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How would implementing an Arkansas-style work requirement affect Medicaid enrollment?

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Executive summary

Congressional Republicans are expected to consider a Medicaid work requirement as part of upcoming reconciliation legislation (e.g., Sanger-Katz 2025; Hooper and Payne 2025). Proposals vary in who they apply to and what they direct enrollees to do, but they generally require adult enrollees to be employed (or engaged in another approved activity, like schooling) for some number of hours per month to remain eligible for Medicaid, subject to some exemptions.

To inform this pending debate, this paper examines one of the few precedents for such a policy: a work requirement implemented by the state of Arkansas under a waiver granted by the first Trump administration. Arkansas applied its policy for nine months, from June 2018 through February 2019, before being stopped by a court ruling. The Arkansas policy closely resembles the federal work requirement passed by the House of Representatives as part of the Limit, Save, Grow Act of 2023, a potential starting point for upcoming reconciliation legislation.

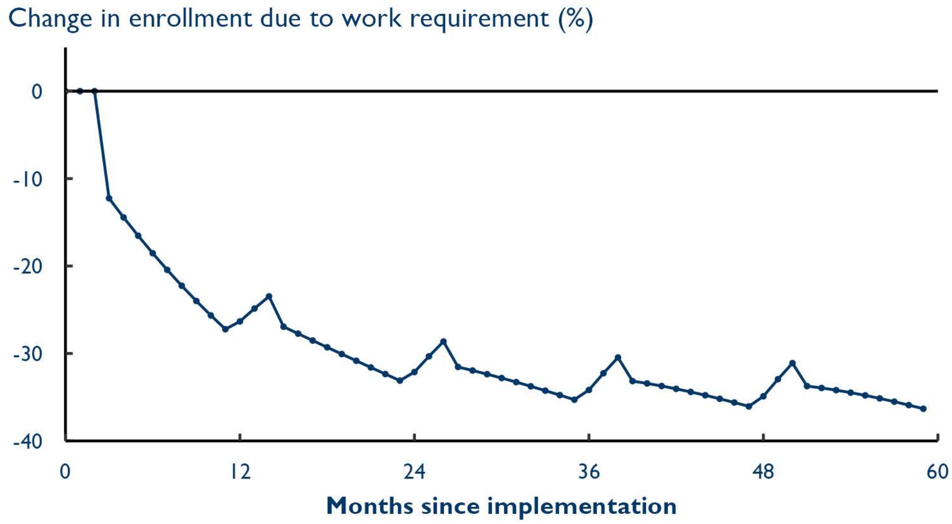
Prior research has shown that Arkansas' policy reduced Medicaid enrollment and increased uninsurance among low-income adults in Arkansas (Sommers et al. 2019; 2020; Gangopadhyaya and Karpman 2025). This research also finds that the number of enrollees that the state deemed non-compliant with the requirement far exceeded the number of enrollees who neither qualified for an exemption nor were engaged in the required activities, indicating that most enrollees lost coverage due to challenges in reporting information to the state, not true non-compliance with the policy. Meanwhile, this research finds no evidence that the Arkansas requirement increased employment among the population subject to the policy, contrary to proponents' hopes.

Because the Arkansas policy was in effect for a short time—and, indeed, was still phasing in when it was stopped in court—it is not straightforward to use the Arkansas experience to assess how this policy would have affected Medicaid enrollment over the long run. To overcome this obstacle, this paper develops a dynamic structural model suitable for using the Arkansas experience to forecast the long-run enrollment effects of the Arkansas policy and similar policies.

My main findings are as follows:

- **A work requirement like Arkansas' would reduce Medicaid enrollment by an estimated 27% at the end of the policy's first year and by 34%, on average, over the long run.** Figure ES.1 (see top of next page) plots how the simulated effect on enrollment evolves over time. Enrollment declines grow over time because, as time passes, more enrollees experience temporary spells of non-compliance that cause them to lose coverage and because removed enrollees are slow to reenroll once eligible to do so.
- **If outcomes under the 2023 House policy mirrored my estimates for Arkansas' policy, Medicaid enrollment would fall by around 4.5 million people nationwide in the long run.** Because of the close resemblance between the Arkansas policy and the 2023 House policy, my estimates offer a good starting point for assessing the impacts of this prospective federal policy. This 4.5 million estimate assumes that the policy would apply to people enrolled under the Affordable Care Act (ACA) Medicaid expansion who fall in

Figure ES.1. Effect of Work Requirement on Enrollment



Note: The figure plots the percent difference between monthly enrollment with and without a work requirement when simulating the model using the base parameter estimates. The work requirement is assumed to be implemented in January of an unspecified year. See text for details.

the 19 to 55 age range specified in the House bill; it was obtained by multiplying my estimate of a 34% long-run enrollment reduction and the Karpman, Haley, and Kenney (2025a) estimate that there will be 13.3 million Medicaid enrollees in this group in 2026.

- **States might implement an Arkansas-like policy differently than Arkansas did, which could lead to larger or smaller enrollment declines.** States might operationalize a work requirement differently due to differences in their administrative capacity or policy goals. It is unclear whether this would lead to larger or smaller enrollment declines. When Karpman, Haley, and Kenney (2025a) analyze data from New Hampshire (which paused its work requirement before beginning disenrollments) alongside data from Arkansas, they find that the New Hampshire data point to somewhat larger enrollment declines. By contrast, data from Michigan (which also paused a Medicaid work requirement shortly before beginning disenrollments) suggest somewhat smaller declines, although this could partly reflect differences in the design of Michigan's policy. In addition, other evidence suggests that Arkansas' administrative capacity was neither especially strong nor weak (Brooks, Roygardner, and Artiga 2019; Brooks et al. 2023; 2025).

As the Congressional Budget Office (CBO) emphasized in its analysis of the 2023 House policy, some states might elect to use state funds to cover enrollees who were no longer eligible for federal funding under a federal requirement (CBO 2023). If many states did so, this could substantially reduce enrollment declines, albeit at a large state cost. While predicting state decisions is beyond the scope of this analysis, there is reason to doubt that this would be the typical outcome. Before creation of the ACA Medicaid expansion option, relatively few states covered adults without children (the group at risk of disenrollment under the 2023 House policy), and when they did so, they often received at least some federal support (Heberlein et al. 2011), which would not be the case here.

- **Changes in macroeconomic conditions or differences in enrollee mix between Arkansas and other states could also affect enrollment outcomes.** Many forecasters believe that the national economy will weaken in the coming months (e.g., Santilli and DeBarros 2025). A weaker economy would likely increase Medicaid enrollment, especially among the expansion population, increasing the number of people subject to a work requirement (Jacobs, Hill, and Abdus 2017). It would likely also push the unemployment rate above where it was in Arkansas while its policy was in effect. During those months, Arkansas' unemployment rate was 3.7% or lower, lower than the national unemployment rate was in 89% of months since the year 2000. Lower employment might increase the rate at which enrollees subject to a work requirement lose coverage, perhaps most importantly by reducing the number of enrollees for whom states could use existing data on enrollee income to automatically ascertain whether enrollees are in compliance with the policy. On the other hand, there is some evidence that the Arkansas Medicaid enrollees are employed at lower rates independent of the state of the business cycle (Garfield, Rudowitz, and Damico 2018; Tolbert et al. 2025), which could operate in the other direction.

- **Work requirement policies designed differently from Arkansas' could cause larger or smaller enrollment declines.** For example, a policy that removed enrollees from Medicaid after one month of non-compliance, rather than after three months as under the Arkansas policy and the 2023 House policy, would lead to larger enrollment declines, while changes in the opposite direction would lead to smaller declines. Other changes—including changes to which enrollees are subject to the policy, to the list of available exemptions, to the activities enrollees are required to engage in, to how states ascertain compliance with the policy, or to how long removed enrollees must wait before returning to the program—could also result in markedly different enrollment effects.

- **My estimates are subject to meaningful uncertainty, but a suite of sensitivity analyses all find enrollment declines of between 23% and 43% over the long run.** The data also offer some hints that my base estimates overstate how quickly enrollees removed from the program for non-compliance would reenroll once eligible to do so, which suggests that outcomes may be more likely to fall in the upper part of this range than the lower part. The main source of uncertainty in my estimates is that developing a model suitable for estimating long-run enrollment effects requires making some strong assumptions.

- **My estimates are similar to Karpman, Haley, and Kenney's recent estimates of the effects of the 2023 House bill, but much larger than CBO's estimate for that bill.** When using data from Arkansas, Karpman, Haley, and Kenney (2025a) project that a work requirement similar to the 2023 House bill would cause 34% of those subject to the policy to lose eligibility for federal funding; this rises to 39% when using data from New Hampshire. These estimates are very similar to my estimates despite substantial differences in our analytic methods. By contrast, CBO's analysis of the 2023 House bill concluded that only 10% of those subject to the policy would lose eligibility (CBO 2023). It is unclear how CBO arrived at its estimate. However, taken together, my estimate and the Karpman, Haley, and Kenney estimate suggest that the CBO estimate may be too small.

Introduction

Congressional Republicans are expected to consider a Medicaid work requirement as part of upcoming reconciliation legislation (e.g., Sanger-Katz 2025; Hooper and Payne 2025). Work requirement proposals vary, including in who they apply to and what they require enrollees to do.[1] In general, however, such proposals require adult enrollees to be employed for a specified number of hours per month (or engaged in another approved activity, like schooling) to remain eligible for Medicaid, subject to some exemptions. Importantly, states often lack the information needed to determine whether enrollees qualify for an exemption or are engaged in the required activities, so enrollees often must report additional information to maintain eligibility.

In assessing the consequences of a Medicaid work requirement, a central question is how much it would reduce Medicaid enrollment. Larger reductions in enrollment generally result in larger reductions in insurance coverage and, in turn, larger reductions in access to care, health outcomes, and financial security (e.g., Baicker et al. 2013; Levy and Buchmueller 2025). Larger reductions in enrollment generally also result in larger reductions in program spending.

