Price-Linked Subsidies and Health Insurance Markups*

Sonia Jaffe[†] Mark Shepard[‡]

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Abstract

Subsidies in the Affordable Care Act exchanges and other health insurance programs depend on prices set by insurers – as prices rise, so do subsidies. We show that these "price-linked" subsidies incentivize higher prices, with a magnitude that depends on to the growth in insurance demand when the price of the outside option (the mandate penalty) rises. To estimate this effect, we use two natural experiments in the Massachusetts subsidized insurance exchange. In both cases, we find that a $1 increase in the relative monthly mandate penalty increases plan demand by about 1%. Using this estimate, our model implies a sizable distortion of $48 per month (about 12%). This distortion has implications for the tradeoffs between price-linked and exogenous subsidies in many public insurance programs. We discuss an alternate policy that would eliminate the distortion while maintaining many of the benefits of price-linked subsidies.

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†Harvard University, sjaffe@fas.harvard.edu. Jaffe gratefully acknowledges the support of a National Science Foundation Graduate Research Fellowship, as well as the hospitality of the Becker Friedman Institute for Research in Economics at the University of Chicago.

‡Harvard University, mshepard@fas.harvard.edu. Shepard gratefully acknowledges the support of National Institute on Aging Grant No. T32-AG000186 via the National Bureau of Economic Research, and a National Science Foundation Graduate Research Fellowship.

An increasingly important model for public health insurance programs is the coverage of enrollees through organized marketplaces offering a choice among subsidized private plans. Long used in Medicare's private plan option (Medicare Advantage), this model was adopted for the Medicare drug program (Part D) in 2006 and most recently, by the Affordable Care Act (ACA) exchanges in 2014. The goals of this program design are to leverage the benefits of choice and competition, while ensuring affordability through subsidies. In this paper, we argue that the method for setting subsidies can affect the strength of insurer price competition, leading to an important interaction between these two goals.

There are two basic approaches to setting subsidies. First, subsidies may be set "exogenously" – based on factors not controlled by market actors, such as an actuarial estimate of expected cost. While exogenous subsidies create clear-cut incentives, they risk leaving consumers with higher than expected premiums (when prices are higher than expected) or giving them windfalls at government expense (when prices are lower than expected). To remedy this problem, recent reforms (including Medicare Part D and the ACA) follow a second approach: setting subsidies endogenously as a function of insurers' prices. These "price-linked" subsides allow the state to ensure the affordability of insurance in the face of cost uncertainty. For instance, the ACA sets a consumer-specific subsidy so that consumers' post-subsidy premium for the second-cheapest "silver-tier" plan equals a specified "affordable" share of their income. This ensures that at least two silver plans will be affordable, even if prices grow faster than anticipated.

We point out an overlooked disadvantage of price-linked subsidies: they risk distorting firms' pricing incentives in imperfectly competitive markets. The basic intuition for the distortion is simple: if higher prices yield higher subsidies, firms have an incentive to raise prices. However, this intuition is only correct if higher prices increase the *relative* subsidies for a firm's plans. Since in the ACA there is a single flat subsidy,[1] if it applied to all options in the market, then (under standard assumptions) there would be no pricing distortion. However, though the subsidy applies to all plans, it does not apply to the "outside option" of not purchasing insurance. When the subsidy goes up, it decreases the cost of buying a plan relative to not buying insurance. Each firm will gain some of the consumers brought

[1] In other settings, subsidies vary across plans, and a plan's subsidy is directly increasing in its own price. These settings include Medicare Advantage – which decrease subsidies by between 30-50 cents for each $1 of price decrease below a county benchmark – and many employers that subsidize a fixed percent (e.g., 85%) of each plan's premium. In these settings with "marginal" subsidies, the intuition for a price distortion is even clearer than in the case with flat subsidies, which we focus on. Cutler and Reber (1998) show that marginal subsidies can be advantageous to offset mispricing due to adverse selection. In theory, the distortion we study could also mitigate adverse selection between insurance and uninsurance, but increasing the mandate penalty would achieve the same effect without distorting the relative prices of the plans most likely to be subsidy pivotal.

