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Eric Helland
Claremont-McKenna College and RAND

“Does Treatment Respond to Reimbursement? Evidence from Trauma Care

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Does Treatment Respond to Reimbursement Rates? Evidence from Trauma Care

Paul Heaton
RAND

Eric Helland
Claremont-McKenna College and RAND

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ABSTRACT

Some models of provider behavior predict that physicians, like other experts, may respond dysfunctionally to financial incentives by recommending unnecessary treatment. We empirically test this relationship using data from inpatient hospitalizations surrounding a 2003 Colorado auto insurance reform. The reform shifted a large fraction of auto injury patients from coverage through auto insurers to less generous sources of reimbursement, such as health insurance and self-pay. Despite negligible changes in auto injury characteristics during this period, treatment supply increased following the reform. Procedure use rose by 5-10% and billed charges rose by 5%, and these increases are specific to auto but not other types of traumatic injury. These changes appear to reflect an increase in real resources devoted to treatment, but do not improve mortality outcomes. Our findings are consistent with models of physician-induced demand.

JEL Classification: I11, G22, K13
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I. Introduction

Provision of expert services is complicated by the fact that in some cases the financial incentives of suppliers and consumers may be misaligned, leading experts to exploit their informational advantage in ways that harm consumers. Although in extreme cases such information asymmetries can lead to a total breakdown of markets (Akerlof 1970), researchers have identified a growing number of functioning markets in which experts appear to behave in ways that do not benefit their customers. Chevalier and Ellison (1997), for example, demonstrate that mutual fund managers attempt to increase investment inflow at the expense of investors, while Hubbard (2002) shows that auto inspectors may respond dysfunctionally to incentives to generate repeat business. Helland and Tabarrok (2003) find that attorneys whose compensation is determined independently of case outcome induce clients to pursue poor cases. More recently, Iizuka (2008) shows that physicians are more likely to prescribe drugs for which they earn high markups, while Levitt and Syverson (2008) document distortions that arise from information asymmetries in the real estate market.

In some cases such agency problems can be minimized through contractual solutions. Plaintiff's lawyers are typically paid by contingent fees in personal injury cases, which induces them to more truthfully reveal the quality of the case to their client. Realtor contracts typically have a relatively short duration to induce effort in selling the house. In medicine, by contrast, contracts to align patient and doctor incentives are far less common.

In the theoretical health literature, the expert-patient agency problem has been labeled physician-induced demand. Studies typically hypothesize a situation in which a physician exploits her agency relationship with the patient to maintain income in the face of falling reimbursement. In the presence of such demand effects, polices for controlling costs by expanding physician entry or changing the structure of fees may actually have detrimental effects on health system efficiency and patient welfare. Given the myriad of proposals for reforming the health care payment system, properly characterizing the effects of changes in the reimbursement structure on physician behavior remains vital for policy evaluation.

However, empirical research attempting to isolate the effects of changes in financial incentives on physician behavior confronts formidable challenges. Two persistent problems have been identifying sources of variation in reimbursement that affect providers but not patients and accounting for potentially observed factors such as patient health that may also be related to both reimbursement rates and treatment. For example, early studies of physician-induced demand (Fuchs 1978, Rossiter and Wilensky 1983, Cromwell and Mitchell 1986) examined differences in the number of physicians across locations as a proxy for reimbursement. However, subsequent research has demonstrated that these studies likely suffer from omitted variable problems (Auster and Oaxaca. 1981, Feldman and Sloan 1988, Dranove and Wehner 1994). More recently, Gruber and Owings (1996) use exogenous shocks to fertility to identify effects of availability of patients, a proxy for physician income, on physician use of Cesarean sections. Although providing the most compelling evidence in favor of physician-induced demand to date, even in this setting there remain potentially uncontrolled factors related to both fertility and patient demand for C-sections, such as risk-aversion and patient income.

An alternative test examines how procedure intensity varies as fees for particular types of treatment are adjusted. Examples of studies employing this methodology include Rice (1983), Christensen (1992), Escarce (1993), Nguyen (1996), Yip (1998), Zuckerman (1998), Mitchell et. al. (2000), and Hadley and Reschovsky (2006).[1] An advantage of focusing on fee changes is that such adjustments are more plausibly exogenous from the perspective of the physician. However,

[1] McGuire (2000) provides a more thorough review of this literature.

an important limitation of this approach is that because of co-payments, decreases in fees may directly lower the price of medical services for consumers, making it difficult to cleanly separate changes in actual demand from induced demand.[2]

In this paper we use an exogenous change in a payment system for auto injuries to examine how treatment responds to a fall in reimbursement rates. Prior to 2003, medical payments for auto injuries in Colorado were handled by auto insurers, but a controversial law change in that year shifted the handling of these injuries to the traditional health insurance system. Provider reimbursement for these injuries dropped roughly 40% as a consequence. Because the reimbursement change was largely unforeseen and affected only certain types of medical conditions, we are able to credibly address the confounding problems that have arisen in past studies of the effects of reimbursement. Essentially, individuals were randomly assigned to a reimbursement rate based upon whether their accident occurred before or after the law change. Additionally because we focus on a class of injuries for which patient demand is likely relatively price inelastic (traumatic injuries treated in hospitals), the effects we capture most plausibly reflect supplier behavior.

Our analysis proceeds in three steps. We first demonstrate that the insurance reform had a profound effect on the payment composition for auto injuries. Figure 1 plots the proportion of claims involving auto injuries in Colorado with a liability insurer coded as the primary payer. Prior to 2003 around half of claims involved liability insurers, but beginning in July of 2003 this proportion began to fall dramatically. Over the twelve-month sunset period, the liability insurer share of claim payments fell by about 50%, reaching a new steady-state at approximately 25% of

[2] We focus on U.S. studies given that our empirical application also uses policy variation arising from features of the American health care system, but there is also a notable literature investigating physician induced demand in non-U.S. settings. International studies provide mixed evidence for induced demand and include Hurley et al. (1990), Grytten et al. (1995), Chalkley and Tilley (2005), Xirasagar and Lin (2006), Richardson and Peacock (2006), and Grytten et al. (2008) .

all claims. Thus, as a result of the insurance reform roughly a quarter of all treatment episodes were shifted from a generous reimbursement source to relatively less generous sources.

Figure 2 captures much of the intuition behind our remaining analysis. We next demonstrate that there were no substantial changes in injury characteristics or demographic characteristics of auto accident victims surrounding the reform. The top panels of Figure 2, for example, plot the average age of auto accident patients and proportion of patients whose primary diagnosis was a head injury. These characteristics are both stable over time and provide no evidence of substantial changes during or after the adjustment period. Given that the medical and other characteristics of patients appear similar both before and after the reform, any differences in treatment we observe are likely due to the changes in physician reimbursement as opposed to other factors.

The next panels of Figure 2 provide evidence of increases in treatment for auto patients, as measured by both the average number of medical procedures performed on each patient and the average charges for care. Notably, for both measures, it additional to higher averages in the post-reform period, the time-series evidence suggests that most of the increases occurred precisely during the year-long transition period from no-fault to tort. Taken alone, this evidence suggests that suppliers increased care in response to the lower reimbursement rates, but could also reflect other omitted factors, such as an improvement in medical technology that was introduced in 2003. However, examining injuries similar to those sustained by auto victims but which did not involve motor vehicles (bottom panels, Figure 2), we observe no evidence of changes in care surrounding the reform period.

Overall, our evidence indicates that the reform generated increases in procedures of 5-10% and charges of 5%. These changes appear to involve increases in real resource utilization as opposed to simple re-coding of treatment for administrative purposes. We also find that the increased treatment does not improve patient mortality. Our findings are consistent with models of physician-induced demand in which physicians increase treatment to compensate for income losses.

