Impact Of Decreasing Copayments On Medication Adherence Within A Disease Management Environment

Value-based cost sharing can increase patients' adherence to important medications.

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ABSTRACT: This paper estimates the effects of a large employer's value-based insurance initiative designed to improve adherence to recommended treatment regimens. The intervention reduced copayments for five chronic medication classes in the context of a disease management (DM) program. Compared to a control employer that used the same DM program, adherence to medications in the value-based intervention increased for four of five medication classes, reducing nonadherence by 7–14 percent. The results demonstrate the potential for copayment reductions for highly valued services to increase medication adherence above the effects of existing DM programs. [*Health Affairs* 27, no. 1 (2008): 103–112; 10.1377/hlthaff.27.1.103]

In 2002 Pitney Bowes reduced copayment rates for several classes of prescription drugs that are important in the treatment of chronic disease. This intervention represents an early example of a Value-Based Insurance Design (VBID) because it connects patients' cost sharing to the value of health care services.[1] This initiative received considerable attention in the employer and policy communities.[2] Although Pitney Bowes reported favorable clinical results and cost

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savings, the analysis was conducted without an external control, and it is unclear whether or not the experience is replicable in other settings.[3]

In this paper we evaluate a similar VBID initiative undertaken by a different employer. In addition to providing insight regarding the generalizability of the Pitney Bowes results, we make two contributions to the literature on the effects of copayments on utilization. First, the body of evidence on the effects of raising copays does not take into account other concurrently implemented interventions that could have either a direct or an indirect effect on medication adherence. For example, many employers and health plans have adopted disease management (DM) programs designed to improve patients' compliance with recommended treatments.[4] The presence of these programs, which are typically unobserved in copay studies, may confound existing studies if adoption of DM is related to copay changes. Relative to other literature that examines copay rate changes, the presence of a common DM program across treatment and control firms in this study allows us to better control the information environment.

We cannot predict how DM will affect the impact of copay changes because DM programs influence which patients are not complying with treatment regimens at baseline and because DM programs change patients' awareness, which could influence their response to copays. Moreover, because we do not observe the prevalence of DM in other studies of copay effects, we cannot ascertain how controlling for DM will influence findings. Nevertheless, given the popularity of these programs and their potential to confound the results from other copay studies, it is important to assess the responsiveness of adherence to copay changes, controlling for the presence of DM programs.

A second contribution of this work is to examine the effects of copayment rates in a setting in which copays are reduced, as opposed to increased. The literature examining the effects of copay changes on utilization is very large and has been summarized elsewhere.[5] Most of the literature either compares adherence across firms with different copay rates or examines the effects of copay increases. However, because of concerns about the adverse clinical effects of high copayment rates, several large employers have reduced these rates for selected high-value services, and there has been limited evaluation of these copay declines.[6]

There are several reasons why we might expect the impact of copay-lowering schemes to differ from copay-raising initiatives. Specifically, with the latter, employees are losing something by being forced to pay more. With the former, they are being given something (lower copays). Although neoclassical economics might suggest similar but opposite effects associated with increases versus decreases in copay rates, considerable research in behavioral economics suggests that the results might not be symmetrical because of employee anchor points and, perhaps, endowment effects.[7]

Study Data And Methods

■ **The intervention.** In January 2005, a large employer reduced copayment rates for five classes of medication: angiotensin-converting enzyme (ACE) inhibitors and angiotensin receptor blockers (ARBs), beta-blockers, diabetes medications (including oral therapies and insulin), HMG-CoA reductase inhibitors (statins), and inhaled corticosteroids (steroids).[8] Copayment rates for generic medications were reduced from $5 to zero. Copays for brand-name drugs were lowered 50 percent (from $25 to $12.50 for preferred drugs and from $45 to $22.50 for nonpreferred drugs).