A major obstacle to answering this question is the dearth of prior experience with work requirements in Medicaid. In this paper, I focus on a notable exception: a work requirement implemented by the state of Arkansas under a waiver granted by the first Trump administration, which was applied for a nine-month period from June 2018 through February 2019 before being stopped by a federal court ruling.[2] Conveniently, the Arkansas policy is very similar in structure to the federal work requirement passed by the House of Representatives as part of the Limit, Save, Grow Act of 2023, which is a likely starting point for pending reconciliation legislation.

Prior research has shown that Arkansas disenrolled a large number of people from Medicaid under its policy (Rudowitz, Musumeci, and Hall 2019; Hill and Burroughs 2019), which markedly increased the uninsured rate among low-income adults in Arkansas (Sommers et al. 2019; 2020; Gangopadhyaya and Karpman 2025). However, because the Arkansas policy was in effect for such a short time—and, indeed, was still phasing in when it was stopped in court—it is not easy to use this short-run experience to predict how such a policy would affect Medicaid enrollment over the long run. Because the state assessed enrollees' compliance with the requirement on an ongoing basis, enrollment declines would likely have grown over time as more enrollees experienced episodes of non-compliance. However, this would have been mitigated to some

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[1] For a recent comparison of work requirement proposals, see Karpman, Haley, and Kenney (2025a).

[2] While the Trump administration approved work requirement waivers in 13 states, Arkansas' was one of only two that were ultimately implemented (Hinton, Raphael, and Diana 2024). The other is Georgia's, which was initially approved by the Trump administration, then rescinded by the Biden administration, and finally implemented in July 2023 after the state successfully challenged the rescission in court. Georgia's waiver is not suitable for isolating the effect of a work requirement because it was implemented as a component of a new program expanding Medicaid eligibility to certain low-income adults. However, the very low enrollment observed under the program date may at least partly reflect the program's stringent work requirement. See Chan (2024) for a detailed discussion of the Georgia program.

extent by the fact that enrollees who were removed from Medicaid could return to the program at the start of the next calendar year and by the entry of new enrollees via normal "churn." The state was also just beginning to apply the requirement to some groups of Medicaid enrollees, and these groups may have lost coverage at different rates than those already subject to the policy.

To overcome this problem, I develop a dynamic structural model of how a requirement like Arkansas' affects enrollment outcomes, and I estimate the parameters of that model using aggregate data reported by the state during the nine-month implementation period. I then use the estimated model to simulate how Arkansas' work requirement would have affected Medicaid enrollment if in place permanently, which can then provide a rigorous basis for forecasting how similar federal policies would affect Medicaid enrollment over the long run.

The rest of this paper proceeds as follows. I first provide some background on the Arkansas policy and how it compares to a prospective federal policy, after which I describe the monthly state enrollment reports that form the foundation of my analysis. I then present a series of descriptive analyses of trends under the policy that help motivate my modeling approach. Next, I present and estimate my model and use it to simulate the long-run effects of the Arkansas policy. I close by discussing what my results imply about the effects of a federal work requirement.

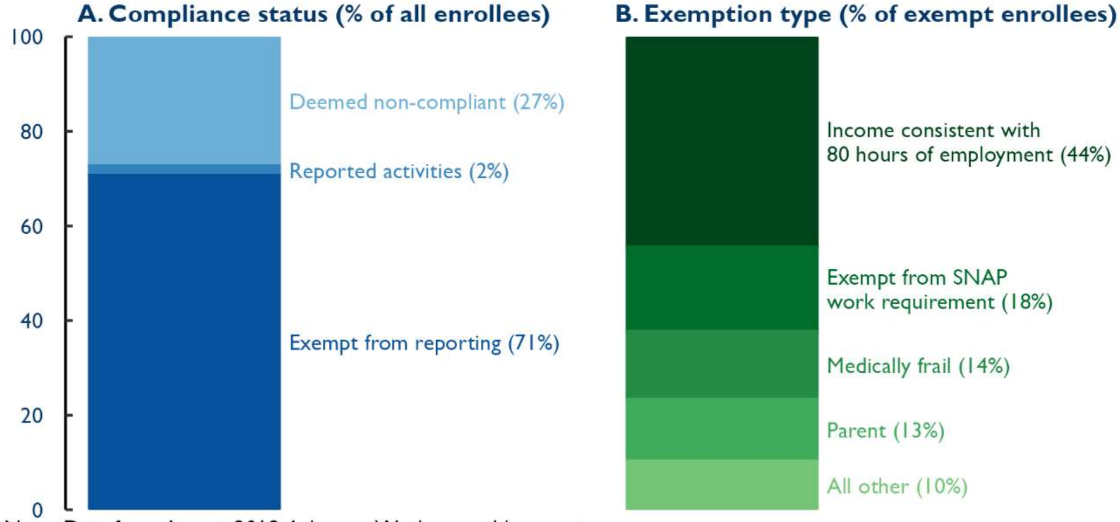
Background on Arkansas' Medicaid Work Requirement

Arkansas adopted the Affordable Care Act (ACA) Medicaid expansion in January 2014 as part of the initial group of states adopting expansion. In June 2018, Arkansas began phasing in a work requirement for people ages 19 to 49 enrolled in its expansion program, then called Arkansas Works, under a waiver granted by the first Trump administration (Arkansas Department of Human Services 2018; 2019). In what follows, I refer to expansion enrollees ages 19 to 49 as the population "subject to the policy" or "requirement," following Karpman, Haley, and Kenney (2025a).

Arkansas phased in its work requirement policy gradually. Enrollees ages 30 to 49 with income below 100% of the federal poverty level (FPL) became subject to the policy in four groups from June 2018 through September 2018, while those with income above 100% of the FPL became subject in January 2019. Enrollees ages 19 to 29 were scheduled to phase in from January 2019 through June 2019, but this phase-in was cut short by a federal court ruling in March 2019 that the Trump administration's approval of Arkansas' waiver violated federal law. February 2019 was the final month for which the state assessed enrollees' compliance with the policy.

Many enrollees subject to the policy qualified for an exemption from its main requirements. Exemptions were available for having income "consistent with" working 80 hours per week, as well as being a full-time student, medically frail or otherwise medically unfit for employment, pregnant, a parent, or a caregiver for an incapacitated person. People exempt from work requirements in the Supplemental Nutrition Assistance Program (SNAP), enrolled in an alcohol and drug treatment program, or receiving unemployment compensation or certain cash

Figure 1. Arkansas Work Requirement Compliance Status, August 2018



Note: Data from August 2018 Arkansas Works monthly report.

assistance were also exempt.[3] The state could sometimes determine exemption eligibility using data it already held, but enrollees had to proactively demonstrate their eligibility in other cases. In some cases, enrollees had to reconfirm exemption eligibility periodically (e.g., every two months).

Enrollees not receiving an exemption were required to engage in and report on at least 80 hours per month of "work or community engagement activities," which could consist of a combination of employment, formal education, volunteering, job search, health education classes, or activities that satisfy work requirements in SNAP.[4] Enrollees who failed to report for a month were deemed non-compliant with the requirement for that month. (Here and throughout, I again follow Karpman, Haley, and Kenney (2025a) in using the term "deemed non-compliant" or simply "non-compliant" to refer to enrollees subject to the policy who did not receive an exemption or report suitable activities for a month.) Enrollees who were deemed non-compliant for three months in a calendar year, whether consecutive or non-consecutive, were disenrolled effective the beginning of the next month, and they were not eligible to reapply until the beginning of the next calendar year.

Figure 1 depicts enrollees' compliance status as of August 2018, the last month before Arkansas began removing enrollees from the program for non-compliance. Approximately 71% of enrollees subject to the policy received an exemption; almost all other enrollees were deemed non-compliant, as only 2% of enrollees successfully reported acceptable activities. Four exemption

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[3] Arkansas also exempted American Indians and Alaska Natives, although it stated that it intended to eliminate that exemption at a later date. In August 2018, 2% of enrollees were exempt for this reason.

[4] The state set out special rules for counting the "work activity" hours involved in many activities. For example, employment hours were measured by dividing the enrollee's reported income by the Arkansas minimum wage, and instructional hours for those attending school or training were generally counted as multiple hours of work activity (generally 2.5 or 3). Additionally, job search activities could only be counted up to 39 hours per month, while health education classes could only be counted up to 20 hours per year.

types accounted for 90% of the exemptions granted, with close to half of those exemptions being for having income consistent with 80 or more hours of employment.

Notably, survey estimates suggest that only 4% of low-income adults in the age group targeted by the Arkansas policy during 2018 were neither eligible for an exemption nor engaged in activities that would satisfy the work requirement (Sommers et al. 2019). This fraction is far smaller than the 27% share of enrollees deemed non-compliant in August 2018. While the surveyed population may not have exactly aligned with the population subject to the policy, this is a clear indication that many enrollees deemed non-compliant were, in fact, satisfying the policy's substantive requirements and merely failed to demonstrate that to the state. This may reflect the fact that many enrollees reported confusion about whether reporting was required and difficulty navigating the state's reporting system, which was widely viewed as complex and poorly designed (Musumeci, Rudowitz, and Hall 2018; Sommers et al. 2019; Hill and Burroughs 2019).