In the next section of the paper we describe theoretical models of physician response to financial incentives. Depending on the model of physician optimization, treatment may increase, decrease, or remain unchanged following a drop in reimbursement rates. We then describe the Colorado legislative reform that we use to identify the effect of reimbursement rates. Section IV of the paper presents our data sources and empirical analysis, and section V concludes.

II. How Should Care Respond to Changes in Reimbursements?

In this section we discuss theoretical models of the provision of medical care that provide predictions regarding the effects of a change in insurer reimbursements. An important contribution of our paper is providing empirical evidence in evaluating these models. Models of induced demand assume that physicians are agents with uninformed patents, allowing physicians to create demand for their services by recommending unnecessary treatment. Typically these models also assume that physicians' utility is increasing in income but that doctors receive disutility from deviating from appropriate levels of care. Such disutility can arise because of professional ethics (McGuire and Pauly 1991) or future demand reductions as patients learn they have received "excessive" treatment (Dranove 1988).

A simplified version of the McGuire and Pauly (1991) inducement model, along the lines of Gruber and Owings (1996), can capture the impact of our quasi-experiment. Assume that providers have a utility function

$$U = U(Y, I) \tag{1}$$

where Y is income minus the value of leisure and I is the level of inducement. For simplicity we have assumed that the provider cannot vary the level of inducement by payer, although this is not essential for our argument.

Provider income from treating auto accidents is given by

$$Y = Q \cdot T(I) \cdot r \tag{2}$$

where $T(I)$ is the amount of treatment received by each patient, which depends on the level of inducement, and r is the exogenous average rate of reimbursement. We assume that Q, the number of auto accident victims, is a constant (or at least determined by factors outside the provider's control). Treatment is increasing in the amount of inducement but at a decreasing rate ($T_I > 0$ and $T_{II} < 0$).

Following McGuire and Pauly we adopt standard regularity conditions for the utility function ($U_Y > 0$, $U_I < 0$, $U_{YY} < 0$ and $U_{II} < 0$), and also assume that the cross partial terms are zero, i.e. $U_{YI} = 0$, and that the utility function is separable. The first order condition is

$$U_Y \cdot T_I \cdot rQ + U_I = 0 \tag{3}$$

As is typical in inducement models, the provider trades off the disutility of further inducement against the increased utility from increasing income. We are primarily interested in the impact of a change in the reimbursement rate, *r*. Taking the total derivative of (3) yields:

$$\frac{\partial I}{\partial r} = \frac{T_I Q(U_{YY} Y + U_Y)}{T_I{}^2 U_{YY} r^2 Q^2 + T_{II} U_Y r Q + U_{II}} \tag{4}$$

The denominator of this expression is negative but the sign of the numerator depends on the curvature of the utility function of the provider.[3] Intuitively, if the marginal utility of income is large or diminishes slowly, then the provider will find it beneficial to increase inducement when faced with a drop in reimbursement rates. An appealing feature of this model is that induced demand can be directly tested by examining how treatment responds to reimbursement rates, since an increase in treatment also implies greater inducement.

Although the induced-demand model provides one prediction regarding the effects of a shift to a less generous reimbursement system, other reasonable models of physician behavior yield differing predictions. For example, in a model with altruistic physicians, in which care is determined primarily by medical necessity, we would not expect a change in the payment system to affect treatment.[4] Alternatively, physicians may provide optimal care for patients but may adjust care in response to budget constraints arising from differential reimbursement. For

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[3] Rizzo and Blumenthal (1994) provide estimates of wage and income elasticities specific to physicians, which can be used to calculate to coefficient of relative risk aversion using the approach of Chetty (2006). Their estimates imply a risk-aversion coefficient sufficiently small to ensure that $\frac{\partial I}{\partial r} < 0$.

[4] This can be seen formally in our model by assuming that U_Y is zero, in which case equation (4) predicts no effect from a change in the share of patients covered by the more generous payment source.

example, suppose that the physician optimization problem is to choose a vector of treatments *t* to maximize a patient health function *H(t)* subject to the constraint that the total cost of treatment be at or below a specified reimbursement level, *R* (i.e. $\sum_i p_i t_i \leq R$). Clearly a drop in *R* in this setting would lead to a decline in treatment. Haas and Goldman (1994) and Doyle (2005) provide empirical evidence in favor of this viewpoint specifically for auto accidents, demonstrating that individuals without health insurance receive 20% less care than those with insurance. Under these models, a shift to less generous reimbursement sources such as self-pay or Medicare/Medicaid following the reform would reduce treatment.

Differentiating across these models of physician optimization remains important for policy evaluation because the models have different implications regarding the desirability of certain policies. In the physician-induced demand model, a reduction in fees may generate cost savings but also can increase inefficiency in the health care system as physicians substitute towards unnecessary treatment. In a model with altruistic physicians and a soft budget constraint, fee reductions will save money without negatively impacting patient care. In the resource-constrained model, fee reductions will actually harm patient health. In the next section we describe a policy experiment that offers an opportunity to empirically evaluate these competing models.

III. Colorado Auto Reform

In 1974 Colorado passed a law requiring all motorists to purchase $100,000 in coverage for medical expenses related to motor vehicle accidents from their auto insurer, a system called

Personal Injury Protection (PIP).[5] Under the law auto-related injuries were handled differently from other types of health care, with payment for these injuries made by auto insurers, who were designated the primary payers and required to pay benefits regardless of the fault of the injured party.[6] Although providing excellent benefits for injury victims, these generous policies led to rapidly escalating auto premiums in the 1990's. Faced with political fallout over high premiums, legislators passed a bill in 2001 mandating that the PIP/no-fault system be automatically repealed unless specifically reauthorized.

No-fault was temporarily extended in 2002, but then-Governor Bill Owens stated he would not extend it further without reform to reduce premiums. During the 2003 legislative session, no-fault proponents introduced several bills, including HB 1225, which would have preserved the no-fault system while implementing a number of reforms. Because there was considerable uncertainty concerning whether PIP/no-fault would be allowed to expire, insurers issued standard PIP policies during this period. However, none of the bills garnered sufficient votes for passage and PIP/no-fault was allowed to sunset on July 1st. At the time of the sunset, insurers were required to honor these PIP policies currently in force, leading to a gradual elimination PIP policies over the next year as policies came up for renewal.

Following the reform auto insurers in Colorado offered optional MedPay policies that covered payments for medical treatment associated with auto accidents regardless of fault. Roughly 1/3 of Colorado drivers elected to purchase policies with MedPay coverage (BBC, 2008), but among those purchasing MedPay, the median coverage amount was $5000, well below the mandatory minimum coverage of $120,000 under no-fault (IRC, 2008). Overall, the

[5] Similar no-fault auto insurance systems exist in several states, including New York, Florida, and Michigan.
[6] No-fault protections were also automatically extended to out-of-state drivers involved in accidents within the state through statute 10-4-711(4). Unfortunately, this precludes the use of out-of-state drivers as a control group.

role of auto insurers as primary payers diminished substantially as a result of the new law, with auto injuries being handled much more similarly to other types of injuries.

The elimination of PIP/no-fault substantially reduced reimbursement rates for physicians treating injuries related to auto accidents. Although conceptually it is not apparent a shift away from coverage by auto insurers need affect reimbursement rates, as a practical matter, PIP policies provided much higher levels of reimbursement than other payers for several reasons. Prior to 1991 auto insurers in Colorado were legally restricted from offering managed care policies, so these insurers has substantially less experience with managed care than health insurers.[7] Auto insurers typically also had less resources than other insurers to devote to monitoring care, since this was not their core business, and were required to process PIP claims quickly. Thus, auto insurers were more likely to simply pay billed medical charges as opposed to negotiate reimbursements. Finally, the uninsurance rate for auto insurance was substantially below that of health insurance, so a sizable fraction of those previously covered by auto polices had no insurance coverage following the reform. A recent study of the Colorado trauma system (BBC 2008) estimates that reimbursement rates fell from 60% to 36% for inpatient auto injury cases as a result of the reform. Since 2005, trauma care providers in Colorado have lobbied aggressively for a return to mandatory PIP.

