Can Physician Induced Demand Benefit Patients? Evidence from a Major Change to Medicare Chemotherapy Reimbursement Policy

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Mireille Jacobson,
Joseph P. Newhouse,
and
Craig C. Earle, M.D.*

Abstract While much of health care reform focuses on the supply-side, consumer response to demand-side changes remains more widely understood. We study a major supply-side change – a 2005 reform to Medicare reimbursement policy for physician-administered drugs such as chemotherapy. The reform switched from list-price to transacted-price based reimbursements, reducing drug margins by almost 30% on average. Chemotherapy treatment probabilities declined sharply for beneficiaries diagnosed with lung cancer just before relative to after the reform; the choice of drugs changed consistent with relative margin changes. Surprisingly, the likelihood of death declined, implying that survival outcomes improved as a result of physician-induced demand.

* Jacobson: RAND and NBER; Newhouse: Department of Health Care Policy, Harvard Medical School, Department of Health Policy and Management, Harvard School of Public Health, Harvard Kennedy School and NBER; Craig C. Earle: The Ontario Institute for Cancer Research, Toronto, Canada.
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Introduction

Medicare's payments for both physician and non-physician practitioners were set to decline by almost 27% in 2013 due to the pending expiration of a series of temporary fee increases (MedPAC 2012).[1] Although Congress, as part of the fiscal cliff deal, postponed the cuts, as they have done every year since the first and only fee reduction was implemented in 2002, understanding the implications of such a large change in fees is of clear policy importance. Most directly, this knowledge would inform the annual debate over Medicare fee cuts. It would also provide broader policy guidance on the importance of "volume offsets" or the supply increases that are assumed to follow legislated price declines and, as a consequence, factor explicitly into Medicare cost projections (CBO 2007).

With some important exceptions (Clemens and Gottleib, 2012; Hurley et al. 1990; Hurley and Labelle 1995; Escarce 1993) much of the literature finds a negative relationship between changes in administratively set fees and the supply of medical care (Rice and Labelle 1989; Rice 1983; Rice 1984; Christensen 1992; Yip 1998; Nguyen and Derrick 1997; Grant 2009; Gruber, Kim and Mayzlin 1999). As the theoretical literature on physician-induced demand demonstrates, however, the relationship between administrative price changes and volume is a priori ambiguous and depends on the relative strength of the income effect (McGuire and Pauly 1991).[2] When the income effect is small, physicians act like profit-maximizing firms and reduce the volume of services in response to a price reduction. When the income effect dominates the substitution effect, the physician's supply curve will have a negative slope such that the volume of services increases in response to a fee cut. The case of extremely large income effects corresponds to a model of "literal target income," where changes in administered prices are fully offset by changes in volume (assuming a full offset is feasible).

[1] In fact the 27% reduction is due to the combined effect of the expiration of fee increases and the accumulation of delayed cuts to Medicare's Sustainable Growth Rate (SGR), the statutory formula for annually updating Medicare fee schedule for physicians and non-physicians. See pages 211-212 in MedPac (2012) for a full discussion of the issue,

[2] This conclusion from McGuire and Pauly (1991) comes out of a very general model where physicians maximize utility that depends positively on net revenue and leisure and negatively on the amount of demand inducement.

We contribute to the literature on volume offsets by analyzing a 2005 change to Medicare fees that likely had a large impact on the income of a particular specialist group, oncologists. Specifically, we study a major reform to Medicare reimbursement policy for physician-administered (Part-B) drugs, primarily injectable and intravenous (IV) oncology, rheumatology, urology and infectious disease agents. Unlike more traditional orally administered products, i.e. pills, physicians purchase Part-B drugs from wholesalers, administer them in their office or clinic and are reimbursed for them directly by payers. The 2005 reform, which replaced a reimbursement system based on list prices with one based on national average transaction prices, sought to remedy well-known Medicare overpayments for Part-B drugs. In the case of chemotherapy, reimbursement was estimated to average 29% above transaction price in 1997 (OIG 1997) and 22% in 2004, after an initial fee reduction took effect (GAO 2005). In contrast, beginning in 2005, average margins were administratively set at 6% above transaction price across all Part-B drugs. Thus, margins fell by nearly a factor of five. Since chemotherapy drug reimbursements accounted for over three-quarters of oncologist practice revenues in 2005 (Asckin et al. 2007), the drug mark-up accounts for more than half of their reported net income (Butcher 2008) and because Medicare is the primary payer for many cancers, the reform almost assuredly represented a large negative shock to oncologist income.[3]

Importantly, we assess the impact of the reform on not only chemotherapy treatment but also on health outcomes. To our knowledge, we present the first evidence suggesting that physician-induced demand can have real implications for patient health. To study these issues, we employ an interrupted time series analysis that takes advantage of the sharp timing and context of the reform. We focus on the case of lung cancer because, as we will demonstrate, several drugs commonly used to treat the disease lost significant margin due to the reform and because it is among the most common cancers, with approximately 210,000 new cases each year and 160,000 deaths per year (U.S. Cancer Statistics Working Group 2012). In addition, because almost 70% of new lung cancer cases are among those ages 65 and over (Howlader et

[3] Furthermore, although GAO has found that the payments oncologists receive for practice expenses – primarily evaluation and management – are higher than they would be under a charge-based payment system, oncologists maintain that these fees do not adequately cover expenses (GAO 2001),

al. 2011), Medicare payment policy is relevant for much of this group. Finally, because lung cancer survival rates are low – with five-year survival rates of only about 16% (Howlader et al. 2011) – we can reasonably assess the impact of the reform on disease outcomes.

Our analysis centers on comparing the outcomes of Medicare beneficiaries diagnosed with lung cancer in the few months before relative to the few months after the payment reform took effect. We first demonstrate that the observable characteristics of patients diagnosed with lung cancer were smooth across the payment change. We then show that those diagnosed just after relative to just before the payment change were significantly more likely to receive chemotherapy treatment – on the order of 10% more likely within 30 days of diagnosis. The timing of this change lines up precisely with the payment change. Moreover, the change is specific to the physician's office setting in contrast to the hospital outpatient setting, where the reimbursement change did not take effect until 2006 (Barlas 2011). In addition, we show that changes in the mix of drugs used to treat patients correspond to the changes predicted by economic theory: drugs that lost the most margin were used less frequently among the chemotherapy-treated population and conversely, expensive drugs favored by the reform's equalized 6% margin, were used more often than previously.

In addition to analyzing treatment, a unique feature of this study relative to other analyses of physician-induced demand is that we assess the implications of the observed changes for survival. We find that the treatment changes improved patient survival; the likelihood of death at 3, 6, and 9 months after diagnosis declined between 2%–4%, suggesting that, consistent with the clinical literature, lung cancer may be undertreated among the elderly and, perhaps more surprisingly, survival outcomes improved at lower total cost.

In addition to contributing to the literature on physician-induced demand, this work aims to provide more credible quasi-experimental evidence on how reforms to the supply-side of the health care market affect both physician treatment decisions and patient health outcomes. Much of the current health policy agenda involves supply-side reforms, whether accountable care organizations (ACOs), health information technology or explicit payment reforms. Although some experimental work examines the impact of

changes in physician reimbursement on the practice of medicine (Hickson, Altemeier and Perin 1987, Krasnick et al 1990), the strongest evidence – both quasi-experimental and experimental – remains on the demand-side of the health care market (see, for example, Chandra, Gruber and McKnight (2010), Manning et al. (1987), Newhouse and the Insurance Working Group (1993), Finkelstein et al. (2012)). Our work demonstrates that supply-side payment reform can have meaningful effects on both the practice of medicine as well as patient outcomes.

The rest of the paper is organized as follows. The next section provides institutional background on chemotherapy treatment and reimbursement policies for these drugs. Section 3 describes the data and analytic approach. Section 4 presents our analysis of treatment changes, tests for structural breaks in treatment, and estimates of the effect of the payment change on mortality. Section 6 concludes.

Section 2. Institutional Background

Although Medicare did not offer a drug benefit for pills until 2006, Medicare Part-B, "supplemental Medicare" for outpatient services, has from inception covered physician-administered drugs such as IV chemotherapy, anti-nausea and pain medicine used in cancer treatment. Since at least the mid-1990s, it was well understood that Medicare overpaid for many chemotherapy and other Part-B drugs (OIG1996; OIG 1997).

The Medicare Prescription Drug, Improvement and Modernization Act (MMA) of 2003 aimed to resolve the overpayment issue by redesigning the way Medicare reimburses physician-administered (Part-B) drugs. Prior to the reform, Medicare reimbursed physicians and outpatient hospital departments for these drugs as a percentage of the average wholesale price (AWP) – a list price published in several pricing catalogues that reflects neither wholesale nor average prices (Berndt and Newhouse, 2012). From 1998 to 2003, reimbursements were 95% of AWP and in 2004 the MMA reduced reimbursements to 85% of AWP in an effort to smooth the transition to the new payment system.

In principle manufacturers could have changed their reported list prices between 2003 and 2004 (or in any other years); in practice, however, such changes are rare. Most importantly, for many drugs the AWP exceeded acquisition costs on average by

anywhere from 13 to 34% (GAO 2004). Some agents, such as paclitaxel, a chemotherapy agent commonly used to treat patients with lung, breast and ovarian cancer, were reimbursed at prices almost an order of magnitude above their acquisition cost (GAO 2004). The reform replaced this system with a new average sales price (ASP) payment system whereby Medicare reimburses for Part-B drugs based on the national average of manufacturers' sales prices, including rebates, from two quarters prior plus a 6% mark-up. The reform affected physician's offices, the primary setting for chemotherapy treatment, in 2005 and the outpatient hospital setting, the next most common setting for chemotherapy, in 2006.

When this policy took effect on January 1st, 2005, the profit margins for many chemotherapy drugs were substantially reduced. Fees for chemotherapy administration were increased between 2003 and 2004 but reduced, albeit not fully, between 2004 and 2005 as MMA transition payments were eliminated (GAO 2004). Even the 2003 to 2005 changes, however, were small in dollar terms (on the order of $50 to $100 per session) compared to the 2004 to 2005 drug reimbursement change, which could total hundreds and even thousands of dollars for a monthly dose of some commonly infused drugs.[4]

Some work in health policy has analyzed the impact of the MMA on chemotherapy treatment, though none to our knowledge has estimated the impact on health outcomes. First, several papers have sought to evaluate the claim by many community oncologists that the fee changes would make it too expensive to treat Medicare beneficiaries, forcing them to shift some patients to hospital settings where they would face unnecessary treatment delays (Community Oncology Alliance 2006). These papers find to the contrary; wait times, travel distance and treatment setting (outpatient hospital or physician's office) did not change in response to the payment change (Friedman et al., 2007; Shea et al. 2008). However, both studies – in one case, a web-survey of a convenience sample of patients undergoing chemotherapy (Friedman et al., 2007) and in the other a 5% sample of claims restricted to beneficiaries with leukemia, lymphoma or breast, colorectal, lung cancer and receiving chemotherapy – condition on treatment, a potentially important margin of adjustment.