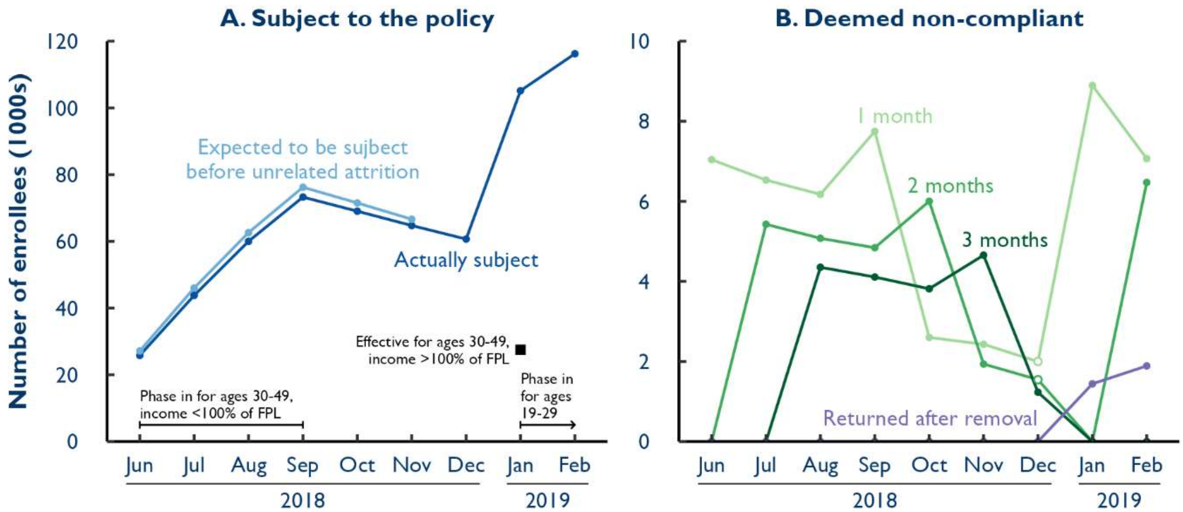
Prior research has documented that a large number of enrollees were disenrolled from Medicaid under Arkansas' policy (e.g., Rudowitz, Musumeci, and Hall 2019; Hill and Burroughs 2019; Wagner and Schubel 2020). Research that compares trends in Arkansas to trends in neighboring states shows that these disenrollments translated into a reduction in the share of people in the targeted population with Medicaid and a corresponding increase in the share who were uninsured (Sommers et al. 2019; 2020; Gangopadhyaya and Karpman 2025). The same studies find no evidence that the policy increased employment, which was one of the state's key rationales for implementing the policy (Arkansas Department of Human Services 2018). The finding that Arkansas' reduced program enrollment without improving labor market outcomes mirrors the findings of studies of work requirements in SNAP (Bauer and East 2025).

The federal work requirement included in the Limit, Save, Grow Act of 2023, which was passed by the House of Representatives in April 2023, closely mirrored the Arkansas policy. Both make three months of non-compliance the threshold for disenrollment and bar reenrollment through the end of the calendar year. They also exempt similar populations and have a similar list of acceptable work and related activities. Additionally, the House-passed bill required states to attempt to use existing data to assess compliance before requesting additional information from enrollees, something Arkansas aimed to do under its policy. The House-passed proposal does encompass a somewhat broader population, as it would apply to enrollees through age 55, versus age 49 under the Arkansas policy. It might also encompass some enrollees outside the ACA expansion population, although whether this would be the case in practice is not clear (Karpman, Haley, and Kenney 2025a). Karpman, Haley, and Kenney (2025a) provide a more comprehensive comparison of the Arkansas policy, the Limit, Save, Grow Act, and various other recent state policies.

Data

This analysis relies on monthly Arkansas Works reports published by the Arkansas Department of Health and Human Services, which provide aggregate information on outcomes under the

Figure 2. Arkansas Work Requirement Data, June 2018-February 2019



Note: Data from Arkansas Works monthly reports. Hollow markers indicate that the data were imputed. See text for details.

state's work requirement for the period it was in effect.[5] The state published these reports for each month the requirement was in effect: June 2018 through February 2019.

Each report provides the number of enrollees subject to the policy in that month. It generally also provides the number of enrollees deemed non-compliant in that month, disaggregated by whether they have accumulated one, two, or three months of non-compliance in the current year. The exception is the December 2018 report, which reports the number of enrollees who accumulated three months of non-compliance (and, thus, were being disenrolled), but not the number who had accumulated one or two months of non-compliance. While it is feasible to proceed without the missing data, I elect to impute them because it simplifies the exposition; however, I present sensitivity analyses that exclude the imputed data. Appendix A describes my imputation method.

The reports for June 2018 through November 2018 also provide the number of enrollees who were enrolled on a date early in the prior month and who would have been subject to the policy in the report month but were disenrolled before the end of the month for reasons unrelated to the work requirement (e.g., an income change or failure to return renewal paperwork), which is useful for estimating the baseline attrition rate without a work requirement. Additionally, reports for January and February 2019 provide the number of current enrollees who returned to the program after being removed due to non-compliance with the work requirement during 2018.

Figure 2 depicts all the raw data I rely upon. Panel A shows that the number of enrollees subject to the policy rose steadily from June 2018 through September 2018 as the policy phased in for enrollees ages 30 to 49 with income below 100% of the FPL. This number then fell through the rest of 2018 as some enrollees were removed for non-compliance before jumping in January 2019 as the policy took effect for enrollees ages 30 to 49 with incomes above 100% of the FPL and

[5] These reports are available for download at https://humanservices.arkansas.gov/newsroom/medicaid-arworks-and-other-reports/.

began phasing in for enrollees ages 19 to 29. The gap between the two lines in Panel A also shows that a modest fraction of enrollees expected to be subject to the policy in any given month disenroll for reasons unrelated to the work requirement prior to the end of that month.

Panel B depicts how the number of enrollees deemed non-compliant evolved over time. I highlight three features of these trends that help motivate the model laid out in the next section. First, the number of enrollees who accrue a first month of non-compliance is largest in months where many enrollees become subject to the policy (June to September 2018 and January to February 2019), but still sizeable in other months. This suggests that many enrollees are non-compliant at entry but also that some enrollees transition from compliance to non-compliance over time. Second, the number of enrollees with two months of non-compliance in a given month is similar to but somewhat smaller than the number with one month of non-compliance in the prior month; for example, the number of enrollees with two months of non-compliance in July 2018 is about 77% of the number with one month of non-compliance in June 2018. A similar pattern holds for the number with three months of non-compliance in a month versus the number with two months of non-compliance in the prior month. This pattern suggests that most, but not all, enrollees who are non-compliant in one month remain non-compliant in the next month. Third, while some enrollees who were removed for non-compliance during 2018 did return to the program during early 2019, the cumulative number of returning enrollees was a small fraction of the total number of enrollees removed during 2018, suggesting that enrollees return at a relatively slow pace.

Model and estimation

This section presents the model I use to simulate the long-run enrollment effects of an Arkansas-style work requirement and then discusses how I estimate the parameters of that model using the monthly data described above. I begin with a high-level overview of my approach.

In the model, each enrollee enters the program in either a compliant or a non-compliant state, and enrollee compliance status then evolves probabilistically in each subsequent month. Enrollees who accrue three months of non-compliance in a calendar year are disenrolled but begin to reenroll probabilistically once eligible to do so at the start of the next calendar year. Additionally, some enrollees probabilistically attrit from the program each month for reasons unrelated to the work requirement (e.g., an income change or a failure to return renewal paperwork).

I estimate the model parameters using a minimum distance procedure that finds the parameters that align the model's predictions and the observed data as closely as possible. I seek to match three main features of the data. The first is the share of enrollees who leave the program for reasons unrelated to the work requirement in each month. Naturally, this allows me to estimate the rate at which enrollees attrit from the program for reasons unrelated to the work requirement.

The second is the share of enrollees with 1, 2, or 3 months of non-compliance in each month. The share of enrollees with 1 month of non-compliance provides information about two types of parameters: the share of enrollees who are non-compliant at program entry and the rate at which existing enrollees transition from compliance to non-compliance over time. I can separately estimate the two types of parameters because the number of people entering the program varies markedly over time due to how Arkansas phased in its policy, so the degree to which these shares reflect each parameter varies over time as well. In months with many new entrants, this share mostly reflects the share of enrollees who are non-compliant at entry, and in months with few new entrants, this share mainly reflects the rate at which existing enrollees transition from compliance to non-compliance. The share of enrollees with 2 or 3 months of non-compliance in a month (relative to the share with 1 or 2 months in the prior month) then allows me to estimate the rate at which enrollees transition from non-compliance to compliance over time.

The third relevant feature of the data is the share of enrollment during 2019 accounted for by enrollees who were disenrolled during 2018. This is the feature of the data that allows me to estimate the rate at which enrollees disenrolled for non-compliance return to the program.

The remainder of this section provides full details on the model and estimation.

Model

Each enrollee i becomes subject to the work requirement in some month M_i, which is either when the enrollee enters Medicaid or when the phase-in of the requirement reaches enrollee i (if later). Enrollees lose eligibility for reasons unrelated to the work requirement at a monthly rate β. Thus, letting E_{im}^0 denote counterfactual enrollment status without a work requirement for enrollee i, the probability of enrollment for any $m \geq M_i$ is given by $\mathbb{P}(E_{im}^0 = 1 \mid M_i) = (1-\beta)^{m-M_i}$.

Enrollment status with a work requirement, which I denote E_{im}^1, depends on compliance with the requirement in addition to the factors driving baseline attrition. I let $D_{im} \in \{0,1\}$ be an indicator for being deemed non-compliant in month m. Compliance evolves via a Markov process. People who are compliant in month m become non-compliant in month $m+1$ with probability ρ_{01}, while those who are non-compliant in month m become compliant in month $m+1$ with probability ρ_{10}. I assume that enrollees from entry cohort $M_i = m$ start in a non-compliant state with probability λ_m, where the subscript m allows this probability to vary by entry cohort.