III. Data Sources and Empirical Analysis

Our primary data source is the Colorado State Inpatient Databases (SID) covering the years 2000-2006 compiled by the Agency for Healthcare Research and Quality (AHRQ). These files

[7] In interviews with the authors, auto insurance industry executives claimed that managed care policies in Colorado were viewed positively by their companies in the early 1990's, but that by the late 1990's many insurers had abandoned such plans because it became overly cumbersome to manage increasingly complex claims.

report on the universe of inpatient hospital admissions in Colorado, with roughly 450,000 records each year, 5000 of which involve automobile injury victims. We use E code designations to identify injuries associated with automobile use. As our control sample, we examine patients with primary diagnoses that cover 14 diagnostic categories, all of which involve traumatic injury.[8] Over 90% of auto patients have one of these 14 codes as their primary diagnosis.

For each inpatient record we observe information about patient demographics, diagnoses, and treatment. Demographic measures include patient age, sex, race, and ZIP code. Diagnoses are coded using multiple ICD-9 diagnosis codes, while treatment information includes procedures performed and length of stay. We observe total charges for each treatment episode but not the actual amounts reimbursed. We also can observe patient disposition, including in-hospital mortality, but lack other measures of health outcomes. Table 1 provides selected summary statistics for the data used in our analysis.

Because our data cover inpatient hospitalizations only, we are unable to analyze treatment conducted exclusively in the emergency room or in outpatient settings, such as clinics or specialist offices. In a survey of closed auto claims conducted in 2002, approximately 6% of claimants reported overnight hospital stays, suggesting that outpatient services remain the predominant means of receiving care. However, individuals reporting inpatient care accounted for 39% of total medical claims charges, indicating that impatient care represents an important component of overall auto injury costs. Also, it seems reasonable to expect that there is reduced flexibility in treatment for more seriously injured patients in a facility setting, meaning that this analysis provides a particularly stringent test of the theory.

[8] The Clinical Classification Software (CCS) codes are 225-236, 239, and 244.

The Colorado SID codes whether the expected source of payment is a liability insurer, private health insurer, governmental health insurer, or self-pay. We first demonstrate that as a result of the insurance reform, a substantial fraction of payment for auto injuries shifted away from auto insurers. Table 2 reports regression estimates of the effect of the law change on the composition of auto injury payers. The left column reports estimates from regressions limited to auto injuries; each row entry represents the coefficient estimate from a unique regression. The dependent variables in these regressions are indicators for payer types, and the primary explanatory variable is an indicator for the law change.[9] These regressions also include month indicators and a yearly time trend as additional controls.

Based upon pre-post differences, as a result of the reform 22% of auto injuries shifted from liability insurers to other types of insurers. Relative to the pre-reform rate of roughly 50% coverage by auto insurers, this represents a decline of about half. Sixteen percent of those no longer covered under auto were covered by health insurers, while 6% had no coverage. All of the estimated effects are highly statistically significant.

The right column of Table 2 reports analogous differences-in-differences (DD) estimates of the effect of the law change, using non-auto-related traumatic injuries as an additional control group. The DD framework permits the inclusion of a full set of month/year interactions as controls and thus accounts for any general trends in insurance coverage that may have occurred during this time period. The DD estimates are similar to the pre-post estimates, although a slightly higher share of patients (10%) are estimated as having shifted to self-pay using this methodology. Overall, the data indicate a substantial shift in the composition of payers resulting from the law change.

[9] We code this indicator as 0 prior to July 1, 2003; 1 after June 30, 2004; and intermediate values between 0 and 1 for the months between July 2003 and June 2004.

In order to obtain valid estimates of the effect of reimbursement rates on supply of treatment, we also require that demand for treatment be stable across individuals subject to different levels of reimbursement. A standard criticism of studies using fee changes to examine how reimbursement rates affect treatment is that a drop in price may increase quantity consumed due to a supplier inducement of demand or true increases in demand. It seems likely that consumers are fairly price insensitive with regard to more serious traumatic injuries, as in our setting. However, an additional concern is that individuals under different levels of reimbursement may possess unobservable characteristics that bias simple regressions of treatment on reimbursement rates. For example, if individuals with greater health endowments purchase lower levels of insurance, we may observe less treatment provided when lower levels of reimbursement are available. However, such differences would not represent effects of the reimbursement system.

A key advantage of focusing on the Colorado insurance reform is that there is no reason to expect that payment rates in our setting are correlated with latent demand for treatment since the source of reimbursement is determined primarily by the timing of the injury as opposed to other factors, such as victim income. Using observable characteristics, we can test this notion formally by examining average characteristics before and after the reform and testing for changes across the reform period. Table 3 reports such estimates, focusing on the demographic and injury characteristics of patients.

The top rows of Table 3 report comparisons of characteristics directly recorded in the inpatient hospital records, such as age, sex, and race. With the exception of admission through the ER, which rose slightly following the law change, there is little evidence of changes in patent demographics. The middle rows use the average characteristics in the reported ZIP code of each patient to consider a wider variety of characteristics, including measures of socioeconomic status. There is no statistically significant difference in income or educational attainment of patients across the reform period. Examining injury characteristics, we see that the null hypothesis of equal pre-post injury rates cannot be rejected for any injuries except lower limb fracture, which fell slightly on the post-reform period.

If the lower reimbursement rates led hospitals to be less willing to accept patients, patients may have spent longer times in transit prior to admission. In this case, patients presenting after the reform may been less healthy in ways that are not captured by the diagnosis codes due to longer waits. To test this possibility, the bottom rows of Table 3 attempt to proxy for travel times by comparing the residential ZIP codes of patient to the ZIP codes of the hospitals in which they were treated.[10] If patients were more likely to be turned away from local hospitals following the reform, we might expect to observe a larger proportion of patients residing out of ZIP and longer average distances between patient and hospital ZIPs. However, Table 3 demonstrates no change in these measures, suggesting that travel times were similar before and after the reform.[11] Overall, it does not appear that the characteristics of patients changed around the reform period in ways that would affect their demand for treatment or the medically appropriate methods for treatment.

An additional concern may be that drivers could have changed their driving behavior in response to the change in insurance law, leading to different types of accidents following the reform. Heaton and Helland (2008), however, demonstrate that drivers under no-fault are no more likely to cause accidents and some evidence suggesting tort drivers may be involved in less

[10] A clear disadvantage of this proxy is that residence ZIP may not always correctly capture the location where the accident occurs.

[11] BBC (2008) also reports that none of the 67 first-responder agencies it surveyed reported an increase in average response times arising from the law change.

severe accidents. In an unreported analysis using a sample of 16120 Colorado accidents from the National Highway Traffic Safety Administration's General Estimates System (GES), we also find no evidence of changes in alcohol involvement or reckless driving associated with the law change. There is some evidence that accident severity-- as measured by vehicle damage severity and occupant injury severity--fell following the reform, which could reflect increased driver caution. Changes in driver behavior, if any, seem likely to favor lower treatment, suggesting our estimates may represent a lower bound on the actual effects of the new reimbursement system.

