Cost Sharing Among Those Who Can't Pay: Evidence from Medicaid Disenrollments

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October 2018

Abstract

Cost-sharing for medical services could lower spending through reductions in moral hazard and consequent decreases in low-value care. Simultaneously, cost-sharing could introduce behavioral hazard through potential reductions to high-value care, which could lead to offsetting increases in avoidable care. The net effect of cost-sharing, through these two opposing mechanisms, is ultimately an empirical question and could furthermore be population dependent. I examine this question in the context of a high-cost and financially needy group: dual-eligibles simultaneously enrolled in Medicaid and Medicare, who account for 35% of overall program spending. I leverage an exogenous court ruling that resulted in a substantial cost-sharing increase from approximately 0% to 15% for 25,000 dual-eligibles, through subsequent loss of Medicaid coverage, and isolate the effects of cost-sharing here from other accompanying changes. I find that this cost-sharing bump surprisingly increases overall spending by 6%, through increases in preventable inpatient services, which more than offset concurrent outpatient reductions. I also find that the spending increases come from the sickest subset of the population, with spending actually falling among everyone else. These results highlight potential benefits from expanding accessibility of supplemental Medicare insurance among the Medicare disabled, particularly through Medicaid and Medigap, given existing coverage limitations.

*I thank Zarek Brot-Goldberg, Mark Duggan, Jon Gruber, Jon Kolstad, Dan Sacks, Mike Sinkinson, Amanda Starc, Bob Town, and Jacob Wallace for valuable comments. This research was supported by the U.S. Social Security Administration through grant #1 DRC12000002-03 to the National Bureau of Economic Research as part of the SSA Disability Research Consortium. I thank Jared Wasserman for outstanding research assistance and the Dartmouth Institute for data and research support, under NIA POI-AG019783. Additional funding was obtained through a Leonard Davis Institute pilot grant. The content is solely the responsibility of the author and does not necessarily represent the official views of the SSA, any agency of the federal government, the Dartmouth Institute, LDI, the NBER, the National Institutes of Health, or Stanford University. All errors are my own.

1 Introduction

Medicaid and Medicare were jointly introduced in 1965, as part of the Social Security Amendments Act. Medicaid was created as a health care program for the poor, while Medicare was primarily meant to serve the elderly. Although Medicaid and Medicare were not originally designed to work together, over 9 million individuals are now simultaneously in both. These individuals, who are referred to as "dual-eligibles", account for a disproportionate 34% of Medicaid and Medicare spending (equivalent to 2% of GDP) while making up only 13% of enrollees (CBO, 2013). Individuals qualifying for both are typically either disabled or among the poor elderly.

Dual-coverage under Medicare and Medicaid differs from coverage under Medicare-only along a number of dimensions, such as the overall set of services covered and the specific payer responsible for each service. For example, Medicare-only coverage excludes extended nursing home stays, even though such stays get covered for dual-eligibles. Among duals, Medicaid can sometimes serve as a primary payer for home care services, with Medicare otherwise serving as the primary payer among non-duals (Grabowski 2007). Dual-coverage is additionally characterized by program interactions between Medicaid and Medicare, which may distort care and ultimately lead to cost-shifting, particularly shifting from long-term to acute care (Bubolz 2012).

This study examines differences between dual and Medicare-only coverage, and attempts to isolate the impact of one specific channel, cost-sharing. First, cost-sharing may be the dimension along which these coverage types differ most meaningfully, being effectively nonexistent under dual-enrollment while averaging around 15% under Medicare-only coverage (MedPAC, 2004). Second, while cost-sharing has significant policy relevance in general and for this population in particular, its effects on this specific group have not been closely examined. This gap in the literature is unfortunate, since cost-sharing could differentially impact this population, and since secondary insurance policies for this group should be tailored accordingly. This subject is particulary topical, given ongoing policy debates around boosting this

population's secondary coverage rates from low current levels, specifically through enhanced Medicaid and Medigap access.[2] Along with the Medicare disabled and dual-eligibles, these results could potentially generalize to non-disabled Medicare beneficiaries and to non-Medigap forms of secondary coverage, particularly since 90% of the broader Medicare population carries some form of secondary insurance.[3]

The study of cost-sharing, in this particular setting, could also be of more general economic and theoretical interest. Specifically, it could be used to evaluate competing theories on the mechanisms driving cost-sharing's effects, and help identify the accompanying implications for patient welfare. Purportedly, cost-sharing could lower utilization through reduced moral hazard (Manning et al 1987, Brot-Goldberg et al 2016) and accompanying reductions in low-value care. At the same time, cost-sharing could increase utilization through behavioral hazard, by reducing valuable care and creating offsetting increases in preventable utilization (Chandra et al, 2010, Brot-Goldberg et al 2016). The relative importance of each mechanism is ultimately an empirical question, and one that warrants separate examination across each individual population. In addition, there is limited understanding of the mechanisms through which offsets may occur, in terms of the specific care reductions that lead to offsets, and the particular care areas in which offsets are most likely to arise.

Past work looking to identify the impact of dual-enrollment, particularly its impact through cost-sharing, has been hindered by a number of empirical issues which I attempt to overcome. In particular, compositional differences between dual-eligibles and Medicare-only enrollees make it difficult to separate the effect of Medicaid coverage status from other in-

[2]First, about 21% of disabled Medicare recipients under 65 lack secondary coverage, compared to 12% of those over 65 (Neuman and Cubanski 2016).Moreover, the disabled Medicare population has limited access to Medicaid, given that half of states require incomes below 75% of FPL for Medicaid eligbility, and the remainder of states require incomes below 100% of FPL (Watts et al 2016). Meanwhile, this population has limited access to Medigap, as only 31 states have guaranteed-issue Medigap provisions for the under-65 population. In addition, even when they are in place, guaranteed-issue provisions only apply over the first six months of Medicare enrollment. Finally, irrespective of the presence of guaranteed-issue provisions, many states allow Medigap plans to price discriminate premium levels based on disability status (Neuman and Cubanski 2016).

[3]Past work on secondary Medicare coverage, apart from looking at Medicaid, has examined employer retiree benefits (Chandra et al, 2010) and Medigap (Cabral et al, 2014).

dividual characteristics. I do so using a novel identification strategy, leveraging involuntary court-driven disenrollments from Medicaid, among those previously dually-enrolled in Medicaid and Medicare. Specifically, I leverage a 2009 court ruling allowing Tennessee to kick ineligible individuals off Medicaid, which followed a 20 year long court prohibition barring Tennessee from disenrolling anyone ever qualifying for Medicaid through SSI (Wadhwani 2010).[4] As a result, these Medicaid disenrollments should not coincide with individual-level financial or health status changes, but instead should result from pre-existing and possibly long-standing ineligibility for Medicaid. In my analyses, I focus on those disenrolled through financial ineligibility, who otherwise remained disabled over the sample period. Despite its financial ineligibility for Medicaid, the sample population here appears financially worse off than typical Medicare beneficiaries, although still better off than typical dual-eligibles.[5]

An additional challenge lies in decomposing dual-enrollment's impact through one specific channel, cost-sharing, from other dimensions along which dual and Medicare-only enrollment might differ. In particular, dual-enrollment is associated with broader service coverage than Medicare-enrollment alone, specifically for long-term and institutional care. To tackle this issue, I exclude dual-eligibles who made use of uncovered services including long-term care, prior to the 2009 Medicaid disenrollments. As a result, the remaining sample should be minimally impacted by coverage differences between dual and Medicare-only coverage, allowing me to isolate cost-sharing effects from all others.[6]

In my analyses, I focus on Tennessee residents who were simultaneously in Medicaid and

[4]Incidentally, the 2009 Medicaid disenrollment is distinct from the one used in Garthwaite et al (2014), given that the 2009 episode is judicially rather than legislatively driven. Coincidentally, both take place in the same state.

[5]Specifically, the vast majority of those getting disenrolled remained eligible for prescription drug subsidies following disenrollment, and also voluntarily opted to remain in Medicaid prior to it. First, over 95% of those disenrolled from Medicaid remain eligible for Medicare Part D cost-sharing subsidies following disenrollment. The eligibility standard for these subsidies, meanwhile, is an income below 150% of the federal poverty limit, which corresponds to around the 30th income percentile among all Medicare beneficiaries. Altogether, this means that over 95% of the treatment group here has an income below the Medicare average. Second, while individuals had the ongoing option to remain in Medicaid, they also could have voluntarily dropped coverage.

[6]One shortcoming of this approach is that it does not account for possible changes in long term care use over time, and specifically for potential first time use of long term care among certain beneficiaries. However, additional analyses indicate that long-term care usage is actually quite persistent, and that only a small percentage of beneficiaries are first-time users of LTC, at any given moment.

Medicare as of 2008. I make use of Medicare administrative data, which comprehensively tracks health utilization for those in Medicare-only as well as for the dually-enrolled, given that Medicare functions as the primary payer for both populations. Employing a difference-in-differences approach, I find that Medicaid disenrollments among previous dual-eligibles increase overall medical spending by a highly signfiicant 6%. The effect size does not appear to be attenuated by substitution to alternative forms of secondary coverage, as I find no meaningful substitution to either Medigap, employer-coverage, or Medicare Advantage following disenrollment from Medicaid. In addition, increased overall spending appears to be driven by a 15% jump in inpatient care, which more than offsets a 3% outpatient spending reduction. This inpatient spending increase arises largely through jumps in ER admissions, which could result from accompanying reductions in preventive outpatient care. The mechanisms for the overall effect may simultaneously include reduced moral hazard as well as increased behavioral hazard, given observed reductions in low as well as high-value care, along with increases in preventable care. Finally, I find that the overall spending effect varies with health status, with spending actually decreasing for the median beneficiary. Unfortunately, I am not able to effectively identify the corresponding impact on health outcomes, nor the general implications for patient welfare.

Altogether, my findings contribute to the existing economic as well as health policy literature. First, I add to the existing work on medical offsets, deepening understanding of them as well as the mechanisms through which they operate. My work is also unique in focusing specifically on medical-driven offsets, unlike previous studies that have examined such offsets only in combination with drug-driven ones (Chandra et al 2010). I also study the effect of cost sharing on a previously overlooked population, made up of disabled Medicare recipients, along with the related impact of secondary insurance. This issue is particularly timely, given ongoing policy debates around expanding this population's secondary coverage access, through Medicaid as well as Medigap (Neuman and Cubanski 2016, Watts et al 2016). Finally, my study could inform cost-sharing policies beyond the Medicare disabled, as my

results might extend to any coverage setting with high-cost patients.

In Section 2, I go over relevant institutional features of Medicaid, Medicare, and overlapping Medicaid-Medicare coverage. I also review the institutional details surrounding the court-driven Medicaid disenrollment in Tennessee. In Section 3, I go over the data used in these analyses. In Section 4, I review my empirical design and implementation. In Section 5, I go over the results. In Section 6, I conclude.

2 Medicaid and Medicare

2.1 Background

Medicare and Medicaid were created in 1965, through the Social Security Amendments Act. Medicare is federally financed and controlled, with a program design that is effectively uniform nationwide. Medicaid, meanwhile, is directly administered by individual states, with each state having some local control over program design, subject to federally imposed limits. Each state also bears up to 50% of the cost of Medicaid, with the remainder of the cost shouldered by the federal government (KFF, 2012).