into the market by this price decrease; therefore, each firm has an incentive to raise the price of any plan it thinks might affect the subsidy. This has the potential to increase government subsidy costs, distort consumer choices, and raise prices for higher-income consumers who do not receive subsidies.[2]

We use a simple choice model based on the rules in the ACA exchanges to show this price distortion theoretically and derive sufficient statistics for its magnitude. We focus on the ACA case because of its timeliness and policy relevance during the early years of implementation. (We return to the broader implications for other markets in Section 4.) We show that the pricing incentive distortion depends on the price-responsiveness of consumers' decisions to purchase insurance in the exchange. Specifically, the price distortion for the subsidy-pivotal plan depends on its demand semi-elasticity with respect to the price of the outside option – the fractional increase in demand when the non-purchase price rises by \$1. In the case of the ACA, the main outside option is uninsurance, whose price is the mandate penalty.[3] To estimate the magnitude of the distortion, we need to estimate how much changes in the mandate penalty affect demand for insurance.[4]

To do so, we study the pre-ACA subsidized insurance exchange in Massachusetts, called Commonwealth Care (or CommCare). A key model for the ACA exchanges, CommCare offered subsidized non-group insurance for eligible individuals earning less than 300% of poverty, a similar group as the ACA's subsidized population of 100-400% of poverty.[5] We use

[2] If silver plan prices rise enough, the implied subsidy may exceed the price of some bronze plans, creating a dilemma of whether to allow *negative* consumer premiums or to penalize the cheapest bronze plans by capping their subsidies. The ACA does not allow negative premiums, and the phenomenon of subsidies exceeding the prices of some plans appears to have happened widely in the bidding for 2014 plans (the first year of exchanges). McKinsey Center for U.S. Health System Reform (2013) finds that 6-7 million uninsured Americans will have access to a plan whose post-subsidy premium is \$0. This is a substantial fraction of the projected steady-state enrollment in exchanges of about 25 million (CBO, 2013).

[3] Other sources of insurance are unlikely to be an option for the ACA's subsidy-eligible population. Anyone eligible for "affordable" employer-sponsored insurance (with a premium less than 9.5% of income) is not eligible for exchange subsidies, and a similar provision applied in Massachusetts during our study period. Non-group insurance purchased outside of exchanges is likely to be a dominated choice relative to the heavily subsidized exchange plans.

[4] While this is conceptually related to past work estimating the response of insurance demand to price in settings with a fixed price of outside options (e.g. Gruber and Poterba (1994) on non-group insurance for the self-employed and Gruber and Washington (2005) on employer-sponsored insurance), there are no similar estimates of the response of coverage to the mandate penalty in a low-income exchange setting. The closest related work is that of Chandra et al. (2011), who, like us, study the introduction of the mandate penalty in Massachusetts. However, their focus is on the effect of the mandate penalty on adverse selection, so they do not report estimates of the increase in total coverage.

[5] Prior to 2014, CommCare was separate from Massachusetts' unsubsidized exchange for individuals above 300% poverty, which has been studied by Ericson and Starc (2012). Under the ACA, the two Massachusetts exchange populations merged together, and most individuals below 100% poverty are shifting to Medicaid (with the exception of some legal immigrants remaining in the exchange). Individuals earning less than 400% of poverty receive subsidies, while those above 400% poverty can purchase the same plans without subsidies.

administrative data on enrollment and consumer demographics for all CommCare enrollees from the start of the program in November 2006 until June 2011.

