECHO, we attempted manual searches to identify them (three percent of providers were manually matched).

We were not able to identify matches for the remaining 34 percent of ECHO participants. For four percent of ECHO participants, there were multiple possible matches that could not be resolved. For the remaining 30 percent, we were not able to find any appropriate matches. This was primarily because either a) the individual was not a health care provider but was attending ECHO for other reasons (e.g., MN state legislators, or staff from Minnesota's Department of Human Services) or b) the provider was not enrolled in Medicaid. Unmatched ECHO participants were not included in the analysis.

Of the set of ECHO providers who were successfully linked with MMIS, we were able to identify 19 percent in the DATA-waiver dataset. We used a similar process to the one described above. The remaining 81 percent of ECHO participants were not found in the DATA-waiver dataset, indicating that they did not have a DATA-waiver.

Table 3. Number and % of ECHO providers linked using each linking approach

Linking approach	**Datasets being linked**			
	iECHO – MMIS		iECHO – DATA-waiver	
	# of individuals	% of sample	# of individuals	% of sample
Name + credential + clinic address	558	52%	122	17%
National Provider Identifier (NPI)	119	11%	8	1%
Manual match	34	3%	6	1%
TOTAL MATCHED	**711**	**66%**	**136**	**19%**
Not matched	*359*	*34%*	*575*	*81%*

Appendix C: Statistical model details

We identify the effect of ECHO participation (for providers) or clinical exposure (for patients) by way of weighted regression models. These models estimate the marginal effects of ECHO on outcomes during the 1-6, 7-12, and 13-18-month follow-up periods by comparing changes since the pre-enrollment baseline period in the ECHO and comparison groups, following a generalized difference-in-differences methodology. Under this approach, time since exposure is aggregated into follow-up periods, and average treatment effects are estimated for each of those periods independently, relative to baseline. This has the advantage of permitting effects to vary over time, as the effects of ECHO training on clinical practice could either lessen or strengthen over time.

All effect estimates were weighted using the IPTW defined in Appendix A. In general, weighting allows us to create a "pseudopopulation" in which ECHO training is not associated with variables we expect a priori to affect outcomes at follow-up. Variables used in weighting are identified in Appendix A. As with other post-stratification methods for control of confounding, we must assume that no unmeasured confounding exists after the weights are applied. In this way, uncontrollable factors that may select against individuals who would otherwise be impacted by the program are mitigated to recover model precision and generalizability.

Provider outcome models

Effects of ECHO on providers were estimated by fitting the following generalized linear mixed-effects model (GLMM) to observed data. All models were fitted with provider random intercepts, to distinguish residual person-level random error from between-person individual variation.

$$y_{igp} = \alpha_g + \lambda_p + \mathrm{x}_{gt}\beta + \mathrm{a_i} + \epsilon_{igp}$$

The variables in the model are defined as follows:

y_{igp}: Measured outcome of interest for provider *i* in group *g*, in follow-up period *p.* For the two binomial outcomes (DATA waiver, any vs no buprenorphine prescribing) y_{igp} is assumed to follow a binomial distribution with the expected value given by the linear combination of terms expressed above, such that $\Pr(Y = 1|y_{igp}) \sim y_{igp}$

α_g: Estimate of average baseline (pre-enrollment) measure of outcome of interest in each treatment group

λ_p: Set of period effects for 1-6 month, 7-12 month, and 13-18 month follow-up periods

x_{gt}: Indicator variable for ECHO treatment group at each follow-up period

β: Difference in differences for ECHO treatment group at each follow-up period (the estimand of interest)

$\mathrm{a_i}$: Random intercept for provider *i* used to account for individual variation, expected to follow a normal distribution $\eta(a_0, \sigma^2_0)$ where a_0 is the average provider outcome at baseline

ϵ_{igp}: Residual variation for individual provider *i* in ECHO group *g* in follow-up period *p*, expected to follow a normal distribution $\eta(0, \sigma^2_i)$. This error term captures the within-subject variance over time due to repeated measurement of provider *i*.