Enrollment status in month m depends on the enrollee's compliance history, which I capture in a state variable $H_{im} \in \{0,1,2,3,L\}$, where $L > 3$. The state $H_{im} = L$ captures people who lost eligibility due to non-compliance in a prior calendar year and have not returned, while for other enrollees H_{im} corresponds to the number of months of non-compliance in the current calendar year, up to the maximum of 3. Individuals are non-enrolled in month m if $H_{im} \in \{3, L\}$.[6]

I assume that individuals who were disenrolled during the prior calendar year return to the program with a probability δ_F the first time that they are eligible to do so and a probability δ_O in all later months. Thus, the dynamics of H_m for $m > M$ when m is not a January are

$$H_{im} = \begin{cases} \min\{H_{i,m-1} + D_{i,m-1}, 3\} & \text{if } H_{i,m-1} \neq L \\ 0 \text{ w/ prob. } \delta_O, \ \ L \text{ w/ prob. } 1-\delta_O & \text{otherwise} \end{cases},$$

[6] When person i is not enrolled, the random variables D_{im} and H_{im} continue to evolve as described and can be viewed as capturing person i's "latent" compliance status.

and the dynamics when m is a January are

$$H_{im} = \begin{cases} 0 & \text{if } H_{i,m-1} + D_{i,m-1} \leq 2 \\ 0 \text{ w/ prob. } \delta_O, \ \ L \text{ w/ prob. } 1-\delta_O & \text{if } H_{i,m-1} = L \\ 0 \text{ w/ prob. } \delta_F, \ \ L \text{ w/ prob. } 1-\delta_F & \text{otherwise} \end{cases}.$$

Under the assumptions laid out above, the tuple (D_{im}, H_{im}) is a Markov chain. For any month m that is not a January, the transition probabilities of the Markov chain are given by

$$\mathbb{P}\big(D_{im} = d', H_{im} = h' \mid D_{i,m-1} = d, H_{i,m-1} = h\big) = \rho_{dd'} \begin{cases} 1 & \text{if } h' = \min\{h+d, 3\}, h \neq L \\ \delta_O & \text{if } h' = 0, h = L \\ 1-\delta_O & \text{if } h' = h = L \\ 0 & \text{otherwise} \end{cases},$$

where I have made the definitions $\rho_{11} = 1 - \rho_{10}$ and $\rho_{00} = 1 - \rho_{01}$.

For a month m that is a January, the transition probabilities are given by

$$\mathbb{P}\big(D_{im} = d', H_{im} = h' \mid D_{i,m-1} = d, H_{i,m-1} = h\big) = \rho_{dd} \begin{cases} 1 & \text{if } h' = 0, h+d \leq 2 \\ \delta_O & \text{if } h' = 0, h = L \\ 1-\delta_O & \text{if } h' = L, h = L \\ \delta_F & \text{if } h' = 0, h \neq L, h+d \geq 3 \\ 1-\delta_F & \text{if } h' = L, h \neq L, h+d \geq 3 \\ 0 & \text{otherwise} \end{cases}$$

The initial state probabilities for enrollees in cohort $M_i = k$, which I denote by $\pi_k(d,h)$, take the form $\pi_k(0,0) = 1 - \lambda_k$, $\pi_k(1,0) = \lambda_k$ and $\pi_k(d,h) = 0$ otherwise. Probabilities of the form $\mathbb{P}(D_{im} = d, H_{im} = h \mid M_i = k)$ can then be calculated by iteratively multiplying the transition matrices implied by the probabilities specified above to the initial state vector π_k.

I assume that baseline attrition unfolds independent of the Markov chain (D_{im}, H_{im}).[7] Thus, with a work requirement in effect, a member of cohort M_i is enrolled in $m \geq M_i$ with probability

$$\mathbb{P}\big(E^1_{im} = 1 \mid M_i\big) = (1-\beta)^{m-M_i}\mathbb{P}(H_{im} \leq 2 \mid M_i),$$

and the joint probability of being enrolled and in a state with $D_{im} = d$ and $H_{im} = h \leq 2$ is given by

$$\mathbb{P}\big(E^1_{im} = 1, D_{im} = d, H_{im} = h \mid M_i\big) = (1-\beta)^{m-M_i}\mathbb{P}(D_{im} = d, H_{im} = h \mid M_i).$$

It will also be useful to calculate the probability that a 2019 enrollee is a "returnee," meaning a person who had been removed for non-compliance during 2018 but then returned to the program. For a member of cohort $M_i = k$ in a month m in 2019, this probability is given by

$$\mathbb{P}\big(E^1_{im} = 1, H_{\text{i,Dec18}} + D_{\text{i,Dec18}} \geq 3 \mid M_i\big) = (1-\beta)^{m-M_i}\mathbb{P}\big(H_{im} \leq 2, H_{\text{i,Dec18}} + D_{\text{i,Dec18}} \geq 3 \mid M_i\big).$$

[7] An equivalent formulation would be to augment the Markov chain (D_{im}, H_{im}) with an additional absorbing state $D_{im} = H_{im} = B > L$ that captures enrollees who have attritted out for reasons unrelated to the work requirement. In this modified structure, the transition probabilities among the existing states are reduced by a factor of $1 - \beta$, and each state gains a probability β of transitioning to the absorbing attrition state. This form of the model is modestly easier to work with for computational purposes.

The second term on the right-hand side can be calculated by defining a vector of probabilities $\pi_k^L(d,h)$ where $\pi_k^L(d,h) = \mathbb{P}\big(D_{i,\text{Dec18}} = d, H_{i,\text{Dec18}} = h \mid M_i = k\big)$ whenever $h + d \geq 3$ and $\pi_k^L(d,h) = 0$ otherwise. Applying the relevant transition matrices to the vector π_k^L then yields the joint probability of being disenrolled in December 2018 and in each state of interest in month m.

To facilitate estimation of the model, I place restrictions on the λ_m parameters, which capture the probability that a member of entry cohort m is non-compliant at entry. Specifically, I assume that λ_m takes the same value for all 2018 entry cohorts but allow λ_m to take a different value for each of the two 2019 cohorts. These constraints on λ_m are structured to accommodate variation in enrollee characteristics across entry cohorts due to how Arkansas phased in its policy. The 2018 entry cohorts consisted solely of enrollees ages 30 to 49 with incomes below 100% of the FPL. By contrast, the January 2019 cohort included a large group of enrollees ages 30 to 49 with income above 100% of the FPL plus an initial group of enrollees ages 19 to 29, and the February 2019 cohort contained an additional group of enrollees ages 19 to 29. (Both the January 2019 and February 2019 cohorts also included normal inflows from the groups phased in previously.)

Estimation

I estimate the model using a minimum distance approach. Recall that a minimum distance estimator of a parameter vector α is based on a sequence of vector-valued functions of the data and parameters $\hat{g}(\alpha)$ such that, at the true parameter value α_0, $\hat{g}(\alpha_0) \to 0$ in probability as the sample size becomes large. The minimum distance estimator of the parameter vector α is then the vector that minimizes the function $\hat{g}(\alpha)'\widehat{W}\hat{g}(\alpha)$, where $\widehat{W}$ is a potentially data-dependent positive-semi-definite matrix. For simplicity, I use $\widehat{W} = I$ in what follows. For a comprehensive review of the theory of minimum distance estimation, see Newey and McFadden (1994).

In this application, the function $\hat{g}$ is a vector of differences between various observed statistics and the model-predicted value of those statistics given the parameters. To specify $\hat{g}$, it is useful to establish some notation to refer to data elements from the Arkansas Works monthly reports. To that end, I let S_m denote the number of enrollees subject to the work requirement policy in month m, and I let N_m^j denote the number of enrollees who did not comply with the requirement in the month m and who have accumulated $j \in \{1,2,3\}$ months of non-compliance as of the end of the month. Similarly, I let R_m denote the number of enrollees in month m who were disenrolled during 2018 and then returned to the program during 2019. Finally, I let A_m denote the number of enrollees who attrit out of the program for reasons unrelated to the work requirement between early in month $m-1$ and the end of month m.

I then define the components of the function $\hat{g}$ as follows:

- <u>Baseline attrition rate</u>: The first set of components corresponds to the baseline attrition rate. For each month m where A_m is observed (June to November 2018), I define:

$$\hat{g}_m^1(\beta) = \frac{S_m}{A_m + S_m} - (1-\beta)^{53/30}.$$

The $1 - \beta$ term is raised to the 53/30 power because, as reported by the state, A_m captures attrition over a period that is typically roughly one week less than two months.

- Entry cohort sizes: For reasons that will become clear below, estimating the remaining model parameters requires estimating how many enrollees become subject to the work requirement policy in each month. Formally, it requires knowing the (relative) entry cohort sizes $\theta_m \equiv \mathbb{P}(M_i = m \mid M_i \in \mathcal{M})$, where $\mathcal{M}$ is the set of months the work requirement was in effect (i.e., June 2018 through February 2019).

  A challenge is that I do not observe the number of enrollees who become subject to the policy in each month m, which I denote C_m. However, C_m can be approximated by $\hat{C}_m = S_m - (1-\beta)(S_{m-1} - N_m^3) - (R_m - [1-\beta]R_{m-1})$, since the number of new enrollees in month m equals the total number of enrollees, S_m, less the number of continuing enrollees, $(1-\beta)(S_{m-1} - N_m^3)$, as well as the number of new returnees, $R_m - [1-\beta]R_{m-1}$.

  For each entry cohort $m \in \mathcal{M}$, I then define:

$$\hat{g}_m^2(\beta, \theta_m) = \frac{\hat{C}_m}{\sum_{m'} \hat{C}_{m'}} - \theta_m.$$

- Non-compliance rates: The next components correspond to the share of enrollees who are non-compliant in month m with $j \in \{1,2,3\}$ months of accumulated non-compliance:

$$\hat{g}_{mj}^3(\beta, \{\theta_m\}, \rho_{01}, \rho_{10}, \{\delta_t\}, \{\lambda_m\}) = \frac{N_m^j}{S_m} - \mathbb{P}(D_{im} = 1, H_{im} = j-1 \mid E_{im}^1 = 1).$$

  The theoretical share can be written in terms of cohort-specific probabilities as

$$\mathbb{P}\big(D_{im} = 1, H_{im} = j-1 \mid E_{im}^1 = 1\big) = \frac{\sum_{k \in \mathcal{M}} \theta_k \mathbb{P}\big(E_{im}^1 = 1, D_{im} = 1, H_{im} = j-1 \mid M_i = k\big)}{\sum_{k \in \mathcal{M}} \theta_k \mathbb{P}\big(E_{im}^1 \mid M_i = k\big)}.$$

  The cohort-specific probabilities can then be computed in terms of the underlying model parameters using the iterative method described above.