Table 4 reports our primary results estimating the effects of a change in reimbursement rates on treatment and patient disposition. As in Table 2, the left column reports estimates for auto injuries alone, and the right column reports differences-in-differences estimates focusing on all types of traumatic injury. Each entry represents a coefficient from a unique regression, and the regressions also control for patient age, sex, race, residence ZIP, hospital, and diagnosis. To account for time-series patterns in treatment and outcomes, the pre-post comparisons include month fixed-effects and a time trend while the DD specifications include a full set of month/year interactions.

The first row of Table 4 considers log(total charges) as the dependent variable. This measure reflects the amount billed by the hospital for all services. Consistent with the evidence presented in Figure 2, charges increased by an average of 4-5% as a result of the change in reimbursement system, and this difference is statistically significant. In 2006, this change would have represented $13 million in additional charges. Examining the reported number of procedures, we find similar evidence of an increase in treatment, with an additional .08-.13 procedures performed per patient, an appreciable change relative to the mean number of procedures of 1.9. Length of stay increased as well, although these changes are only marginally significant. Interestingly, despite these longer stays, it appears that patients spend shorter amounts of time in the hospital prior to the initial onset of treatment following the reform.

To further explore the nature of the increase in treatment, we identified diagnostic and therapeutic procedures using the ARHQ Clinical Classification Software. An additional 4% of patients received therapeutic treatment after the law change, while there was little change in the use of diagnostic procedures. Overall, across several different measures of treatment there is evidence that providers increased treatment following the drop in reimbursement rates.

The bottom rows of Table 4 indicate how the reform affected the disposition of patients. Despite the increase in treatment noted above, the point estimate indicates that patient mortality actually increased under the new insurance system, although this change is not statistically different from zero. The treatment changes did not translate into improvement in this key outcome. For surviving patients, we do not observe important changes in the types of follow-up care accessed, although the available measures are somewhat limited.

One possibility is that the increase in procedures we observe following the reform represents a simple change in coding of procedures to correspond with the new billing rules of medical providers. In other words, it may be that the actual resources devoted to patient care remain the same, but care is upcoded to allow for different billing rates. Although observing an increase in charges is in and of itself interesting, from a policy perspective it would be more useful to know whether moving to a less generous payment system entails a true shift in resource utilization.

To examine this possibility, we first turn to the CPEP resource input files published by the Centers for Medicare and Medicaid Services. For each clinical procedure, the CPEP files provide an estimate of the labor, equipment, and supply inputs required for that procedure as estimated by expert panels comprising physicians and medical administrators. Inputs are also assigned monetary weights.[12] For each patient, we match each coded procedure to its associated resource input cost and then sum across all procedures to form an estimate of the total resources used in treating the patient.[13]

Table 5 reports estimates analogous to those in Table 4 focusing on changes in real resources devoted to care. Based upon the changes in procedure coding, quality-adjusted labor hours increased by approximately 8% as a result of the insurance reform. Equipment utilization, which refers to durable machinery such as diagnostic equipment, as well as supply utilization saw increases of a similar magnitude. Thus, over a range of inputs there is evidence that increased resources were devoted to care after the reform. Total cost-weighted care inputs rose by roughly 11%. This increase also persists controlling directly for the number of coded procedures (bottom row of Table 5), indicating that the reform not only increased the number of procedures coded, but also shifted care towards procedures that were more resource-intensive.

Insurers have incentives to monitor claims and ensure that patients actually receive reported treatments. As an additional test of resource use, we compare reimbursement patterns for audited versus unaudited claims in Colorado to patterns from other states. We assume that auditing reveals the true nature of treatment received, so that if providers are simply upcoding procedures without actually increasing treatment, insurers will detect this in audited claims and reduce reimbursements accordingly. If the treatment changes in Colorado represent upcoding as

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[12] These estimates were originally prepared to help in constructing the physician fee schedules for Medicare and Medicaid and have been updated periodically by committees convened by the AMA.

[13] These estimates are best understood as representing the marginal cost of conducting an additional procedure, and do not incorporate fixed costs of treating patients, such as providing facilities and general monitoring equipment. Because of this, monetized resource inputs tend to be substantially below reported charges, and there is no obvious way to compare inputs to charges in order to assess the "profitability" of procedures. Additionally, resource inputs could not be calculated for a small fraction (4.8%) of our sample due to invalid or missing procedure codes.

opposed to real treatment increases, we expect reimbursement for audited claims to be lower in Colorado than in other states.

Our claims data is drawn from a sample of closed auto insurance claims collected by the Insurance Research Council in 2007.[14] We limit the sample to claims involving hospital visits that occurred in tort states, leaving us with a total sample of 13,297 claims, 289 of which originated in Colorado and 12% of which were audited. Table 6 reports regression estimates of the relationship between reimbursement rates and audit status in Colorado. The dependent variable is the amount of medical expenses reimbursed divided by claimed medical expenses (mean=.87). The primary explanatory variable is an interaction between whether a claim was audited and whether in occurred in Colorado. In column I we control for audit status and a full set of state fixed effects, thus obtaining a difference-in-difference estimate that compares claims in Colorado to claims in other states and claims that were audited to claims that were not audited. The coefficient estimate on the interaction implies that audited claims in Colorado received an average of 3.6% greater compensation than audited claims in other states.[15] Controlling for claimant demographics and injury (column II) increases the estimated coefficient. After accounting for patient characteristics, there is no evidence that reimbursements fell in Colorado when claims from the post-reform period were audited.

There are clear limitations of this claims analysis. Our sample size is relatively small and only involves a single type of insurer. Upcoding may be prevalent in other tort states, in which case comparing Colorado to other locations may not be informative regarding the overall extent of upcoding. Additionally, it is possible that audit procedures are insufficiently thorough to detect upcoding. At a minimum, however, our analysis indicates that when insurers in Colorado

[14] IRC (2008b) provides a more detailed description of this data set.
[15] As we would expect, the coefficient on audit status is negative and statistically significant and indicates that insurers reduce reimbursements by 7% after auditing a claim.

more closely scrutinized selected claims involving hospital care, they were not inclined to reduce reimbursements. This observation is consistent with the notion that the treatment increases we have documented involve actual resource use.

Taken alone, our finding that resource use increased following the decline in reimbursement rates does not allow us to draw strong conclusions about patient welfare. For example, if medical insurers are better able to monitor doctors than auto insurers, the effects we document may represent reduced shirking by doctors, in which case patients receive more care at potentially lower cost. However, the fact that the additional resources devoted to care did not improve mortality outcomes argues that this additional care may have had limited benefit, a clear prediction of models of physician-induced demand.

As an additional test of the monitoring versus reimbursement hypothesis, we re-estimate specifications from Tables 4 and 5, focusing on the more flexible diffs-in-diffs approach. In specification 1 of Table 7, we first control directly for insurer type in our regressions, thus identifying the effect of the law change comparing individuals with the same type of insurer before and after the reform. Our above argument regarding monitoring relies on shifts across insurer types, which we specifically account for in these regressions. The next specification limits the analysis to those without insurance, a group that presumably has limited ability to monitor doctors. In both cases we see evidence of increases in treatment for auto injuries following the reform, suggesting that our results are not explained solely by changes in monitoring.

The remaining columns of Table 7 demonstrate that our primary conclusion that the insurance reform led to significant changes in treatment is borne out across different samples and estimation methodologies. The third specification of Table 7 limits that analysis to treatment episodes involving at least one procedure. Nationwide, roughly 40% of all inpatient hospitalizations have zero coded procedures (AHRQ 2006); the bulk of these cases represent patients admitted to the hospital for observation purposes and subsequently released. Because these types of patients may be different from more traditional patients, we examine whether their exclusion affects our results. Excluding observational patients yields results that are consistent with our baseline specifications in both sign and magnitude.