Patient outcome models

Patient effects were estimated with generalized estimating equation (GEE) models.[13] The covariance structure was clustered on the individual patient, with an exchangeable working correlation matrix. All patient outcomes were fitted as linear probability models with binomial distributions and identity links.

$$y_{igp} = \alpha_g + \lambda_p + \mathrm{x}_{gt}\beta + \epsilon_{igp}$$

The variables in the model are defined as follows:

y_{igp}: Measured outcome of interest for patient *i* in patient group *g*, in follow-up period *p*. y_{igp} is assumed to follow a binomial distribution with the expected value given by the linear combination of terms expressed above, such that $\Pr(Y = 1|y_{igp}) \sim y_{igp}$

α_g: Estimate of average baseline (pre-enrollment) measure of outcome of interest in each patient group

λ_p: Set of period effects for 1-6 month, 7-12 month, and 13-18 month follow-up periods

x_{gt}: Indicator variable for ECHO patient group at each follow-up period

β: Difference in differences for ECHO patient group at each follow-up period (estimand of interest).

ϵ_{igp}: Residual variation for individual patient *i* in patient group *g* in follow-up period *p*

[13] We analyzed the patient effects using both GEE and GLMM and found nearly identical difference-in-differences estimates. We chose GEE models for patient outcomes because the model estimates reflect the marginal means in whole population – that is, that the baseline means of outcomes for ECHO and comparison patients were equivalent in the GEE models. Due to the effects of random intercepts, that baseline equivalence in outcomes is not maintained in GLMM estimates, but differences-in-differences are unaffected by model choice.

Appendix D: Results tables

Table 4. Unweighted baseline characteristics for ECHO and comparison providers

	Study group, Mean (SD) or No. (%)		
Characteristic	ECHO providers (n = 314)	Comparison providers (n = 628)	*P* value[a]
Age (years)	46.1 (12.0)	46.7 (12.0)	0.46
Follow-up time (months)	18.6 (8.9)	18.8 (9.0)	0.65
Provider type (N [%])			0.88
Physician	197 (62.7)	396 (63.1)	
Nurse Practitioner	84 (26.8)	163 (26.0)	
Clinical Nurse Specialist	3 (1.0)	10 (1.6)	
Physician Assistant	30 (9.6)	59 (9.4)	
Region (N [%])			0.59
Minneapolis-Saint Paul metro region	97 (30.9)	178 (28.3)	
Greater Minnesota	207 (65.9)	424 (67.5)	
Outside Minnesota	10 (3.2)	26 (4.1)	
Unique Medicaid patients per month (count)	70.7 (60.1)	69.4 (58.5)	0.76
Unique Medicaid patients per month with SUD diagnosis history (count)	23.2 (30.1)	21.8 (24.9)	0.46
Unique Medicaid patients per month with OUD diagnosis history (count)	9.5 (15.1)	9.0 (13.5)	0.64
Opioid prescriptions (MME per unique patient per month)	56.8 (134.8)	57.2 (145.4)	0.97
Opioid prescriptions > 90 MME per day per 1000 patients per month	8.7 (34.1)	7.9 (25.3)	0.69
DATA-Waiver (N [%])	55 (17.5)	99 (15.8)	0.49
1+ buprenorphine prescription in previous 12 months (N [%])	53 (16.9)	96 (15.3)	0.53
1+ MOUD prescription in previous 12 months (N [%])	80 (25.5)	138 (22.0)	0.23

Abbreviations: MOUD, medications for opioid use disorder; MME, morphine milligram equivalent; OUD, opioid use disorder; SD, standard deviation; SUD, substance use disorder

[a] *P*-values for count and continuous variables from 2-sided t-test. *P*-values for binary and categorical variables from Chi-square test.