- Returnee rates: The final components of $\hat{g}$ correspond to the share of enrollees who have returned to the program after being removed for non-compliance. For months m that are in calendar year 2019, these components take the form:

$$\hat{g}_m^4(\beta, \{\theta_m\}, \rho_{01}, \rho_{10}, \{\delta_t\}, \{\lambda_m\}) = \frac{R_m}{S_m} - \mathbb{P}\big(H_{\mathrm{i,Dec18}} + D_{\mathrm{i,Dec18}} \geq 3 \mid E_{im}^1 = 1\big)$$

  For earlier months, they are identically zero. For months in 2019, the theoretical share can be written in terms of cohort-specific probabilities as

$$\mathbb{P}\big(H_{\mathrm{i,Dec18}} + D_{\mathrm{i,Dec18}} \geq 3 \,\big|\, E_{im}^1 = 1)$$

$$= \frac{\sum_{k=\text{Jun18}}^{\text{Dec18}} \theta_k \mathbb{P}\left(E_{im}^1 = 1, H_{\text{i,Dec18}} + D_{\text{i,Dec18}} \geq 3 \mid M_i = k\right)}{\sum_{k=\text{Jun18}}^{\text{Dec18}} \theta_k \mathbb{P}\left(E_{im}^1 \mid M_i = k\right)}.$$

Those probabilities can again be computed using the iterative method described above.

It is easy to see that, at the true parameter values, the vector $\hat{g} = \left[\{\hat{g}_m^1\}, \{\hat{g}_m^2\}, \{\hat{g}_{mj}^3\}, \{\hat{g}_m^4\}\right]$ converges to zero in probability as enrollment becomes large, as required.[8]

While it would be feasible to estimate the model parameters in a single step by minimizing the function $\hat{g}'\hat{g}$, it is computationally more convenient to use a sequential estimation procedure.[9] First, I estimate β using only the components $\hat{g}_m^1$. Second, I plug the resulting estimate of β into the functions $\hat{g}_m^2$ and obtain minimum distance estimates of θ_m based on $\hat{g}_m^2(\hat{\beta}, \theta_m)$. Third, I plug the estimates of β and θ_m into $\hat{g}_m^3$ and $\hat{g}_m^4$ and obtain minimum distance estimates of ρ_{01}, ρ_{10}, $\{\delta_t\}$, and $\{\lambda_m\}$ based on $\hat{g}_{mj}^3(\hat{\beta}, \{\hat{\theta}_m\}, \rho_{01}, \rho_{10}, \{\delta_t\}, \{\lambda_m\})$ and $\hat{g}_m^4(\hat{\beta}, \{\hat{\theta}_m\}, \rho_{01}, \rho_{10}, \{\delta_t\}, \{\lambda_m\})$.

Appendix B describes how I calculate standard errors.

Estimation results

Table 1 (next page) reports the resulting estimates. I focus on the base estimates. The estimated baseline attrition rate β, which captures the rate at which enrollees churn out of the program for reasons unrelated to the work requirement, is 2.3% per month. The estimates of ρ_{01} and ρ_{10} indicate enrollees move between compliance and non-compliance over time, but at a moderate rate: 4.2% of enrollees who are compliant in one month transition to non-compliance in the next month, while 20.3% of enrollees who are non-compliant in one month transition to compliance in the next month. Removed enrollees who are eligible to return to the program are estimated to do so at an 8.4% rate in the first month eligible (δ_F) and at a lower 3.4% monthly rate (δ_O) thereafter.

The share of enrollees who enter the program in a non-compliant state, which is captured by the λ_m parameters, varies widely across entry cohorts. This pattern likely reflects differences in income mix across entry cohorts driven by the program phase-in schedule. As noted above, the

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[8] One subtlety is that the $\hat{g}_m^2$ components depend on the *approximated* cohort sizes $\hat{C}_m$ rather than the true cohort sizes C_m. However, note that

$$\frac{\hat{C}_m}{\sum_{k\in\mathcal{M}} C_k} = \frac{C_m}{\sum_{k\in\mathcal{M}} C_k} + \frac{S_m - C_m - \tilde{R}_m - (1-\beta)(S_{m-1} - N_{m-1}^3)}{\sum_{k\in\mathcal{M}} C_k} + \frac{R_m - \tilde{R}_m - (1-\beta)R_{m-1}}{\sum_{k\in\mathcal{M}} C_k},$$

where $\tilde{R}_m$ is the number of new returnees in month m. The first term clearly converges to θ_m in probability as $\sum_{k\in\mathcal{M}} C_k \to \infty$, while the fact that $S_m - C_m - \tilde{R}_m \sim \text{binom}(S_{m-1} - N_{m-1}^3, 1-\beta)$ and $R_m - \tilde{R}_m \sim \text{binom}(R_{m-1}, 1-\beta)$ implies that the variance of the latter two terms converges to zero as $\sum_{k\in\mathcal{M}} C_k \to \infty$ and, thus, that each of those terms converges to zero in probability as $\sum_{k\in\mathcal{M}} C_k \to \infty$. The conclusion that $\hat{g}_m^2$ has the desired convergence properties then follows immediately.

[9] If the preliminary steps deliver consistent estimators of the relevant parameters, then plugging those estimates into the latter components of $\hat{g}$ will still generate conditions that converge to zero in probability as enrollment gets large. Newey and McFadden (1994) discuss sequential estimation in the context of generalized method of moments estimators, but the basic idea carries over directly to this setting.

Table 1: Parameter Estimates

Parameter	Rate of…	Base Estimates		Ex. Imputed Data	
		Estimate	Std. Err.	Estimate	Std. Err
β	Baseline attrition	0.023	0.000	0.023	0.000
ρ_{01}	Transition into non-compliance	0.042	0.001	0.046	0.001
ρ_{10}	Transition into compliance	0.203	0.002	0.206	0.002
δ_F	Return post-removal, Initial	0.084	0.002	0.083	0.002
δ_O	Return post-removal, Other	0.034	0.001	0.033	0.001
λ_{2018}	Entry non-compliance, 2018	0.281	0.002	0.280	0.002
λ_{Jan19}	Entry non-compliance, Jan. '19	0.047	0.003	0.038	0.003
λ_{Feb19}	Entry non-compliance, Feb. '19	0.221	0.010	0.192	0.009

Note: The table reports the results from estimating the model using the minimum distance procedure described in the text. The base estimates are obtained using the full dataset, including the two imputed data points for December 2018, while the other estimates are obtained using a dataset that excludes the imputed datapoints.

state exempted people with high enough reported incomes from reporting activities, so an entry cohort's income mix is likely to have a large effect on its initial compliance outcomes.

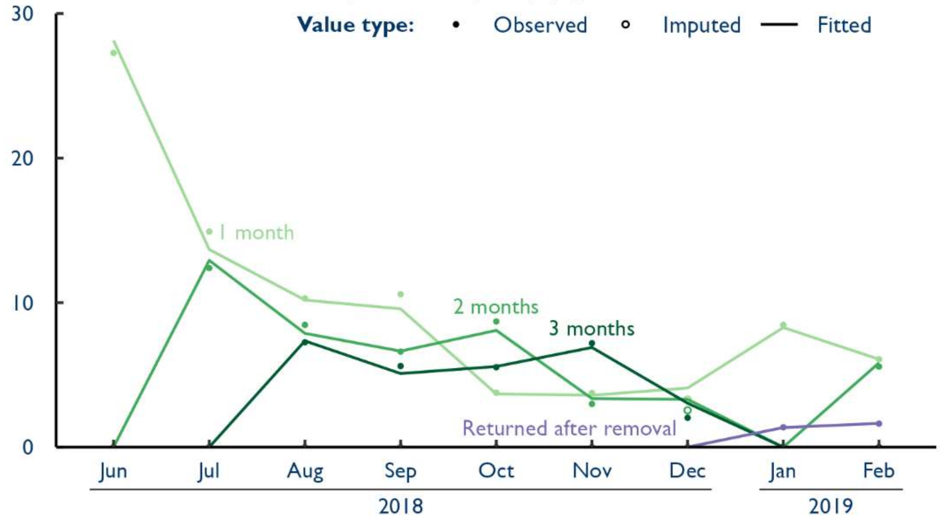
Indeed, the highest value of λ_m is for 2018 entrants, all of whom had incomes below 100% of the FPL since these were the only enrollees subject to the requirement in 2018. The lowest value, by contrast, is for January 2019 entrants, the large majority of whom likely had incomes above 100% of the FPL since this was the month in which the state extended the requirement to enrollees ages 30 to 49 with incomes above 100% of the FPL. The February 2019 entry cohort is intermediate between the 2018 and January 2019 entrants, likely because both sources of new entrants in this month (the continued phase-in of the requirement for enrollees ages 19 to 29 and inflows of enrollees from groups already subject to the work requirement via normal "churn") brought a mixture of lower- and higher-income enrollees into the subject population.

For what follows, it is useful to calculate a weighted average of the λ_m parameters that can be interpreted as the average non-compliance rate at entry for the full population subject to the policy, which I denote $\bar{\lambda}$. To do so, I weight each entry cohort by its estimated size $\hat{C}_m$, except that I quintuple the weight of the February 2019 entry cohort to reflect the fact that this cohort consisted mostly of enrollees ages 19 to 29, and the phase-in of that age group was scheduled to continue for four more months. For the base estimates, the resulting $\bar{\lambda}$ equals 20.7%.

Table 1 shows that excluding the imputed data points for December 2018 has little effect on the parameter estimates. It also shows that all model parameters are precisely estimated, which implies that the main potential source of error is specification error. To offer some insight on the potential for specification error, Figure 3 examines the quality of the model fit and shows that the model does a good job of matching the evolution of non-compliance and return rates over time.

Figure 3. Observed vs. Fitted Non-Compliance Rates

Share of current enrollees subject to the policy (%)



Note: Points are data or imputations from Arkansas Works monthly reports. Fitted values are predictions from the estimated model. See text for details.