Specification 4 of Table 7 re-estimates our baseline models employing different functional forms. For total charges and resource inputs, we utilize a specification in levels rather than logs; for binary outcomes we employ a linear probability model, and our count outcomes (number of procedures and length of stay) are re-estimated using poisson regression. Varying functional forms does not alter our conclusions. Specification 5 limits the analysis to treatment episodes occurring during 2003 and 2004, which focuses attention on the changeover period. This specification essentially tests for a dose-response effect, examining whether there was in increase in treatment as policies were moved from one system to the other. Although the coefficients are in many cases only marginally significant--which is not surprising given the much smaller sample size--the direction and magnitude of the estimated effects accord with our primary findings.

Specification 6 includes a full set of month/year/injury class interactions for injuries occurring between 7/2003 and 6/2003, so that these injuries do not contribute to identification of the effects of the reform. This approach might be preferred if individuals switched to tort policies unevenly during the transition year. Omitting the transition period does not appreciably alter our conclusions.

Specification 7 performs placebo regressions testing our identification approach by replacing those injured in auto accidents with a sample of 12,713 patients who were injured in non-motor vehicle related transport accidents, such as accidents involving bicycles or boats. These patients were not affected by the insurance reform because they were not covered under the no-fault insurance system. If our diff-in-diffs methodology properly accounts for changes across groups over time in treatment, we should not observe statistically significant changes in treatment for this group. Indeed, for all but one of the treatment measures our estimates are statistically indistinguishable from zero, and the point estimates are typically much closer to zero than our estimates for truly affected patents. For resource inputs the coefficient estimate is positive and significant, but substantially below the estimate we obtained for auto injury patients. Even accounting for the possibility that transport-related injuries exhibited differential growth of 4% in resource requirements, we would still find a substantial additional increase in inputs used in care of auto patients.

V. Conclusions

This paper demonstrates that physicians in Colorado increased treatment following an insurance reform that shifted a large number of patients to insurers who offered lower reimbursements for trauma care. This change in treatment can not be explained by differences in medical or other characteristics of patients and is unlikely to reflect patient demand. Real resources devoted to patient care appear to have increased, but there were no improvements in patient mortality.

Our finding that shifting the burden of payments from auto insurers to other insures has a direct bearing on treatment has important implications for the regulation of auto insurance.

Currently over 20% of all automobiles in the U.S. are covered by no-fault policies similar to those which existed in Colorado prior to 2003, and a number of states have recently considered modifying their laws regarding no-fault. Although critics of no-fault systems argue that their nearly unlimited PIP benefits generate incentives for patients to obtain unnecessary treatment, our analysis indicates that even eliminating no-fault may cause providers to offer excess treatment. The fact the providers may not exercise their discretion in ways most beneficial for auto injury patients suggests a potential role for treatment schedules, a reform that has been implemented in a number of states.

More generally, the Colorado experience provides evidence supportive of induced-demand models of physician behavior. In the presence of induced-demand effects, policies that attempt to reign in medical costs by limiting reimbursements to medical providers may entail real resource costs as some treatment is diverted to unproductive ends. Additionally, failing to account for induced demand may lead analysts to overstate the financial effects of changes in the reimbursement system. In the case of Colorado, although hospital reimbursement rates fell by around 40% following the reform, some of this decrease was offset by increased billing.

Our analysis also suggests several avenues for further research. Given that physicians likely have less discretion over treatment for trauma patients than they do for patients in outpatient settings, it seems plausible that larger demand effects than those documented in this study may be observable in other care settings. With suitable data, an approach similar to ours could be employed to examine outpatient care. Data on a richer set of outcomes, such as recovery times and disability, would provide a more comprehensive look at the welfare effects of different methods for reimbursing auto injuries.