The intervention was implemented by ActiveHealth Management (AHM), an integrated care management company. The program was added to an already existing accredited DM program used by both the treatment and control firms. The DM program was a comprehensive, telephonic, and nurse-staffed program. This is a fairly typical telephonic DM program, except for its broad scope—covering thirty-two clinical conditions—and its linkage to a system of clinical alerts, in which medical, drug, and lab claims; lab results; and a large electronic database of clinical recommendations from the medical literature are used to identify opportunities to improve clinical care. Although all eligible employees and dependents have access to the program, participation is voluntary. Participation rates in the DM program were similar in both the treatment and control firms, both before and after the intervention. All employees and dependents were covered by the clinical alert system, without an option to "opt out."

When the clinical data provided an indication (or contraindication) for (or against) the use of a specific test or medication, the physician and patient were notified. This "clinical alerting" program was run for the employer's entire insured population several times a month. Physicians were notified of potential clinical improvement opportunities via telephone, fax, or mail as appropriate to the urgency of the clinical alert; members were notified by telephone and mail if enrolled in the DM program or by mail alone if not enrolled. To permit physicians to respond first to any clinical alerts, member notification was lagged by two weeks.

All patients in the treatment firm who were already taking any of the intervention medications without a contraindication were eligible for the copay reduction, beginning with their next prescription fill. Copay relief was also available for those who were not taking the medication if they were identified by the clinical alert system as patients who would benefit from the medication. The list of eligible patients was compiled by AHM and transmitted to the pharmacy benefit manager (PBM), which facilitated the reduced copayments at the point of service. Eligible people received a letter explaining the importance of taking the recommended drug therapy class and an appended intervention letter notifying them of the copay reduction program.

■ **Analytic strategy.** We used a quasi-experimental, pre-post study design with a control group (difference-in-difference design), using data for a year before and a year after the copayment change for the intervention employer and for a second large

employer that used the same DM program but did not adjust copayments.[9]

For each eligible employee and dependent, adherence to the relevant medication for each quarter was ascertained. Thus, our unit of observation was the patient-quarter, yielding a maximum of eight observations per patient over the two-year study period. The results were not sensitive to the different specifications, so we present results using linear regression models, estimated separately by drug class.

■ **Sample.** Employees and dependents ages 18–64 who were continuously enrolled for the relevant quarter and the entire previous quarter were eligible for the study. The control group was determined using these same selection criteria. People age sixty-five or older were excluded because their medical claims data from Medicare were incomplete.

The study was divided into two periods (pre- and postintervention). People were entered into the sample each year using an identical sampling process. Specifically, for each drug class, people were selected for the sample in a given year if they used a medication within three months of the start of the study year and did not have a contraindication to its use, or if they were identified by AHM as having a clinical indication for the medication's use but did not receive it in the previous six months. People could be included in multiple drug class samples and could enter the study at any point during either study year (pre or post), as long as they qualified for a drug class sample as described above.

This approach maintains comparability between the pre and post samples because the exact same rules were used to construct both samples. Because we did not have a full year of data for 2003 (the year before the "pre" year), we did not use the full year of data in 2004 to construct the "post" (2005) sample.

Because of the comparability of sample construction between the pre and post years and between the control and treatment firms, any flaws in sample construction should be controlled for by our difference-in-difference study design, minimizing selection bias. Results are robust to the use of a sample limited to continuously enrolled beneficiaries.[10]

■ **Variables.** *Adherence.* Our measure of adherence is based on the Medication Possession Ratio (MPR), defined as the number of eligible days in the quarter the person was in possession of the medication divided by the number of days in the quarter.[11] If patients were on multiple medications in different categories within the same class (for example, an ACE inhibitor and an ARB, or two different medications to treat hyperglycemia), the patient was assumed to be taking the medications concurrently. We made the conservative assumption that a patient was noncompliant only if he or she had no medication available in the category (neither an ACE inhibitor nor an ARB). We also estimated separate logistic regression models that used MPR 80 percent or MPR Î percent as dependent variables, which are commonly used thresholds to distinguish between adherent and nonadherent groups.[12]

Explanatory variables. The primary explanatory variables were binary variables

denoting the following: employment in the treatment firm, observation post-implementation, and an interaction of these two variables. The estimated effects are based on the coefficient on the interaction term.