[4] Many chemotherapy regimens are given in 3-week cycles, meaning that patients receive about 1.33 treatments per month.

Indeed, our prior work suggests that, at least for lung cancer patients, the likelihood of chemotherapy treatment and the mix of drugs used changed in response to the payment change (Jacobson et al. 2010). These findings are consistent with analyses of aggregate chemotherapy billing data by the Medicare Payment Advisory Committee (MedPAC 2006). Other work supports the view that responses on the extensive margin are important. In response to the MMA, use of medical castration or androgen-deprivation therapy (ADT), which was subject to AWP-reimbursement, to treat prostate cancer declined and surgical castration, which did not face a reimbursement change, increased (Weight et al. 2007). Although analysis of ADT suggests that the decline was largest among those for whom the benefits of therapy were unclear (Shahinian et al 2010), the welfare consequences in the case of lung cancer are less obvious.[5]

An obvious question is how much of these changes were driven by physicians versus patients: since Medicare beneficiaries, in principle face 20% coinsurance for Part-B services, a decline in drug prices could have plausibly increased patient demand. In practice, however, about 90% of beneficiaries have supplemental insurance that pays this coinsurance (KFF 2010). Moreover, anecdotal evidence indicates that oncologists were less likely to collect the coinsurance when reimbursements were based on AWP, implying that out-of-pocket spending may have actually increased for some beneficiaries even as average drug costs declined for Medicare (Mullen 2007).

We seek to understand the consequences of this specific payment reform for chemotherapy treatment as well as provide broader lessons about the return to increased chemotherapy use in this population. Below we review our prior evidence on the effect of the policy change in treatment, more rigorously demonstrating that the observed changes are driven by the policy, and then establish its impact on an important measure of patient welfare – survival.

[5] Other work finds that after the policy change chemotherapy treatment declined in the last 14 days of life among those who received chemotherapy in the 6 months before death in a physician's office (Colla et al. (2012). Since limited use of chemotherapy in the last 14 days of life is a marker of quality, this is one potential marker of improved patient welfare. However, this analysis does not take into account the changes in the likelihood of treatment overall due to the reform.

Section 3. Data and Methods

Data Sources

We analyze Medicare claims from all beneficiaries who had at least one claim in a physician's office or in an outpatient-hospital setting with a lung cancer diagnosis (ICD-9 162.0-162.9) between 2003 and 2005 (N=878,923). For these beneficiaries, we have all of their claims for physicians' services, hospital outpatient care, durable medical equipment, short-stay, long-stay and skilled nursing facility services, and hospice care for 2002 to 2006.[6] Dates of death for our analytic cohort (defined below) are from the Medicare Vital Status File through 2012. These claims capture only the Medicare fee-for-service population, the group affected by this payment change. Although in principle beneficiaries with lung cancer who are enrolled in Medicare Advantage plans, Medicare-approved private health plans that are paid on a risk-adjusted per-enrollee basis, could serve as a control group for this study, there is no comparable source of treatment data for this population.[7]

Cohort Definition

A claim with a lung cancer diagnosis need not indicate the presence of cancer but rather could capture evaluation for a suspected cancer that is subsequently deemed benign or a miscoded diagnosis code. Thus, to confirm lung cancer, we restrict analysis to Medicare beneficiaries with two or more non-institutional (i.e., physician, durable medical equipment or outpatient hospital) claims separated by at least 28 days but no more than 365 days or one institutional (short-stay, long-stay, skilled nursing facility or hospice) claim with a lung cancer diagnosis. This is a common approach to defining cancer in claims data that has been shown to have high sensitivity (i.e., identify a high proportion of actual cancer cases) and very high specificity (i.e., exclude beneficiaries without cancer) in Medicare claims data (e.g., see Warren et al. 1999; Ramsey et a.

[6] These data come from the Medicare Carrier, Outpatient, Durable Medical Equipment (DME), Medicare Provider Analysis and Review (MedPAR) and Hospice files.

[7] We had access to some data from Kaiser Northern California and had intended to use the treatment experience of their Medicare enrollees as a control group. However, there is substantial geographic variation in the response to the payment reform, which we document in Jacobson, Earle and Newhouse (2011), and fee-for-service Medicare in Northern California exhibited almost no change in treatment in response to the payment change. We do not have a good explanation for the geographic variation in response, although such variation is widespread in Medicare (Skinner, 2012).

2009; Vera-Llonch et al. 2011). To isolate lung cancer treatments, we exclude beneficiaries with more than one confirmed primary cancer. We use the first claim with a lung cancer diagnosis to date the onset of disease. To ensure new diagnoses and thus treatment trajectories that had not been influenced by prior clinical decisions, we eliminate beneficiaries with no claims history in the 12 prior months.

We eliminate the roughly 5% of beneficiaries in the fee-for-service claims who were enrolled in a Medicare Advantage plan at some point during the study period and thus lacked complete treatment and billing information. The lung cancer cohort included 216,119 beneficiaries enrolled in Medicare Parts A and B who were diagnosed between January 2003 and November 2005. We further restrict the primary analytic cohort to the 132,768 beneficiaries diagnosed between February 2004 and November 2005 to capture 11 months of data on either side of the payment change.

Defining Chemotherapy Treatment and Survival Outcomes

Chemotherapy treatment is identified using relevant diagnosis and billing codes (Warren et al. 2002). Specifically, we use ICD-9-CM procedure codes of 99.25 or diagnosis codes of V58.1, V66.2 or V67.2, diagnosis-related group code 410, or HCPC codes 96400 through 96549, J9000 through J9999 or Q0083 through Q0085, C8953-C8955 (2006 only), G0345-G0362 and revenue center codes 0331, 0332 and 0335. Beneficiary date of death is drawn from the Medicare Vital Status File, which we had updated through June 30, 2012.

Analytic Approach

To assess whether the January 2005 payment change plausibly affected treatment and health outcomes, we take a graphical approach that analyzes patients by month of diagnosis relative to the reform. Specifically, we plot estimated month-relative-to-reform (January 2005) fixed effects along with 95 percent confidence intervals from a regression of the likelihood a newly diagnosed lung cancer patient received any chemotherapy treatment, chemotherapy treatment by setting (e.g., physician's office), treatment with specific agents conditional on receiving any chemotherapy, all within 30 days of diagnosis. We choose a 30-day treatment period because it enables us to relatively cleanly define treatment (diagnosed post-reform) and control (diagnosed pre-

reform) cohorts.[8] The month-relative-to-reform fixed effects are simply the set of coefficients δ from the following regression:

$$Y_{ism} = \beta_0 + X_{ism}\gamma + \mu_s + \delta_{m-m^*} + \varepsilon_{ism} \quad (1)$$

where Y_{ism} is the treatment or survival outcome of individual i residing in state s and diagnosed in month m, X_{ism} are a set of patient characteristics including gender, race/ethnicity (7 categories), as well as the patient's age and its square, pre-cancer comorbidities, as measured by the Deyo-Charlson score (Deyo, Cherkin and Chiol 1992; Charlson et al. 1987), an indicator for metastatic disease at the time of diagnosis, defined as 30 days from the date of diagnosis[9]; μ_s are state fixed effects, δ_{m-m^*} are month-relative-to-reform fixed effects (e.g., m-m*=-1 in December 2004 and +1 in February 2005) with January 2005 omitted, and ε_{ism} is an error term. The display of these month fixed effects helps us to verify that the time-series patterns are consistent with a causal impact of the reform on treatment and survival. In addition, we use these plots to guide the specification of models estimating the magnitude of the reform's impact. We focus on a window of 11 months prior to and 11 months post reform. The length of window was based on the limited post-reform data available to us.[10] As will be discussed further below, the results are similar for shorter windows as well as when using all 24 months of pre-reform data.

We use the same basic regression framework and visual approach to analyze by month of diagnosis, the likelihood of death within 3, 6, 9, or 12 months of diagnosis. To minimize any bias from right truncation due to differences in follow-up time across cohorts, we separately obtained dates of death from administrative records through June 30, 2012. Because of the low rate of lung cancer survival, this long a follow-up period means that almost 90% of patients have died in both cohorts by the end of our

[8] Longer treatment windows capture cohorts that may not have been diagnosed in the post-reform period but received care in this time period. We show results for these groups in our sensitivity checks.
[9] To confirm metastasis we required two or more non-institutional claims separated by at least 28 and no more than 365 days or one institutional claim with a secondary cancer diagnosis [ICD-9 codes: 197-199. Although this algorithm is likely to miss many beneficiaries with secondary cancer, it has been shown to have reasonable specificity (Earle et al. 2002). Results are not sensitive to the exclusion of this measure.
[10] Although we, in principle have 12 months of data post—reform, the cohort counts show a sharp drop off suggesting these data were incomplete.

study period. In addition we compare the characteristics of the cohorts diagnosed before and after January 2005. The goal is to establish that those individuals diagnosed just before relative to just after the payment change are similar on observable dimensions, a key identifying assumption for our analysis.

Based on these figures, we model the effect of the January 2005 switch to ASP-based payments as follows:

$$\overline{Y}_t = \beta_0 + \beta_1 post + \beta_2(t-t^*) + \beta_3(t-t^*) * post + \overline{X}_t\gamma + \varepsilon_{ct} \qquad (2)$$

where $\overline{Y}_t$ is an outcome such as the mean rate of chemotherapy treatment for beneficiaries diagnosed in week t, post is an indicator that equals 1 after the payment change and 0 otherwise, (t-t*) is a linear function of time relative to the reform, t*, that controls for smooth trends in outcomes around the time of the payment change and that we allow to differ on either side of the payment change. These regressions also control for the share of patients treated in a given week that were male, the share by race/ethnicity, the median age and median age squared of these patients, the mean Deyo-Charlson score,[11] and the share with metastatic disease. We computed Newey-West standard errors, which allow for heteroskedasticity and autocorrelation in the error of an unknown form up to a lag of 52 weeks (Newey and West 1987). We allowed for this long a lag to account for correlation of annual events that affect treatment, such as the Christmas holidays. However, inference is not meaningfully affected by alternative lag structures. In sensitivity checks, we estimate regressions that both include month-relative to Jan 2005 trends and that omit these trends but include calendar month (e.g., Jan, Feb, …) fixed effects to control for cyclicality in the timing of treatment.