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Tables and Figures

Figure 1: Trends in Coverage for Colorado Auto Injuries

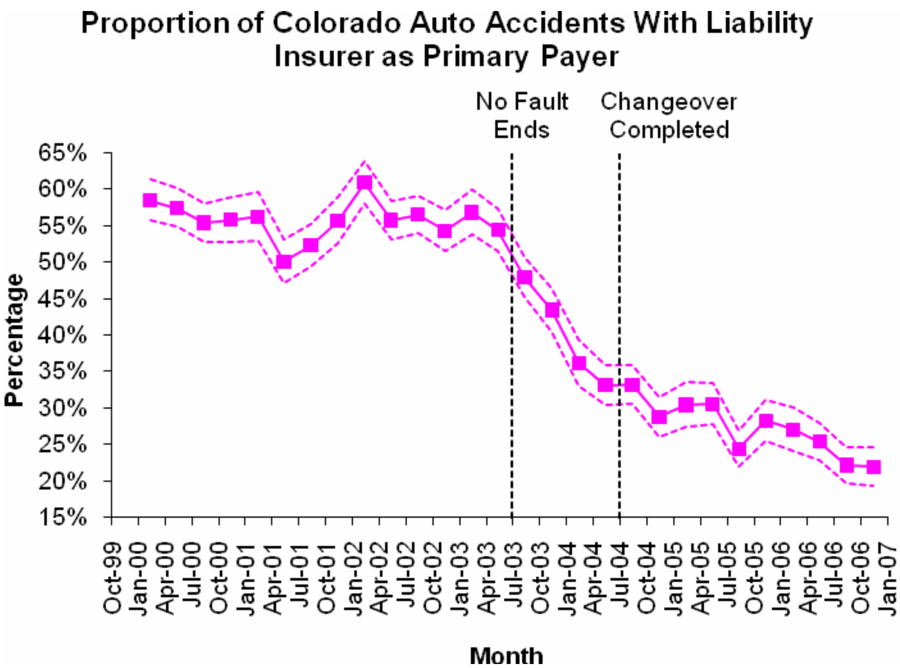
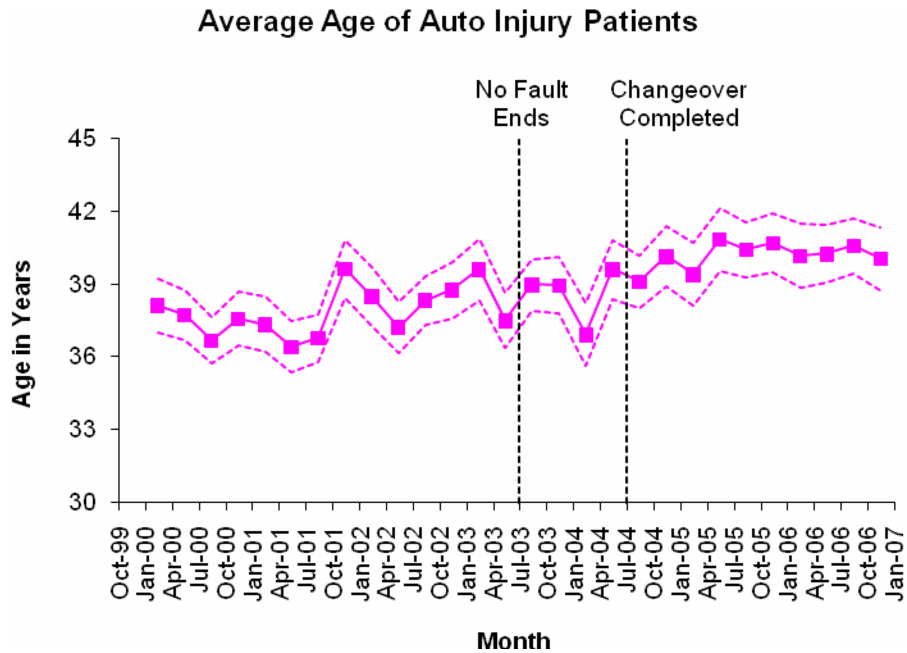
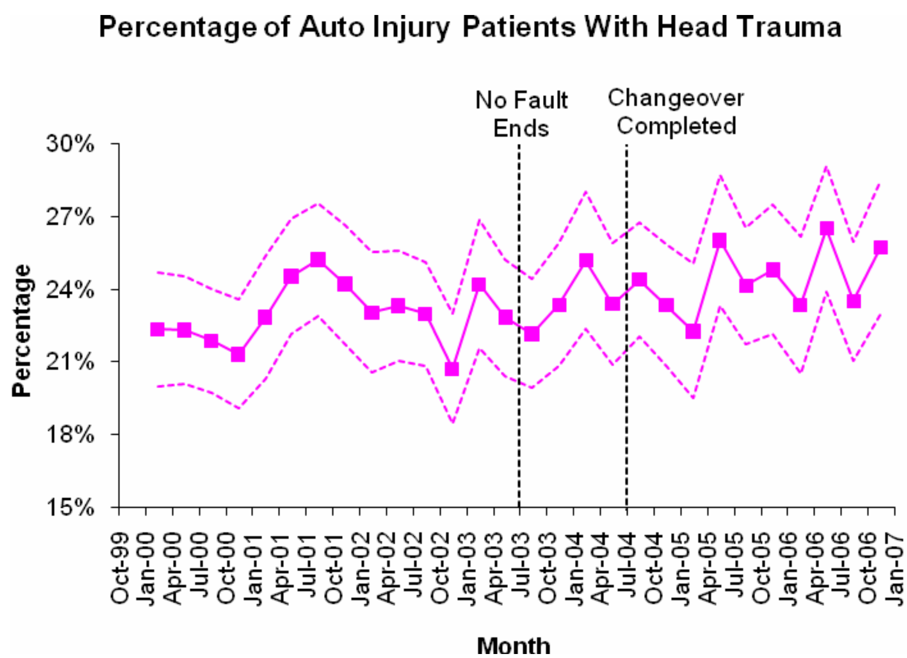
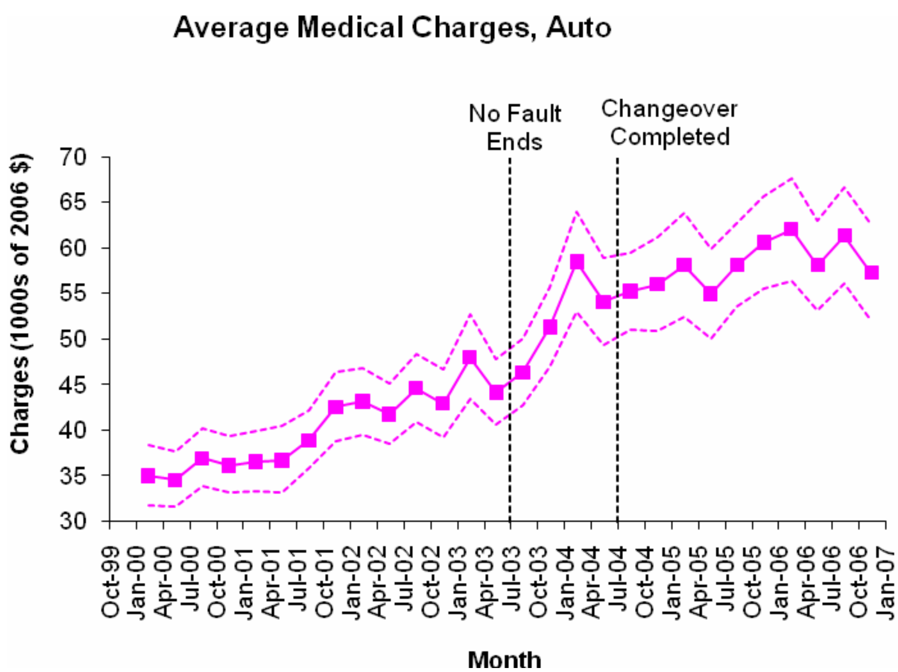
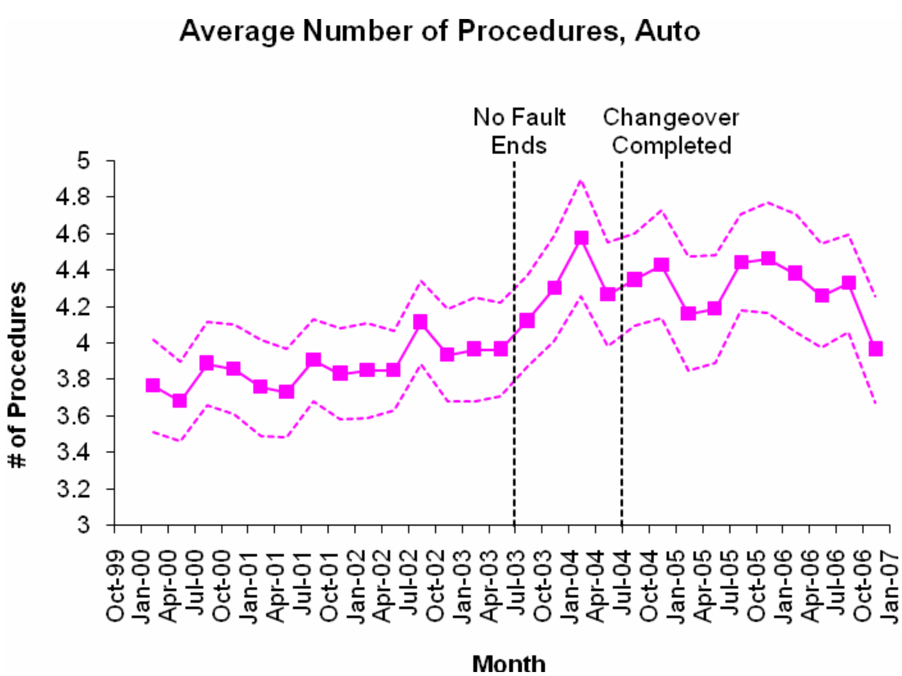


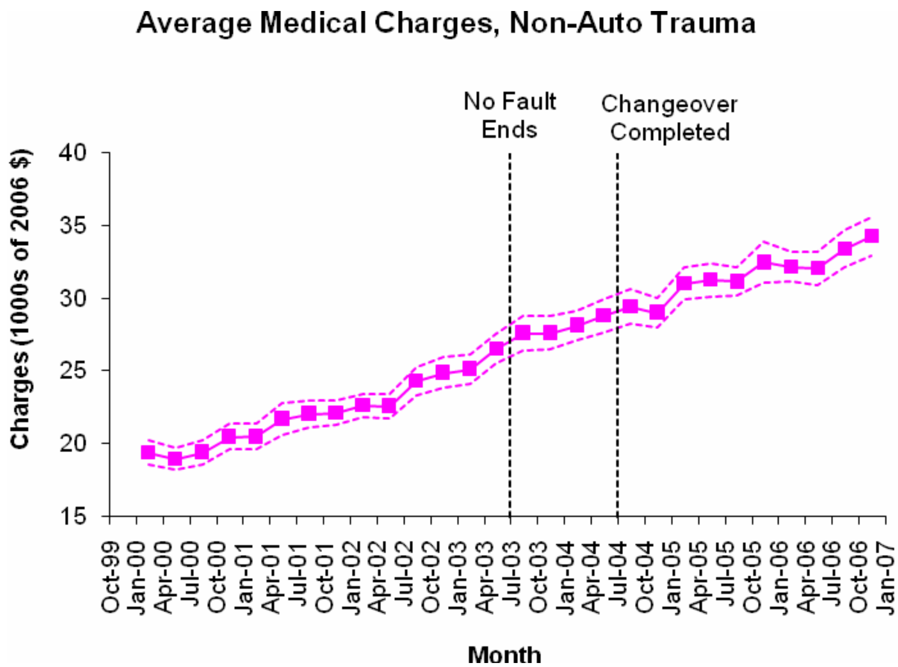
Figure 2: Trends in Patient and Treatment Characteristics