To estimate our key statistic, we use variation from two natural experiments that occurred in 2007-2008. The first experiment is the introduction of the mandate penalty in December 2007 for individuals earning above 150% of poverty. This group shows a clear spike in new enrollments in December and subsequent months, with no concurrent changes for a control group of enrollees below poverty not subject to financial penalties. Using difference-in-differences regression, we find that the number of new enrollees in the cheapest plan exceeded trend by about 23% of its steady-state size. Scaling this by the mandate penalty amounts (which varied by income), these results imply that on average each $1 increase in the monthly penalty increased enrollment by 0.97%.

The second experiment uses a policy change in July 2007 that decreased all plans' premiums for consumers 100-150% poverty. Because a lower price of all inside options has an equal effect on *relative* prices as a higher price of the outside option, this experiment can be used to estimate the response of insurance demand to the relative mandate penalty. We again find a visible spike in new enrollment for the 100-150% poverty group in absolute terms and relative to a control group of higher income enrollees whose prices were essentially unchanged. Our preferred estimates imply that new enrollment in the cheapest plan grew by 17% of steady-state size. Given the price changes, these estimates imply a 0.94% increase in enrollment for each $1 increase in the relative mandate penalty. The similarity of results across these two quite different experiments is striking and reassuring.

To estimate the distortion, our model also requires an estimate of the own-price semi-elasticity of demand. For this, we use the estimates of Chan and Gruber (2010), who studied the same market, time period (2007-2008), and income groups (100-300% poverty). Their coefficient implies that each $1 premium increase reduces demand for the cheapest plan among new enrollees by 1.97%. Plugging their and our estimates into the model, we estimate a upward distortion due to price-linked subsidies of $48 per member per month. This amount is substantial – about 12% of the average CommCare insurance price at the time. Since this distortion applies to the subsidy-pivotal cheapest plan, it implies an equal distortion of the subsidy amount.

Though the ACA exchanges differ from Massachusetts, we expect the incentive distortion will continue to be important. In particular, our theory predicts the distortion will be larger in more concentrated markets. Data for 2014 suggest that many ACA markets will be highly concentrated, with over half of (county-level) markets having just one or two participating insurers (Abelson et al., 2013). However, the distortion may be mitigated by the presence of unsubsidized consumers (projected to be about 20% of the market).

The policy implications of our results for the ACA exchanges depend on balancing several considerations. One option would be to shift to exogenous subsidies, which do not distort prices. But exogenous subsidies cannot guarantee the availability of "affordable" post-subsidy plan premiums if prices grow without an accompanying increase in subsidies. Adjusting subsidies based on exogenous factors likely to correlate with local plan prices – e.g., local cost growth in fee-for-service Medicare[6] – could mitigate but would not eliminate the problem. Furthermore, the experience with persistently high payments through exogenous subsidies in Medicare Advantage (see MedPAC, 2013) indicate potential political problems with exogenous subsidies.

Our model suggests a simple alternative to remove the distortion while preserving the affordability advantages of price-linked subsidies: apply the subsidy to the outside option as well. While normally, the cost of not purchasing a good is fixed at zero, the ACA's mandate penalty for uninsurance makes such a subsidy for the outside option possible. Specifically, if the second-cheapest silver price exceeds an expected target level, the difference would be applied to *reduce* the mandate penalty (and vice versa if its price is below the target). Under this system, subsidies would still ensure the "affordability" of at least two silver plans. But a higher subsidy would not affect the relative prices of insurance vs. non-insurance, removing the distortionary incentive.

Another way to see this is to consider the *net* subsidy for insurance vs. non-insurance, which equals the subsidy plus the mandate penalty, $S + M$. With exogenous subsidies, $S + M$ is exogenous to plan prices, so there is no distortion. Under the ACA's price-linked subsidies, higher prices increase S but do not affect M, implying a positive effect of prices on $S + M$. Under our alternate proposal, higher prices increase S and decrease M, leaving $S + M$ unchanged. As with exogenous subsidies, there is no distortion because plan prices do not affect the net subsidy for insurance. However, a potential downside is that unexpectedly high prices could (by reducing the mandate penalty) increase uninsurance, an outcome the current policy forestalls. We discuss this issue further in Section 4.