Table 5. Unweighted baseline characteristics for ECHO and comparison patients

	Study group, Mean (SD) or No. (%)		
Characteristic	ECHO patients (n = 9,327)	Comparison patients (n = 13,399)	*P* value[a]
Age (years)	42.5 (14.2)	43.4 (14.7)	<0.001
Follow-up time (months)	12.0 (7.9)	11.3 (8.0)	<0.001
Region (N [%])			0.078
Minneapolis-Saint Paul 7-county metro area	4,993 (53.5)	7,014 (52.4)	
Greater Minnesota	4,334 (46.5)	6,385 (47.7)	
Sex (N [%])			<0.001
Male	4,369 (46.8)	5,794 (43.2)	
Female	4,958 (53.2)	7,605 (56.8)	
Race (N [%])			<0.001
White	4,751 (50.9)	7,647 (57.1)	
Non-white	3,178 (34.1)	3,830 (28.6)	
Unknown	1,398 (15.0)	1,922 (14.3)	
Language code (N [%])			0.16
English	9,096 (97.5)	13,043 (97.3)	
Non-English	64 (0.7)	123 (0.9)	
Unknown	167 (1.8)	233 (1.7)	
Plan type (N [%])			0.93
Managed Care	7,579 (81.3)	10,882 (81.2)	
Fee-for-service	1,748 (18.7)	2,517 (18.8)	
Eligibility code (N [%])			<0.001
Income-based eligibility	6,252 (67.0)	8,525 (63.6)	
Medical/health-based eligibility	3,075 (33.0)	4,874 (36.4)	
Received 1+ buprenorphine prescription in previous 6 months (N [%])	1,835 (19.7)	1,791 (13.4)	<0.001
Received 1+ MOUD prescription in previous 6 months (N [%])	3,127 (33.5)	3,709 (27.7)	<0.001
1+ nonfatal opioid overdose in previous 6 months	406 (4.4)	432 (3.2)	<0.001

[a] *P*-values for count and continuous variables from 2-sided t-test. *P*-values for binary and categorical variables from Chi-square test.

Table 6. Results from main provider regression models

	Estimated means (95% confidence intervals)				Difference-in-differences analysis (95% confidence intervals)		
Outcome	Baseline	6 months	12 months	18 months	Baseline to 6 months	Baseline to 12 months	Baseline to 18 months
DATA-waiver (%)							
ECHO providers	16.7 (12.6 to 20.8)	23.7 (18.8 to 28.6)	29.5 (23.9 to 35.1)	35.4 (29.1 to 41.6)	6.6*** (2.6 to 10.6)	12.1*** (6.8 to 17.4)	16.8*** (10.5 to 23.3)
Comparison providers	16.0 (13.1 to 18.9)	16.4 (13.4 to 19.4)	16.7 (13.6 to 19.7)	17.8 (14.6 to 21.0)			
1+ bup. prescription (%)							
ECHO providers	16.6 (12.5 to 20.7)	21.3 (16.6 to 26.0)	24.8 (19.6 to 30.0)	27.4 (21.7 to 33.2)	7.8*** (3.7 to 11.9)	11.0*** (6.1 to 16.0)	14.3*** (8.5 to 20.1)
Comparison providers	15.6 (12.7 to 18.4)	12.5 (9.8 to 15.2)	12.8 (10.1 to 15.5)	12.2 (9.4 to 14.9)			
% of OUD patients prescribed bup. (% per month)							
ECHO providers	4.5 (2.9 to 6.0)	7.8 (5.6 to 10.1)	10.1 (7.5 to 12.7)	11.7 (8.8 to 14.6)	3.0*** (1.4 to 4.6)	5.1*** (2.9 to 7.3)	6.5*** (3.9 to 9.1)
Comparison providers	3.1 (2.2 to 4.1)	3.5 (2.4 to 4.5)	3.7 (2.6 to 4.8)	3.9 (2.8 to 5.0)			
% of OUD patients provided MOUD (% per month)							
ECHO providers	4.7 (3.2 to 6.3)	8.2 (5.9 to 10.5)	10.6 (7.9 to 13.0)	12.0 (9.1 to 15.0)	3.0*** (1.4 to 4.6)	5.2*** (3.0 to 7.4)	6.3*** (3.7 to 9.0)
Comparison providers	3.5 (2.5 to 4.4)	3.9 (2.9 to 5.0)	4.1 (3.0 to 5.2)	4.4 (3.3 to 5.6)			
Opioid prescribing (MME per patient per month)							
ECHO providers	56.1 (41.3 to 70.8)	53.0 (32.1 to 73.9)	44.7 (32.1 to 57.2)	40.6 (28.5 to 52.7)	6.7 (-6.1 to 19.5)	2.9 (-6.9 to 12.8)	6.6 (-6.0 to 19.2)
Comparison providers	57.2 (45.9 to 68.5)	47.5 (37.2 to 57.7)	42.9 (32.3 to 53.4)	35.2 (25.9 to 44.4)			
Opioid prescription >90 MME per day (per 1,000 patients per month)							
ECHO providers	8.5 (4.7 to 12.4)	8.3 (3.8 to 12.8)	7.0 (3.6 to 10.3)	6.2 (2.7 to 9.6)	0.80 (-2.7 to 4.3)	0.36 (-2.0 to 2.8)	0.56 (-2.5 to 3.6)
Comparison providers	7.8 (5.9 to 9.8)	6.8 (4.9 to 8.7)	5.9 (3.9 to 7.9)	4.9 (3.3 to 6.5)			
****p* < .001							