While Medicaid and Medicare had little overlap at their time of formation, subsequent program expansions have resulted in a beneficiary population of over 9 million that is simultaneously in both. To this point, Medicare was originally restricted to individuals over 65, who had been legal U.S. residents for over five years. Meanwhile, Medicaid was initially restricted to poor families with children, who were the recipients of cash or welfare assistance. In 1972, both programs were expanded to cover individuals who were disabled, with Medicare covering disabled recipients of SSDI[7], and Medicaid covering disabled recipients of SSI[8]. As a result, the under-65 in SSI and SSDI could be on Medicaid and Medicare

[7] To qualify for SSDI, individuals needed to be disabled and also have a certain amount of work experience, with the specific work requirement varying by age.

[8] Some states retained the more stringent financial requirements that were in place prior to 1972, in lieu of the relatively less stringent ones associated with SSI.

simultaneously, along with the over-65 in SSI-only (SSA 2011, CBO 2013).

Although dual-eligibles make up about 15% of Medicare and Medicaid enrollees, they account for a disproportionate 35% of all Medicaid and Medicare spending. This group's high spending could be partly attributable to underlying health characteristics, particularly given higher rates of chronic illness than among the overall population (CBO, 2013). However, this high spending could also be attributable to program design issues, particularly differences in cost-sharing and service coverage across dual and Medicare-only enrollment.

Specifically, Medicare-only and dual-coverage may differ most meaningfully in terms of patient cost-sharing. Medicaid covers effectively all traditional Medicare cost-sharing for those dually-enrolled, with this otherwise being the patient's responsibility under Medicare-only enrollment (in the absence of other secondary insurance). Altogether, this difference in cost-sharing exposure could end up being significant, given that traditional Medicare cost-sharing will typically amount to 15-20% of the original spending amount. The precise level of cost-sharing under Medicare will ultimately be care-dependent, given its varied and non-linear structure (Carpenter, 1998). In particular, Medicare cost-sharing features separate Part A and Part B deductibles, which effectively means separate deductibles for inpatient and non-inpatient care. In 2008, the Part A deductible amounted to $1024, while the Part B deductible amounted to $135. For spending above the deductible, Part A imposes cost-sharing only once a patient has stayed in the hospital for over 60 days or in a skilled-nursing facility for over 20 days, over a single benefit period. Meanwhile, for above-deductible spending, Part B imposes a co-insurance of 20%. While cost-sharing under Medicaid is typically de minimus, for Medicaid-only as well as dual-enrollees, a handful of states such as Indiana have imposed additional cost-sharing (Saloner et al, 2014). As such, findings from this study could shed light on the advisability and effectiveness of these new policies.

Medicare-only and dual-coverage may differ along other dimensions, apart from cost-sharing, but these other differences should only be pronounced for users of long-term and post-acute care services. Given this, restricting to non-long term care users should allow

me to focus in on coverage differences through the particular channel of cost-sharing, while isolating out other potential factors. First, Medicare-only and dual-coverage may differ in terms of the underlying scope of services covered, particularly for long-term care. While most Medicare services are also covered by Medicaid, extended nursing home stays are covered by Medicaid only, and consequently would not be covered for Medicare-only enrollees.[9] In addition, dual-enrollment may differ from Medicaid-only coverage on account of program interaction issues between Medicare and Medicaid, which could ultimately distort care. Such program interactions can arise as a result of ambiguous or overlapping payment responsibilities, which again would be concentrated among long-term care services. For example, Medicaid and Medicare can both serve as primary payers for dual-eligibles' home care, which can lead to confusion among home care providers and also to gaming around obtaining the typically higher Medicare rate. In addition, program interactions may perversely incentivize nursing homes to have patients readmitted to hospitals, given that nursing homes would then be eligible for the higher Medicare rate over the 30 days following a patient's return. (MedPAC 2011).

2.2 Medicaid Disenrollment in Tennessee

Since 1972, SSI receipt in Tennessee ensured automatic eligibility for Medicaid, while the loss of SSI receipt could mean Medicaid disenrollment. During the 1980's, a number of individuals filed litigation to,contest their disenrollment from Medicaid, following the loss of SSI receipt (this case was designated as 'Cluster Daniels', in reference to the main plaintiff). A federal court issued an injunction in 1987, preventing Tennessee from dropping anyone from Medicaid who had originally qualified for it through SSI, including individuals no longer on SSI. While the state was prohibited from kicking this subpopulation off Medicaid, individuals could voluntarily drop Medicaid coverage at any time (Wadhwani, 2010).

Final resolution of this case in January 2009 lifted the longstanding prohibition, allowing

[9] For all beneficiaries, including dual-eligibles, Medicare pays for all nursing home costs during the 30 days following an inpatient discharge (MedPAC, 2011)

Tennessee to check the Medicaid eligibility of those originally qualifying through SSI, and also to kick off those no longer eligibile. At the time, about 150,000 Tennessee Medicaid recipients had originally qualified on this basis (or about 13% of all Tennessee Medicaid enrollees), with this population accounting for about $1.2 billion in annual Medicaid expenditures. The group was comprised of those enrolled in Medicaid-only, along with others simultaneously in Medicaid and Medicare (Wadhwani, 2010). Among this group, about 60,000 individuals were ultimately deemed to no longer be eligible for Medicaid and consequently kicked off, including 25,000 individuals who had simultaneously been in Medicare.[10] The disenrollments were concentrated among those no longer qualifying for SSI, either on the basis of health or financial ineligibility.[11] In these particular analyses, I focus on individuals who remained classified as disabled at the time of the court ruling, whose ineligibility for SSI and Medicaid would have therefore stemmed from financial considerations.

3 Data

In this paper, I use several administrative datasets from CMS, covering Tennessee for the 2008-2011 period. These datasets contain information on demographic characteristics, Medicaid and Medicare enrollment status, as well as inpatient and outpatient utilization. Critically, this data comprehensively tracks utilization among those in Medicare-only, and also among those concurrently in Medicare and Medicaid ('dual-eligibles').

Compared to other studies on Medicaid disenrollments, mine is unique in tracking individual medical utilization over the entire study period, including following exit from Medicaid. I am able to do so using Medicare claims data, given that Medicare serves as the primary payer for dual as well as Medicare-only enrollees. Meanwhile, other studies focusing on

[10] About seven to eight months elapsed between the original court ruling and the implementation of disenrollments, which could have led to intertemporal substitution of care on the part of patients. Specifically, patients may have pushed forward elective care, in anticipation of higher future cost-sharing.

[11] SSI eligibility requirements, for those under 65, are based on continuing disabled status as well as certain financial criteria. The financial requirement for SSI is specified as an income of under 75% of the federal poverty line, combined with financial assets below a certain threshold. Meanwhile, for those over 65, SSI eligibility is entirely financially dependent and not at all contingent on disability status.

Medicaid disenrollments have had to rely on discharge and patient reported data in lieu of claims, given that many of the disenrolled subsequently had no formal insurance coverage (Finkelstein et al, 2012).

Using information contained in the CMS administrative data, I can construct my sample in accordance with my research design. First, I restrict to Tennessee residents who were simultaneously enrolled in Medicaid and Medicare, as of the beginning of my study period (early 2008). I further restrict to those who originally qualified for Medicaid (and SSI) by virtue of disability, and who remained disabled as of the start of the study period. In addition, I restrict to those who were not enrolled in the private version of Medicare (Medicare Advantage) at any point in the study period.[12] Finally, I focus on the under-65 population throughout my primary analysis, for reasons detailed in the following section. In a set of accessory analyses, I restrict to the over-65 population.

3.1 Individual Characteristics and Enrollment Information

I track individual-level demographic and insurance enrollment information, using administrative data from CMS. While this data was originally compiled by CMS's Medicare program, it contains person-month level Medicare as well as Medicaid enrollment indicators, along with accompanying fields tracking the basis for Medicaid and Medicare eligibility (such as age or disability). This data also separately identifies whether each Medicare enrollee is enrolled in the public fee-for-service version of Medicare, or is instead under the private equivalent of Medicare Advantage. Finally, the data includes demographic information such as county of residence, age, and date of birth, all of which can subsequently be incorporated as regression controls.

While this data tracks enrollment into and out of Medicaid, it does not explicitly identify the reasons for these enrollment changes. As such, those getting removed from Medicaid as a result of the 2009 court ruling are identified indirectly, based on whether their exit

[12] This restriction is necessary, given the absence of Medicare Advantage claims in standard CMS datasets.

from Medicaid coincides in timing with the mass Medicaid disenrollments (which took place in July, August, or September of 2009). The resulting identifier will not be completely accurate, given that individuals could have exited Medicaid for alternative reasons over this same period, but ought to be precise to a high degree.[13]

3.2 Inpatient and Outpatient Utilization Metrics

I track inpatient and outpatient usage for my study population, including dual-eligibles as well as those in Medicare-only, using various administrative files from CMS. This data was originally compiled by the Medicare program, with the relevant subset of this data covering all Tennessee beneficiaries for the 2008-2011 period. Notably, this data comprehensively tracks utilization for the dually-enrolled as well as Medicare-only populations, given that Medicare serves as the primary payer for both. The data takes the form of an individual-level panel, which I construct by linking together several different CMS administrative files, including individual-level Medicare enrollment data, claims-level inpatient data, and claims-level outpatient data. This linking is done using standardized beneficiary ID's common to all these data.

This data provides information on the timing of each visit, at a month-year level. The data also provides visit-level information on treatment intensity and composition, including the length of stay (for inpatient visits), types and number of procedures performed, and total visit cost. These cost measures reflect actual payment amounts owed to providers, and so would not be mechanically affected by changes in secondary coverage, given that payment amounts are inclusive of cost sharing. For most of my analyses, I aggregate this data to a person-quarter level, and include those without any utilization as part of the sample (as such, sample selection is not conditional on usage of inpatient or outpatient care).

In Table 1, I present average person-quarter level utilization for my main analytic sample,

[13] Examining periods outside of the mass 2009 disenrollments, I find exit rates from Medicaid that are 5% of rates under the mass disenrollments. This could imply that 5% of the Medicaid exits flagged as court-driven, under the present methodology, actually took place for other reasons if assuming that the rate of non-court driven Medicaid exits remained constant throughout.

consisting of Tennessee dual-enrollees as of early 2008. I break these measures out for two separate groups, based on whether an individual was subject to involuntarily disenrollment from Medicaid following the 2009 'Cluster Daniels' court ruling. I find that the involuntarily disenrolled group has substantially lower overall utilization (~25%), although this figure doesn't decompose treatment impact from underlying selection differences.

3.3 Utilization Composition Metrics

I also track health care composition and value, using the original inpatient and outpatient Medicare claims data, which I combine with proprietary algorithms to granularly categorize different types of care.

For outpatient care, I break out utilization into different service categories, particularly those associated with higher as well as lower value care. Specifically, I identify care that is diagnostic or imaging based, which is considered relatively more wasteful. I also identify physician office visits, which are typically considered more valuable and preventive in nature. Finally, I identify visits that might be more elective or procedure based, such as outpatient surgeries.

For inpatient care,. I similarly break out care into higher as well as lower value categories, as well as into discretionary and non-discretionary types. Specifically, I measure the preventability of hospital visits based on whether the visit originated in the ER, given that ER admissions are relatively more prevention sensitive. In addition, I measure whether a hospitalization is elective using an algorithm put together by the Dartmouth Institute.