The specification laid out in (2) assumes that the effect of the payment change is permanent in the post-reform period. This modeling choice - the permanence of the reform's effect is motivated by our event-study plots, which as will be discussed below, show a shift up in outcomes in the post-reform period. We have conducted a Quandt likelihood ratio (QLR) test for structural break that confirms this choice. Moreover, as we show below, results are similar as we narrow the window around the reform, providing further support that the effect is permanent.

[11] The median Deyo-Charlson score is actually 1 across all weeks.

Section 4. Results

Descriptive Statistics

Table 1 provides demographic characteristics for our analytic sample overall and by diagnosis before versus after January 2005. We focus on the 22-month period from February 2004 to November 2005 so that we have an equal number of months before and after the payment change. Median age at diagnosis was 74 years. Just over half the patients were male, 88% were white and 9% were black. Patients had an average Deyo-Charlson comorbidity score of 1.08, meaning on average one non-cancer comorbidity in the year leading up to diagnosis, and almost 29% had metastatic disease documented in claims within 1 month of diagnosis.[12] While the Charlson score and the proportion of males differed before and after the payment change and the differences are statistically distinguishable from zero, neither difference (0.02 more comorbidities or 0.4% fewer males) is meaningful in magnitude. There were no other differences in observable demographic characteristics.

Payment rates for carboplatin, paclitaxel, and etoposide, drugs commonly used to treat lung cancer, declined dramatically after the payment reform but were relatively flat for docetaxel, a high-priced drug at about $2,500 per standardized monthly dose, and gemcitabine at $1,300 per monthly dose (see Figure 1, which shows that the changes in quarterly reimbursements for carboplatin and paclitaxel occurred around the time of the new payment system's introduction (time 0). The changes were most striking for carboplatin and paclitaxel; reimbursement rates for a standard monthly dose declined from over $2,270 to $225 for paclitaxel and from $1,845 to $930 for carboplatin. The large discrepancy between payment rates and acquisition costs for paclitaxel and carboplatin was identified as early as 1997. The General Accounting Office (now the Government Accountability Office) (GAO) reported widely available discounts of about 20% below AWP for both drugs in 2001(GAO 2001) and a later analysis calculated that reimbursement for paclitaxel in 2004 was six times higher than actual cost (GAO 2004).

[12] We use a 1-month period to define metastasis because our models estimate the impact of the reform on monthly treatment. Given this relatively short period, this measure should yield conservative estimates of metastasis.

A complication in the payment changes is that carboplatin went off patent in October 2004, the quarter before ASP implementation. Because of the lag in Average Sales Price determination – quarterly reimbursements are based on wholesale prices two quarters prior – carboplatin likely maintained a margin above the 6% mandated by the legislation for at least the first quarter of 2005.[13] To ensure that our results for chemotherapy are not driven entirely by carboplatin, where the observed changes in reimbursement combine the effects of the patent expiration and the payment reform, we examine both chemotherapy use of regimens with and without carboplatin.

The summary statistics indicate significant changes in the likelihood of chemotherapy treatment after the payment change. Whereas 16.6% of patients diagnosed in the 11 months prior to the reform received chemotherapy within 1 month of diagnosis, those diagnosed in the 11 months after the reform were almost 2 percentage points (or 11% off the pre-reform base) more likely to receive chemotherapy within 1 month of diagnosis. Nearly three-quarters of this increase (1.5 percentage points) was among patients receiving regimens without carboplatin. In addition, this increase came almost entirely from treatment in physician offices. The level of the increase is similar in relative terms (3.2 percentage point increase in chemotherapy treatment rates off a pre-reform base of 26%) when a window of 3 months since diagnosis is considered. The composition of the changes across setting is also quite similar to the 1-month case.

Conditional on chemotherapy treatment within 1 month of diagnosis, the percent of patients receiving carboplatin declined from almost 56 to 53, and the percent receiving paclitaxel declined from 30 to 26, consistent with the large decline in payment rates for these agents. Since carboplatin and paclitaxel are often given in combination and because carboplatin likely retained a margin above 6% of ASP for a few quarters, some of carboplatin's decline may be driven by the sharp reduction in paclitaxel's profitability. Trends in use of etoposide were comparatively flat while use of docetaxel, a relatively high-priced regimen, increased 1.3 percentage points off a base of just 8.3%,

[13] The gradual decline in carboplatin's payment rates shown in Figure 1, which may capture a gradual switching to generics, suggests it maintained a margin above 6 percent of the average sales price for longer than the first quarter of 2005.

possibly reflecting the incentive created by the uniform 6% margin across all drugs.

Among cohorts diagnosed in the 11 months prior to January 2005, 33.7% died within 3, 46.7% within 6, 52.4% within 9, and 61.6 % within 12 months of diagnosis. For cohorts diagnosed in the 11 months after January 2005, the proportion dying was 1-2 percentage points (3-4%) lower in each of these four periods. Shortening the diagnosis window to 9 or 6 months around the reform yields slightly smaller effects: the likelihood of death is about 3% lower for those diagnosed after the reform (not shown).

We also tested the stability of patient characteristics around the payment change. As in a randomized controlled trial (RCT), patient characteristics should look similar just before and after Average Sales Price implementation. To do this, we estimated variants of equation (1) but with the observable characteristics (e.g., gender or log age) as the dependent variable. The coefficients and 95 percent confidence intervals for the month relative to reform fixed effects are shown in Figure 2a-e. With the possible exception of the comorbidity score, which is noisy, the observable characteristics of patients (e.g., share metastatic, share white, share male and log age) are smooth by month of diagnosis relative to the reform (time 0). And none of the characteristics shows a sharp change around the time of the reform. Estimates of the discontinuity in these variables on January 1, 2005 are provided in Appendix Table 1. There is no reason to expect that the clinical characteristics of newly diagnosed lung cancer patients would change on January 1, 20120. And, in general, the estimates are small and indistinguishable from zero. When the estimatess are distinguishable from zero (specifically, log age and the share of patients of other/unknown race), the changes are modest in size. To the extent that the gap in age is meaningful, which we do not believe it is, it should bias us against finding improvements in survival. Thus, on balance we interpret the results in Figure 2 and Appendix Table 1, as evidence that a key identifying assumption of the empirical approach – the stability of the patient population across the reform – is satisfied.

Changes in Treatment

Figures 3a – 3d show the regression-adjusted mean changes in the likelihood that patients received any chemotherapy, any chemotherapy regimen excluding carboplatin, any chemotherapy in a physician's office and any chemotherapy in an

outpatient clinic by month of diagnosis relative to January 2005. With the exception of the outpatient clinic setting (Fig 3d), which did not experience a payment change until the following year, all the panels show a discrete increase in the likelihood of chemotherapy treatment for those lung cancer patients diagnosed just after relative to just before January 2005 (time 0). The timing of the increase strongly suggests that the switch to the Average Sales Price system drove the change. The similarity across outcomes confirms that the increase in treatment is not driven by the sustained margin on carboplatin and that most of the increase came in physician offices, the only setting subject to the payment reform in 2005.

Table 2 shows the estimates of the discontinuous jump in chemotherapy treatment rates for those diagnosed just after January 2005. The estimates are based on the specification in (2) and using 11 months (col 2) or 9 months (col 3) on either side of the payment change. The overall change in the likelihood of chemotherapy treatment rates within 1 month of diagnosis is about 1.5 percentage points. Off a base of 16.6% of those diagnosed prior to the reform receiving treatment within one month, this represents about a 9% increase in the likelihood of treatment in response to the reform. When we look at treatment that does not include carboplatin, the relative increase is much larger – almost 20% (1.2 to 1.4 percentage points off a base of 7.3%). This implies that the overall change in chemotherapy treatment is not driven solely by the interaction between the payment reform and the patent expiration of carboplatin. In addition, the main increase is driven by treatment in the physician's office setting, which was the setting subject to the reform. As shown in Appendix Table 2, the results are largely insensitive to the use of relative month trends (cols 1 and 2) or calendar month fixed effects (cols 3 and 4) instead of relative-week trends. The main difference is that the effects are slightly larger when calendar month instead of relative-week or relative-month trends are used, suggesting a 10% increase in treatment and an increase in treatment in the outpatient setting. However, the patterns in Figure 3 suggest that the specification with relative trends that are allowed to vary on either side of January 1, 2005 more appropriately captures treatment trends before and after the reform.

Figures 4a-4d and Table 3 consider chemotherapy service counts within thirty

days of diagnosis. The change in service counts, unlike the likelihood of treatment, combines changes on both the extensive and intensive margins. The trends in Figure 4 are analogous to those in Figure 3 but suggest larger changes after the payment reform at least outside of the outpatient clinic setting, where the change is also essentially zero. As shown in Table 3, the mean number of chemotherapy services within 30 days was 1 prior to the reform; among those diagnosed after the reform, services increased almost 33%. Given the 9% increase in treatment on the extensive margin, these results suggest that chemotherapy services likely increased even among those who would have received some treatment absent the reform. As on the extensive margin, the increase is seen among patients receiving treatments that do not contain carboplatin, implying that the interaction of the reform and carboplatin's patent expiration do not explain our main treatment result. The increase in the physician office setting accounts for almost all (about 94% or 0.916 *0.347/0.339) of the change. Services appear to increase modestly in the outpatient clinic setting as well, about 1.3%, although this change is quite modest compared to the 34% increase in the physician office setting. As with relative-week trends, estimates from models with relative-month trends or calendar month fixed effects (Appendix Table 3) are similar but larger without the inclusion of linear time trends.

As a more formal test of a structural break in chemotherapy treatment, we adopt an approach from the time series literature. Specifically, we employ a Quandt likelihood ratio (QLR) test (Quandt 1960) that tests for structural breaks at all possible dates between two time periods, $\tau_{0\,0}$ to τ_1. Operationally, the test amounts to a modified set of sequential Chow tests based on equation (2), where a post-period is defined at each cutoff date τ between the two time periods, i.e. $post_\tau = 1(t \geq \tau^*)$ is an indicator equal to zero for all weeks before τ^* and one for all subsequent weeks; this indicator is also interacted with the linear trend. Under the null hypothesis of no break, the coefficients on $post_\tau$ and $post_\tau$* (t-t*) are jointly equal to zero. The QLR statistic, also known as the sub-Wald statistic, is just the maximal of the F-statistics testing the null of no break at all dates τ between $\tau_{0\,0}$ to τ_1; it provides an estimate of the structural break date.