Average Medical Charges, Auto
No Fault Ends
Changeover Completed
Charges (1000s of 2006 $)
70
65
60
55
50
45
40
35
30
Oct-99 Jan-00 Apr-00 Jul-00 Oct-00 Jan-01 Apr-01 Jul-01 Oct-01 Jan-02 Apr-02 Jul-02 Oct-02 Jan-03 Apr-03 Jul-03 Oct-03 Jan-04 Apr-04 Jul-04 Oct-04 Jan-05 Apr-05 Jul-05 Oct-05 Jan-06 Apr-06 Jul-06 Oct-06 Jan-07
Month


Average Number of Procedures, Auto
No Fault Ends
Changeover Completed
# of Procedures
5
4.8
4.6
4.4
4.2
4
3.8
3.6
3.4
3.2
3
Oct-99 Jan-00 Apr-00 Jul-00 Oct-00 Jan-01 Apr-01 Jul-01 Oct-01 Jan-02 Apr-02 Jul-02 Oct-02 Jan-03 Apr-03 Jul-03 Oct-03 Jan-04 Apr-04 Jul-04 Oct-04 Jan-05 Apr-05 Jul-05 Oct-05 Jan-06 Apr-06 Jul-06 Oct-06 Jan-07
Month


Average Medical Charges, Non-Auto Trauma
No Fault Ends
Changeover Completed
Charges (1000s of 2006 $)
40
35
30
25
20
15
Oct-99
Jan-00
Apr-00
Jul-00
Oct-00
Jan-01
Apr-01
Jul-01
Oct-01
Jan-02
Apr-02
Jul-02
Oct-02
Jan-03
Apr-03
Jul-03
Oct-03
Jan-04
Apr-04
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Oct-06
Jan-07
Month

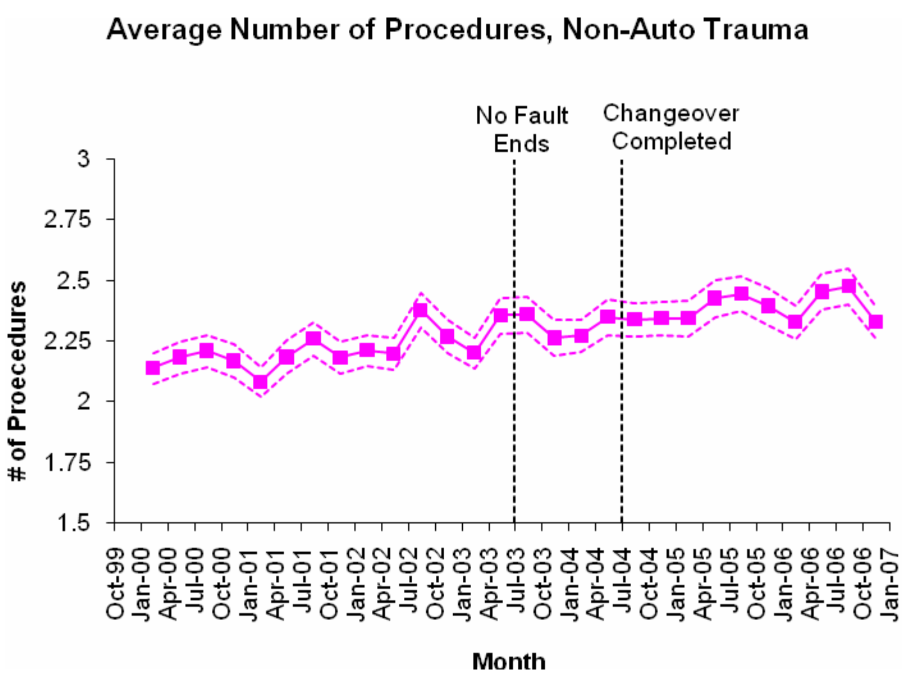
Average Number of Procedures, Non-Auto Trauma
No Fault Ends
Changeover Completed
# of Proecedures
3
2.75
2.5
2.25
2
1.75
1.5
Oct-99
Jan-00
Apr-00
Jul-00
Oct-00
Jan-01
Apr-01
Jul-01
Oct-01
Jan-02
Apr-02
Jul-02
Oct-02
Jan-03
Apr-03
Jul-03
Oct-03
Jan-04
Apr-04
Jul-04
Oct-04
Jan-05
Apr-05
Jul-05
Oct-05
Jan-06
Apr-06
Jul-06
Oct-06
Jan-07
Month

Table 1: Summary Statistics for Selected Variables

Variable	N	Mean	Std. Dev.	Min	Max
Patient Characteristics					
Female	154045	0.468	0.499	0	1
Age (years)	154047	51.9	26.3	0	108
Black	113753	0.036	0.187	0	1
Hispanic	113753	0.138	0.345	0	1
Expected Payment Source					
Liability insurer	151419	0.172	0.377	0	1
Government health insurer	151419	0.395	0.489	0	1
Private health insurer	151419	0.283	0.450	0	1
Self-pay	151419	0.151	0.358	0	1
Injury and Treatment					
Number of diagnoses	154052	7.02	3.89	1	15
Auto-related injury	154052	0.200	0.400	0	1
Total hospital charges ($)	153954	28506	45004	80	973162
Number of procedures	154052	1.88	2.58	0	15
Length of stay (days)	154044	4.56	6.02	0	303
Days before first procedure	106004	0.98	2.25	0	87
Received major diagnostic procedure	154052	0.021	0.142	0	1
Received major therapeutic procedure	154052	0.512	0.500	0	1
Disposition					
Died in hospital	153998	0.022	0.147	0	1
Transferred for home health care	150616	0.064	0.244	0	1
Transferred to other medical facility	150616	0.345	0.475	0	1

Table 2: Effect of Insurance Reform on the Primary Payer for Accident Injuries

	Estimation Method	
Dependent Variable	**Pre-Post**	**Diffs-In-Diffs**
Probability primary payer is:		
Liability insurer	-.224**	-.234**
	(.013)	(.006)
Private health insurer	.062**	.076**
	(.011)	(.006)
Governmental health insurer	.100**	.059**
	(.009)	(.005)
Self-pay	.061**	.099**
	(.010)	(.005)
N	31980	171065

Note: This table reports regression estimates of the effect of the Colorado repeal of no-fault on the probability that different types of insurers were the primary payer for medical charges resulting from auto injuries. Each table entry reports a coefficient estimate from a unique regression. The Pre-Post specifications limit the analysis to auto injuries and include month of year fixed effects and a yearly time trend as additional controls. The Diffs-In-Diffs specifications include auto injuries as well as non-auto related traumatic injuries in the sample. These specifications include an indicator for an auto-related injury and a full set of month/year interactions as additional controls. These specifications thus identify the effects of the reform comparing the post-pre change in the compostion of insurers for auto injuries to the change for similar non-auto related injuries. Heteroskedasticity-robust standard errors are in parentheses. * denotes statistical significance at the 5% level and ** at the 1% level.