4 Identification and Empirical Strategy

In addition to their cost-sharing exposure, Medicare-only and Medicare-Medicaid dual enrollees will also differ in their underlying health and demographic characteristics; dual-eligibles typically suffer from worse health and greater co-morbidities than Medicare-only

enrollees, even within the disabled population specifically (CBO, 2013). As such, any naïve comparison between those in Medicare-only and those simultaneously in Medicare and Medicaid may reflect differences in enrollee characteristics, rather than in cost-sharing exposure. To decompose the effect of cost-sharing from that of underlying patient composition, I focus on situations where individuals involuntarily switch from dual-enrollment to enrollment in Medicare-only. In such situations; only Medicaid enrollment status will change, while other patient-specific characteristics will remain fixed over that limited time window.

As part of my identification strategy, I implement a difference-in-differences approach, leveraging a large-scale Medicaid disenrollment that arose in Tennessee as a result of the 2009 "Cluster Daniels" court decision. The decision lifted a 20 year court injunction that had prohibited Tennessee from kicking anyone off Medicaid who had originally qualified through SSI, thereby allowing Tennessee to disenroll those no longer eligible for Medicaid. Altogether, the treatment group will be made up of disabled individuals who were simultaneously in Medicare and Medicaid, as of the start of the study period (2008), and who were kicked off Medicaid in 2009 as a direct consequence of the court ruling. Their Medicaid ineligibility would arise entirely from economic ineligibility for SSI, and not at all be due to health factors,[14] The control group, meanwhile, is identical to the treatment apart from its continuing eligibility for Medicaid, as of the time of the ruling. My primary analyses focus on the under-65 population, while a companion set of analyses focuses on those over 65. This research strategy is somewhat analogous to the approach employed in Finkelstein et al (2012), which instrumented for Medicaid status using Oregon's lottery for Medicaid coverage. This approach is also analogous to that of Garthwaite et al (2014), which leveraged legislation-driven Medicaid disenrollments in examining related labor market effects.[15]

Altogether, the estimating equation for my analysis takes the following form, for individ-

[14] These economic requirements, which coincide with the requirements for SSI, are income as well as asset based.

[15] Coincidentally, that disenrollment also took place in Tennessee, although it was driven by legislative action, rather than by a judicial ruling. Further, that disenrollment affected a very different population from the one examined here, as it impacted adults who weren't disabled and who weren't simultaneously in Medicare.

ual i, at time t, where the underlying data is aggregated to an individual-quarter level. Also included below are gender, quarter-year, county, and age fixed effects.

$$y_{it} = \alpha + \beta_0 * GroupDisenrolled_{it} + \beta_1 * PostDisenrollment_t$$
$$+ \beta_2 * GroupDisenrolled_{it} * PostDisenrollment_t + X_{it} * \gamma + \varepsilon_{it}$$

(Equation #1)

Not everyone in the treatment group will be subject to the actual treatment over the entire post-period, as a small fraction ends up re-enrolling in Medicaid. As such, the the estimated value of β_2 would capture an intent-to-treat effect, and would need to be appropriately scaled to reflect the effect of the actual treatment.

4.1 Endogeneity and Other Empirical Concerns

The primary identifying assumption is that the timing of Medicaid disenrollment is exogenous, and does not coincide with changes in health or in other relevant criteria. Altogether, multiple aspects of my research design and institutional setting could make this assumption plausible. First, many of those eventually disenrolled likely became ineligible for Medicaid long before 2009, given that Tennessee was prohibited from checking or enforcing this group's Medicaid eligibility for the 20 years prior. My assumptions could therefore appear plausible, given that most existing group differences would likely have developed long before the disenrollments and not in simultaneity to them. Moreover, any existing differences between these groups should be primarily socioeconomic rather than explicitly health based, as my sample is restricted to the continuing disabled.

A related identifying assumption is that no other statewide changes were taking place around the time of the disenrollments, particularly those differentially impacting the treatment group. I find some support for this assumption through the absence of differential pre-trends, specifically over the period prior to the court ruling, with corresponding results shown in the bottom panel of Table 3 and in Tables B.4 through B.6. This assumption is

also aided by the judicial nature of the disenrollments, compared to the alternative of being legislation-driven, as court rulings may be more disconnected from statewide developments and trends. I also examine trends over the eight month interval between the original court ruling and the actual disenrollments, to account for the effects of intertemporal substitution and other anticipatory dynamics. As discussed in further detail in Section 5.2, I do find some evidence for such anticipatory effects, and address them in part by excluding this interim period from many analyses. In any case, these anticipatory effects do not appear to have significant implications for the overall results.

In addition to general assumptions around Medicaid disenrollment, a further implicit assumption is that disenrollment affects care through cost-sharing, and not through alternative mechanisms. Specifically, this approach assumes away possible effects from differences in services covered, specifically from long-term care being covered only under dual and not under Medicare-only enrollment. The approach similarly assumes away program interaction issues associated with dual-coverage, which also appear largely specific to long-term care.[16] To mitigate for this possibility, I restrict to beneficiaries with zero long term care use over the pre-period, with the results appearing robust to this restriction. One limitation of this approach is that I am unable to exclude individuals who would have been first-time long-term care users under a counterfactual scenario, although this should not be consequential given high persistence in its use. Finally, my approach assumes away possible concurrent changes to either prescription drug coverage or drug subsidy eligibility. As shown in Table 2, I find no evidence for such coverage changes, given that that the vast majority remain in Part D and also under LIS (low-income drug subsidy) following disenrollment. Similarly, I find no evidence of shifts within Part D to different types of coverage, following disenrollment.[17]

[16] To cite one example of program interactions, dual-eligibles may end up being readmitted to hospitals at higher rates than Medicare-only enrollees, as readmission would qualify nursing homes for the higher Medicare payment rate for the 30 days following patient return as this would count as post-acute care (MedPAC, 2011). Meanwhile, Medicaid would typically continue paying for the resident's nursing home while they're in the hospital.

[17] Shifts to different types of Part D plans should not have material consequences here in any case, given that Part D plans differ primarily in terms of cost-sharing and formulary coverage. Further, those in the sample would get insulated from the cost-sharing piece of this.

Another empirical threat could come from potential take-up of other secondary insurance, following Medicaid disenrollment, as secondary insurance would insulate the treatment group from the full effects of cost-sharing.[18] To tackle this issue, I consider alternative sources of secondary coverage such as Medigap and employer-sponsored coverage, finding that these do not materially affect my results due to their very limited take-up. Relatedly, I exclude those in Medicare Advantage from my sample, given that Medicare Advantage typically has more generous cost-sharing coverage and also given that my data contains no MA claims. I go over these findings in further detail in Section 5.1.

A final concern comes down to the generalizability of these results to the broader dual-eligible and disabled populations. First, I argue that these results are broadly applicable, given the sample population's modest finances. Specifically, the vast majority of those disenrolled appear to financially qualify for Part D low-income subsidies, implying incomes in the bottom third (below 150% FPL) of Medicare beneficiaries (Jacobson et al 2017). Another concern around generalizability could come from treatment non-compliance, given that a portion of those disenrolled subsequently re-enroll into Medicaid, with those remaining off Medicaid presumably being relatively healthier and wealthier. However, I attempt to address this issue, by accounting for degree of non-compliance in interpreting my estimates.

5 Results

5.1 Effect of Disenrollment on Medicaid, Drug, and Secondary Insurance Coverage

The effect of the 2009 'Cluster Daniels' court ruling on Medicaid enrollment is documented in Figure 1, showing a precipitous drop in Medicaid coverage rates among the treatment group, with no corresponding change among the control group; note that this is by construction,

[18] Unfortunately, standard Medicare administrative data does not track individuals' enrollment status in secondary insurance.

based on the underlying group definitions. Further, among this treatment group, I document a steady increase in Medicaid coverage rates over the course of the post-period, implying that some of those disenrolled later re-entered Medicaid. The high observed rate of re-entry into Medicaid could plausibly come from renewed Medicaid eligibility, through such channels as the medical spend down program (on the basis of high ongoing medical expenses) or financial restructuring (on basis of shedding assets).

These basic findings are confirmed by regression results, shown in Table 2, which suggest that about 1/3 of those originally disenrolled will re-enter Medicaid by the end of the study period. This is reflected in the coefficient estimates under column one, at the bottom of Panel B, which capture the difference in Medicaid enrollment rates between the pre-period effectively level of 100% and the end of the post-period. My main estimates will consequently reflect an 'intent-to-treat' effect, and will need to be scaled appropriately to reflect the actual impact on those treated. These analyses are conducted using the baseline specification (Equation #1), where the outcome of interest is at a person-quarter level. Meanwhile, the sample is restricted to Tennessee residents who were under-65 and disabled as of the start of the study period, and who were also initially dually-enrolled in Medicare and Medicaid. It is further restricted to those who were never in Medicare Advantage, at any point over the study period.

I also try to identify other potential changes coinciding with Medicaid disenrollments, which could similarly have implications for utilization and health. First, I examine potential accompanying changes in individual drug-coverage. Altogether, I find no impact on rates of Part D drug coverage, with the corresponding results presented in Table 2. These coverage rates could have theoretically shifted, given that Part D enrollment would go from being automatic among dual-eligibles, to being voluntary for Medicare-only enrollees. Similarly, I find no meaningful reductions in receipt of Part D cost-sharing subsidies, which are available to those with low income.[19] Further, I conclude that potential changes in the specific Part

[19]The eligibility threshold for these Part D subsidies is at incomes of around 150% FPL, compared to a typical eligibility threshold of 75% FPL for SSI and Medicaid.

D plan of enrollment should not be a major factor here, given that Part D plans differ most materially in cost-sharing policies; these differences would be largely blunted for the sample population, however, through its high-rates of cost-sharing subsidy receipt.

Alongside these analyses, I also investigate how much individuals substitute to alternate secondary insurance, following Medicaid disenrollment. Such substitution could affect the overall impact of Medicaid disenrollments, given that it would insulate beneficiaries from the accompanying imposition of cost-sharing. I ultimately find no meaningful take-up of other secondary insurance following Medicaid disenrollment, meaning that secondary insurance should not be expected to blunt Medicaid disenrollment's effects.

First, I find no meaningful substitution to employer-sponsored supplementary coverage, specifically the form typically carried by the under-65 population. For this population, such coverage typically takes the form of comprehensive rather than wrap-around insurance, and could be available to the disabled through spouses or past employers (Jacobson et al, 2014, McArdle et al 2014).[20] In particular, I find that this coverage type's market share only increases from three to five percent of the underlying population, under Medicaid disenrollment. Similarly, in results not shown, I find that my main estimates are not sensitive to exclusion of those with employer-sponsored supplementary coverage.

In addition, I find no significant take-up of supplementary coverage in the form of Medigap, with such coverage furthermore largely non-existent among this population.[21] First, I argue that this under-65 population was effectively locked out of Medigap, due to the lack of guaranteed issue Medigap provisions in Tennessee over this time period, along with Medi-

[20] I can explicitly track comprehensive employer-sponsored coverage in the data, as under coordination of benefit rules, this coverage will be primary to Medicare and will pay first. As such, the presence of this coverage can be tracked indirectly, using coordination of benefit fields in the Medicare data. Unfortunately, I will not be able to track employer-sponsored coverage when it comes from firms under 100 employees in size, as for these cases Medicare gets designated as the primary payer. I also will not be able to track employer-sponsored coverage in wrap-around form, but this limitation should not be problematic, given that such coverage is only common among the over-65.