We used a range of demographic variables to adjust for population differences, but, given the research design, we would not expect inclusion of these covariates to affect the results. They included age; sex; previous use of the medication (if the drug in the class was filled within six months prior to the first quarter of the year); duration, defined as the number of quarters that the subject was eligible for the study (reset to 1 at the first quarter of the post period); and comorbidities, measured by a series of indicator variables measuring whether the subject had one of several diseases related to the class of medications (as identified in the claims data).[13]

Models that included interactions between duration and the binary variables measuring coverage by the treatment firm and observation in the post year were also estimated to test whether the effects changed over the year. All analyses were adjusted for multiple observations on the same person using generalized estimating equations.

Study Results

■ **Subjects.** There were several statistically significant differences between the employees at the intervention and control firms. Intervention-firm employees were, on average, about six years younger and slightly more likely to be female (Exhibit 1). Moreover, the subjects insured by the intervention firm were more likely to be employees than dependents.

■ **Impact of copayments on medication adherence.** In 2004, before the intervention, both the control and treatment firms had similar copayment rates for brand-name drugs ($29.72 versus $28.55). For generic medications, copayment rates were higher in the control firm than in the treatment firm in 2004 ($16.22 versus $5). Between 2004 and 2005, copays for targeted drugs in the control employer rose about $1 per prescription for brand-name drugs (about 4 percent), while copays

EXHIBIT 1
Demographic Comparison Of Intervention And Control Employers In Study Of A Disease Management Intervention, 2004 And 2005

	Year	No. of members[a]	Age (years)	Percent female	Percent employee	Percent spouse	Percent child
Intervention firm	2004	35,807	37.4	53.5	73.0	21.4	5.6
Control firm	2004	74,345	43.9	51.2	65.6	29.4	5.0
Intervention firm	2005	37,867	38.0	53.5	72.2	21.5	6.3
Control firm	2005	70,259	44.7	51.2	65.7	29.1	5.2

SOURCE: Authors' tabulations of administrative data.

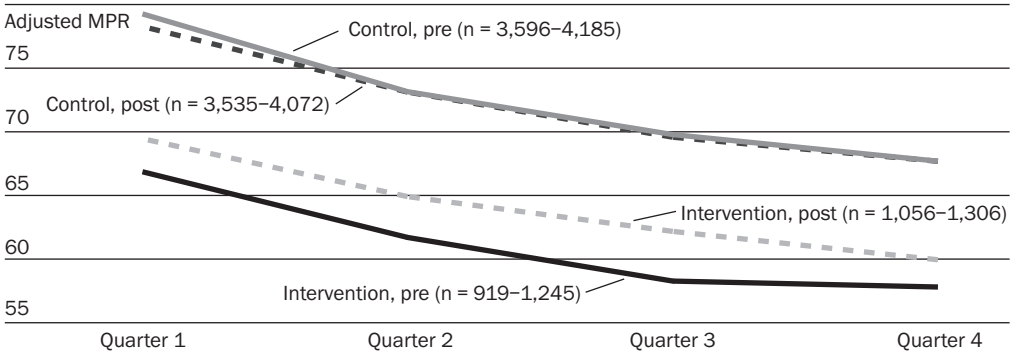
[a] Average per quarter, adjusted for enrollment or disenrollment.

in the intervention firm fell 29.9 percent over this period. This is less than the full 50 percent reduction for several reasons. First, initial prescriptions were filled at the higher copay rate for patients not yet identified as needing the medication. Second, any delay in transferring information to the pharmacy could result in prescriptions' being filled at the higher copay rate. Finally, in cases in which the prescription cost was lower than the copay rate, the reduction might not be 50 percent. The effects for generic drugs were similar in magnitude. In particular, copayments for targeted generic drugs in the control firm dropped about twenty-one cents per prescription (less than 1 percent), while they dropped about 70 percent (more than $3) for the intervention firm. Weighted average copay rates (brand and generic) fell in the intervention firm by 33.9 percent compared to a 2 percent increase in the control firm.