The distortion we identify is relevant to the choice between exogenous and price-linked subsidies in a variety of settings, including the ACA exchanges, Medicare Advantage and Part D, and employer-sponsored plan choices like the Federal Employees Health Benefits Program. Most of these programs set subsidies using plan prices, either based on a pivotal plan (as in the ACA) or based on a weighted average of plan prices (as in Medicare Part D for non-low-income enrollees). Although estimating the distortion in these other markets is beyond the scope of this paper, our theory suggests that it will be relevant wherever firms

[6]While local premiums in employer-sponsored insurance seem appealing as an adjustment factor, they would not be truly exogenous if the same insurers offered both exchange and employer plans.

have market power and there is meaningful substitution between the in-market plans and the outside option. We discuss the implications for other markets in Section 4.

We are not aware of past research that has analyzed the distortion we discuss. The closest related work is Decarolis (2013), which highlights a different pricing distortion in Medicare Part D. By increasing its plan prices for higher-income enrollees, insurers can increase their payments for low-income subsidy recipients, who do not pay prices. However the structure in Part D is different, creating different and often more subtle incentives to game the systems. Also, the subsidized consumer share of the Part D market (about 40%) is substantially smaller than their share in the ACA exchanges (about 80%).

The paper proceeds as follows. Section 1 sets up a standard choice model and derives the formula for the change in markup due to the endogeneity of the subsidy, first for single-plan insurers (as in Massachusetts) and then for multi-plan insurers (as in the ACA). Section 2 uses two natural experiments from Massachusetts to calibrate the relevant semi-elasticity of demand with respect to the mandate penalty. Section 3 combines this estimate with the estimate from Chan and Gruber (2010) of the sensitivity of demand to own price to get a quantitative estimate for the distortion in the markup due to endogenous subsidies. Section 4 proposes an alternative subsidy framework that would eliminate the distortion while maintaining price-linked subsidies and compares it to the existing systems of exogenous and endogenous subsidies. It also discusses the broader relevance of our findings to markets besides the ACA. Section 5 concludes.

1 Theory

We analyze the competitive effects of linking subsidies to plan prices in a simple model of consumer choice and insurer bidding. Insurers offer differentiated products and compete by setting prices. The exchange collects price bids and uses them to determine subsidies based on a formula that insurers know before bidding. Subsidy-eligible consumers then choose which (if any) plan to purchase based on post-subsidy prices and plan attributes. If they choose not to purchase a plan, they are subject to the legally applicable mandate penalty. We assume that insurers set prices simultaneously to maximize static profits, knowing the effects of these choices on demand and cost.[7] As in most recent work on insurance (e.g. Einav et al. (2013)), we assume that plan attributes are held fixed, focusing instead on the effect of subsidies on pricing conditional on plan attributes.[8]

[7]We therefore abstract from pricing dynamics or incomplete information. As noted below, uncertainty would spread the distortion out across multiple plans, but would not eliminate it.

[8]This assumption is less problematic in the insurance industry, where many attributes are severely constrained by regulation. For instance, in the Massachusetts exchange we study empirically, the regulator

We use the necessary first-order conditions for Nash equilibrium to derive sufficient statistics that capture the effect of subsidy rules on insurers' optimal prices. We do so in two institutional settings: a simpler one with single-plan insurers (based on the situation in the Massachusetts exchange before 2014; for additional background, see Section 2) and a more complicated setting with multi-plan insurers (based on the ACA exchanges). The first illustrates the intuition more clearly and generates the formula for our analysis of Massachusetts data, but the second is more relevant for future policy. Our analysis suggests reasons that the distortion may be *larger* in the ACA case.