[21] Only 2% of all under-65 disabled on Medicare appear to have supplemental Medigap coverage. Moreover, this figure is inclusive of states with guaranteed issue and open enrollment Medigap provisions for that population. In such states, Medigap enrollment is actually likely to be higher than in Tennessee, where such provisions are absent.

gap's limited accessibility to the disabled in their absence (Neuman and Cubanski 2016).[22] Moreover, I find that statewide Medigap enrollment only increases by 2,000 following Medicaid disenrollment, even inclusive of the over-65 population that had more ready access to it; unfortunately, I am unable to identify the specific change among the under-65 population, due to data limitations.[23] This compares to a 25,000 overall decrease in Medicaid enrollment among dual-eligibles, for the combined under as well as over 65 populations (KFF 2013).

While Medicare Advantage could theoretically serve as additional secondary insurance option, given that it typically offers more generous cost-sharing terms than Traditional Medicare, I conclude that such coverage is not applicable to the study population.[24] First, I find that Medicare Advantage enrollment rates do not meaningfully change following Medicaid disenrollment, as shown in Table A.1. Further, I already address this issue through the full exclusion of Medicare Advantage enrollees from the main sample.[25]

5.2 Effect of Medicaid Disenrollment on Utilization and Spending

I then consider the effect of Medicaid disenrollments, specifically through the accompanying imposition of cost-sharing, on spending and utilization measures. For these analyses, my sample restrictions remain unchanged from before, with the observation-level being at a person-quarter level, and the outcome measures reflecting individual-quarter level utilization and spending. In addition, as before, I restrict to beneficiaries under 65.

First, I find that Medicaid disenrollment counterintuitively results in increased rather than decreased overall spending. Specifically, as indicated in the first panel of Table 3,

[22] Prior to 2011, Medigap plans in Tennessee were only offered on a guaranteed-issue basis to the over-65 population, with that guaranteed-issue also limited to the 6 month initial period of eligibility. In January 2011, as a result of new regulations, this policy was extended to the under-65 population (State of Tennessee, 2010).

[23] Unfortunately, standard CMS claims and enrollment data does not track information on Medigap enrollment at an individual-level.

[24] Medicare Advantage cannot be combined with other forms of traditional secondary insurance, such as Medigap.

[25] Unfortunately, I am not able to examine the effect of Medicare Advantage enrollment on health care use, particularly through Medicare Advantage's effects on cost sharing, as my data does not contain claims for Medicare Advantage.

overall spending increases by $163 (or 6%) following disenrollment. In addition, this increase appears to be driven entirely by inpatient care, given a $199 (or 15%) inpatient spending increase that more than offsets a concurrent $36 (or 3%) decrease on the outpatient side. These original point estimates reflect an intent-to-treat effect and so will need to be scaled appropriately, since some of those initially disenrolled will subsequently re-enter Medicaid. In addition, the main pre-post analyses here exclude the 8 month interval between disenrollment notification and its actual implementation, to avoid capturing anticipatory effects; these effects are separately examined and discussed below.

Figures 3 and 4 confirm these basic findings, documenting spending effects for the period following the court ruling and just for the treatment group, made up of outpatient spending decreases and inpatient increases. Consistent with the results in Table 3, Figures 3 and 4 also show no differential pre-trends over the period preceding the court ruling. In the figures, the leftmost vertical line corresponding to the time of notification and the rightmost corresponding to the time of actual disenrollment.

In these analyses, I also consider trends over the period between disenrollment notification and its actual implementation. Theoretically, intertemporal substitution may produce higher utilization over this specific period, with individuals pushing elective care forward in anticipation of future cost-sharing increases. The reverse effect is also theoretically possible, as individuals may reduce utilization due to misinterpretation, by mistaking disenrollment notification for already being disenrolled. Altogether, I find evidence for both phenomena, with their net overall impact appearing ambiguous. First, consistent with an intertemporal substitution story, I find increases in overall outpatient utilization over this period that are concentrated in elective care. Simultaneously, consistent with individuals assuming they've already been disenrolled, I find reductions in certain outpatient utilization such as office visits. Similarly consistent with individuals assuming they're already off Medicaid, I find increases in overall inpatient care, particularly in offset-sensitive emergency visits. To deal with these anticipatory and substitutionary effects, I have excluded this interim period be-

tween disenrollment notification and implementation from my main analyses, and have also undertaken other relevant robustness tests.

5.3 Mechanisms Underlying Main Effects

I proceed by examining the mechanisms driving the observed increase in overall inpatient spending, alongside the accompanying decrease in outpatient spending. In looking at mechanisms, I am particularly interested in identifying how moral as well as behavioral hazard might drive the overall results, given that cost-sharing could reduce moral hazard while simultaneously increasing behavioral hazard. At first glance, the results appear consistent with the presence of both, although this is suggestive rather than definitive. For example, behavioral hazard could plausibly drive the observed inpatient effect, given that inpatient care is more prevention sensitive and would increase with increased behavioral hazard. Similarly, moral hazard could plausibly drive the observed outpatient effect, as outpatient care is more discretionary and would decrease under increased moral hazard. To investigate this more definitively, I break out inpatient and outpatient utilization into different subclasses of care, specifically to examine differential impacts on preventable as well as discretionary care.

Focusing first on the outpatient setting, I examine whether the overall outpatient effect is heterogeneous across care types, finding a comparable effect across low as well as high-value care. This result is consistent with previous findings from the RAND Health Insurance experiment and a more recent high-deductible health plan study (Manning et al 1987, Brot Goldberg et al 2017), since these also found reductions in necessary as well as wasteful care. These outpatient results could plausibly come through moral hazard, particularly the observed reductions in low-value care, which could come from lower moral hazard under cost-sharing. Altogether, detailed estimates are presented in Table 4, showing reductions in imaging spending under Medicaid disenrollment, which is considered a relatively more unnecessary and wasteful form of care. Meanwhile, the estimates indicate even more pro-

nounced reductions in primary care and specialist office spending, care categories considered to be of relatively higher-value.

I then turn to inpatient care, examining whether there is a differential effect across preventable and non-preventable visits, as well as across discretionary and non-discretionary care. Altogether, I find that the observed increase in inpatient spending is driven almost entirely by prevention-sensitive and non-discretionary visits. This inpatient result could be consistent with a behavioral hazard explanation, given that it is driven by prevention-sensitive hospitalizations and coincides with a decrease in preventive outpatient care. To differentiate between preventable and non-preventable visits, I look specifically at whether an admission originated directly in the ER, given that ER admissions are generally thought to be more prevention-sensitive than non-ER ones. Meanwhile, I define whether a visit is discretionary based on whether it is classified as elective. The corresponding results, which are shown in Panel A of Table 5, indicate a 20% increase in spending on emergency hospitalizations alongside a 10% increase in non-emergency inpatient spending. Similarly, the results indicate a 20% increase in spending on non-elective hospitalizations, compared to only a 3% increase in elective inpatient spending. In Panel B, I find comparable effects on the number of hospitalizations, suggesting that these spending effects are driven by changes in the number of hospitalizations rather than by utilization changes conditional on hospitalization.

Finally, I rule out other plausible mechanisms for these observed effects, apart from moral and behavioral hazard. In particular, I rule out cross-service cost-sharing differences as a driver of my results. First, I find no evidence that individuals are deliberately substituting towards services with lower cost-sharing, given no reductions in traditional Medicare cost-sharing liability following Medicaid disenrollment. The traditional Medicare cost-sharing measure is inclusive of all patient as well as secondary insurance liability, meaning it should not be mechanically affected by secondary insurance coverage, and moreover should decrease under substitution towards lower cost-sharing services. Even independent of this, cost-sharing minimization could not explain most of the inpatient care increase, given that the

increase is concentrated among non-discretionary hospitalizations that should not be directly sensitive to cost-sharing. Altogether, corresponding regression results are presented in Table B.2. While only marginal and not average cost-sharing rates would be relevant factors here, it's worth noting that average cost-sharing rates for this population are actually lower on the inpatient than outpatient side, given total inpatient cost-sharing of 11% and outpatient of 20% as shares of corresponding spending.[26]

5.4 Heterogeneity in Cost-Sharing Effects Across Populations

Finally, I examine whether cost-sharing imposition might differentially affect population with different health characteristics. I specifically explore whether cost sharing imposition might lead to greater spending increases among sicker populations, and whether such increases could come largely from preventable care, in line with past research (Chandra et al 2010). For these analyses, I make use a number of different approaches and metrics.

First, I introduce a modified spending measure that is denoted in logs rather than in levels, which is meant to capture effects on the median individual in the sample rather than being disproportionately weighted towards higher-cost beneficiaries. These results indicate reduced rather than increased spending from cost-sharing for median beneficiaries, in contrast to the non-logged results showing spending increases, which presumably driven by higher-cost beneficiaries.[27] These differential impacts across higher and lower cost groups appear to be specifically attributable to differential inpatient spending effects, with inpatient spending increases appearing to be more modest among lower cost groups. Detailed estimates are presented in Table 6, with the specific coefficients implying an arc-elasticity from cost-sharing of -.19, for the median beneficiary.[28] This arc-elasticity figure is compa-

[26] Cost-sharing in the Medicare setting is non-linear, given the presence of separate inpatient and outpatient deductibles. Above the outpatient (Part B) deductible, cost-sharing applies in the form of 20% co-insurance. Finally, above the inpatient deductible, co-pays apply to all hospital days beyond the first 60, within a single benefit period.

[27] Specifically, this is because the logged spending results could be seen as capturing impact for the median beneficiary, while the non-logged spending results could capture impact on relatively higher cost beneficiaries. This is given the sample weighting that is implicit under both approaches.

[28] This calculation assumes effective cost-sharing of 15%, consistent with averages for this population. This

rable with the -.2 estimate for a commercially-insured population, from the RAND health insurance experiment, as well as the an estimate of -.11 from the Medicare and Medigap setting (Cabral and Mahoney 2014). Altogether, this finding highlights the significant implications of heterogeneous treatment effects, particularly since the overall effect ends up being opposite in sign from the effect on the typical beneficiary.

I proceed by examining differential cost-sharing effects more granularly, based on the CCI (Charlson Co-Morbidiy Index) assigned to patients, given that this could more precisely capture patient sickness.[29] Consistent with the previous set of results, I find spending decreases among the healthiest subpopulation, alongside increases among the sickest. I also find these heterogeneous effects to stem largely from inpatient care, particularly from ER-based admissions, given a much more pronounced increase in admissions among the sick than among the healthy. As shown in Table 7, the inpatient spending increase offsets 95% of the corresponding outpatient decrease for the 1 and 3 Charlson Score range, alongside an offset of 438% for the 4+ range (corresponding to the sickest 10% of this sample).

In additional analyses, I confirm the robustness of these results, while ruling out mechanisms and explanations other than those described above. First, I rule out the possible explanation of differential pre-trends, given the absence of such pre-trends in separate results not shown. Second, I eliminate the possible explanation of cross-group differences in Medicaid disenrollment, as Table By shows these rates to be comparable. Finally, I find that these results are not accounted for by differential cost-sharing exposure, for example from higher cost groups facing different cost-sharing and substituting to different services as a result. Specifically, as shown in Table By, I find no meaningful changes in cost-sharing liability (inclusive of patient and secondary insurance portions) across any of these differ-

represents an approximation, given that a 20% coinsurance applies only to outpatient Medicare services, while a copay applies for inpatient services and is charged for each day in hospital. Further, deductibles are in place for Part A as well as Part B services. The arc-elasticity estimate is given by [q2-q1/((q2+q1)/2)]/[(p2-p1)/(p2+p1)/2)], where q1 and p1 correspond to dual-eligibles' utilizations and prices, and q2 and p2 correspond to those of Medicare-only enrolled; note that for dual-eligibles, p1 is defined as 0.