Table 7. Results from main patient regression models

	Estimated means (95% confidence intervals)				Difference-in-differences analysis (95% confidence intervals)		
Outcome	Baseline	6 months	12 months	18 months	Baseline to 6 months	Baseline to 12 months	Baseline to 18 months
1+ buprenorphine prescription (%)							
ECHO patients	16.1 (15.4 to 16.8)	22.1 (21.2 to 23.0)	19.3 (18.3 to 20.2)	19.2 (18.0 to 20.4)	3.1*** (2.2 to 3.9)	3.0*** (1.9 to 4.0)	4.2*** (2.7 to 5.7)
Comparison patients	15.5 (14.9 to 16.2)	18.4 (17.7 to 19.2)	15.7 (14.9 to 16.5)	14.5 (13.5 to 15.4)			
1+ MOUD prescription/procedure (%)							
ECHO patients	30.3 (29.5 to 31.2)	36.0 (35.0 to 37.0)	31.8 (30.7 to 32.9)	30.7 (29.3 to 32.1)	2.5*** (1.5 to 3.5)	2.1** (.81 to 3.4)	2.2* (.50 to 3.9)
Comparison patients	29.7 (28.9 to 30.5)	32.9 (32.0 to 33.7)	29.1 (28.1 to 30.0)	27.9 (26.8 to 29.0)			
1+ non-fatal opioid overdose (%)							
ECHO patients	3.67 (3.31 to 4.04)	2.95 (2.56 to 3.33)	2.80 (2.33 to 3.27)	3.07 (2.39 to 3.74)	.51 (-.12 to 1.13)	.85* (.12 to 1.59)	.72 (-.22 to 1.66)
Comparison patients	3.67 (3.32 to 4.02)	2.44 (2.12 to 2.75)	1.94 (1.60 to 2.28)	2.34 (1.83 to 2.86)			
*$p < .05$ **$p < .01$ ***$p < .001$							

Appendix E: Exploratory analyses

We conducted two exploratory analyses. The first examined whether the number of ECHO sessions providers attended affected their outcomes. The second looked at the amount of time that had passed since providers initiated their ECHO training, and whether that affected patient outcomes. We consider these "exploratory" because, unlike our main analyses, they are not causal analyses and are influenced by omitted variable bias. Given that there are very few studies of OUD-focused ECHO programs, we believe that these analyses still provide useful information that may improve programming.

Number of ECHO sessions providers attended

We were interested in whether there were different outcomes for providers who attended more vs. fewer ECHO sessions. The number of sessions a provider attended is voluntary and likely correlated with other unmeasured (or measured) factors, such as motivation to treat OUD or clinic culture. Therefore, any significant findings cannot be attributed to the ECHO program itself but may offer some clues about the association between different "doses" and provider outcomes.