To implement this test, we adopt the conventional choice for $\tau_{0\,0}$ to τ_1 of 0.15T and 0.85T or -34 weeks and + 33 weeks relative to the first week, i.e. week 0, in January 2005. In this way, the post period cutoff or break date, τ^*, is sequentially defined over the inner 70% of the 96 week sample. We take the critical values of the QLR statistic for the endpoints 0.15T and 0.85T and 2 restrictions tested from Stock and Watson (2003), which are adapted from Andrews (2003).[14]

Appendix Figure 1 plots the F-statistics testing for a break at -34 to +33 weeks relative to week 0. We show theses test for the following 30-day treatment series: any chemotherapy, chemotherapy that does not include carboplatin, chemotherapy in a physician's office, and chemotherapy in an outpatient hospital setting. Based on the QLR statistic, a structural break in treatment occurs two weeks prior to January 2005 for any chemotherapy treatment (App Fig 1a) and treatment in a physician's office (App Fig 1c). Excluding Carboplatin, the break occurs 1 week prior to ASP implementation (App Fig 1b). The pre-implementation break is not worrisome given that we are analyzing 30-day treatment and patients diagnosed at the end of 2004 almost certainly began treatment after the new payment system took effect. Importantly, in all 3 cases, the maximal F-statistic exceeds the 1% critical value of 7.78, implying we can reject the null of no structural break in treatment. In contrast, we cannot reject the null at any point for chemotherapy treatment in the outpatient setting (App Fig 1d).

Together the results above suggest that chemotherapy treatment within 30 days of diagnosis increased on both the extensive and intensive margins for those diagnosed just after relative to just before the reform. As another check on the credibility of these results, we analyze the likelihood of treatment within 90 days of diagnosis. Looking at 90 instead of 30 days means that those diagnosed just before the reform may still be affected by the reform if they survive past implementation. Consistent with this fact, Appendix Figures 2a-2c show a gradual increase in the likelihood of 90-day treatment for those cohorts diagnosed within three to one months prior to the reform. The change flattens out for those cohorts diagnosed just after the reform, suggesting that the earlier

[14] The QLR critical values are necessarily larger than those for an individual F-test since searching over a large set of F-tests provides many opportunities to reject the null. In other words, the QLR critical values effectively implement a multiple-hypothesis correction.

gradual increase suggests only partial exposure to the reform relative to the time of diagnosis. As indicated in Appendix Table 4, once this gradual rise is accounted for (via an indicator for those diagnosed in the quarter prior to the reform), the increase in 90-day treatment for those fully exposed to the reform, i.e. diagnosed in 2005, was about 11% overall and almost 15% in the physician's office-setting. In a parallel fashion, Figure 3d shows that those cohorts diagnosed in the last few months of 2005 experienced an increase in the likelihood of 90-day treatment in the outpatient setting, after it would have been subject to the payment reform. In other words, the treatment response to the payment change in the physician's setting is not unique.

We next analyze changes in the types of drugs used. Figure 5a-5e show regression-adjusted changes in the probability that chemotherapy-treated patients received carboplatin, paclitaxel, docetaxel, etoposide and gemcitabine, which are the most commonly prescribed agents in our sample. Although more patients received chemotherapy after the new payment system was implemented, patients treated within 30 days of diagnosis were less likely to receive a mix of agents that included carboplatin or paclitaxel. The timing of the sustained decline in use of these agents preceded the introduction of the average sales price payment system by about 3 months, which may reflect the quarterly purchasing that is common among oncologists.[15] Given the publicity the reform received in the oncology community, physicians likely understood that the reduction in payment rates for these drugs would be large and were reducing their reliance on these agents in advance. Failure to do so could have meant a considerable loss of income.

In contrast, the probability of receiving docetaxel, a relatively expensive agent implicitly favored by the 6% margin on all drugs, increased modestly for patients receiving chemotherapy treatment. The increase preceded the change in reimbursement by about a month, further suggesting physicians were rearranging their stock of agents in anticipation of the ASP system. The likelihood of receiving etoposide or gemcitabine and other less commonly used agents did not change systematically.

Table 4 quantifies the changes illustrated in this figure (using the same

[15] This fact was relayed to us by a community oncologist from Phoenix, AZ.

specification as above) but first considers the unconditional change in use of these agents within 30 days of diagnosis. Whereas carboplatin use within 30 days did not change when considered unconditionally, use of paclitaxel, the drug that lost the most margin, declined by about 10% and use of docetaxel and etoposide increased by about 30 and 9%, respectively. The changes combine the change in chemotherapy treatment and the choice of drugs. As a share of all agents, physicians were less likely to give patients carboplatin or paclitaxel after average sales price implementation. The probability that chemotherapy-treated patients received carboplatin declined 3 to 4 percentage points. The decline in the probability of receiving paclitaxel was more dramatic – about 6 to 7 percentage points or over 20%. Some physicians switched patients to docetaxel, a relatively expensive regimen favored by the 6% uniform mark-up. The likelihood patients received docetaxel increased about 2.6 percentage points or over 30% off a base of 8.3%. The estimated change for etoposide is small, indistinguishable from zero and even changes sign as the study window is narrowed. The estimate for gemcitabine is larger in relative terms (0.7 percentage points off a base of 10%) but based on Fig 5e may not reflect a policy-related change.

Changes in Survival

The results presented above demonstrate that chemotherapy treatment increased on both the intensive and likely the extensive margin for lung cancer patients diagnosed just after relative to just before the payment reform. The mix of drugs administered to patients also changed, with drugs that lost the most margin used less and expensive agents favored by the 6% average margin used slightly more. These treatment changes beg the question of what happened to patient welfare. We have one important measure of welfare – survival – that we exploit here to address this issue. A limitation is that we can say nothing about changes in quality of life, which could deteriorate in response to harsh chemotherapy treatment (e.g., see Ballatori et al. 2007).[16]

[16] Despite the commonly held view that chemotherapy treatment decreases quality of life, a recent randomized trial in patients with advanced lung cancer found that, compared to placebo, chemotherapy treatment not only increased survival but also improved some measures of quality of life (specifically the worsening of pain and the coughing up of blood) while leaving others quality of life measures unchanged (Belani et al 2012).

To begin, we first plot in Appendix Figure 3 the unadjusted Kaplan-Meier survival curves for patients diagnosed in the 11 months before and after the payment change.[17] The median survival based on the Kaplan-Meier estimator was 261 days for those diagnosed in the pre-period and 284 days for those diagnosed in the post period, a difference of just over 3 weeks. To assess whether this survival difference is credibly related to the reform, Figure 6a-6d plot the regression-adjusted mean changes in the proportion of patients dying within 3, 6, 9 and 12 months by month of diagnosis relative to the January 2005 payment change. Consistent with the Kaplan-Meier curves, it demonstrates a discrete decline in the likelihood of dying within each interval for the post-reform cohorts. The decline, at least for the likelihood of death between 3 and 9 months, corresponds closely to the timing of ASP implementation and the previously demonstrated increase in chemotherapy utilization.

Table 5 quantifies the mortality changes in Figure 2a-2d. Among patients diagnosed just after relative to just before the reform, the likelihood of death within three months of diagnosis decreased about 1 percentage point or about 4% relative to the base of 33.9% dying in this interval. The results are virtually identical using only patients diagnosed 9 months before to 9 months after the payment change (col 3) or using alternative specifications (see Appendix Table 6). At 6 months from diagnosis, the reduction in the likelihood of death increased to about 1.6 percentage points or 3.4% relative to the base rate of 46.9%. At 9 months the effect was 1.1 percentage points, a relative decrease in mortality of about 2.1%. At 1 year, the survival effects appear to have disappeared. While the estimated reductions in likelihood of death at 3 to 9 months post-diagnosis are seemingly small, these estimates average survival across all patients, whereas the reform increased chemotherapy in less than 2 out of every 100 patients. Of course, the reform also changed the intensity of treatment and the types of drugs used, making it difficult to isolate the specific source of the survival changes.

Section 5. Conclusions

This work is among the first to demonstrate health changes associated with physician-induced demand. Patients diagnosed just after relative to just before a major

Medicare payment change that reduced the profitability of chemotherapy treatment for oncologists, were more likely to receive chemotherapy treatment. Although cancer treatment is constantly evolving, no other change in management was so suddenly implemented that could account for changes of the magnitude we observed.[18] Likewise, there were also no changes in the observed clinical or sociodemographic characteristics of patients diagnosed before and after the policy change that could explain our results. In addition to changes on the extensive margin, the mix of drugs used to treat patients also changed, reflecting the change in relative margins caused by the reform, as did the intensity of treatment (i.e., the number of administrations). The observed changes in treatment were unlikely to have been driven by patient demand as almost all beneficiaries have supplementary insurance that covers their coinsurance and anecdotal evidence suggests that after the reform physicians were less likely to waive the coinsurance for those without supplemental coverage.

Perhaps most surprisingly, those diagnosed just after Medicare switched from an AWP to an ASP-based reimbursement system for chemotherapy had higher rates of survival than those diagnosed just before the payment change. The discrete change in chemotherapy treatment, the mix of drugs and the likelihood of death after January 2005 suggests that the changes were causally related to the reform and, more generally, that physician-induced demand can in some instances improve outcomes.

Why might the switch to ASP-based reimbursement and the associated increase in chemotherapy use have improved survival for lung cancer patients? Other work suggests that despite its clear survival benefits, chemotherapy treatment is underutilized in seniors with advanced non-small cell cancer, who account for the majority of cases, as well as in the adjuvant setting, meaning after the primary tumor has been removed by surgery (Davidoff et al 2010; Booth et al. 2010b). Consequently, one explanation is that the MMA prompted some oncologists, particularly those who may have been underutilizing it, to use more chemotherapy to offset some of the decline in income from

[18] Evidence for adjuvant chemotherapy was emerging in the mid-2000s but the timing of major presentations and publications on this subject are unlikely to cause a sudden change in practice in January 2005 (LeChevalier 2003; Arriagada et al. 2004; Winton et al. 2004; Winton et al. 2005; Stauss et al. 2004; Stauss et al. 2006; Stauss et al. 2008; Douillard et al. 2005; Douillard et al. 2006). Furthermore, evidence on uptake suggests utilization was stable across the policy change (Booth et al. 2010a), and our own data (not shown) show no increase in the use of cisplatin-based regimens, as would be expected if the diffusion of adjuvant chemotherapy were to explain our findings.

lower drug reimbursement, resulting in an improvement in overall survival for patients with lung cancer.

Decreasing reimbursement for chemotherapy may be an instance where better outcomes were attained at lower cost (MedPAC 2006). In this case, oncologists, particularly those who may have been undertreating lung cancer, were induced by the reform to use chemotherapy more. Although increasing intervention may not be desirable in many other situations in oncology or medicine more generally (Fisher et al. 2003a; Fisher et al 2003b), our findings illustrate the power of provider incentives to improve quality of care and patient outcomes. Designing those incentives to take full account of behavioral responses and thus more fully align with desired practice, however, is a difficult task and an important area of ongoing investigation, particularly given ongoing emphasis on reforms to the supply-side of the health care market.