[29] The index is assigned based on patient utilization for the period preceding the Medicaid disenrollments, and thus could be considered an ex-ante measure.

ent populations, which would otherwise be expected if patients were substituting to lower cost-sharing services.

Finally, to explore differential effects of cost-sharing, I compare its impact on the over-65 and the under 65 populations. Notably, the over-65 population is of policy interest in their own right, as it makes up a significant share of dual-enrollment and an even higher share of spending. Consistent with my previous findings, I find a more pronounced spending increase among the sicker over-65 population, whose overall medical spending is 53% greater than the under-65 group's. As shown in Table A2, Medicaid disenrollment is associated with a 10% spending increase among the over 65 population, compared to a 6% increase among the under 65. The more pronounced increase in the over-65 group comes largely from inpatient spending, particularly from increased ER-based admissions, consistent with the previous results. Further, differences in effect sizes across the under and over-65 populations do not appear to be driven by alternative mechanisms, which would be unconnected to the groups' relative sickness. For example, these differential effects do not appear to come from differences in underlying Medicaid disenrollment rates, given that these rates are comparable across the two populations.

6 Conclusion

I examine the effects of cost-sharing among a previously understudied population, the dually enrolled in Medicaid and Medicare, who account for 35% of all Medicare and Medicaid spending altogether. For doing so, I leverage a 2009 court-driven Medicaid disenrollment in Tennessee, which shifted 25,000 beneficiaries from dual to Medicare-only coverage. While Medicaid disenrollment could affect care through a variety of mechanisms, I isolate its impact through cost-sharing specifically, finding that cost-sharing surprisingly produces an increase in this population's medical spending. From this, I conclude that cost-sharing could be counterproductive in this setting, as well as among similarly sick or financially needy groups.

Looking more closely at mechanisms, I find that this increase could arise from greater behavioral hazard under cost-sharing, as the increase comes specifically from prevention-sensitive inpatient care. Consistent with this behavioral hazard explanation, I find a concurrent reduction in preventive outpatient visits. Although behavioral hazard's overall effect outweights moral hazard's here, reductions in moral hazard under cost-sharing appear to simultaneously impact spending, through reductions in low-value outpatient care. I additionally find that cost-sharing's effects vary by patient sickness, with the median beneficiary actually experiencing a spending reduction, due to more modest increases in their inpatient care.

There are a few notable limitations to my study, which future research could tackle. First, I am unable to identify effects on mortality and various other measures of patient well-being, given the absence of corresponding data. This prevents me from drawing more sweeping conclusions on cost sharing's welfare impact, particularly in terms of individual health outcomes. An additional limitation of this study is its focus on a small subset of dual-eligibles within a single state, a group that may not be fully generalizable to dual-eligibles nationwide or to the entire disabled population. Given these limitations, future work can more comprehensively examine how cost sharing's effect varies across populations, particularly those with differing health and socioeconomic characteristics.

My study carries a number of policy implications for the Medicaid and Medicare programs, as well as for other insurers with higher-cost enrollees. First, the study suggests that imposing cost-sharing on sicker and more financially needy individuals may actually be counterproductive, as it may increase rather than reduce overall medical spending. Moreover, any such spending increase would be borne by Medicare and the federal government, while individual states would actually see reduced spending through lower Medicaid expenditures. This highlights possible conflicting interests between state and federal governments, buttressing the case for federalizing dual-eligible coverage. This study also identifies possible benefits from impoving access to supplemental Medicare coverage, for high-cost beneficia-

ries in particular, consistent with proposals to broaden under-65 Medigap access through guaranteed issue provisions (114th Congress 2016). These findings are also consistent with proposals to expand supplemental Medicare coverage through Medicaid, particularly by raising the disabled eligibility threshold from 75% to 100% FPL of income, as some states have done (Watts et al 2016).

Notably, my study finds that cost-sharing reduces spending among less sick beneficiaries, which in combination with the earlier result offers some additional implications.. First, it highlights the need to tailor cost-sharing rules to specific populations and also to specific care types, for which cost-sharing might be most productive. These findings also imply a need for cost-sharing alternatives, for cases where cost-sharing might be productive but is otherwise impractical or infeasible. For dual-eligibles specifically, alternatives could come in the form of managed care, along the lines of integrated managed care initiatives currently being rolled-out in select states. Another policy option could involve the pairing of cash vouchers with increased cost-sharing, as this combination could properly incentivize patients without reducing affordability. Several states have previously proposed similar policies, with Indiana having been the first actual adopter (Saloner et al 2014).

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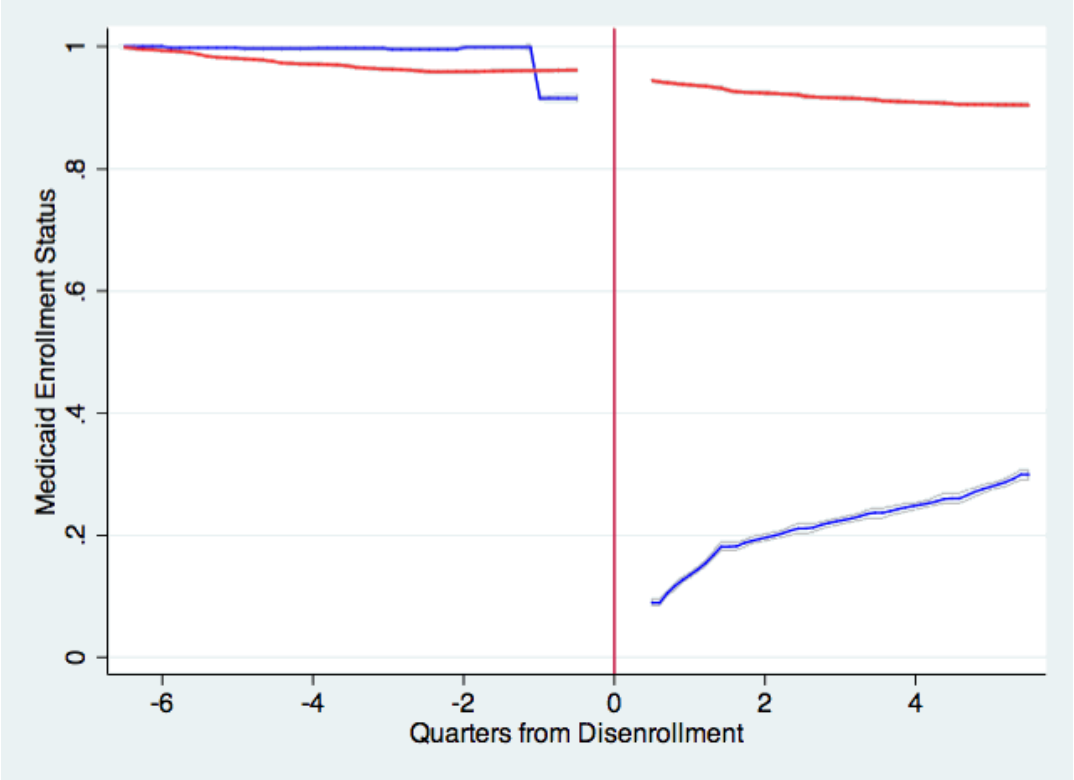
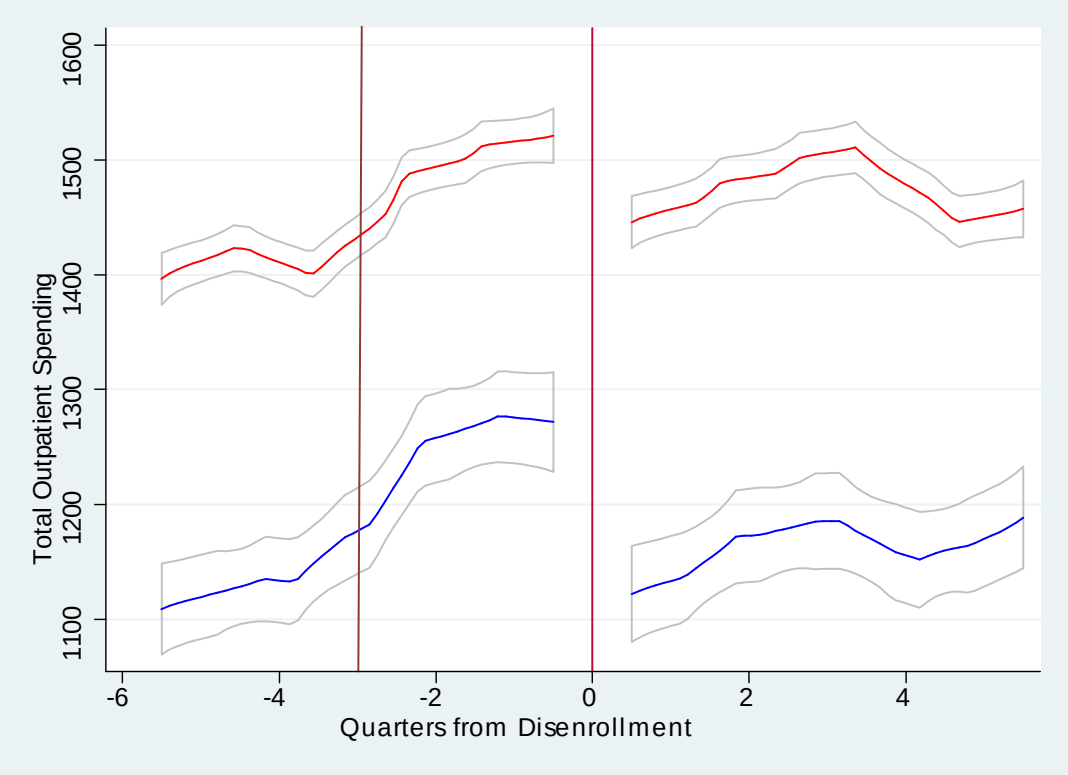
Figure 1: Effect of Court-Ruling on Medicaid Enrollment Status