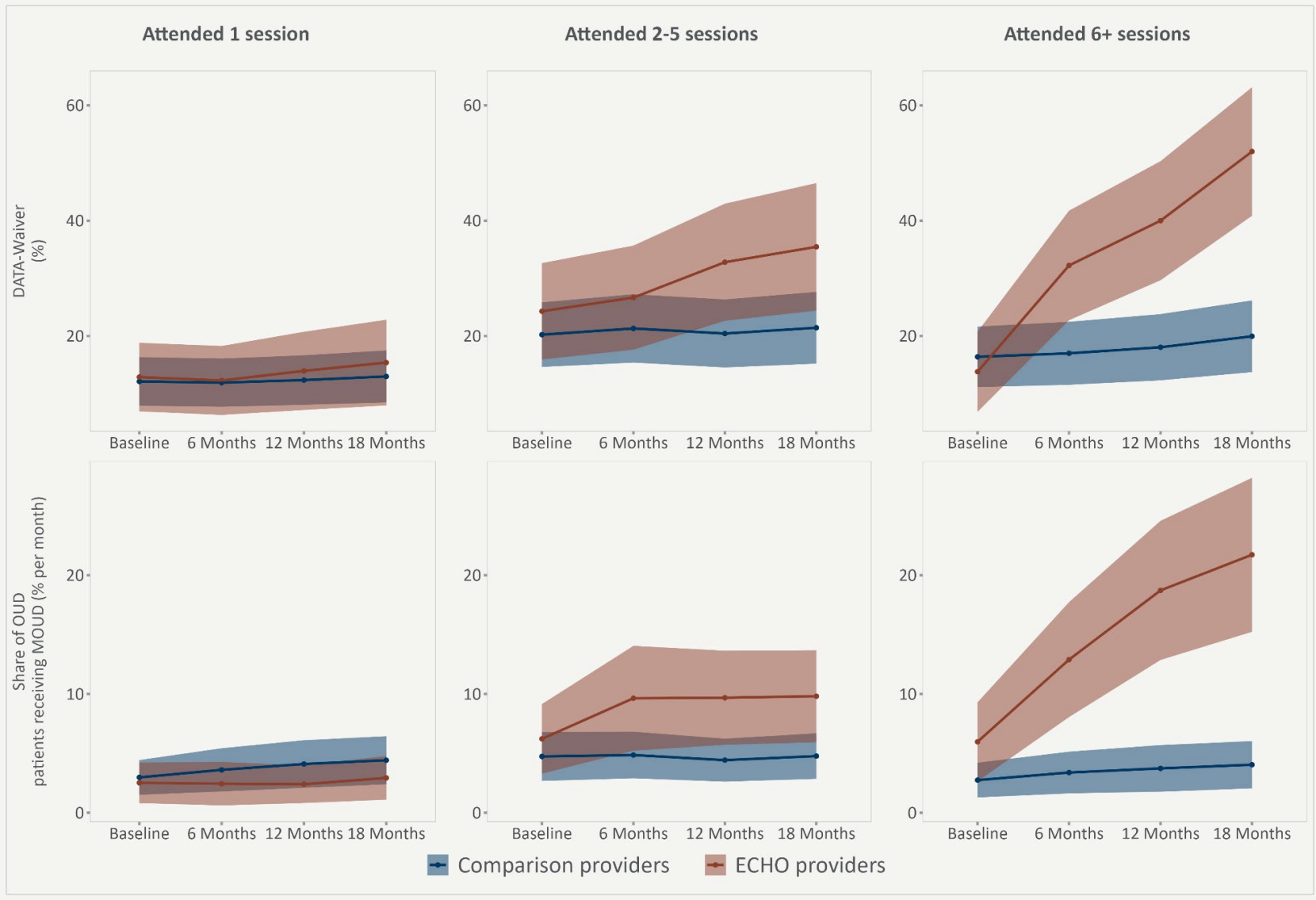
We created three groups of ECHO providers: those who attended one session (38 percent), between two and five sessions (32 percent), and six or more sessions (31 percent). Comparison providers were assigned the same number of sessions as their matched ECHO provider from the propensity score matching process. We found evidence of a significant dosage effect on four of the six outcomes: DATA-waivers, ever prescribing buprenorphine, the percentage of OUD patients prescribed buprenorphine, and the percentage of OUD patients receiving MOUD. There were no dosage effects on opioid MMEs or high-dose opioid prescriptions.

At the 18-month follow-up, ECHO providers who attended six or more sessions were more likely to obtain a DATA-waiver (52 percent) and to prescribe buprenorphine (46 percent) compared to providers who attended only one session (15 percent and 10 percent, respectively) or between two and five sessions (36 percent and 23 percent, respectively). This pattern is shown in Figure 8, top panel (a similar pattern emerged for ever/never prescribing buprenorphine, but it is not pictured). Notably, we estimate that, among ECHO providers who attended six or more sessions, the effect of ECHO attendance on obtaining DATA-waivers was a 35-percentage point increase within 18 months. There was no increase in DATA-waivers at 18 months for providers who attended just one session.