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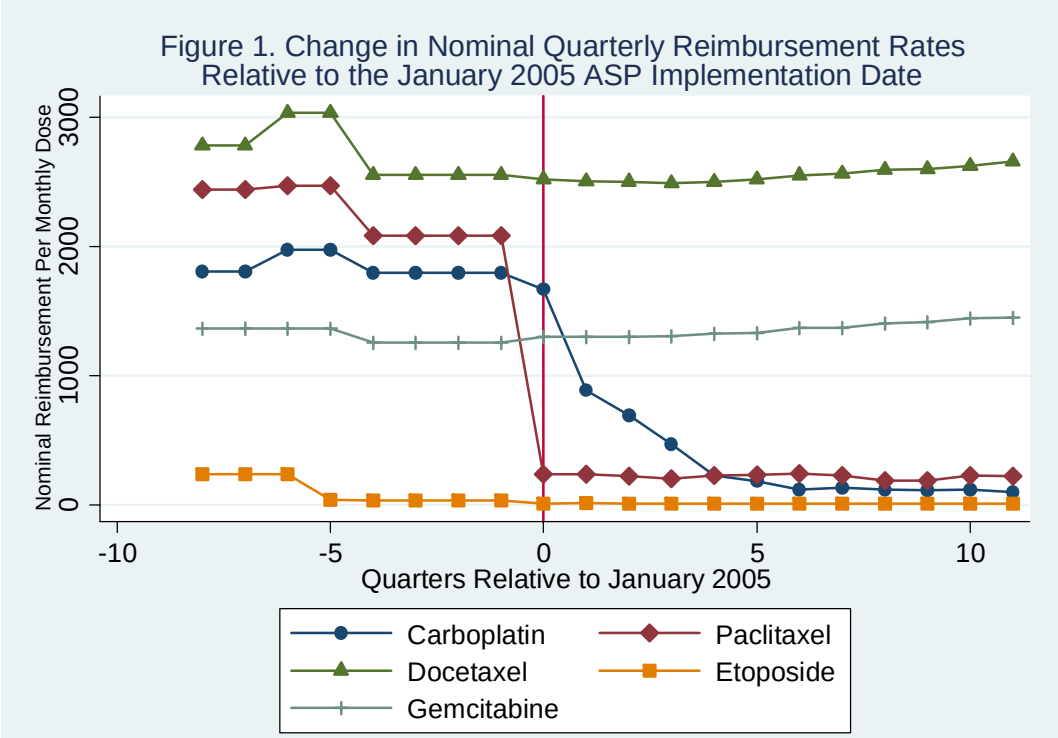
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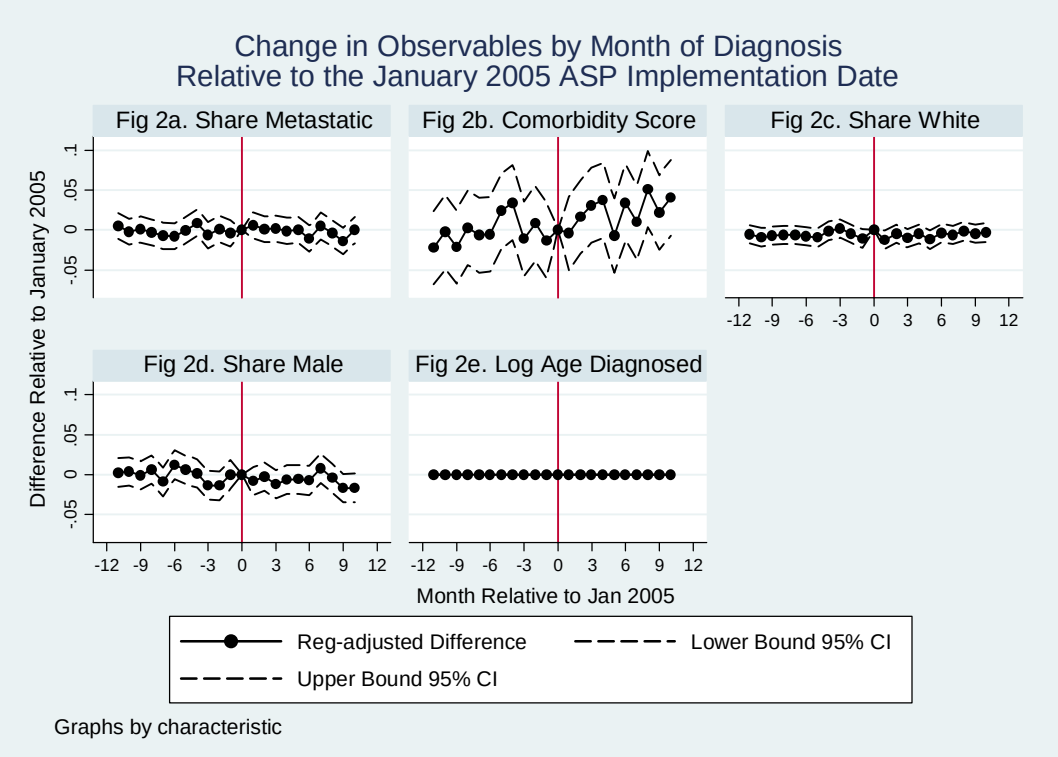
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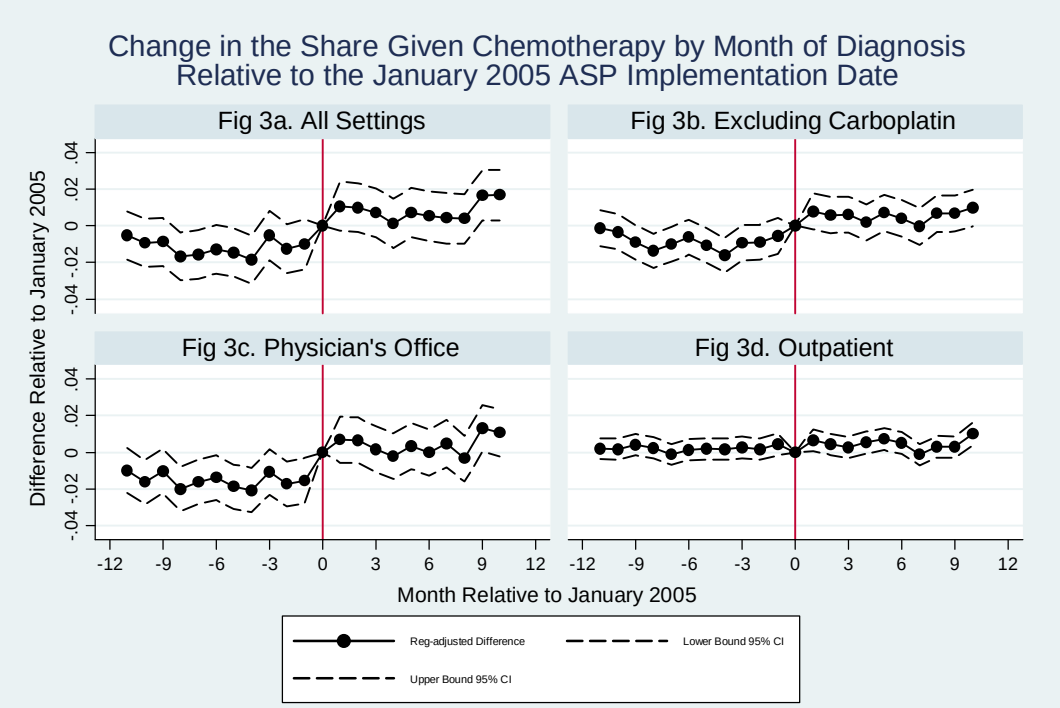
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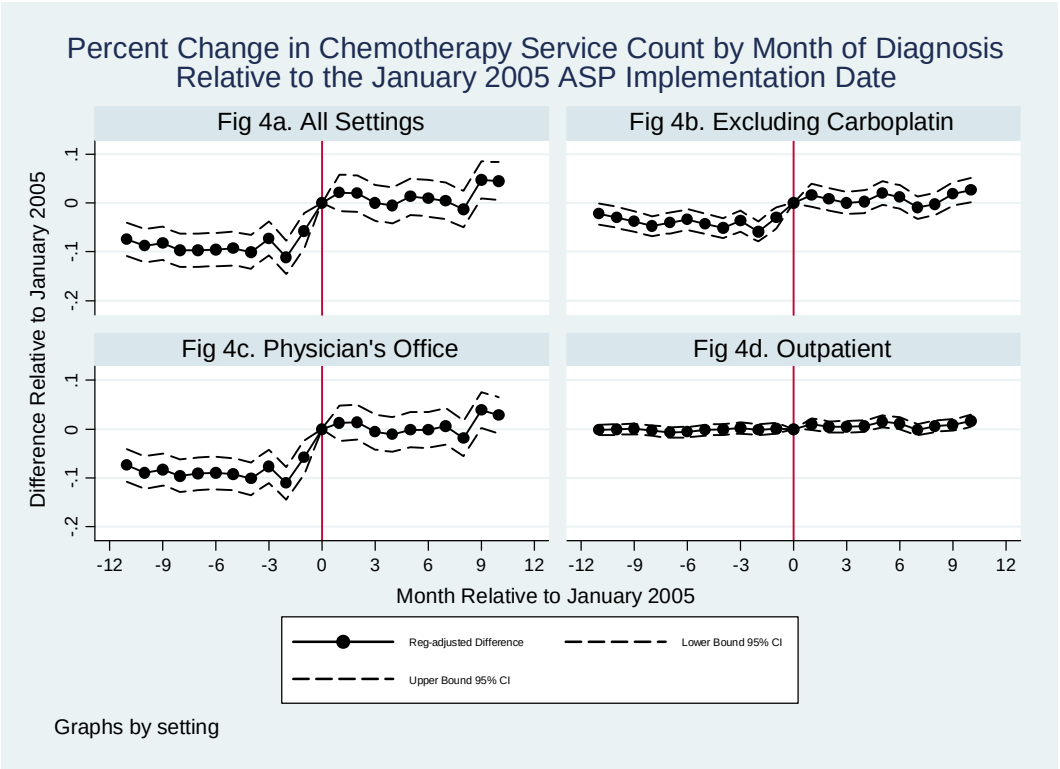
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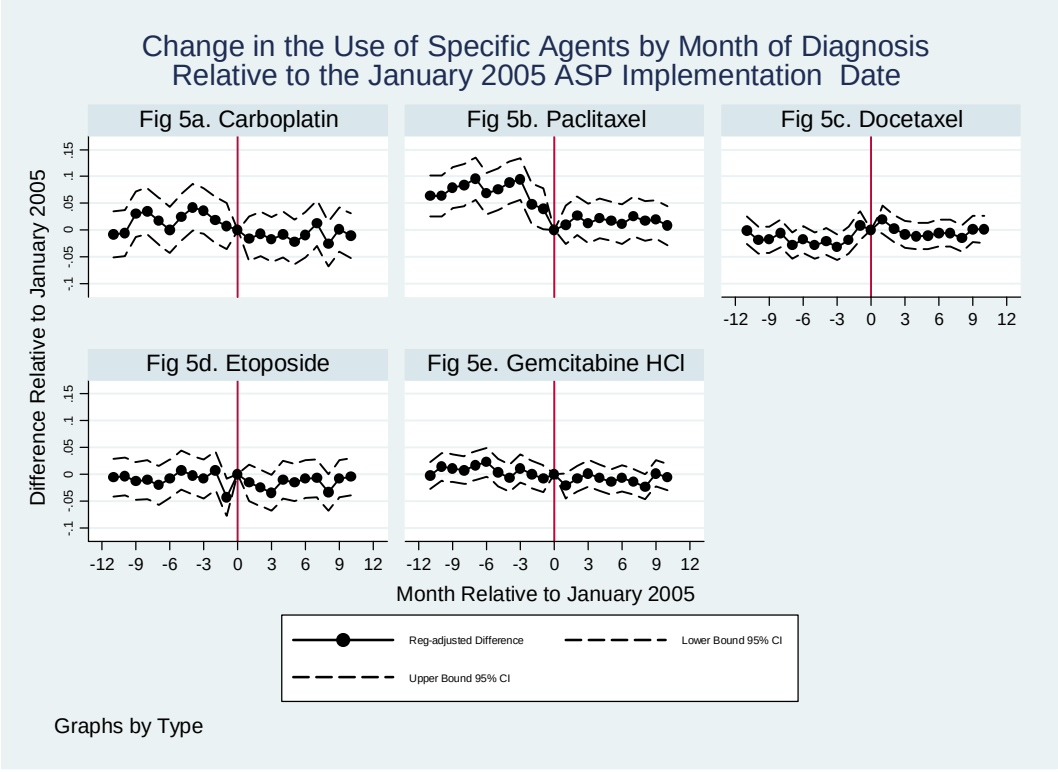
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Figure 1. Change in Nominal Quarterly Reimbursement Rates
Relative to the January 2005 ASP Implementation Date
Nominal Reimbursement Per Monthly Dose
0
1000
2000
3000
-10
-5
0
5
10
Quarters Relative to January 2005
Carboplatin
Paclitaxel
Docetaxel
Etoposide
Gemcitabine