The estimates are also internally consistent in two notable respects, which offers some additional reassurance about the model specification. First, $\bar{\lambda}$ is relatively close to the value of λ_m for the February 2019 cohort, which is consistent with the fact that the February 2019 cohort consisted of a mix of enrollees with incomes above and below 100% of the FPL that was likely similar to that of program enrollees overall. Second, $\bar{\lambda}$ is fairly close to the "steady state" non-compliance share implied by transition parameters ρ_{01} and ρ_{10} which is 17.1%.[10] This indicates that the share of the subject population estimated to be in a non-compliant state at any given point in time is reasonably compatible with the estimated dynamics of the compliance process.

Simulated long-run enrollment effects

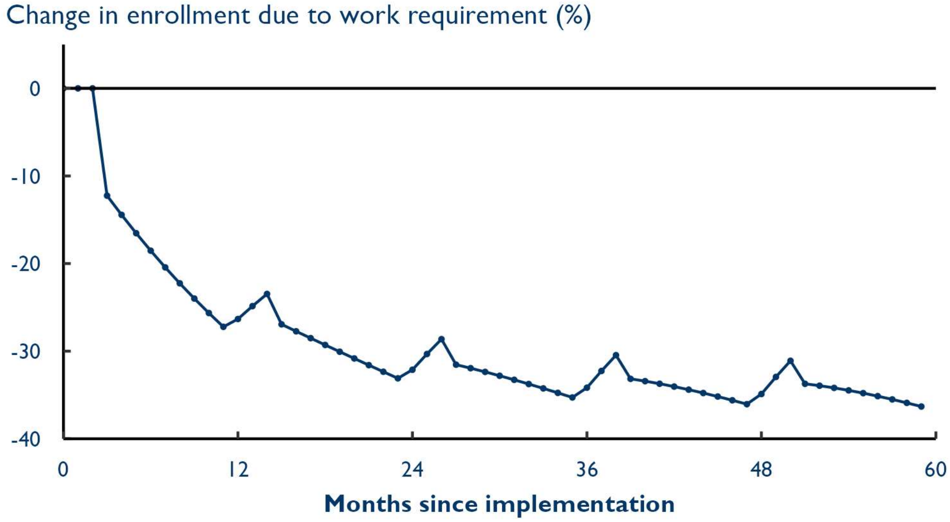
I now use the estimated model to simulate how an Arkansas-style work requirement would affect enrollment over the long run. To do so, I make a few additional assumptions.

First, I assume that implementation of the requirement occurs in January of an unspecified year. Second, I normalize pre-implementation enrollment to one and assume that new enrollees enter the program at a rate β per month, which implies that enrollment was in steady-state before implementation of the requirement (and would have remained so without the requirement). Third, I assume that model parameters take the values shown for the base estimates in Table 1 and that the share of enrollees who are non-compliant at entry takes the corresponding value of $\bar{\lambda}$.

Figure 4 (next page) reports the simulated effect of a work requirement on enrollment. Enrollment falls sharply in the April following implementation as the first enrollees accrue three months of non-compliance and are removed from the program. Enrollment falls steadily through December of year 1 as additional enrollees accrue three months of non-compliance and are removed from

[10] At the steady state non-compliant share s, $s\rho_{10} = (1-s)\rho_{01}$, which implies $s = \rho_{01} / [\rho_{01} + \rho_{10}]$.

Figure 4. Effect of Work Requirement on Enrollment



Note: The figure plots the percent difference between monthly enrollment with and without a work requirement when simulating the model using the base parameter estimates. The work requirement is assumed to be implemented in January of an unspecified year. See text for details.

the program, and December enrollment is 27% below what it would have been without the policy. In year 2, enrollment rises through March; this reflects the pause in removals since enrollees cannot accrue three months of non-compliance until the end of March, plus the return of some enrollees who were removed for non-compliance during year 1. Enrollment then begins declining again in April as removals for non-compliance resume. A similar seasonal pattern repeats and gradually stabilizes in the following years. By years 4 and 5 of the policy, average monthly enrollment is 34% below what it would have been in the absence of the work requirement.[11]

To provide insight on which features of the model and parameter estimates drive my conclusions, Figure 5 (next page) presents short-run (year 1) and long-run (year 5) enrollment effects for my base parameter estimates and various alternative estimates. Because of the aggregate nature and short duration of the available data, my modeling exercise necessarily requires making some strong assumptions, particularly that the patterns of enrollee behavior observed while Arkansas' policy was in effect would have persisted over the long term; these sensitivity analyses can help quantify the uncertainty these assumptions introduce. They can also help guide the discussion in the next section on how outcomes might differ when looking beyond Arkansas.

The figure shows that the policy would result in large enrollment declines under any of the parameter values considered. While the magnitude of the decline does vary across scenarios, these results indicate that enrollment dynamics would need to differ radically from those observed during the period Arkansas' requirement was in effect to overturn the conclusion that a work requirement like Arkansas' would lead to very substantial declines in enrollment.

The simulated long-run enrollment decline is most sensitive to the parameters ρ_{01} and ρ_{10}, which control the rate at which enrollees move between compliant and non-compliant states and, thus,

[11] The corresponding estimates for year 1, 2, and 3 are 15%, 29% and 33%, respectively.

Figure 5. Effect on Enrollment with Alternative Parameters

Change in enrollment due to work requirement (%)

Note: The figure plots the percent difference between simulated average monthly enrollment with and without a work requirement in year 1 and year 5 after implementation for the base parameter estimates and various alternative parameter values. See text for details.

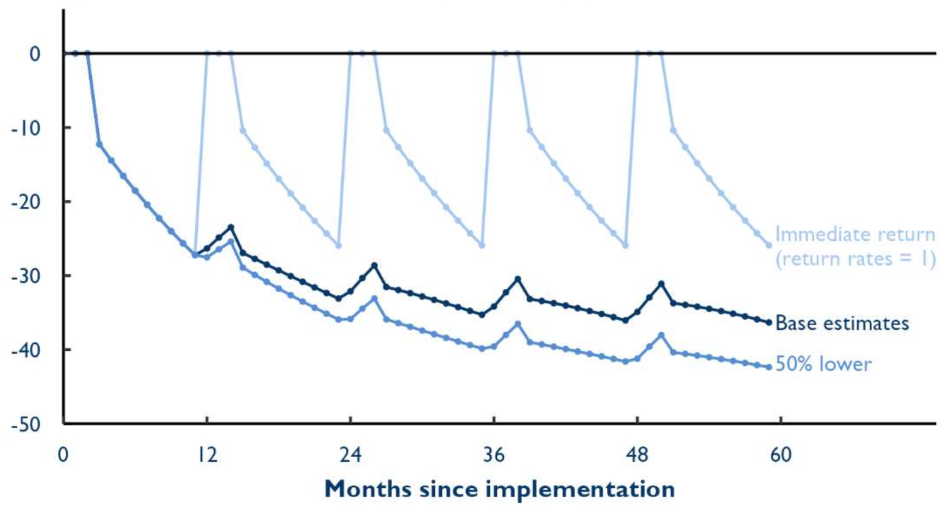
how much time enrollees spend in non-compliant states, on average, over the long run. Given the evidence discussed earlier suggesting that an overwhelming fraction of enrollees were, in fact, compliant with the policy, the central determinant of these parameters is likely the efficacy—or lack thereof—of the systems the state used to ascertain compliance.

Long-run estimates are also sensitive to δ_F and δ_O, which control the rate at which removed enrollees return to the program and, thus, how long removals persist. For two reasons, these scenarios (for which Figure 6 on the next page displays month-by-month estimates) merit special attention. First, there are only two months of data available for estimating the return rates since the Arkansas policy was paused just two months after removed enrollees were first eligible to return, which makes my estimates of these parameters especially uncertain. Moreover, the fact that the estimated return rate in February (captured in δ_O and equal to 3.4%) was so much lower than the return rate in January (captured in δ_F and equal to 8.4%) raises the question of whether the return rate would have continued to decline in subsequent months. If so, then the scenario with lower values of δ_F and δ_O might be a better guide to actual outcomes than the base scenario.

Second, while the extreme scenario in which all removed enrollees reenroll immediately once they become eligible to do so (i.e., the scenario with $\delta_F = \delta_O = 1$), is starkly at odds with the Arkansas data, it may nevertheless have some policy relevance. The Congressional Budget Office (CBO) analysis of the work requirement in the Limit, Save, Grow Act suggested that some states might use state funds to continue covering enrollees who lost eligibility for federal funding due to non-compliance with the work requirement (CBO 2023). If a state adopted that approach, non-

Figure 6. Effect on Enrollment With Alternative Return Rates

Change in enrollment due to work requirement (%)



Note: The figure plots the percent difference between simulated monthly enrollment with and without a work requirement for three parameter sets: the base estimates; a set where the δ parameters are 50% lower; and a set where all removed enrollees return immediately when eligible, which corresponds to setting both δ parameters equal to 1. See text for details.

compliant enrollees would never be separated from the program and would immediately qualify for federal funding at the beginning of the next year. Thus, this scenario might offer a better guide to the reduction in federally funded enrollment and the effect on federal outlays for the subset of states that adopted this approach. A caveat is that states that did adopt this approach would remove enrollees' incentive to demonstrate eligibility for an exemption or report suitable activities, which could cause an offsetting increase in non-compliance rates.

Varying the baseline attrition rate β has a more modest, but still meaningful effect on the long-run enrollment decline; this effect arises because it determines how quickly older enrollee cohorts that have been depleted by episodes of non-compliance are replaced by new cohorts. The long-run estimates are largely insensitive to the entry non-compliance rate $\bar{\lambda}$ because what matters for long-run enrollment outcomes is how much time enrollees spend in non-compliant states over the long run, which depends primarily on the transition parameters ρ_{01} and ρ_{10}, not enrollees' initial non-compliance rate. The value of $\bar{\lambda}$ does have a larger effect on short-run outcomes.