After six months, providers who attended six or more sessions provided any MOUD to 13 percent of OUD patients per month, compared with 10 percent for providers who attended between two and five sessions, and three percent for providers who only attended one session. At the 18-month follow-up, providers who attended six or more sessions provided MOUD to 22 percent of OUD patients per month, while there was no further change among either providers who attended one session or those who attended between two and five sessions (see Figure 8, bottom panel). The same pattern was evident (but not pictured) for the percentage of OUD patients receiving buprenorphine.

This exploratory analysis tells us that providers who attended six or more ECHO sessions were more likely to get waivered and prescribe higher levels of MOUD, but it cannot tell us whether ECHO *caused* providers to do these things. However, these findings do indicate that ongoing participation is likely an important component of the effectiveness of ECHO training.

Figure 8. Differences in provider impacts by number of ECHO sessions attended (one, two to five, or more than six sessions)



Length of time since first ECHO session

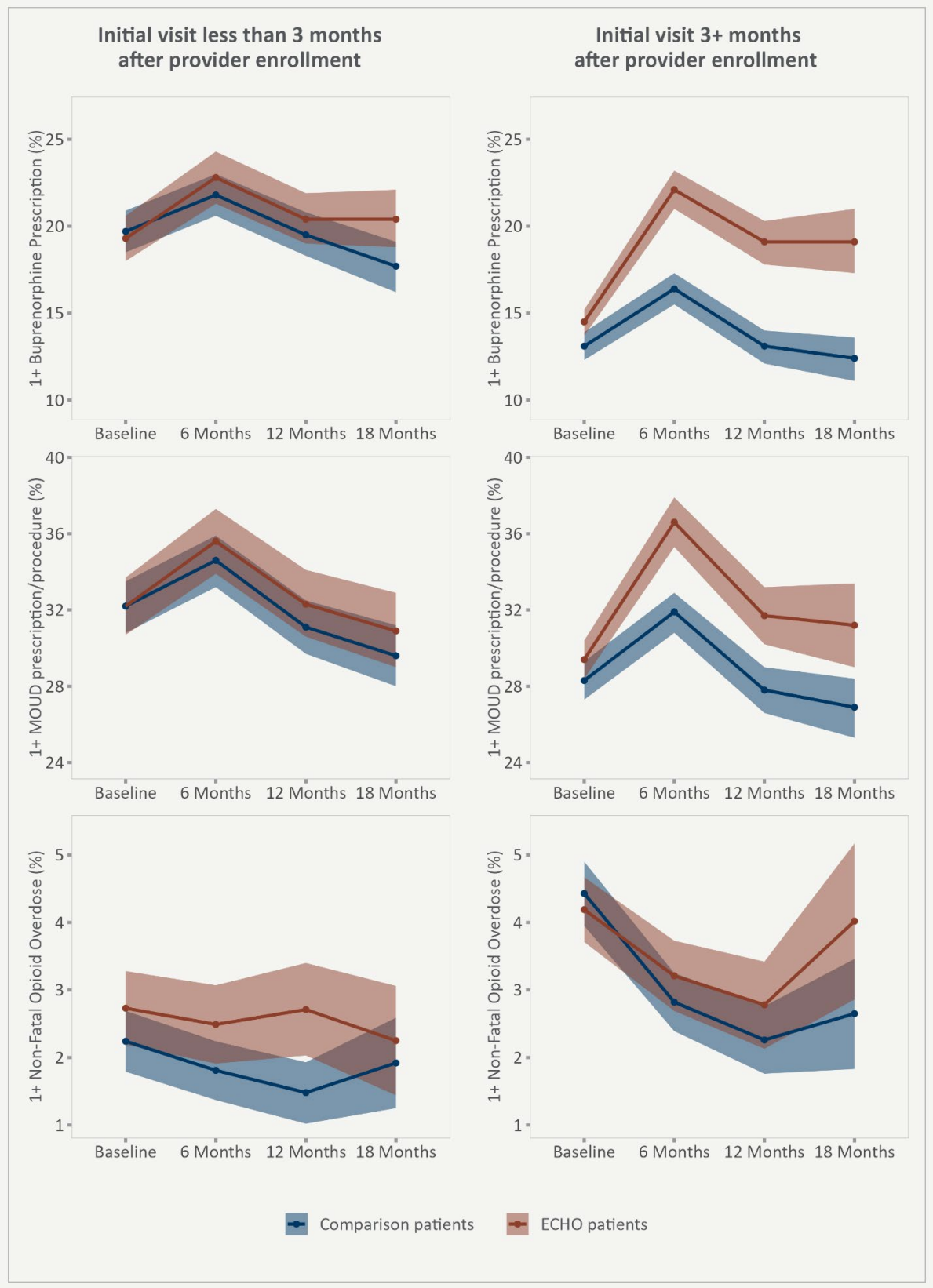
This study was designed in a way that enrolled patients on a rolling basis, meaning that some patients in our sample saw an ECHO provider shortly after the provider attended his or her first ECHO session, while other patients saw an ECHO provider many months after the provider attended his or her first ECHO session. We expect that ECHO does not take effect immediately; it takes time for providers to change their behaviors and develop new practices.