Change in Observables by Month of Diagnosis
Relative to the January 2005 ASP Implementation Date
Fig 2a. Share Metastatic
Fig 2b. Comorbidity Score
Fig 2c. Share White
Fig 2d. Share Male
Fig 2e. Log Age Diagnosed
Difference Relative to January 2005
-.05
0
.05
.1
-12 -9 -6 -3 0 3 6 9 12
Month Relative to Jan 2005
Reg-adjusted Difference
Lower Bound 95% CI
Upper Bound 95% CI
Graphs by characteristic


Change in the Share Given Chemotherapy by Month of Diagnosis
Relative to the January 2005 ASP Implementation Date
Fig 3a. All Settings
Fig 3b. Excluding Carboplatin
Fig 3c. Physician's Office
Fig 3d. Outpatient
Difference Relative to January 2005
-.04 -.02 0 .02 .04
-12 -9 -6 -3 0 3 6 9 12
Month Relative to January 2005
Reg-adjusted Difference
Lower Bound 95% CI
Upper Bound 95% CI
Graphs by setting


Percent Change in Chemotherapy Service Count by Month of Diagnosis
Relative to the January 2005 ASP Implementation Date
Fig 4a. All Settings
Fig 4b. Excluding Carboplatin
Fig 4c. Physician's Office
Fig 4d. Outpatient
Difference Relative to January 2005
-.2 -.1 0 .1
-12 -9 -6 -3 0 3 6 9 12
Month Relative to January 2005
Reg-adjusted Difference
Lower Bound 95% CI
Upper Bound 95% CI
Graphs by setting


Change in the Use of Specific Agents by Month of Diagnosis
Relative to the January 2005 ASP Implementation Date
Fig 5a. Carboplatin
Fig 5b. Paclitaxel
Fig 5c. Docetaxel
Fig 5d. Etoposide
Fig 5e. Gemcitabine HCl
Difference Relative to January 2005
-.1 -.05 0 .05 .1 .15
-12 -9 -6 -3 0 3 6 9 12
Month Relative to January 2005
Reg-adjusted Difference
Lower Bound 95% CI
Upper Bound 95% CI
Graphs by Type

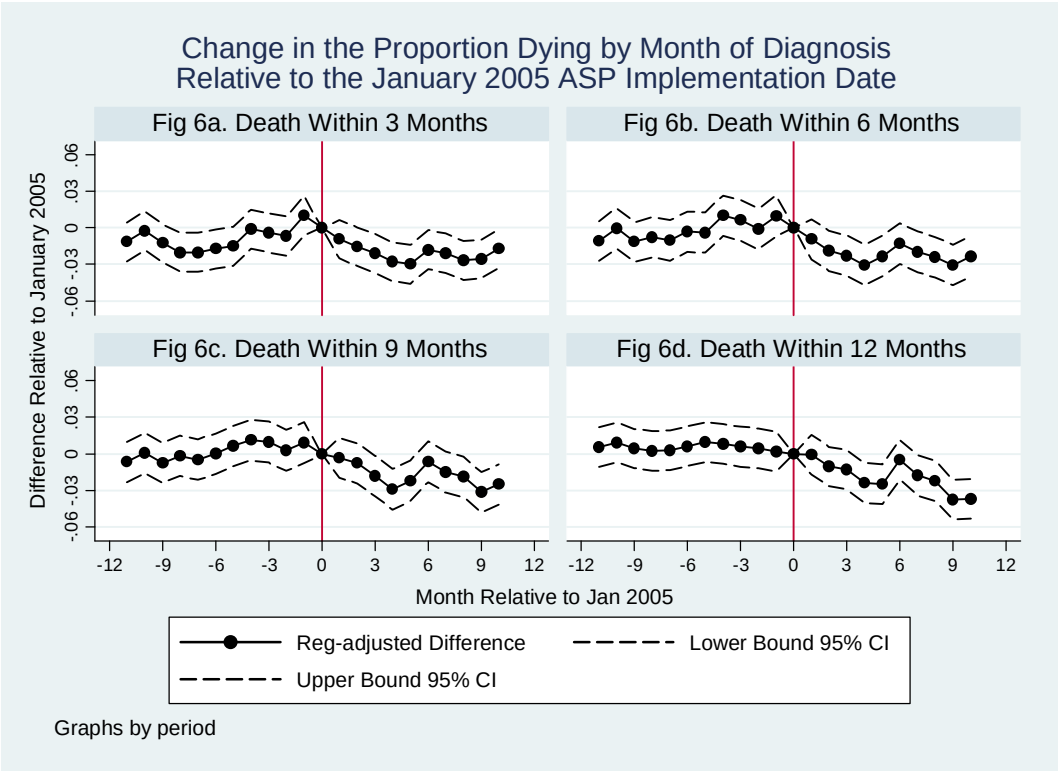

Change in the Proportion Dying by Month of Diagnosis
Relative to the January 2005 ASP Implementation Date
Fig 6a. Death Within 3 Months
Fig 6b. Death Within 6 Months
Fig 6c. Death Within 9 Months
Fig 6d. Death Within 12 Months
Difference Relative to January 2005
-.06 -.03 0 .03 .06
-12 -9 -6 -3 0 3 6 9 12
Month Relative to Jan 2005
Reg-adjusted Difference
Lower Bound 95% CI
Upper Bound 95% CI
Graphs by period

Table 1. Characteristics of the Study Sample Overall and Relative to the January 2005 Payment Change for 11 Months Pre versus Post January 2005*

	Overall N=132,768	Pre N=66,501	Post N=66,267
Age at Diagnosis – mean (sd)	74.0 (8.35)	74.0 (8.32)	74.1 (8.38)
Share Male	0.513	0.516	0.510
Distribution by Race/ethnicity			
White	0.877	0.876	0.877
African-American	0.089	0.090	0.088
Asian	0.0108	0.0105	0.011
Other/Unknown	0.0091	0.0087	0.0095
Hispanic	0.0102	0.0104	0.0099
Native American	0.0039	0.0038	0.0039
Deyo-Charlson Score – mean (sd)	1.07 (1.30)	1.06 (1.30)	1.08 (1.31)
Metastasis within 1 month	0.289	0.289	0.289
Quarterly Reimbursement Rates Per Standard Monthly Dose ($)			
carboplatin	1540 (153)	1845 (29)	930 (261)
paclitaxel	1590 (294)	2272 (70.9)	225 (9.28)
docetaxel	2657 (58.4)	2732 (74.8)	2506 (6.87)
etoposide	77.6 (27.8)	111(36.8)	11.4(0.369)
gemcitabine	1311 (13.5)	1313 (20.6)	1305 (1.71)
Share Receiving Chemotherapy Within …			
1 month	0.175	0.166	0.185
1 month, excluding carboplatin	0.080	0.073	0.087
1 month, physician's office	0.140	0.130	0.149
1 month, outpatient hospital	0.0285	0.0295	0.0275
3 months	0.279	0.263	0.295
3 months, excluding carboplatin	0.103	0.093	0.113
3 months, physician's office	0.228	0.213	0.244
3 months, outpatient hospital	0.067	0.065	0.069
Chemotherapy Drugs Conditional on Treatment within 1 month			
Share carboplatin	0.543	0.557	0.531
Share paclitaxel	0.285	0.315	0.257
Share docetaxel	0.090	0.083	0.096
Share etoposide	0.208	0.211	0.206
Share gemcitabine	0.092	0.100	0.085
Patients Dying Within…			
3 months	0.333	0.337	0.328
6 months	0.458	0.467	0.449
9 months	0.516	0.525	0.507
12 months	0.605	0.616	0.594
No date of death	0.110	0.100	0.120

* Percentages may not sum to one due to rounding. Race/ethnicity categories are provided by the Social Security Administration and the Indian Health Service and are contained in the Medicare Denominator File. Reimbursement rates were downloaded directly from the CMS website.The sample includes Medicare beneficiaries with a confirmed lung cancer diagnosis between February 2004 and November 2005. Associated claims are available for 2002 to 2006. The post-period is January through November 2005. Standard deviations are in parentheses. "Any treatment Excluding carboplatin" means that a patient was treated with chemotherapy and the patient received no carboplatin. The Deyo-Charlson score is a comorbidity index, which we measure in the year prior to a cancer diagnosis.