1.1 Theory with Single-Plan Insurers (Massachusetts Case)

We start with a model where each insurer offers a single plan. There are J insurers, each of which chooses a "price bid" P_j, which is submitted to the exchange regulator and is the total amount the insurer receives per enrollee. Using a pre-determined rule that may incorporate the vector of prices P, the regulator sets a subsidy $S(P)$. This subsidy applies equally to all plans, so consumers face subsidized premiums denoted $P_j^{cons} = P_j - S(P)$. Consumers can also choose the outside option of not buying a plan, in which case they face a mandate penalty, M. Consumer demand for plan j, $Q_j(P^{cons}, M)$, is a function of all premiums and the mandate penalty. As in most discrete choice models, we assume that utility is (locally) quasi-linear in price, so consumers only care about prices *relative* to other prices and to mandate penalty. We assume constant marginal cost and we abstract from adverse selection by assuming ideal risk adjustment; we can therefore specify a constant per-enrollee marginal cost c_j for insurer j.[9]

Given this setup, the insurer profit function is:

$$\pi_j = (P_j - c_j) \cdot Q_j(P^{cons}, M).$$

In simultaneous-pricing Nash equilibrium, all insurers set prices to maximize profits, given their opponents' strategies. Thus, the necessary conditions for Nash equilibrium are that

specifies the services insurers must cover and all of the associated co-payments.

[9]Risk adjustment works by adjusting the payment insurers receive to be higher than P_j for sick, high-cost enrollees and lower for healthy enrollees. In ideal risk adjustment, the quantity $(P_{ij} - c_{ij})$ is constant across enrollees, allowing us to write it in the standard form $(P_j - c_j)$. Both the Massachusetts and ACA exchanges include sophisticated (though likely still imperfect) risk adjustment. Nonetheless, the effects we identify all carry over to a model with adverse selection, with more complicated formulas for the sufficient statistics.

each firm price according to its first-order conditions for profit maximization:[10]

$$\frac{d\pi_j}{dP_j} = Q_j\left(P^{cons}, M\right) + \left(P_j - c_j\right) \cdot \frac{dQ_j}{dP_j} = 0. \tag{1}$$

This differs from standard oligopoly pricing conditions in two respects. Because of the subsidies, the firm's price P_j enters consumer demand indirectly, through the subsidized premiums, P^{cons}. Also, the term dQ_j/dP_j is not the slope of the demand curve, but a composite term that combines the slope of demand and the effect on demand via the effect of the price on the subsidy and mandate penalty. The total effect of the premium on demand is:

$$\frac{dQ_j}{dP_j} = \frac{\partial Q_j}{\partial P_j^{cons}} - \left(\sum_k \frac{\partial Q_j}{\partial P_k^{cons}}\right)\frac{\partial S}{\partial P_j} + \frac{\partial Q_j}{\partial M}\frac{\partial M}{\partial P_j}, \tag{2}$$

which depends on the specific policy for determining subsidies and the mandate penalty.

Exogenous Subsidies

Exchanges could set flat, *exogenous* (i.e., based on factors not controlled by market actors) subsidies and mandate penalties.

$$\begin{aligned} \text{Exogenous Subsidy: } & P_j^{cons} = P_j - S, \\ \text{with } & \frac{\partial S}{\partial P_j} = \frac{\partial M}{\partial P_j} = 0 \qquad \forall j. \end{aligned} \tag{3}$$

As a result of subsidies and the mandate penalty being unaffected by any plan's price, dQ_j/dP_j in (2) simplifies to the demand slope $\partial Q_j/\partial P_j^{cons}$. Even though there are subsidies, the equilibrium pricing conditions are not altered relative to the standard form for differentiated product Bertrand competition. Under this exogenous subsidies benchmark, firms set markups as:

$$Mkup_j^{\text{Ex}} \equiv P_j - c_j = \frac{1}{\eta_j} \qquad \forall j,$$

where $\eta_j \equiv -\frac{1}{Q_j}\frac{\partial Q_j}{\partial P_j^{cons}}$ is the own-price semi-elasticity of demand.