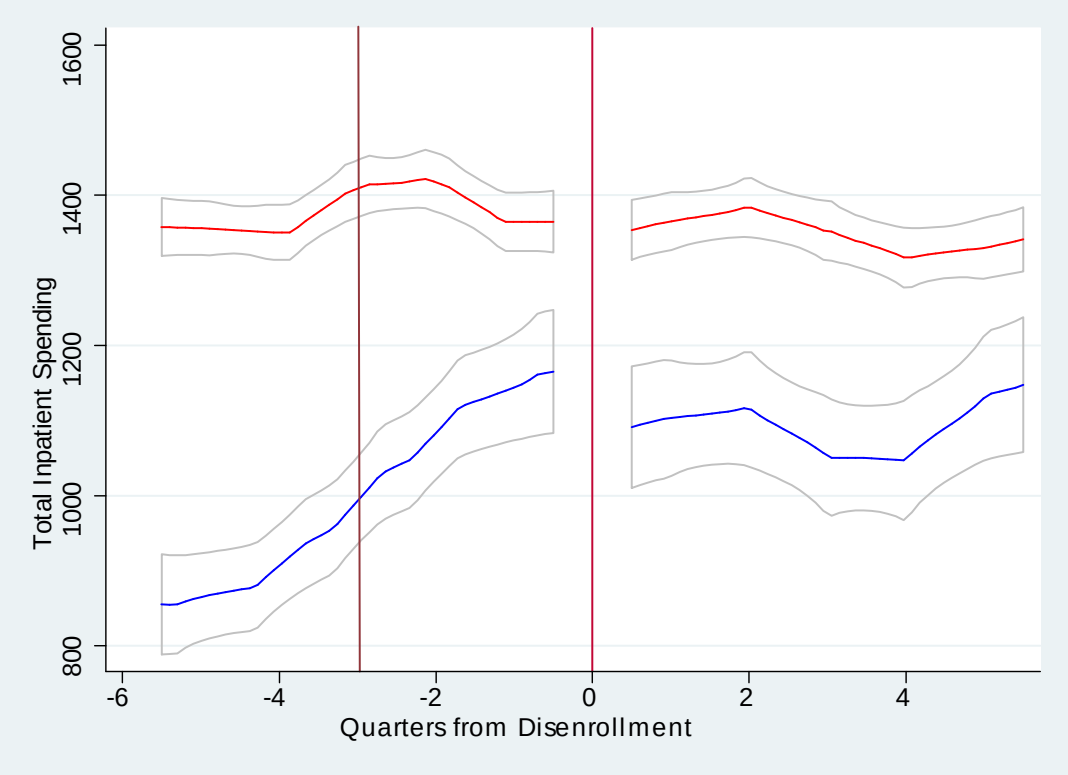
Blue: Invol. and Exogenously Disenrolled from Medicaid, via Cluster Daniels (Treatment)
Red: Not Disenrolled via Cluster Daniels (Control)

Figure 2: Effect of Court-Driven Medicaid Disenrollments on Outpatient Spending



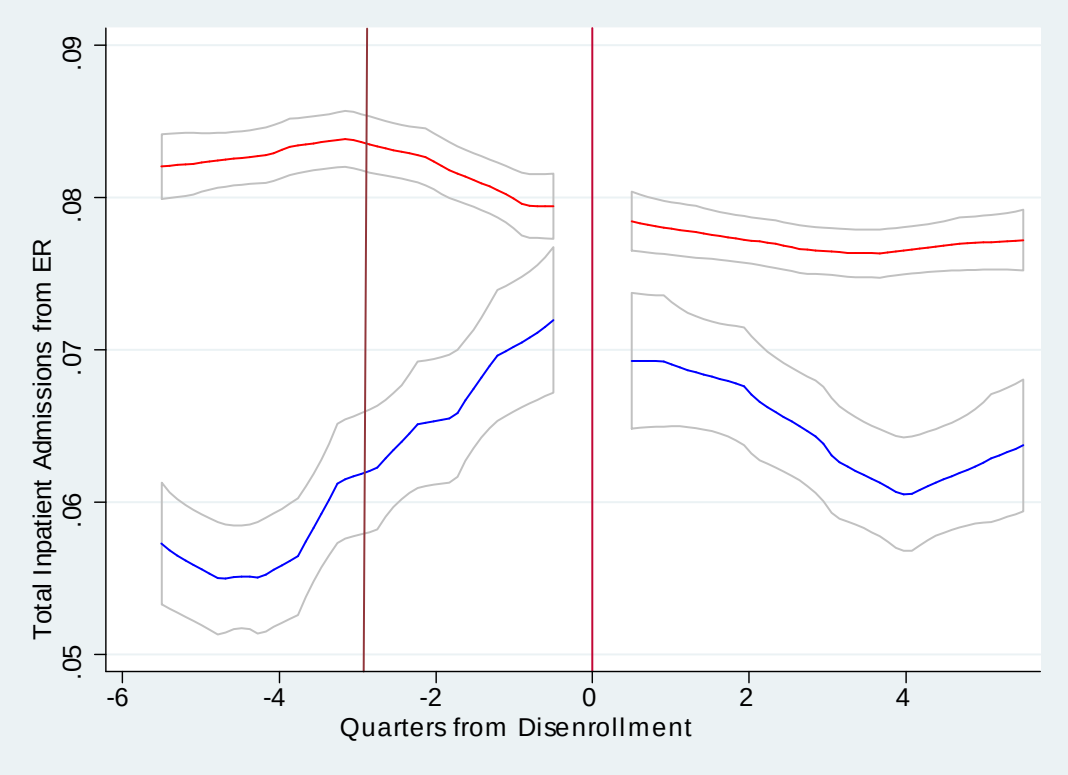
Blue: Invol. and Exogenously Disenrolled from Medicaid, via Cluster Daniels (Treatment)
Red: Not Disenrolled via Cluster Daniels (Control)

Figure 3: Effect of Court-Driven Medicaid Disenrollments on Inpatient Spending



Blue: Invol. and Exogenously Disenrolled from Medicaid, via Cluster Daniels (Treatment)
Red: Not Disenrolled via Cluster Daniels (Control)

Figure 4: Effect of Court-Driven Medicaid Disenrollments on Inpatient ER Admissions



Blue: Invol. and Exogenously Disenrolled from Medicaid, via Cluster Daniels (Treatment)
Red: Not Disenrolled via Cluster Daniels (Control)

Table 1: Summary Statistics

	Disenroll Group	Non-Disenroll Group
Coverage		
Medicaid Enrollment Status	0.585	0.944
	(0.493)	(0.228)
Drug Coverage Status	0.999	0.999
	(0.016)	(0.015)
Total Utilization		
Total Spending	2,078	2,824
	(7,415)	(8,585)
Inpatient Spending	978	1,363
	(5,751)	(6,658)
Outpatient Spending	1,100	1,461
	(2,912)	(3,342)
Composition of Care		
Outp Procedure Spending	235	306
	(739)	(827)
Inp Elective Spending	298	444
	(2,910)	(3,461)
Inp ER Spending	538	660
	(3,747)	(3,935)
Prevention		
Primary Care Visits	0.893	1.200
	(1.973)	(2.354)
N	134,661	682,314

Notes: Table presents summary statistics for various outcome variables of interest, where the unit of observation is at an individual-quarter level, for the 2008-2011 period. Summary statistics are broken out for those who were and who weren't disenrolled from Medicaid as a consequence of the 'Cluster Daniels' court ruling. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. Finally, the sample is restricted to those who were not in Medicare Advantage at any point in the sample period. Standard-errors are clustered at the individual level. Data is taken from CMS administrative data.

Table 2: Effect of Disenrollment on Medicaid and Drug Status

	(1)	(2)	(3)	(4)
	Medicaid Enrolled	Drug Cov		
		Any	*Part D*	*Subsidized Part D*
Mean	0.872	0.996	0.991	0.990
		Panel A: Main Specification		
Medicaid Disen.*Post	-0.767***	-0.005***	0.000	0.001
	(0.003)	(0.001)	(0.000)	(0.001)
N		679,565		
		Panel B: Leads and Lags		
Medicaid Disen*Pre-5 Q	-0.006***	0.001	0.000	0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Medicaid Disen*Pre-4 Q	0.009***	-0.003***	0.000	0.011***
	(0.001)	(0.001)	(0.000)	(0.000)
Medicaid Disen*Pre-3 Q		Baseline		
Medicaid Disen*Pre-2 Q	0.007***	-0.006***	0.001	0.009***
	(0.001)	(0.001)	(0.000)	(0.000)
Medicaid Disen*Pre-1 Q	0.009***	-0.006***	0.001**	0.007***
	(0.001)	(0.001)	(0.000)	(0.000)
Medicaid Disen*Q of Dis	-0.975***	-0.006***	0.000	0.007***
	(0.001)	(0.001)	(0.000)	(0.000)
Medicaid Disen*Post-1 Q	-0.810***	-0.006***	0.000	0.017***
	(0.004)	(0.001)	(0.000)	(0.001)
Medicaid Disen*Post-2 Q	-0.753***	-0.006***	0.000	-0.014***
	(0.004)	(0.001)	(0.001)	(0.002)
Medicaid Disen*Post-3 Q	-0.717***	-0.004***	-0.001	-0.009***
	(0.004)	(0.001)	(0.001)	(0.001)
Medicaid Disen*Post-4 Q	-0.686***	-0.003*	-0.001	-0.009***
	(0.004)	(0.001)	(0.001)	(0.001)
N		816,975		

Notes: Table presents linear regression models, where the outcome variables include Medicaid and drug coverage related indicators. The terms of interest are Medicaid Disen., interacted with pre and post terms. Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', who subsequently were in Medicare-only. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. In addition, the sample is restricted to those who were not in Medicare Advantage at any point in the sample period. Finally, the top panel excludes the time period between the court ruling and disenrollment implementation. Standard-errors are clustered at the individual level. Data is taken from CMS administrative files.

Table 3: Effect of Disenrollment on Medical Spending

	(1)	(2)	(3)
		Spending	
	Outpatient+Inpatient	*Outpatient*	*Inpatient*
	Panel A: Main Specification		
Medicaid Disen.*Post	163***	-36	199***
	(57)	(23)	(43)
Mean Spend (Levels)	2,709	1,402	1,307
Percent Effect	6.0%	-2.6%	15.1%
	(2.1%)	(1.6%)	(3.3%)
N		679,565	
	Panel B: Leads and Lags		
Medicaid Disen.*Pre-5 Q	-104	-29	-75
	(83)	(25)	(73)
Medicaid Disen.*Pre-4 Q	-98	-23	-75
	(72)	(23)	(62)
Medicaid Disen.*Pre-3 Q		Baseline	
Medicaid Disen.*Pre-2 Q	90	33	56
	(89)	(24)	(78)
Medicaid Disen.*Pre-1 Q	205**	38	167**
	(86)	(26)	(73)
Medicaid Disen.*Q of Dis.	257***	4	253***
	(91)	(29)	(76)
Medicaid Disen.*Post-1 Q	71	-74**	145**
	(89)	(30)	(73)
Medicaid Disen.Post-2 Q	174*	-31	205**
	(98)	(31)	(80)
Medicaid Disen.*Post-3 Q	90	-48	139*
	(99)	(33)	(82)
Medicaid Disen.*Post-4 Q	21	-69**	90
	(96)	(33)	(78)
N		826,472	

Notes: Table presents linear regression models, where the outcome variables include person-quarter aggregated inpatient and outpatient spending measures, reflecting the overall amounts paid to providers. The terms of interest are Medicaid Disen., interacted with pre and post terms. Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', who subsequently were in Medicare-only. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. In addition, the sample is restricted to those who were not in Medicare Advantage at any point in the sample period. Finally, the top panel excludes the time period between the court ruling and disenrollment implementation. Standard-errors are clustered at the individual level. Data is taken from CMS administrative files.

Table 4: Effect of Disenrollment on Outpatient Spending

	Effect Estimate	***N***
Overall Outp Spending	-0.275***	679,565
	(0.020)	
Log Effect on Outp Spend		
Surg Amt	-0.153***	679,565
	(0.019)	
Diag Amt	-0.146***	679,565
	(0.020)	
Imaging Amt	-0.104***	679,565
	(0.019)	
Office Visits	-0.230***	679,565
	(0.017)	
All Outp Hospital	-0.155***	679,565
	(0.020)	

Notes: Table presents linear regression models, where the outcome variables include person-quarter aggregated outpatient spending measures, reflecting the overall amounts paid to providers. The coefficients shown reflect the effect of Medicaid disenrollment on the treatment group, corresponding to the original coefficients from the Medicaid Disenroll*Post variable. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. In addition, the sample is restricted to those who were not in Medicare Advantage at any point in the sample period. Finally, the sample excludes the time period between the court ruling and disenrollment implementation. Standard-errors are clustered at the individual level. Data is taken from CMS administrative files.