To examine this, we separated the patients into two groups: those who saw an ECHO provider less than three months after that provider's first ECHO session, and those who did not see an ECHO provider until three or more months after that provider's first ECHO session. We found that there were significant differences between these groups for receiving a buprenorphine prescription (*P* = .003; see Figure 9, top panel). There were not overall significant differences for receiving any MOUD or receiving medical care for a nonfatal overdose (see Figure 9, middle and bottom panels, respectively).

Patients who first saw their provider three or more months after initiating ECHO training were significantly more likely to receive buprenorphine than those who saw first saw their provider less than three months after initiating ECHO training. We estimate that OUD patients who had their first interaction with an ECHO provider three months or more after ECHO participation began were 4 percentage points more likely to receive a buprenorphine prescription, and 3.5 percentage points more likely to receive any MOUD, than if they had seen a comparison provider, in the first six months after study enrollment (see Figure 9, top panel).

As with the other exploratory analysis, these findings do not indicate that a longer period since ECHO initiation caused patients to have higher MOUD receipt. This is because we did not attempt to control for differences between patients who saw providers earlier versus those who saw providers later; for example, patients who saw providers earlier may have done so because they see doctors frequently, perhaps due to having greater medical needs. However, these findings are suggestive of a learning curve for providers to implement what they learn in ECHO and/or for clinics to develop necessary supports to treat patients using MOUD.

Figure 9. Differences in patient impacts by length of time (less than three months, three or more months) since provider's first ECHO session



Appendix F: Sensitivity analyses

In addition to the main analysis, we conducted four sensitivity analyses to test the robustness of our findings. Here, we briefly describe the results of these analyses; more detailed information can be requested by emailing ResultsFirstMN@state.mn.us.

Subgroup analysis by COVID-19 period

The COVID pandemic spanned the last three months of our follow-up period (March – June 2020). During this time, there were substantial shifts in health care, such as most appointments moving from in-person to virtual. Shifts in care could potentially change the patterns of our outcomes, for example, if patients encountered providers less frequently, if providers changed their prescribing patterns, or if there was a change in the prevalence of receiving medical care for nonfatal overdoses.

To examine the possibility that the COVID pandemic changed our results, we ran the main provider models again, removing data that was collected after March 1, 2020. Essentially, this tests what would happen if the study period ended on March 1, 2020 (before COVID-19) instead of June 30, 2020. We then visually compared plots of the average effects over time and their 95% confidence intervals, for models with and without the COVID time period.

None of the six provider outcomes showed patterns that were different when the COVID time period was excluded. We concluded that COVID did not change our estimates of the effects of ECHO, so we included data from March 1-June 30, 2020 in our final models.

Subgroup analysis by ECHO Hub

We explored the possibility that each hub, Hennepin and CHI-St. Gabriel's, had differential treatment effects on provider outcomes. We tested this possibility by adding a three-way interaction term to the provider models: group*time*hub. If this parameter was significant, it would indicate that the treatment effect was different for Hennepin vs. CHI-St. Gabriel's; if it was not significant, it would indicate that there were no detectable differences for providers who attended Hennepin vs. CHI-St. Gabriel's ECHO programs. Providers could attend ECHO sessions led by both hubs; for the purposes of this analysis, we assigned providers to the first hub that they attended (Hennepin = 99 providers, CHI-St. Gabriel's = 215 providers). Comparison providers were assigned to the same hub as the provider they were matched with.