Table 2. Changes in Chemotherapy Treatment After the January 2005 Payment Change

	Before Jan/05	Feb/04 –Nov/05	Apr/04 –Sept/05
Share of Patients Receiving Chemotherapy Treatment…			
within 1 month of Diagnosis	0.166	0.015	0.014
		(0.003)	(0.002)
within 1 month, excluding carboplatin	0.073	0.014	0.012
		(0.002)	(0.002)
within 1 month, in a Physician's Office	0.130	0.016	0.016
		(0.003)	(0.003)
within 1 month, in an outpatient hospital clinic	0.028	-0.001	-0.001
		(0.001)	(0.001)
Number of Observations (weeks)	48	96	72

Notes: Means in column 1 are for the dependent variable for the period before ASP implementation. All cells in columns 2 - 3 present estimates from separate time-series regressions. Estimates are the coefficients on an indicator for the period that the ASP payment scheme was in effect. Regressions control for mean patient characteristics (see Table 1) and relative-week trends that are allowed to differ on either side of the payment change. Excluding carboplatin" means the share of patients treated with chemotherapy and without carboplatin. Newey-West standard errors allowing for autocorrelation up to 52-week lags are given in parentheses.

Table 3. Percent Changes in Chemotherapy Service After the January 2005 Payment Change

	Before Jan/05	Feb/04 –Nov/05	Apr/04 –Sept/05
Percent Change in Monthly Chemotherapy Service…			
within 1 month of Diagnosis	1.00	0.339	0331
		(0.046)	(0.044)
within 1 month, excluding carboplatin	0.304	0.203	0.209
		(0.025)	(0.022)
within 1 month, in a Physician's Office	0.916	0.347	0.340
		(0.047)	(0.046)
within 1 month, in an outpatient hospital clinic	0.087	0.013	0.013
		(0.003)	(0.004)
Number of Observations (weeks)	48	96	72

Notes: Means in column 1 are for the dependent variable for the period before ASP implementation. All cells in columns 2 - 3 present estimates from separate time-series regressions. Estimates are the coefficients on an indicator for the period that the ASP payment scheme was in effect. Regressions control for mean patient characteristics (see Table 1) and relative-week trends that are allowed to differ on either side of the payment change. Newey-West standard errors allowing for autocorrelation up to 52-week lags are given in parentheses.

Table 4. Changes in Types of Chemotherapy Drugs

	Before Jan/05	Feb/04 –Nov/05	Apr/04 –Sept/05
Share of Patients Receiving…			
carboplatin	0.092	0.001	0.0017
		(0.001)	(0.0017)
paclitaxel	0.053	-0.0053	-0.0051
		(0.0014)	(0.002)
docetaxel	0.014	0.0043	0.0037
		(0.002)	(0.002)
etoposide	0.035	0.0032	0.0044
		(0.001)	(0.001)
gemcitabine	0.017	0.0002	0.001
		(0.0008)	(0.0007)
Share of Chemotherapy-treated Patients Receiving…			
carboplatin	0.557	-0.039	-0.026
		(0.008)	(0.008)
paclitaxel	0.300	-0.074	-0.061
		(0.007)	(0.006)
docetaxel	0.083	0.026	0.026
		(0.004)	(0.006)
etoposide	0.210	-0.005	0.004
		(0.010)	(0.007)
gemcitabine	0.100	0.007	0.008
		(0.003)	(0.004)
Number of Observations (weeks)	48	96	72

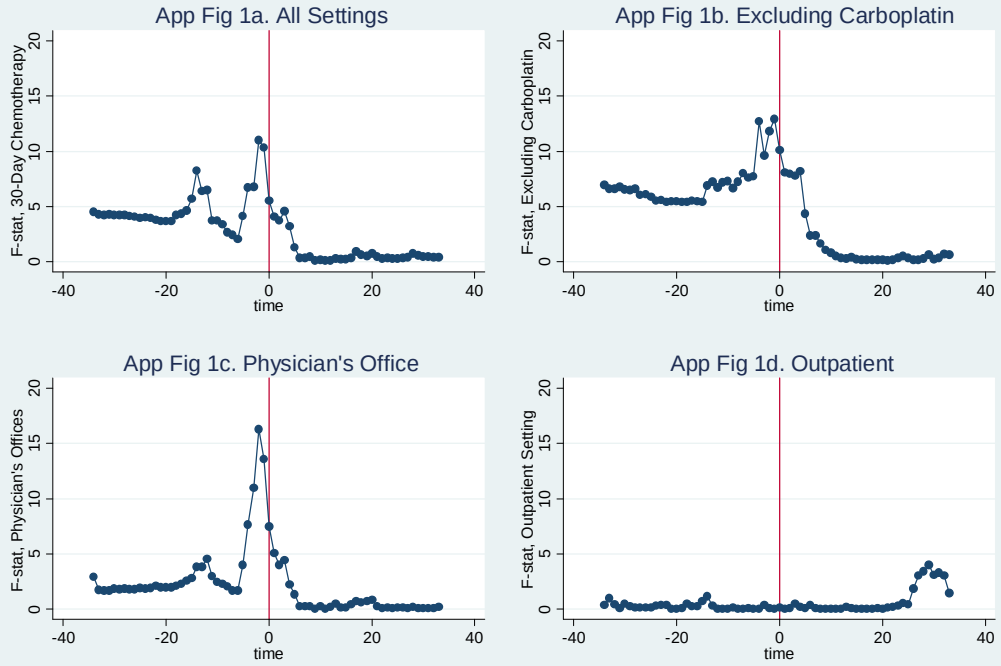
Notes: Means in column 1 are for the dependent variable for the period before ASP implementation. All cells in columns 2 - 3 present estimates from separate time-series regressions. Estimates are the coefficients on an indicator for the period that the ASP payment scheme was in effect. Regressions control for mean patient characteristics (see Table 1) and relative-week trends that are allowed to differ on either side of the payment change. Newey-West standard errors allowing for autocorrelation up to 52-week lags are given in parentheses.

Table 5. Change in the Share of Patients Dying within 3, 6, 9 or 12 Months of Diagnosis

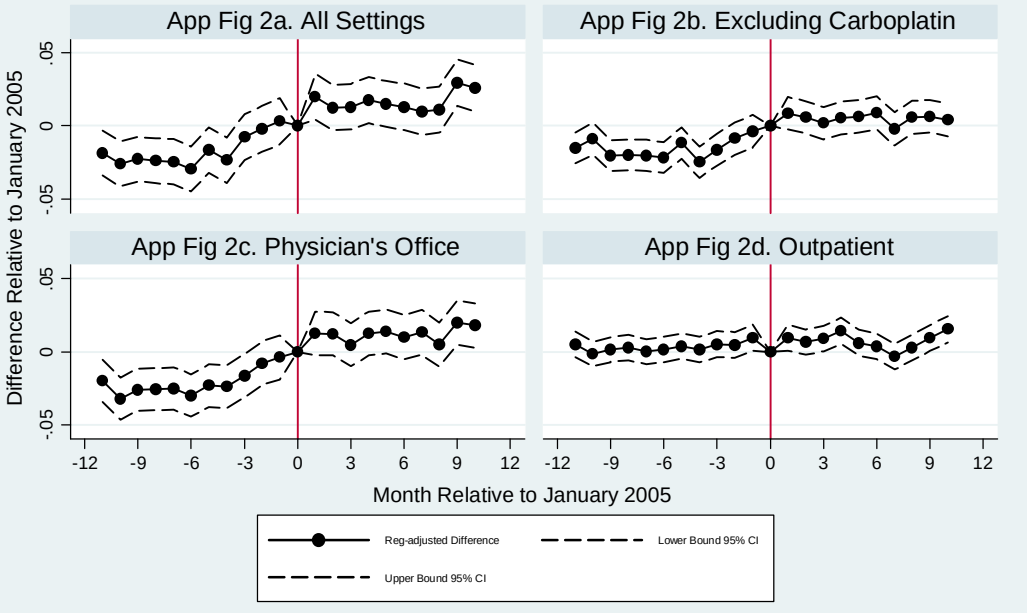
	Mean Before Jan/05	Feb/04 –Nov/05	Apr/04 –Sept/05
Share of Patients Dying within…			
3 months of diagnosis	0.339	-0.009	-0.010
		(0.004)	(0.005)
6 months of diagnosis	0.469	-0.016	-0.016
		(0.002)	(0.004)
9 months of diagnosis	0.527	-0.011	-0.011
		(0.002)	(0.004)
1 year of diagnosis	0. 619	-0.001	-0.001
		(0.003)	(0.003)
Number of Observations (weeks)	48	96	72

Notes: Means in column 1 are for the dependent variable for the period before ASP implementation. All cells in columns 2 - 3 present estimates from separate time-series regressions. Estimates are the coefficients on an indicator for the period that the ASP payment scheme was in effect. Regressions control for mean patient characteristics (see Table 1) and relative-week trends that are allowed to differ on either side of the payment change. Newey-West standard errors allowing for autocorrelation up to 52-week lags are given in parentheses.

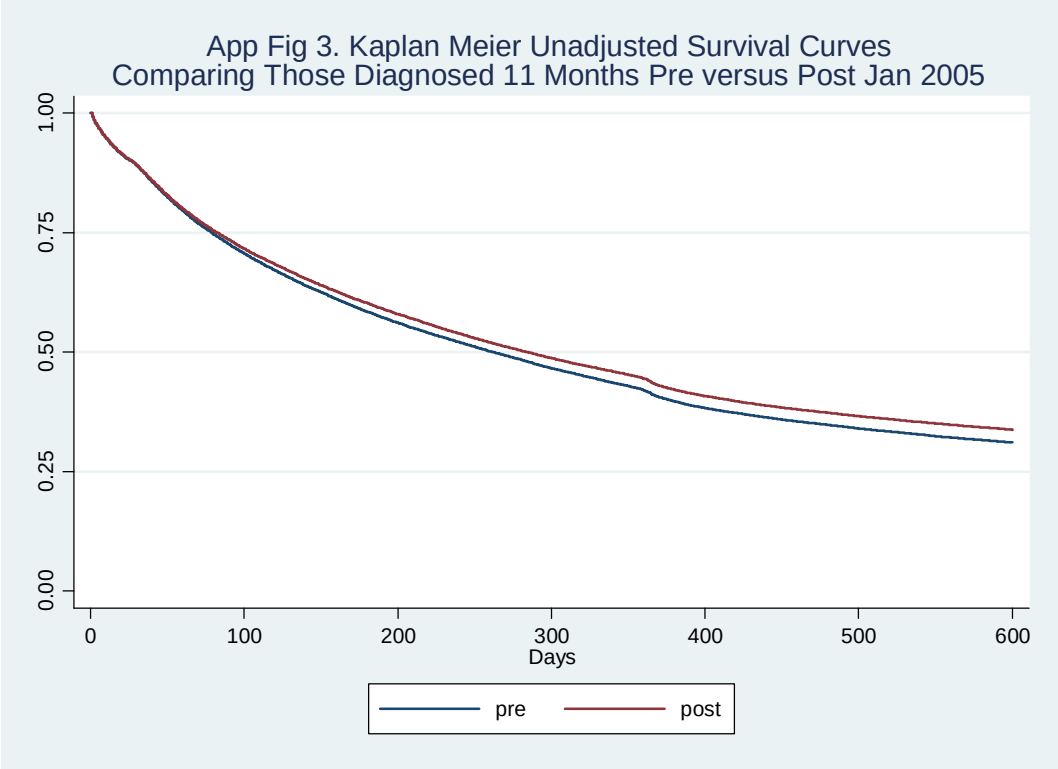
Appendix Figure 1: F-statistics Testing for a Break in Treatment



Change in the Share Given Chemotherapy Within 90 Days By Month of Diagnosis Relative to the January 2005 ASP Implementation Date



Graphs by setting



Note: There is a small discontinuity we cannot explain at about 365 days in both the cohorts diagnosed in the pre and post periods. Since it is of similar magnitude across cohorts, it should not affect our results.