[10]These first-order conditions would be necessary conditions for Nash equilibrium even in a more complicated model in which insurers simultaneously chose a set of non-price characteristics like copays and provider network. Thus, the theoretical point we make about price-linked subsidies holds when quality is endogenous, though there may also be effects on quality and cost levels, which we don't capture.

Price-Linked Subsidies

Alternatively, exchanges could *link subsidies to prices* (but again set M exogenously). This is the approach taken in Massachusetts and in ACA exchanges. A key advantage of this approach is that a state can ensure affordability to consumers even if prices are higher than expected and avoid windfalls to consumers when prices are lower than expected. This transfer of health care cost risk from individuals to the state is a key motivation for price-linked subsidies.

However, linking subsidies to prices distorts insurers' pricing incentives. To see this distortion, consider a subsidy rule (similar to that used in Massachusetts) that sets $S(P)$ so that the post-subsidy premium for the cheapest plan equals a pre-determined "affordable amount" (based on income). With this policy, the subsidy rises with the price of the cheapest plan, but the mandate penalty is still exogenous to plan prices. Formally, the subsidy for a given income group would be set so that:

$$\begin{aligned}\text{Subsidy Linked to Min Price: } S\left(P\right) &= \min_j P_j - \text{AffAmt} \\ \Longrightarrow \frac{\partial S\left(P\right)}{\partial P_{\underline{j}}} &= 1, \qquad \frac{\partial M}{\partial P_j} = 0 \;\forall j,\end{aligned} \tag{4}$$

where $\underline{j}$ is the index of the cheapest plan. For example, in 2007, the affordable amount for a consumer earning between 150-200% of poverty was \$40 per month, and the cheapest plan in the "Outside of Boston" region bid a price of \$295.84 per month. As a result, the subsidy for this income group was set at \$255.84.[11] Equation (4) shows that the subsidy rises one-for-one with this cheapest plan's price. Had that plan bid \$1 higher, the subsidy would increased by \$1 so that the consumer premium was still \$40.

This price-subsidy link changes optimal pricing for an insurer that believes it will have the cheapest plan. Raising this plan's price by \$1 does not affect its own consumer premium (which is offset by the higher subsidy) but lowers the premium of all other plans by \$1. Initially, this may sound the same as the standard case – raising own price by \$1 affects demand identically to lowering all other options' prices by \$1. But critically, the price increase for the cheapest plan *does not lower the price of being uninsured*, i.e. the mandate penalty. A price increase for the cheapest plan does not increase its price relative to being uninsured. This creates an extra incentive for the cheapest plan to markup its price, which in turn increases subsidy costs to the state.

[11] In later years in the Massachusetts exchange, there were "incremental" subsidies to pricing higher, so more expensive plans received larger subsidies. We do not model this feature of the subsidy scheme, which will not be replicated in the ACA and was only present for enrollees earning less than 150% poverty during the 2007-08 period we study.

To see the distortion analytically, consider how the endogenous subsidy affects the total derivative of demand with respect to price for the cheapest plan:

$$\frac{dQ_{\underline{j}}}{dP_{\underline{j}}} = \frac{\partial Q_{\underline{j}}}{\partial P^{cons}_{\underline{j}}} - \left(\sum_k \frac{\partial Q_{\underline{j}}}{\partial P^{cons}_k}\right) \cdot 1 + \frac{\partial Q_{\underline{j}}}{\partial M} \cdot 0 = \sum_{k \neq \underline{j}} \left(-\frac{\partial Q_{\underline{j}}}{\partial P^{cons}_k}\right).$$