The three-way interaction term was statistically significant in one out of the six provider outcome models – active use of the DATA-waiver. The follow-up analysis showed that providers who attended CHI-St. Gabriel's started out with a lower percentage of providers who had ever prescribed buprenorphine compared to providers who attended Hennepin's ECHO. Over the next 18 months, buprenorphine prescribing increased for providers in both hubs, but increased at a slightly faster rate for CHI-St. Gabriel's providers, to close the gap that was present at baseline.

We did not see the same pattern for the five other outcome measures, suggesting that overall, Hennepin and CHI-St. Gabriel's had similar effects in this study. As such, we did not distinguish between

the two hubs in our final models, and instead pooled all providers who attended at least one ECHO session at either of the hubs into the ECHO provider group.

Subgroup analysis by TOWN participation

Beginning in 2017, Minnesota's Department of Health awarded grants to eight clinics across the state under a program called Tackling Opioid Use With Networks (TOWN). The TOWN sites are funded to create coordinated clinical care teams, improve opioid prescribing culture, address unmet social service needs that create barriers to managing pain effectively, and engage community partners to address root causes of opioid abuse and addiction. As part of the grant, they were required to have a waivered provider and received intensive technical support from the CHI-St. Gabriel's ECHO Team, including in-person training and assistance, and strong encouragement for at least some providers to attend ECHO. The TOWN funding began in 2017, with a break in 2018, and picked back up again in 2019.

We wanted to estimate the potential influence that TOWN providers who attended ECHO might have on the overall ECHO impact estimates. Because of their additional funding to focus on opioids, these clinics were not representative of all clinics in Minnesota. This subgroup analysis was designed simply to ensure that our results were not being driven by the TOWN providers who attended ECHO; it was *not* designed to assess the overall effect of the TOWN program. It also did not examine the broader array of outcomes that TOWN targets, such as family-child reunification, employment, reductions in emergency department use, and a strengthened community system to respond to and prevent drug overdose.

To examine the influence of TOWN providers on ECHO impacts, we added a three-way interaction term to the provider models: group*time*TOWN. If this parameter was significant, it would indicate that the ECHO treatment effect was different for TOWN vs. non-TOWN providers; if it was not significant, it would indicate that there were no detectable differences in the impact of ECHO for providers who were affiliated with TOWN clinics vs. providers who were not.

There were no statistically significant three-way interaction terms for four of the provider outcomes (percentage of provider's OUD patients prescribed buprenorphine, percentage of provider's OUD patients prescribed any MOUD, opioid MMEs, and high-dose opioid prescriptions). The models for the other two outcomes, DATA-waivers and active use of the DATA-waiver, would not converge, likely due to difficulties with binary outcomes and non-canonical link functions in GLMMs.

These results indicate that TOWN providers did not have an outsized effect on the observed ECHO impacts for MOUD or opioid analgesic prescribing. In total, there were 39 providers who attended both ECHO and TOWN in our sample, a small number that limits our ability to detect effects. Future studies of TOWN should include measures of systems-level change indicators and a more complete sample of TOWN providers.

Analysis of all patients vs. patients with a history of OUD

For the main patient models, we focused specifically on patients who had ever been diagnosed with OUD[14] before baseline. Additional individuals may have received OUD diagnoses during the study period, but they were not included in our main analysis. We analyzed the full patient sample (not just those with prior OUD diagnoses) and examined ECHO's impact on the three patient outcomes: buprenorphine prescriptions, MOUD receipt, and receiving medical care for nonfatal overdoses. The results mirrored those that we found with the subsample of OUD-diagnosed patients, indicating that the positive effects of ECHO on buprenorphine and MOUD, and lack of effect on receiving medical care for nonfatal overdose, are not dependent on prior OUD diagnosis.

[14] Defined as any ICD-10 code beginning with F11, including all F11 subcodes (see, for example, https://www.icd10data.com/ICD10CM/Codes/F01-F99/F10-F19/F11-)