Appendix Table 1. Changes in Observable Characteristics of Patients, Pre and Post Reform

	Mean Before Jan/05	Feb/04 –Nov/05	Apr/04 –Sept/05
Log Age at Diagnosis	74.1	0.0096	0.010
		(0.0008)	(0.001)
Share Male	0.516	$-2.76X10^{-18}$	$-7.78X10^{-18}$
		$(5.47X10^{-17})$	$(3.56X10^{-17})$
Distribution by Race/ethnicity			
White	0.875	-0.0030	-0.0010
		(0.0032)	(0.0024)
African-American	0.091	0.0026	0.0013
		(0.0031)	(0.0023)
Asian	0.0108	0.0010	0.0003
		(0.0062)	(0.0007)
Other/Unknown	0.009	-.0007	-.00082
		(0.0003)	(0.00046)
Hispanic	0.0103	0.0002	0.0008
		(0.0004)	(0.0005)
Native American	0.0038	-0.0002	-0.00082
		(0.0004)	(0.00034)
Metastatic at Diagnosis	0.289	$-9.86X10^{-18}$	$1.12X10^{-18}$
		$(2.09X10^{-17})$	$(3.04X10^{-17})$
Deyo-Charlson Score	1.06	-0.006	-0.016
		(0.010)	(0.013)
Number of Observations (weeks)	48	96	72

Notes: Means in column 1 are for the dependent variable for the period before ASP implementation. All cells in columns 2 - 3 present estimates from separate time-series regressions. Estimates are the coefficients on an indicator for the period that the ASP payment scheme was in effect. Regressions control for other mean patient characteristics (see Table 1), i.e. except the one used as the dependent variable or any race variables when any race category is a dependent variable, and relative-week trends that are allowed to differ on either side of the payment change. Newey-West standard errors allowing for autocorrelation up to 52-week lags are given in parentheses.

Appendix Table 2. Specification Checks of Reform-related changes in Chemotherapy Treatment within 1 months of Diagnosis

	Relative month trends		Calendar month fixed effects	
	Feb/04 –Nov/05	Apr/04 –Sept/05	Feb/04 –Nov/05	Apr/04 –Sept/05
within 1 month of Diagnosis	0.0150 (0.003)	0.0134 (0.002)	0.0175 (0.001)	.0183 (0.002)
excluding carboplatin	0.0140 (0.002)	.0121 (0.002)	.0126 (0.001)	.0126 (0.001)
in a Physician's Office	0.0166 (0.003)	0.0154 (0.002)	0.0181 (0.001)	0.017 (0.002)
in an outpatient hospital clinic	-0.001 (0.001)	-0.001 (0.001)	0.002 (0.001)	0.002 (0.001)
Number of Observations (weeks)	96	72	96	72

Notes: Each cell is based on a separate regression. Estimates are the coefficients on an indicator for the period that the ASP payment scheme was in effect. Regressions control for other mean patient characteristics (see Table 1). Columns (1) and (2) include relative-month trends that are allowed to differ on either side of the payment change and columns (3) and (4) include calendar month fixed effects. Newey-West standard errors allowing for autocorrelation up to 52-week lags are given in parentheses.

Appendix Table 3. Specification Check of Percent Change in Chemotherapy Treatment Service Counts Within 1 Month of Diagnosis After the January 2005 Payment Change

	Mean	Relative month trends		Calendar month fixed effects	
within 1 month	1.00	0.342 (0.047)	0328 (0.046)	0.445 (0.009)	.439 (0.013)
excluding carboplatin	0.304	0.206 (0.024)	0.208 (0.022)	0238 (0.006)	0.214 (0.007)
in a Physician's Office	0.916	0.350 (0.048)	0.338 (0.048)	0.445 (0.009	0.435 (0.014)
in an outpatient hospital clinic	0.087	0.013 (0.003)	0.012 (0.005)	0.036 (0.002)	0.040 (0.002)
Number of Obs (weeks)		96	72	96	72

Notes: Each cell is based on a separate regression. Estimates are the coefficients on an indicator for the period that the ASP payment scheme was in effect. Regressions control for other mean patient characteristics (see Table 1). Means in column (1) are for the dependent variable for the period before ASP implementation. Columns (2) and (3) include relative-month trends that are allowed to differ on either side of the payment change and columns (4) and (5) include calendar month fixed effects. Newey-West standard errors allowing for autocorrelation up to 52-week lags are given in parentheses.

Appendix Table 4. Changes in Chemotherapy Treatment Within 3 Months of Diagnosis After the January 2005 Payment Change: February 2004- November 2005

	Mean	Relative week trends		Relative month trends		Calendar month fixed effects	
within 3 months	0.263	0.007	0.029	0.007	0.029	0.031	.035
		(0.0037)	(0.003)	(0.0040)	(0.003)	(0.001)	(0.001)
excluding carboplatin	0.093	0.015	0.023	0.015	0.023	0.019	0.020
		(0.002)	(0.003)	(0.003)	(0.003)	(0.001)	(0.001)
in a Physician's Office	0.212	0.014	0.031	0.014	0.031	0.031	0.035
		(0.003)	(0.003)	(0.004)	(0.003)	(0.001)	(0.001)
in an outpatient hospital clinic	0.066	-0.007	-0.0004	-0.007	-0.0004	0.004	0.004
		(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Quarter prior to reform indicator		N	Y	N	Y	N	Y
Number of Obs (weeks)		96	96	96	96	96	96

Notes: Means in column 1 are for the dependent variable for the period before ASP implementation. All cells in columns 2 - 7 present estimates from separate time-series regressions. Estimates are the coefficients on an indicator for the period that the ASP payment scheme was in effect. Regressions control for mean patient characteristics (see Table 1). Columns (2) and (3) include relative-week trends and (4) and (5) relative-month trends that are allowed to differ on either side of the payment change. Columns (6) and (7) include calendar month fixed effects. Newey-West standard errors allowing for autocorrelation up to 52-week lags are given in parentheses.

Appendix Table 5. Specification Checks of Reform-related changes in Chemotherapy Types within 1 months of Diagnosis: February 2004- November 2005

	Relative week trends		Relative month trends		Calendar month fixed effects	
Share of Patients Receiving…						
carboplatin	0.001	0.009	0.001	0.009	0.005	0.006
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
paclitaxel	-0.0053	-0.0030	-0.0053	-0.0032	-0.0053	-0.0059
	(0.0014)	(0.0016)	(0.0013)	(0.0016)	(0.0007)	(0.001)
docetaxel	0.0043	0.0077	0.0043	0.0078	0.0047	0.0051
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.0002)
etoposide	0.0032	0.0036	0.0032	0.0042	0.0031	0.0028
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
gemcitabine	0.0002	0.0004	0.0002	0.0001	-0.002	-0.002
	(0.0008)	(0.001)	(0.0009)	(0.002)	(0.0003)	(0.0002)
Share of Chemotherapy Treated Patients Receiving…						

carboplatin	-0.039	-0.047	-0.038	-0.046	-0.019	-0.029
	(0.008)	(0.012)	(0.008)	(0.013)	(0.005)	(0.005)
paclitaxel	-0.074	-0.084	-0.073	-0.083	-0.059	-0.068
	(0.007)	(0.010)	(0.008)	(0.010)	(0.003)	(0.002)
docetaxel	0.026	0.036	0.026	0.036	0.014	0.013
	(0.004)	(0.005)	(0.004)	(0.005)	(0.002)	(0.003)
etoposide	-0.005	-0.025	-0.003	-0.023	-0.004	-0.007
	(0.010)	(0.007)	(0.010)	(0.007)	(0.004)	(0.003)
gemcitabine	0.007	0.007	0.007	0.008	-0.019	-0.014
	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)
Quarter prior to reform indicator	N	Y	N	Y	N	Y
Number of Observations (weeks)	96	96	96	96	96	96

Notes: Each cell is based on a separate regression. Estimates are the coefficients on an indicator for the period that the ASP payment scheme was in effect. Regressions control for other mean patient characteristics (see Table 1). Columns (1) and (2) include relative-week trends and (3) and (4) relative-month trends that are allowed to differ on either side of the payment change. Columns (5) and (6) include calendar month fixed effects. Odd columns also include an indicator for the 3 months leading up to the reform. Newey-West standard errors allowing for autocorrelation up to 52-week lags are given in parentheses.

Appendix Table 6. Specification Checks of Reform-related changes in the Likelihood of Death by Month of Diagnosis

	Relative month trends		Calendar month fixed effects	
Share Dying within…	Feb/04 –Nov/05	Apr/04 –Sept/05	Feb/04 –Nov/05	Apr/04 –Sept/05
3 months of diagnosis	-0.009	-0.010	-0.012	-0.011
	(0.004)	(0.005)	(0.002)	(0.002)
6 months of diagnosis	-0.015	-0.015	-0.020	-0.020
	(0.002)	(0.003)	(0.002)	(0.003)
9 months of diagnosis	-0.011	-0.010	-0.021	-0.022
	(0.00	(0.003)	(0.003)	(0.003)
1 year of diagnosis	-0.0005	-0.0001	-0.026	-0.025
	(0.003)	(0.003)	(0.002)	(0.002)
Number of Observations (weeks)	96	72	96	72

Notes: Each cell is based on a separate regression. Estimates are the coefficients on an indicator for the period that the ASP payment scheme was in effect. Regressions control for other mean patient characteristics (see Table 1). Columns (1) and (2) include relative-month trends that are allowed to differ on either side of the payment change and columns (3) and (4) include calendar month fixed effects. Newey-West standard errors allowing for autocorrelation up to 52-week lags are given in parentheses.