The unadjusted data on adherence for diabetes medications illustrate the effect of the intervention on adherence (Exhibit 2). The declining slope within the year reflects the system of qualifying subjects for the sample, which included everyone taking the medication within three months prior to the beginning of the year. However, because the sample selection criteria were identical for the intervention and control firms, this pattern is common to both firms. Relative to adherence patterns in control firms, there was a clear increase in adherence in the intervention firm. Results were similar for beta-blockers and ACE inhibitors/ARBs (data not shown). The unadjusted data are more difficult to interpret for statins and suggest no effect for inhaled corticosteroids.

The econometric models based on these data support the conclusions from the raw data (Exhibit 3). Specifically, there is a clear positive effect of the intervention on adherence to diabetic agents, beta-blockers, and ACE inhibitors/ARBs. The effect for statins is also positive and statistically significant. Multivariate analyses suggest a small positive result for inhaled corticosteroids, but this is not statistically significant.

EXHIBIT 2
Adjusted Medication Possession Ratio (MPR) For Diabetic Therapy, In The Pre And Post Periods, For Intervention And Control Groups, Calendar Years 2004 And 2005



SOURCE: Authors' multivariate analysis of administrative data.
NOTE: Pre period is calendar year 2004; post period is calendar year 2005.

EXHIBIT 3
Effect Size For Medication Possession Ratio (MPR)

Drug category	Effect size (percent MPR points)	Baseline MPR	Percent increase	Take-up percentage	Elasticity
ACE inhibitors/ARBs	2.59****	68.37	3.79	8.20	−0.118
Beta-blockers	3.02****	68.30	4.43	9.54	−0.112
Diabetes drugs	4.02****	69.46	5.79	13.16	−0.136
Statins	3.39****	52.99	6.28	7.08	−0.182
Steroids	1.86[a]	31.56	5.88	2.71	−0.202

SOURCE: Authors' multivariate analysis of administrative data.

NOTES: Percent increase is the percentage-point increase divided by base adherence. Take-up percentage is the percentage-point increase divided by nonadherence percentage (for example, 1 − base adherence). Elasticity is the percentage increase/percentage change in copays for each drug class. ACE is angiotensin-converting enzyme. ARB is angiotensin-receptor blocker.

[a] $p = 0.134$

****$p < 0.001$

The magnitude of the findings (Exhibit 3) demonstrates an increase in adherence ranging from 1.86 percentage points ($p = 0.134$) for inhaled corticosteroids to approximately four percentage points for diabetes medications ($p < 0.001$). This represents a 7–14 percent reduction in nonadherence for the four classes where a statistically significant effect was found. The implied elasticities for the drug classes that yielded statistically significant results were −0.11 to −0.20. These elasticities are comparable to those reported in the literature, which suggests an elasticity of demand for chronic disease medications ranging from −0.1 to −0.4, with recent studies reporting results in the range of about −0.1 to −0.25.[14]

It is difficult to assess whether the effects of copay reduction changed over time. To examine this issue, we estimated an expanded model, which allowed the effect to change over time by adding a quarter variable that captures the trend over the year, and an interaction between this variable and the postvariable to allow the trend over the year to vary in the post period. This was interacted with a dummy for the treatment firm, thereby allowing the change in trend between the pre and post periods to vary for the treatment and control firms. The models suggest that these adherence effects of the intervention were increasing over time for ACE inhibitors/ARBs ($p < 0.001$) and diabetes medications ($p < 0.10$). The slope result for statins was consistent with this finding but not statistically significant. The analogous results for beta-blockers and steroids were sensitive to the specification of a linear or logarithmic time trend but were never statistically significant. The logistic models estimating adherence and nonadherence, using MPR thresholds of 80 percent and 20 percent, respectively, confirmed our findings of improved adherence as a result of the intervention.