Discussion

The model developed in this paper predicts that a work requirement like Arkansas' would reduce enrollment among those subject to the policy by 27% at the end of the policy's first year and by 34%, on average, over the long run. Enrollment declines grow over time because, as time passes, more enrollees experience temporary spells of non-compliance that cause them to lose coverage and because removed enrollees are slow to reenroll once they become eligible to do so. The evidence discussed earlier implies that most of this decline would be attributable to enrollees who were, in fact, compliant when disenrolled and merely failed to prove that to the state or who were disenrolled during a temporary spell of non-compliance and had not yet regained coverage.

While my estimates are subject to meaningful uncertainty, the basic conclusion that an Arkansas-style work requirement would substantially reduce enrollment appears robust.

Given the close resemblance between the Arkansas policy and the federal work requirement passed by the House in 2023 as part of the Limit, Save, Grow Act, my estimates offer a good starting point for assessing the impacts of this prospective federal policy. Karpman, Haley, and Kenney (2025a) estimate that, in 2026, there will be 13.3 million Medicaid expansion enrollees in the 19 to 55 age range specified in the 2023 House proposal. Thus, if outcomes under the 2023 House policy mirrored the outcomes I estimate under the Arkansas policy, my estimate of a 34% long-run enrollment decline would imply a long-run enrollment reduction of around 4.5 million (=13.3 million x 34%) people.[12] As emphasized by Karpman, Haley, and Kenney, if the 2023 House bill were applied to enrollees outside the expansion population, the effects would be larger.

Potential differences in state implementation

Importantly, states might operationalize an Arkansas-style requirement differently than Arkansas did due to differences in their administrative capacity or policy goals. For example, states might differ in their willingness or ability to use existing data to automatically determine exemption eligibility, to establish reporting processes that are easy for enrollees to navigate, or to help enrollees who are disenrolled for non-compliance reenroll when eligible to do so. It is not clear, however, whether these differences would result in larger or smaller enrollment reductions.

The limited data on other states' recent experience with Medicaid work requirements does not point clearly in either direction. When Karpman, Haley, and Kenney (2025a) use data on New Hampshire's work requirement (which was paused before the state began disenrollments) to estimate the effect of a federal requirement, they obtain somewhat larger estimates than when they rely on the Arkansas data. On the other hand, the fragmentary data available on Michigan's work requirement (which was also paused before the state began disenrollments) point toward somewhat smaller enrollment reductions, with the caveat that this could at least partly reflect differences in the design of Michigan's policy, rather than its implementation.[13]

Nor is there reason to believe that Arkansas' administrative capacity during this period was particularly strong or particularly weak. Arkansas' enrollee-facing systems did lack some features

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[12] For two reasons, this calculation may understate the actual the long-run enrollment decline. First, Karpman, Haley, and Kenney note that their enrollment estimates are calibrated to match CBO's June 2024 baseline, but recent enrollment data are coming in above those projections. Second, CBO (2024) projects that Medicaid expansion enrollment will rise slightly over time.

[13] Kelly et al. (2022) cite state data indicating that approximately 80,000 enrollees out of 687,000 enrollees in Michigan's Medicaid expansion program were on track to be deemed non-compliant in this first month of Michigan's policy. Accounting for the fact that enrollees ages 63 and 64 were not subject to the Michigan policy, as well as the fact that the state reported having excluded an additional 18,000 enrollees to serve as a control group (Michigan Department of Health and Human Services 2020), this implies that 12.5% (=80,000/[687,000 x (44/46) - 18,000]) of enrollees subject to the policy were on track to be deemed non-compliant at program entry. The comparable figure in my estimates is 20.7%, which suggests that compliance outcomes like Michigan's would likely result in smaller enrollment declines. A caveat is that Michigan's exemptions and required activities were not identical to Arkansas', so this difference could at least partially reflect these design differences rather than implementation differences.

that many other states had then or have today. For example, as of 2019, an Arkansas Medicaid enrollee could not upload verification documents via an online account, something that was possible in 32 states at that time and 44 states (including Arkansas) as of 2025 (Brooks, Roygardner, and Artiga 2019; Brooks et al. 2025). This suggests that Arkansas may have been relatively poorly positioned to establish easy-to-navigate reporting processes, as borne out in practice. On the other hand, Arkansas completed at least three-quarters of eligibility renewals automatically as of 2019, something that was true in only 10 states at that time and only four states in 2023 (Brooks, Roygardner, and Artiga 2019; Brooks et al. 2023). This suggests that Arkansas may have been relatively well-positioned to automatically determine exemption eligibility, something the state does appear to have had considerable success with in practice.

Under a permanent federal work requirement, states would accrue more experience with this type of policy than Arkansas had during its brief implementation period. This could allow states to improve their systems for ascertaining compliance with the requirement. On the other hand, states' attentiveness to implementation could fade over time as the novelty of the policy wore off, which would push in the other direction. In principle, enrollees could also get better at navigating these requirements over time, although given the extremely low reporting rates observed in Arkansas, even large relative increases in reporting rates would likely only modestly increase enrollment. Additionally, enrollees disenrolled for non-compliance might be discouraged from enrolling in the program in the future, which would tend to reduce enrollment.[14]

Operational outcomes under a federal work requirement could also depend on how the executive branch implemented legislation creating a requirement. For example, under a proposal like the 2023 House bill, federal policymakers would likely need to make choices about how broadly to interpret some of the bill's exemption categories and how stringently to enforce the bill's requirement that states use existing data to ascertain enrollees' compliance status wherever possible. Depending on how those choices were made, states could find themselves more constrained than they were under the earlier waiver programs, which, depending on the nature of the federal decisions, could either magnify or attenuate enrollment reductions.

Possible use of state funds to cover enrollees who lose eligibility for federal funding

An additional consideration is whether states that (unlike Arkansas) were *compelled* to adopt a work requirement by the federal government might elect to use their own funds to cover enrollees who were no longer eligible for federal funding. In its analysis of the 2023 House policy, CBO assumed that states would step in to cover 60% of these enrollees using their own funds (CBO 2023). If this occurred, it would meaningfully attenuate the reduction in Medicaid enrollment caused by a federal work requirement, albeit at a substantial cost to state governments.

However, past state behavior offers some reason to doubt that this would be the typical outcome. Before introduction of the ACA Medicaid expansion, states typically did not provide insurance

[14] My model already accounts for this type of discouragement effect *within* an enrollment "spell," meaning from the time of disenrollment through the time at which the enrollee would have left the program in the absence of the work requirement, via the return rate parameters δ_F and δ_O. The model does not, however, capture any effect on the initiation of new enrollment spells.

coverage to any non-disabled adults without children (Heberlein et al. 2011), the types of enrollees who would lose eligibility for federal funding under the 2023 House policy. Moreover, where states did cover members of this group, they often received some federal funding via a Medicaid waiver in order to do so. By contrast, in this case, states would need to be willing to bear the full cost of covering what some may view as a politically unsympathetic slice of this population.[15]

States might exhibit "loss aversion" (Kahneman and Tversky 1979) and thus be more motivated to avoid *reductions* in insurance coverage in this population than they were to *increase* coverage in this group. On the other hand, if a work requirement were implemented alongside other reductions in federal support for Medicaid or during an economic downturn, the resulting fiscal stress could make states less likely to step in. Additionally, if loss aversion does play a key role in motivating states to maintain coverage for this group, it could make continued coverage for this group relatively fragile in the sense that if temporary circumstances (e.g., a recession) caused a state to drop coverage, that coverage might be unlikely to return in later years.

Differences in macroeconomic conditions and state enrollee populations

The enrollment effects of a work requirement would likely also depend on macroeconomic conditions, a salient consideration at present in light of concerns that the Trump administration's tariff policies will lead to a period of slow growth or an outright recession (e.g., Santilli and DeBarros 2025). An economic downturn would likely increase Medicaid enrollment—and expansion enrollment in particular—above currently projected levels (Jacobs, Hill, and Abdus 2017), thereby resulting in larger enrollment reductions than I estimate here.

A downturn could also change the composition of the population subject to a work requirement in ways that would cause a larger share of enrollees to lose coverage. Indeed, Arkansas' labor market was quite strong while its requirement was in effect: the unemployment rate was 3.7% or lower throughout this period, lower than the national rate in 89% of months since the year 2000.[16] In a weaker labor market, enrollees would likely be employed at lower rates. Lower employment would likely increase the share of enrollees who are not engaged in suitable activities and, thus, at risk of disenrollment. Perhaps more importantly, it would likely also reduce the share of enrollees for whom states could use existing income data (including data collected during the ordinary eligibility process, unemployment tax records, or other income data held by the state) to automatically exempt enrollees from reporting. Due to the challenges that enrollees appear to have faced in reporting work and other activities under the Arkansas policy, this would likely increase the share of enrollees deemed non-compliant and, in turn, disenrolled.

State enrollee populations could also differ in ways that are independent of the business cycle but could still influence enrollment outcomes under a work requirement. Notably, in recent years, non-elderly, non-disabled adult Medicaid enrollees appear to have been employed at somewhat

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[15] A caveat is that, as noted in the discussion surrounding Figure 6, a state's decision to cover some enrollees with state funds might also increase the number of enrollees who *are* eligible for federal funding. In this case, the federal government would, in effect, bear part of the cost of the overall increase in Medicaid enrollment spurred by the state's decision to cover non-compliant enrollees.

[16] These data are from the Bureau of Labor Statistics.

lower rates in Arkansas than in other states (Garfield, Rudowitz, and Damico 2018; Tolbert et al. 2025). By the same logic laid out above, this could cause experience under Arkansas' work requirement to overstate the enrollment reductions that other states would experience under a similar requirement. Consistent with this, state-specific estimates of the effect of a work requirement by Karpman, Haley, and Kenney (2025b) that take account of differences in the composition of states' enrollee populations suggest that Arkansas would experience modestly larger enrollment reductions under a work requirement than other states.[17]

Alternative work requirement policy designs

Work requirement policies that differed substantially from the Arkansas policy that I focus on in this paper would, of course, have different effects on enrollment. One potentially salient design difference is the number of months of non-compliance that triggers disenrollment. Reducing this number would result in larger (potentially much larger) enrollment declines, while increasing it would result in smaller declines. Changing how long removed enrollees must wait before reenrolling would also affect the size of enrollment declines, with a longer waiting period increasing them and a shorter period reducing them. The model and estimates obtained here could likely be adapted to simulate the effects of these types of alternative policy designs.