Table 3: Demographic and Injury Characteristics of Accident Victims Before and After the Insurance Reform

	Average		P-Value of Test
Dependent Variable	**Pre-Reform**	**Post Reform**	**H_0: Pre=Post**
Individual Characteristics			
White (N=22383)	.699	.702	.842
Hispanic (N=22383)	.209	.192	.198
Female (N=30805)	.401	.384	.188
Out-of-state (N=30123)	.100	.095	.571
Age (N=30805)	38.4	39.1	.160
Weekend (N=30805)	.346	.336	.406
ER Admission (N=30268)	.849	.889	.000
ZIP Code Characteristics (N=26279)			
Fraction married	.574	.573	.759
Fraction urban	.813	.822	.320
Fraction non-citizen	.066	.070	.050
Fraction college graduate	.285	.282	.495
Median household income	47198	46869	.460
Unemployment rate	.046	.048	.049
Vehicles per household	1.87	1.86	.231
Injury Characteristics (N=30806)			
Head injury	.240	.241	.922
Upper limb fracture	.070	.059	.103
Lower limb fracture	.162	.143	.045
Other fracture	.182	.185	.804
Internal injury	.118	.122	.633
Travel Distance to Hospital			
Same ZIP code (N=29890)	.079	.074	.490
ZIP miles (N=27596)	17.5	17.6	.914

Note: Averages are computed using regressions of each characteristic on an indicator for the post reform period (coded with intermediate values during the sunset period) and month fixed effects. Individual and injury characteristics are reported directly on each patient's medical record; the ZIP code characteristics are calculated by matching the ZIP codes of patients who are Colorado residents to 2000 Census demographic files. The "Travel Distance to Hospital" calculations are based upon the ZIP codes of patients compared to the ZIP codes of the hospitals where they were treated. ZIP miles is the distance in miles between ZIP code centroids, and the averages are evaluated using a log-linear model.

Table 4: Effect of Insurance Reform on Treatment and Outcomes

	Estimation Method	
Dependent Variable	**Pre-Post**	**Diffs-In-Diffs**
Treatment Intensity		
Log(Total charges)	.041*	.048**
	(.019)	(.010)
Number of procedures	.136*	.079**
	(.054)	(.021)
Length of stay (days)	.135	.093
	(.108)	(.049)
Days from admission to first procedure	-.062	-.059*
	(.056)	(.026)
At least one major diagnostic procedure	.003	-.001
	(.002)	(.001)
At least one major therapeutic procedure	.024	.043**
	(.016)	(.008)
Disposition		
Died in hospital	.001	.001
	(.002)	(.001)
Transferred to health care facility	-.009	-.003
	(.011)	(.007)
Provided home health care	.008	-.009**
	(.005)	(.003)

Note: This table reports regression estimates of the effect of the insurance policy reform on patient treatments and outcomes. The unit of observation is a patient and each table entry represents a coefficient estimate from a separate regression. All regressions include fixed effects for patient age, sex, race, residence ZIP code, number of diagnoses, primary diagnosis, and hospital as additional controls. The pre-post specifications also include month fixed effects and a time trend as additional controls while the diffs-in-diffs specifications include a full set of month by year interactions. In the diffs-in-diffs specification, the first difference is Post-Pre and the second difference is Auto-Related Injury-Non-Auto-Related Traumatic Injury. For dependent variables that are counts (length of stay, number of procedures, days to first procedure) analysis is performed using negative binomial regression, while dichotomous dependent variables (diagnostic/therapeutic procedures and disposition outcomes) are analyzed using a probit model. Reported coefficients for negative binomial and probit regressions are marginal effects. The pre-post regressions have approximately 35000 observations and the diffs-in-diffs regressions have approximately 150000 observations. Heteroskedasticity-robust standard errors are reported in parentheses; * denotes statistical significance at the 5% level and ** at the 1% level.

Table 5: Effect of Insurance Reform on Real Resource Use

	Estimation Method	
Dependent Variable	**Pre-Post**	**Diffs-In-Diffs**
Log(Labor inputs)	.075*	.087**
	(.034)	(.017)
Log(Equipment inputs)	.069*	.081**
	(.032)	(.016)
Log(Supply inputs)	.076	.117**
	(.042)	(.021)
Log(Total inputs)	.062	.114**
	(.040)	(.020)
Log(Total inputs)	.027	.050**
	(.022)	(.011)

Note: This table reports pre-post and diffs-in-diffs regressions of the effect of the change in reimbursement structure on the amount of resource inputs used in treating patients. See notes for Table 4. Resources were calculated based upon the composition of procedures administered to each patient combined with expert estimates of the amount of labor, equipment, and supplies required for each procedure. Equipment generally refers to durable machinery and supplies refers to perishable items. The final rows of the table report specifications including a set of fixed effects for the number of procedures as additional controls.

Table 6: Reimbursement Rates for Audited Claims in the Post-Reform Period

Explanatory Variable	(I)	(II)
Audit x Colorado	.036**	.067**
	(.009)	(.012)
N	13297	13297
R^2	.030	.167
Include state fixed effects?	Yes	Yes
Control for patient characteristics?	No	Yes

Note: This table reports diffs-in-diffs regressions examining whether reimbursements were lower when claims were audited in Colorado relative to other states. The dependent variable is the amount of medical expenses reimbursed divided by claimed medical expenses (mean=.87). The primary explanatory variable is an interaction between whether a claim was audited and whether it occurred in Colorado. Each column reports results form a separate regression. Both specifications include an indicator for whether a claim was audited (mean=.12) and a full set of state fixed effects as additional controls. The difference-in-difference estimate thus compares claims in Colorado to claims in other states and claims that were audited to claims that were not audited. Column II also includes fixed effects controlling for type of auto coverage; claimant age, gender, role in accident, seat position, and seatbelt use; accident impact point and damage severity; and patient injuries (fatality, minor cuts, laceration, scarring back sprain, neck sprain, other sprain; knee, shoulder, disc, internal, brain, tempero-mandibular joint, and pregnacy-related injury; fracture, concussion, amputation, paralysis, sensory loss, psychological trauma, headache, and other). Standard errors clustered on state are reported in parentheses; * denotes statistical significance at the 5% level and ** at the 1% level.

Table 7: Additional Tests

Specification	Log(Total Charges)	Number of Procedures	Days to First Procedure	≥ 1 Major Therapeutic Procedure	Log(Total Inputs)
	Dependent Variable				
1. Control for insurance type	.066**	.102**	-.034	.048**	.100**
	(.010)	(.021)	(.026)	(.008)	(.017)
2. Limit to uninsured	.094**	.147*	.082	.068**	.111**
	(.024)	(.061)	(.047)	(.017)	(.043)
3. Omit patients with no procedures	.049**	.040	-.051	.042**	.106**
	(.012)	(.030)	(.026)	(.007)	(.018)
4. Alternative functional form	10,999**	.085**	-.080**	.047**	21.157**
	(808)	(.024)	(.028)	(.007)	(3.493)
5. Limit to 2003-2004	.044	.049	-.007	.008	.058
	(.023)	(.052)	(.053)	(.015)	(.040)
6. Omit transition period	.074**	.099**	-.061*	.052**	.114**
	(.011)	(.025)	(.027)	(.007)	(.019)
7. Placebo regression using non-auto transport injuries	.019	.021	-.070	-.004	.049*
	(.014)	(.035)	(.038)	(.011)	(.022)

Note: This table reports diffs-in-diffs regressions of the effect of the change in reimbursement structure on treatment. These estimates are analogous to those presented in the right columns of Tables 4 and 5. For specification 4, the regressions examining total charges and total resource inputs are estimated in levels instead of logs, the regressions examining number of procedures and days to first procedure are estimated using poisson regression instead of negative binomial regression, and the regression examining major therapeutic procedures is estimated using a linear probability model instead of a probit model. Specification 6 includes a full set of month/year/injury class interactions for injuries occurring between 7/2003 and 6/2003, so that these injuries do not contribute to identification of the effects of the reform. For specification 7, the treated group consists of patients involved in non-auto transport accidents (e.g. bicycle or boat) in the post-reform period; this group of patients should not have been affected by the reform.