Discussion

Given the widespread use of DM programs, it is important to understand how copayment changes affect adherence within a DM environment. This is the first study on copay changes that holds access to DM constant for both treatment and control firms. Moreover, in contrast to much of the existing literature on copayment effects, our methods control for secular trends and for employer fixed effects. This study is also among the first to address the effects of a value-based copay reduction, such as that implemented by Pitney Bowes, and thus adds to our understanding of the impact of copayment reductions.

We found that reductions in drug copayments increased medication adherence. The magnitude of the adherence-improving effect with copay reduction is similar to those estimated in the existing literature for increases in copayment rates.[15] The similarity between our results and the literature could indicate that DM does not affect price responsiveness much, or it could reflect widespread use of DM in the firms whose data were used in other studies. Our analysis suggests that the adherence effects may increase over time for some clinical areas, but with only one year of postintervention data, this conclusion is tentative.

Consistent with the published literature, we observed differences in effect across medication classes.[16] Most notably, we did not observe a statistically significant effect for inhaled corticosteroids. We believe that this reflects the difficulty in measuring adherence for these medications, since there are multiple doses in a single inhaler as opposed to the other medications that allow individual doses to be counted.

This analysis has several other limitations. Most notably, the control group, although facing similar copayments, had higher adherence throughout the study period. This could be attributable in part to demographic differences, but we believe that those differences were not large enough to explain the difference in baseline adherence. We consider it more likely that the difference reflects differences in the attributes of the physicians or preferences of the two patient groups. To the extent that those differences are time invariant, our analysis controls for them. Although this limitation is important, it is shared by much of the literature in this area. For example, studies that rely only on cross-sectional variation in copayment rates do not control for any employer-specific unobservables that may affect adherence. Moreover, some studies that use longitudinal data do not control for unobserved differences across employers. Others that do control for unobserved traits use employer fixed effects, which is analogous to our approach in that the fixed effects control for time-invariant differences across employers, but not differences in trends across employers.

Our results would be biased if existing trends, as opposed to the intervention, could account for the increase in adherence in the treatment firm. We were not able to recreate the exactly analogous database for the entire year prior to the pre period; however, analysis of available data for the period 2003–04 (prior to the in-

"Reform proposals must include safeguards against unwanted clinical effects resulting from misaligned financial incentives."

tervention) did not reveal any consistent trend in adherence in the treatment firm, which suggests that the bias associated with existing trends was likely small.

Another limitation is that the implementation lag partially dampened the reduction in copays. However, this would tend to bias our findings against an effect on adherence, which suggests that the magnitude of our elasticity estimates could be conservative.

Finally, the full clinical and financial consequences are difficult to assess because health gains and financial offsets associated with better adherence may accrue over time. Because clinical evidence supports adherence to these medications, we expect health improvements, although we do not quantify them in this study. Moreover, although existing reports in the press suggest substantial short-term savings associated with this type of value-based insurance program, we have not assessed the financial effects of this initiative. We expect that there will be some savings in nondrug spending associated with improved adherence, and there might be gains in worker productivity or reduced absenteeism or disability. Although a detailed examination of these issues was beyond the scope of this study, estimates based on crude assumptions about effectiveness of these medications on adverse events suggests that adherence results of the magnitude reported here could generate offsets equal to the costs of the additional prescriptions filled.

AS HEALTH CARE COST PRESSURES MOUNT, the prevailing cost containment approaches increasingly shift costs to patients. The evidence is strong, however, that increased cost sharing leads to decreased adherence to potentially life-saving medications, with likely serious deleterious health effects. These adverse health outcomes can be mitigated if cost-sharing provisions are explicitly designed with value in mind. This analysis demonstrates that such value-based insurance design programs can effectively increase adherence to important medications and complement existing DM programs. As policymakers consider future quality and cost containment initiatives, it is important that health benefit reform proposals include safeguards against unwanted clinical effects resulting from misaligned financial incentives.

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NOTES

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