Table 5: Effect of Disenrollment on Inpatient Spending and Utilization

	(1) *All*	(2) *Emerg.*	(3) *Non-Emerg.*	(4) *Elect.*	(5) *Non-Elect.*
Panel A: Total Spending					
Med. Disenroll*Post	199***	131***	68**	13	185***
	(43)	(26)	(28)	(21)	(32)
Mean	1,308	637	671	422	886
Percentage Effect	15.1%	20.6%	10.1%	3.1%	20.9%
	(3.3%)	(4.1%)	(4.2%)	(5.0%)	(3.6%)
Obs			679,565		
Panel B: Visit Counts					
Med. Disenroll*Post	0.015***	0.012***	0.003	0.002	0.013***
	(0.003)	(0.002)	(0.002)	(0.001)	(0.003)
Mean	0.129	0.077	0.052	0.031	0.098
Percentage Effect	11.6%	15.6%	5.8%	6.5%	13.3%
	(2.3%)	(2.6%)	(3.8%)	(3.2%)	(3.1%)
Obs			679,565		

Notes: Table presents linear regression models, where the outcome variables include person-quarter aggregated inpatient spending and utilization measures. The coefficients presented reflect the impact of Medicaid disenrollment on the treatment group. Specifically, Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', who subsequently were in Medicare-only. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. In addition, the sample is restricted to those who were not in Medicare Advantage at any point in the sample period. Finally, both panels exclude the time period between the court ruling and disenrollment implementation. Standard-errors are clustered at the individual level. Data is taken from CMS administrative files.

Table 6: Heterogeneous Effects of Disenrollment: Median versus Overall

	(1)	(2)	(3)
		Spending	
	Total	*Outpatient*	*Inpatient*
Panel A: Log Spending			
Medicaid Disen.*Post	-0.253***	-0.275***	0.062***
	(0.021)	(0.020)	(0.017)
N		679,565	
Panel B: Spending in Levels			
Medicaid Disen.*Post	162.61***	-36.14	198.74***
	(56.58)	(22.65)	(43.07)
Mean Spend (Levels)	2,709	1,402	1,307
Perc Effect	**6.0%**	**-2.6%**	**15.1%**
	(-2.1%)	**(1.6%)**	**(3.3%)**
N		679,565	

Notes: Table presents linear regression models, where the outcome variables include person-quarter aggregated spending measures. The top panel here corresponds to logged spending measures, while the bottom corresponds to the original measures. The coefficients presented reflect the impact of Medicaid disenrollment on the treatment group. Specifically, Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', who subsequently were in Medicare-only. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. In addition, the sample is restricted to those who were not in Medicare Advantage at any point in the sample period. Finally, all panels exclude the time period between the court ruling and disenrollment implementation. Standard-errors are clustered at the individual level. Data is taken from CMS administrative files.

Table 7: Heterogeneous Effects of Disenrollment: By Charlson Score

	Charlson Score		
	0	*1-3*	*4+*
Mean Total Spending:	1,002	2,647	8,110
Panel A: Effect on Tot Spending			
All	-91*	-4	639**
	(47)	(78)	(319)
Perc Effect	-9.1%	0.1%	7.9%
	(4.7%)	(2.9%)	(3.9%)
Panel B: Decomposing Effect			
Outp	-76***	-66**	-189
	(16)	(32)	(131)
Inp	-15	62	828***
	(38)	(60)	(241)
Offset	**19.6%**	**94.5%**	**438.2%**
Panel C: Inpatient Visits			
LOS	0.014	0.080*	0.467***
	(0.026)	(0.043)	(0.147)
Tot Visits	0.000	0.004	0.049***
	(0.003)	(0.005)	(0.016)
Admits from ER	0.002	0.003	0.044***
	(0.002)	(0.003)	(0.013)
N	282,277	304,490	92,798

Notes: Table presents linear regression models, where the outcome variables include person-quarter aggregated spending and utilization measures. The coefficients presented reflect the impact of Medicaid disenrollment on different subsamples, based on their assigned Charlson Score for the period preceding the disenrollments. For these different subsamples, the coefficients presented are for the term Medicaid Disen.*Post, corresponding to the effect of going from dual to Medicare only, on those dual-eligibles involuntarily disenrolled as a result of 'Cluster Daniels'. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. In addition, the sample is restricted to those who were not in Medicare Advantage at any point in the sample period. Finally, I exclude the time period between the court ruling and disenrollment implementation. Standard-errors are clustered at the individual level. Data is taken from CMS administrative files.

Appendix A: Results for Over 65 Population

Table A.1: Over 65: Effect of Disenrollment on Medicaid and Drug Cov

	(1)	(2)	(3)	(4)
	Medicaid Enrolled	**Drug Cov**		
		Any	*Part D*	*Subsidized Part D*
Medicaid Disen.*Post	-0.789***	-0.002**	-0.001	-0.011***
	(0.008)	(0.001)	(0.002)	(0.003)
Mean	0.909	0.998	0.993	0.991
N		168,454		
Medicaid Dis.*Pre-5 Q	-0.006***	-0.000	-0.000	0.000
	(0.002)	(0.000)	(0.001)	(0.000)
Medicaid Dis.*Pre-4 Q	0.010***	0.000	0.001	0.009***
	(0.002)	(0.000)	(0.001)	(0.001)
Medicaid Dis.*Pre-3 Q		Baseline		
Medicaid Dis.*Pre-2 Q	0.010***	-0.002	0.004***	0.007***
	(0.002)	(0.001)	(0.001)	(0.001)
Medicaid Dis.*Pre-1 Q	0.006***	-0.001	0.002	0.005***
	(0.002)	(0.001)	(0.001)	(0.001)
Medicaid Dis.*Q of Dis.	-0.978***	-0.002	0.001	0.005***
	(0.002)	(0.001)	(0.001)	(0.001)
Medicaid Dis.*Post-1 Q	-0.809***	-0.002*	0.000	0.014***
	(0.009)	(0.001)	(0.002)	(0.001)
Medicaid Dis.*Post-2 Q	-0.768***	-0.001	0.000	-0.031***
	(0.010)	(0.001)	(0.002)	(0.005)
Medicaid Dis.*Post-3 Q	-0.740***	-0.002	-0.002	-0.022***
	(0.010)	(0.001)	(0.002)	(0.004)
Medicaid Dis.*Post-4 Q	-0.714***	-0.004**	-0.003	-0.020***
	(0.011)	(0.002)	(0.002)	(0.004)
Mean	0.919	0.997	0.994	0.992
N		203,007		

Notes: Table presents linear regression models, where the outcome variables include Medicaid and drug coverage related indicators. The terms of interest are Medicaid Disen., interacted with pre and post terms. Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', who subsequently were in Medicare-only. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those over 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. In addition, the sample is restricted to those who were not in Medicare Advantage at any point in the sample period. Finally, the top panel excludes the time period between the court ruling and disenrollment implementation. Standard-errors are clustered at the individual level. Data is taken from CMS administrative files.

Table A.2: Over 65: Effect of Disenrollment on Spending

	(1)	(2)	(3)
		Spending	
	Total	*Outpatient*	*Inpatient*
Medicaid Disen.*Post	432**	-11	443***
	(169)	(69)	(129)
Mean Spend (Levels)	4,168	1,661	2,506
Perc Effect	10.3%	0.7%	17.7%
	(4.1%)	(4.1%)	(5.1%)
N		168,454	
Medicaid Disen.*Pre-5 Q	-391	-146**	-245
	(241)	(70)	(204)
Medicaid Disen.*Pre-4 Q	-10	-28	18
	(233)	(62)	(202)
Medicaid Disen.*Pre-3 Q		Baseline	
Medicaid Disen.*Pre-2 Q	322	62	260
	(263)	(64)	(229)
Medicaid Disen.*Pre-1 Q	1,783***	369***	1,414***
	(318)	(88)	(274)
Medicaid Disen.*Q of Disen.	1,334***	195**	1,139***
	(295)	(85)	(246)
Medicaid Disen.*Post-1 Q	406	-165*	571**
	(297)	(85)	(255)
Medicaid Disen.*Post-2 Q	320	-20	340
	(299)	(90)	(250)
Medicaid Disen.*Post-3 Q	364	-41	405*
	(302)	(95)	(245)
Medicaid Disen.*Post-4 Q	481	19	462*
	(300)	(98)	(249)
Mean (Lvl)	4,182	1,673	2,509
N		203,007	

Notes: Table presents linear regression models, where the outcome variables include person-quarter aggregated inpatient and outpatient spending measures, reflecting the overall amounts paid to providers. The terms of interest are Medicaid Disen., interacted with pre and post terms. Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', who subsequently were in Medicare-only. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those over 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. In addition, the sample is restricted to those who were not in Medicare Advantage at any point in the sample period. Finally, the top panel excludes the time period between the court ruling and disenrollment implementation. Standard-errors are clustered at the individual level. Data is taken from CMS administrative files.

Appendix B: Robustness Checks and Additional Results

Table B.1: Robustness Check: Alternative Sample Including MA

	(1) Dual Status	(2) MA Status
Medicaid Disen.*Post	-0.749***	-0.004
	(0.002)	(0.003)
Mean	0.876	0.217
N		1,158,628
Mcd. Disen.*Pre-5 Q	-0.007***	-0.003***
	(0.000)	(0.001)
Mcd. Disen.*Pre-4 Q	0.008***	-0.002**
	(0.001)	(0.001)
Mcd. Disen.*Pre-3 Q		Baseline
Mcd. Disen.*Pre-2 Q	0.006***	-0.011***
	(0.001)	(0.001)
Mcd. Disen.*Pre-1 Q	0.008***	-0.017***
	(0.001)	(0.002)
Mcd. Disen.*Q of Dis.	-0.971***	0.019***
	(0.001)	(0.002)
Mcd. Disen.*Post-1 Q	-0.791***	-0.002
	(0.003)	(0.003)
Mcd. Disen.Post-2 Q	-0.732***	-0.013***
	(0.003)	(0.003)
Mcd. Disen.*Post-3 Q	-0.697***	-0.018***
	(0.003)	(0.003)
Mcd. Disen.Post-4 Q	-0.666***	-0.019***
Mean	0.875	0.213
N		1,392,582

Notes: Table presents linear regression models, where the outcome variables include Medicaid and Medicare Advantage coverage related indicators. Notably, the sample here is inclusive of those enrolled in Medicare Advantage. The terms of interest are Medicaid Disen., interacted with pre and post terms. Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', who subsequently were in Medicare-only. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. Standard-errors are clustered at the individual level. Data is taken from CMS administrative files.

Table B.2: Robustness Check: Effect on Raw Medicare Cost-Sharing

	(1)	(2)	(3)
		Cost Sharing	
	Total	*Outp*	*Inp*
Pre-Disenrollment Stats:			
Spending	2,006	1,117	889
Raw Cost-Sharing	324	225	99
Effective Perc	**16.1**	**20.1**	**11.2**
Effect of Disenrollment on Raw Cost-Sharing			
Medicaid Disen.*Post	1.482	-9.803**	11.285**
	(7.319)	(4.598)	(4.440)
Mean	429.39	304.66	124.73
N		679,565	

Notes: The top panel of the table presents summary statistics on typical medical spending and cost-sharing for the sample population. The cost-sharing statistics reflect the share uncovered by Medicare, and does not necessarily reflect the amount for which patients are ultimately responsible, given that this uncovered might end up covered by secondary insurance such as Medicaid. Meanwhile, the bottom panel reflects the impact of Medicaid disenrollment on this uncovered spending amount, associated with Medicare coverage. Medicaid Disen. here is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', who subsequently were in Medicare-only. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. Standard-errors are clustered at the individual level. Data is taken from CMS administrative files.