Alternative policies might also apply to different populations. For modest changes in the subject population (e.g., a modest narrowing or broadening of the targeted age range), it would likely be reasonable to assume that the alternative population would comply at similar rates to the Arkansas policy and, thus, experience similar proportional enrollment reductions.[18] More substantial changes in the subject population, changes in the set of exemptions or required activities, or changes in how states are required to assess compliance could have more complex effects. While the directional effect of these types of design changes would likely be clear in most cases, obtaining quantitative estimates could require undertaking more disaggregated modeling similar to that performed by Karpman, Haley, and Kenney (2025a) and then using the results of that analysis to adjust the parameters of the dynamic model developed here.[19]

Comparison to other estimates

My estimates of the effects of a policy like the 2023 House bill are similar to estimates from Karpman, Haley, and Kenney (2025a). When relying on data from Arkansas, they estimate that

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[17] When relying on data from Arkansas, Karpman, Haley, and Kenney project enrollment reductions of 62,000 for Arkansas and 4.6 million for the nation overall in 2026. These estimates represent 26% and 22% of total Medicaid expansion enrollment (not just age 19-55 enrollment) in these areas in June 2024. A caveat is that this comparison could be distorted by state-specific differences between the total number of Medicaid expansion enrollees in June 2024 and Karpman, Haley, and Kenney's projection of the number of Medicaid expansion enrollees ages 19-55 in 2026.

[18] Indeed, I have implicitly made this assumption in using my results for Arkansas to predict the effects of the 2023 House policy, which applied to a slightly broader age range (19-55 rather than 19-49).

[19] Concretely, a method like Karpman, Haley, and Kenney's could be used to estimate the share of enrollees expected to be non-compliant at a point in time under the alternative policies. That could be used directly to adjust the share of enrollees who are non-compliant at program entry ($\bar{\lambda}$) and to adjust the transition parameters (ρ_{01} and ρ_{10}) to achieve the requisite change in the "steady state" non-compliant share.

34% of enrollees subject to the 2023 House policy would lose eligibility for federal funding; this almost exactly aligns with my estimate even though we use markedly different analytic methods.[20] (Karpman, Haley, and Kenney arrive at a modestly higher 39% estimate when relying on data from New Hampshire.) By contrast, CBO's analysis of the 2023 House policy predicted that only 10% of enrollees subject to the policy would lose eligibility for federal funding (CBO 2023) CBO has published little information on how it produced this estimate, so it is unclear why CBO's estimate is so much smaller. Regardless, taken together, my estimate and the Karpman, Haley, and Kenney estimate suggest that CBO may be underestimating the effect of such a policy.

Conclusion

The estimates presented in this paper demonstrate that a Medicaid work requirement similar to the requirement implemented in Arkansas in 2018 and 2019 would substantially reduce Medicaid enrollment, especially in the long run. This fact does not, in itself, answer the question of whether a work requirement would be good or bad policy. The answer to that question depends on how policymakers weigh the fiscal cost of delivering coverage to these enrollees against the benefits for enrollees (e.g., Wherry and Miller 2016; Ghosh, Simon, and Sommers 2019; Duggan, Gupta, and Jackson 2022; Zewde et al. 2019; Brevoort, Grodzicki, and Hackmann 2020; Levy and Buchmueller 2025) and health care providers (e.g., Blavin and Ramos 2021; Fiedler 2021). But this paper's estimates do show that pending decisions about whether or not to create a federal Medicaid work requirement will likely have large and long-lasting consequences.

[20] The similarity in our topline estimates does obscure some important underlying differences. Karpman, Haley, and Kenney's estimate essentially equals the share of enrollees that they estimate to be non-compliant at program entry. My estimate of this share (20.7%) is markedly lower than theirs. This may be partly because their method allows them to adjust for differences between the population subject to the Arkansas policy and the national population that would be subject to the 2023 House policy and because their analysis excludes some exemptions that applied under Arkansas' policy that they believe may either be administratively infeasible in other states or impermissible under the 2023 House policy. However, this difference is offset by the fact that the dynamic model that I develop in this paper allows me to account for changes in enrollee compliance status over time, the fact that enrollees must accrue three months of non-compliance to be disenrolled (and may return in subsequent calendar years), and underlying program churn. On balance, accounting for these dynamic factors increases my estimate.

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Appendix A: Imputing Missing December 2018 Data

This appendix describes how I impute the two data elements missing from the December 2018 Arkansas Works report: the number of enrollees deemed non-compliant in that month who had accumulated either one or two months of non-compliance year-to-date. As in the main text, I let N_m^j denote the number of enrollees who are non-compliant in month m who have accumulated j months of non-compliance during the current year, so the missing data are N_m^1 and N_m^2.

To impute N_m^2, I assume that $N_m^2/N_{m-1}^1 = N_m^3 \,/\, N_{m-1}^2$, an assumption that holds almost exactly in prior months; this assumption implies that N_m^2 may be imputed as $N_{m-1}^1[N_m^3/N_{m-1}^2]$. To impute N_m^1, I use the fact that each monthly report reports the total number of enrollees deemed non-compliant in that month (across all durations of non-compliance), and I impute N_m^1 as the residual after subtracting off the observed value of N_m^3 and the imputed value for N_m^2.

One puzzling feature of the state's enrollment reports is the total number of non-compliant enrollees does not exactly equal the sum of the number of non-compliant enrollees with one, two, or three months of accumulated non-compliance (i.e., $N_m^1 + N_m^2 + N_m^3$). The differences run in both directions but are generally modest; the mean absolute difference is 5%, and the largest absolute difference is 13%. Certain notations on the report suggest that the number of enrollees with each number of accumulated months of non-compliance may have been tabulated at a slightly later point in time than the overall total, which could account for the instances where the sum of the disaggregated estimates is smaller than the total if some enrollees are terminated from the program for reasons unrelated to the work requirement in the intervening period. It is unclear, however, what could account for the cases where the sum of the disaggregated estimates is larger than the overall total. In any case, except for the imputation procedure described here, I rely exclusively on the disaggregated tallies and disregard the overall tally.

Appendix B: Calculating Standard Errors

This appendix describes the method used to calculate the standard errors presented in Table 1.

Baseline attrition rate

The sequential minimum distance procedure we use here delivers an estimator $\hat{\beta}$ that satisfies:

$$\hat{\beta} = 1 - \left[\frac{1}{|\mathcal{M}_A|}\sum_{m\in\mathcal{M}_A}\hat{\gamma}_m\right]^{1/\eta},$$

where

$$\hat{\gamma}_m \equiv \frac{S_m}{A_m + S_m},$$

$\eta = 53\ /\ 30$, and $\mathcal{M}_A$ is the set of months for which attrition data are available.

The variance of each $\hat{\gamma}_m$ is given by the usual formula for binomial random variables as $(A_m + S_m)^{-1}\gamma(1-\gamma)$, where $\gamma \equiv (1-\beta)^{\eta}$. If the $\hat{\gamma}_m$ were mutually independent, it would then be straightforward to calculate be the variance of the mean $|\mathcal{M}_A|^{-1}\sum_{m\in\mathcal{M}_A}\hat{\gamma}_m$. However, because A_m captures attrition over a two-month period, the $\hat{\gamma}_m$ are correlated across adjacent months.

To obtain a conservative standard error, I assume that the $\hat{\gamma}_m$ are perfectly positively correlated. Under this assumption, the covariance matrix of the $\hat{\gamma}_m$ can be written as $\{\sigma_m\}\{\sigma_m\}'$, where $\sigma_m = \mathrm{StdDev}(\hat{\gamma}_m)$. The variance of $|\mathcal{M}_A|^{-1}\sum_{m\in\mathcal{M}_A}\hat{\gamma}_m$ is then given by $|\mathcal{M}_A|^{-2}[1'\{\sigma_m\}\{\sigma_m\}'1] = \bar{\sigma}^2$, where $\bar{\sigma} = |\mathcal{M}_A|^{-1}\sum_{m\in\mathcal{M}_A}\sigma_m$. Applying the delta method then implies that that the variance of $\hat{\beta}$ is well-approximated by $\eta^{-2}\gamma^{2(1/\eta-1)}\bar{\sigma}^2$. I use this formula to obtain a standard error for β by plugging in $|\mathcal{M}_A|^{-1}\sum_{m\in\mathcal{M}_A}\hat{\gamma}_m$ for γ each place it appears.

Other model parameters

I calculate standard errors for ρ_{01}, ρ_{10}, $\{\delta_t\}$, and $\{\lambda_m\}$ using a parametric bootstrap procedure.

In the first step, I create synthetic microdata of the same size as the enrollee population reflected in the actual tallies used for estimation. To do so, I draw a synthetic vector of entry cohort sizes for each month m from the distribution $\mathrm{multinomial}(\sum_m \hat{C}_m, \hat{\theta})$, where $\hat{\theta} = \{\hat{\theta}_m\}$. I then create the corresponding number of synthetic enrollees for each month and simulate each enrollee's outcomes forward through February 2019 using the estimated parameters.

In the second step, I use the synthetic microdata to calculate a synthetic replicate of the aggregate tallies used in estimation, and I re-estimate the model using these synthetic tallies. In doing so, I use the original value of $\hat{\beta}$, as I do not simulate synthetic attrition data.

I repeat this procedure 500 times and then calculate standard errors for the parameters of interest as the sample standard deviation of the replicate estimates. I note that this procedure ignores variability stemming from the fact that β and $\sum_m C_m$ are both estimated. However, the error in each is small enough that this is unlikely to meaningfully affect the reported standard errors.