Table B.3: Robustness Check: Expanded Inpatient Results

	(1)	(2)	(3)	(4)
	Inpatient Utilization		**Log Spending**	
	LOS	*Procedures*	*Proc*	*Non-Proc*
Medicaid Disen.*Post	0.153***	0.030***	0.049***	0.031**
	(0.029)	(0.006)	(0.013)	(0.013)
Mean (Lvl)	0.901	0.166	810	497
N		679,565		
Medicaid Disen.*Pre-5 Q	-0.063	-0.005	-0.026	-0.019
	(0.045)	(0.010)	(0.024)	(0.023)
Medicaid Disen.*Pre-4 Q	0.081	0.008	0.021	0.000
	(0.049)	(0.010)	(0.025)	(0.023)
Medicaid Disen.*Pre-3 Q		Baseline		
Medicaid Disen.*Pre-2 Q	0.141***	0.021**	0.061**	0.016
	(0.049)	(0.010)	(0.026)	(0.024)
Medicaid Disen.*Pre-1 Q	0.238***	0.068***	0.106***	0.013
	(0.054)	(0.012)	(0.026)	(0.024)
Medicaid Disen.*Q of Disen.	0.140***	0.040***	0.034	-0.005
	(0.051)	(0.011)	(0.025)	(0.024)
Medicaid Disen.*Post-1 Q	0.165***	0.021*	0.035	0.049*
	(0.053)	(0.011)	(0.027)	(0.025)
Medicaid Disen.Post-2 Q	0.162***	0.031***	0.035	-0.001
	(0.052)	(0.011)	(0.027)	(0.025)
Medicaid Disen.*Post-3 Q	0.128**	0.040***	0.036	-0.020
	(0.054)	(0.012)	(0.027)	(0.025)
Medicaid Disen.*Post-4 Q	0.132**	0.024**	0.050*	0.020
	(0.052)	(0.011)	(0.026)	(0.025)
Mean (Lvl)	0.900	0.166	814	497
N		816,975		

Notes: Table presents linear regression models, where the outcome variables include person-quarter aggregated inpatient utilization and spending measures. The terms of interest are Medicaid Disen., interacted with pre and post terms, which reflect the impact of Medicaid disnerollment on the treated group. Specifically, Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', who subsequently were in Medicare-only. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. In addition, the sample is restricted to those who were not in Medicare Advantage at any point in the sample period. Finally, the top panel excludes the time period between the court ruling and disenrollment implementation. Standard-errors are clustered at the individual level. Data is taken from CMS administrative files.

Table B.4: Robustness Check: Time Trends in Inpatient Visits

	(1)	(2)	(3)	(4)	(5)
	All	*Elect.*	*Non-Elect.*	*Emerg.*	*Non-Emerg.*
Mcd. Disen.*Pre-5 Q	-0.011**	-0.003	-0.008*	-0.008**	-0.004
	(0.005)	(0.002)	(0.004)	(0.004)	(0.003)
Mcd. Disen.*Pre-4 Q	0.002	-0.001	0.003	0.004	-0.002
	(0.005)	(0.003)	(0.004)	(0.004)	(0.003)
Mcd. Disen.*Pre-3 Q			Baseline		
Mcd. Disen.*Pre-2 Q	0.013**	0.002	0.012**	0.010***	0.003
	(0.005)	(0.003)	(0.005)	(0.004)	(0.003)
Mcd. Disen.*Pre-1 Q	0.022***	0.003	0.019***	0.018***	0.004
	(0.006)	(0.003)	(0.005)	(0.004)	(0.003)
Mcd. Disen.*Q of Dis.	0.013**	0.001	0.013***	0.014***	-0.000
	(0.006)	(0.003)	(0.005)	(0.004)	(0.003)
Mcd. Disen.*Post-1 Q	0.013**	-0.001	0.013***	0.016***	-0.003
	(0.006)	(0.003)	(0.005)	(0.004)	(0.003)
Mcd. Disen.Post-2 Q	0.012**	-0.001	0.013***	0.014***	-0.001
	(0.006)	(0.003)	(0.005)	(0.004)	(0.003)
Mcd. Disen.*Post-3 Q	0.009	0.002	0.007	0.008*	0.002
	(0.006)	(0.003)	(0.005)	(0.004)	(0.004)
Mcd. Disen.*Post-4 Q	0.010*	0.000	0.010**	0.005	0.005
	(0.006)	(0.003)	(0.005)	(0.004)	(0.003)
Mean	0.129	0.031	0.098	0.077	0.052
N			816,975		

Notes: Table presents linear regression models, where the outcome variables include person-quarter aggregated inpatient utilization measures, in terms of visit counts. The terms of interest are Medicaid Disen., interacted with pre and post terms, which reflect the impact of Medicaid disnerollment on the treated group. Specifically, Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', who subsequently were in Medicare-only. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. In addition, the sample is restricted to those who were not in Medicare Advantage at any point in the sample period. Standard-errors are clustered at the individual level. Data is taken from CMS administrative files.

Table B.5: Robustness Check: Time Trends in Inpatient Spending

	(1)	(2)	(3)
	Tot Inp Spending		
	All	*Elective*	*Emergency*
Mcd. Disen.*Pre-5 Q	-75	-37	-39
	(73)	(43)	(43)
Mcd. Disen.*Pre-4 Q	-75	-46	-55
	(61.519)	(38)	(37)
Mcd. Disen.*Pre-3 Q		Baseline	
Mcd. Disen.*Pre-2 Q	56	24	16
	(78)	(43)	(51)
Mcd. Disen.*Pre-1 Q	167**	-29	125***
	(73)	(40)	(46)
Mcd. Disen.*Q of Dis.	253***	57	172***
	(76)	(45)	(46)
Mcd. Disen.*Post-1 Q	145**	-18	108**
	(73)	(41)	(47)
Mcd. Disen.Post-2 Q	205**	13	133***
	(80)	(42)	(49)
Mcd. Disen.*Post-3 Q	139*	-17	105**
	(82)	(45)	(48)
Mcd. Disen.*Post-4 Q	90	-10	35
	(78)	(41)	(46)
Mean (Lvl)	1,311	639	423
N		816,975	

Notes: Table presents linear regression models, where the outcome variables include person-quarter aggregated inpatient spending measures. The terms of interest are Medicaid Disen., interacted with pre and post terms, which reflect the impact of Medicaid disnerollment on the treated group. Specifically, Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', who subsequently were in Medicare-only. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. In addition, the sample is restricted to those who were not in Medicare Advantage at any point in the sample period. Standard-errors are clustered at the individual level. Data is taken from CMS administrative files.

Table B.6: Robustness Check: Time Trends in Outpatient Spending

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Outp Spending					
	Surg Amt	*Diag Amt*	*Imaging Amt*	*Prevent. Care*	*Office Visits*	*Hosp Out*
Medicaid Disen.*Pre-5 Q	-0.045	-0.035	-0.047	-0.120***	-0.002	-0.036
	(0.030)	(0.033)	(0.033)	(0.041)	(0.022)	(0.031)
Medicaid Disen.*Pre-4 Q	0.046	0.057*	-0.020	-0.122***	0.022	-0.000
	(0.031)	(0.035)	(0.034)	(0.040)	(0.022)	(0.032)
Medicaid Disen.*Pre-3 Q			Baseline			
Medicaid Disen.*Pre-2 Q	-0.032	0.027	0.003	-0.046	-0.040*	0.010
	(0.032)	(0.035)	(0.034)	(0.041)	(0.022)	(0.033)
Medicaid Disen.*Pre-1 Q	-0.083**	-0.096***	-0.075**	-0.049	-0.129***	-0.115***
	(0.032)	(0.036)	(0.035)	(0.037)	(0.024)	(0.034)
Medicaid Disen.*Q of Disen.	-0.222***	-0.219***	-0.182***	-0.076**	-0.249***	-0.251***
	(0.033)	(0.036)	(0.034)	(0.037)	(0.025)	(0.034)
Medicaid Disen.*Post-1 Q	-0.155***	-0.162***	-0.098***	-0.114***	-0.162***	-0.185***
	(0.033)	(0.037)	(0.035)	(0.042)	(0.026)	(0.035)
Medicaid Disen.Post-2 Q	-0.157***	-0.164***	-0.150***	-0.084*	-0.237***	-0.169***
	(0.034)	(0.037)	(0.036)	(0.043)	(0.027)	(0.035)
Medicaid Disen.*Post-3 Q	-0.198***	-0.152***	-0.162***	-0.079*	-0.241***	-0.191***
	(0.034)	(0.038)	(0.036)	(0.041)	(0.027)	(0.036)
Medicaid Disen.*Post-4 Q	-0.091***	-0.079**	-0.086**	-0.015	-0.213***	-0.114***
	(0.034)	(0.038)	(0.036)	(0.038)	(0.028)	(0.036)
N			816,975			

Notes: Table presents linear regression models, where the outcome variables include person-quarter aggregated outpatient spending measures. The terms of interest are Medicaid Disen., interacted with pre and post terms, which reflect the impact of Medicaid disnerollment on the treated group. Specifically, Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', who subsequently were in Medicare-only. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. In addition, the sample is restricted to those who were not in Medicare Advantage at any point in the sample period. Standard-errors are clustered at the individual level. Data is taken from CMS administrative files.

Table B.7: Robustnes Check: Effect on Medicaid and Cost-Sharing By Charlson Group

	Charlson Score		
	0	*1-3*	*4+*
Effect on Medicaid Status	-0.777***	-0.760***	-0.752***
	(0.005)	(0.005)	(0.009)
Effect on Cost-Sharing Amt			
Tot	-19***	-16	-3
	(6)	(11)	(40)
Outp	-18***	-14**	-47*
	(3)	(6)	(27)
Inp	-1	-2	44*
	(4)	(7)	(23)
N	282,277	304,490	92,798

Notes: Table presents linear regression models, where the outcome variables include person-quarter measures of Medicaid coverage and cost-sharing exposure. The coefficients presented reflect the impact of Medicaid disenrollment on the treated group, broken out for different subgroups, based on their Charlson Scores for the pre-period preceding disenrollments. The top panel shows the corresponding effect on overall Medicaid coverage status, for the post-period. Meanwhile, the bottom panel shows the corresponding effect on traditional Medicare cost-sharing, defined as the spending amount that is uncovered by Medicare. This does not necessarily reflect the net cost-sharing responsibility facing the patient, given the potential presence of secondary insurance. The coefficients presented reflect the impact of Medicaid disnerollment on the treated subgroup, and correspond to the coefficient for the Medicaid Disenroll and Post interaction term. Here, Medicaid Disen. is defined as those dual-eligibles involuntarily disenrolled from Medicaid as a result of 'Cluster Daniels', who subsequently were in Medicare-only. The unit of observation is at the individual-quarter level, for the 2008-2011 period. Year-quarter, county, age, and gender fixed effects are included as part of the analysis. The sample is restricted to those under 65 and disabled, who were dually-enrolled in Medicaid and Medicare at the start of 2008; the sample is also restricted to Tennessee only. In addition, the sample is restricted to those who were not in Medicare Advantage at any point in the sample period. Standard-errors are clustered at the individual level. Data is taken from CMS administrative files.