Since the affordable amount is fixed, raising $P_{\underline{j}}$ is equivalent to lowering consumer premiums for all other plans. Next, we use the fact that with utility linear in price, raising the prices of *all options* (including uninsurance) equally does not affect demand, that is $\sum_k \frac{\partial Q_j}{\partial P^{cons}_k} + \frac{\partial Q_j}{\partial M} = 0 \ \forall j$. Combining these two equations implies that

$$\frac{dQ_{\underline{j}}}{dP_{\underline{j}}} = \frac{\partial Q_{\underline{j}}}{\partial P^{cons}_{\underline{j}}} + \frac{\partial Q_{\underline{j}}}{\partial M}. \tag{5}$$

However, if the firm raises its price so much that it is no longer the cheapest plan, then it is no longer pivotal for the subsidy, and $\frac{dQ_{\underline{j}}}{dP_{\underline{j}}} = \frac{\partial Q_{\underline{j}}}{\partial P^{cons}_{\underline{j}}}$.

Plugging Equation (5) into Equation (1) and rearranging yields the following markup condition for the cheapest plan when subsidies are endogenous

$$Mkup^{\text{End}}_{\underline{j}} \equiv P_{\underline{j}} - c_{\underline{j}} = \frac{1}{\eta_{\underline{j}} - \eta_{\underline{j},M}},$$

where $\eta_{\underline{j},M} = \frac{1}{Q_{\underline{j}}}\frac{\partial Q_{\underline{j}}}{\partial M}$ is the semi-elasticity of demand with respect to the mandate penalty. Price-linked subsidies lower the effective price sensitivity faced by the cheapest plan, leading to a higher equilibrium markup than under exogenous subsidies. In our model without uncertainty, this distortion only applies to the cheapest plan, though there may be strategic responses by other firms.[12] If the distortion is large enough that the cheapest plan would want to price above the second cheapest plan, it instead sets a price equal to the second cheapest plan.

If the semi-elasticities of demand are constant across the relevant range of prices (own-cost pass-through equals 1), the increase in markup for the cheapest plan is:

$$Mkup^{\text{End}}_{\underline{j}} - Mkup^{\text{Ex}}_{\underline{j}} = \frac{\eta_{\underline{j},M}}{\eta_{\underline{j}}\left(\eta_{\underline{j}} - \eta_{\underline{j},M}\right)}, \tag{6}$$

[12] Like much of the related literature, we make the simplifying assumption that firms know what equilibrium they are in, so there is no uncertainty about which plan will be cheapest. In a more realistic model with uncertainty about others' prices (perhaps due to uncertainty about others' costs) then the distortionary term $\eta_{\underline{j},M}$ would be weighted by the probability of being the lowest price plan. The (ex-post) cheapest plan would have a smaller distortion, but there would also be distortions to other plans' prices. Strategic complementarity in prices could further exacerbate these.

though the distortion cannot cause the cheapest plan to raise its price above that of the second cheapest plan. Constant semi-elasticities may be a good approximation in some markets; alternatively, the estimated increase in markups can be thought of as an estimate of how much marginal costs would have had to decrease to offset the incentive distortion generated by endogenous subsidies.[13]

The distortion will tend to be larger when markets are less competitive. When there are fewer firms, on average we expect $\eta_{\underline{j}}$ to be smaller because consumers have fewer other options and $\eta_{\underline{j},M}$ to be higher because a given firm will get a larger share of however many consumers enter the market when the penalty goes up. Both of these effects increase the distortion. Also, with fewer firms, it is less likely that the second cheapest plan will be close in price to the cheapest plan and thereby act as an effective cap on the distortion.

This pricing distortion can have an important effect on social welfare. While the price distortion is on the cheapest plan, it also drives up the subsidy, which raises the government's costs for all enrolled individuals, not just those that chose the cheapest plan. With a marginal cost of government funds above one, this transfer from the state to insurers is socially costly.[14]

1.2 Theory with Multi-Plan Insurers (ACA Case)

The distortion described above is exacerbated when firms offer multiple plans in a market. In the ACA insurers must offer a plan in each of multiple tiers – bronze, silver, gold, and platinum.[15] Subsidies are set equal to the second-cheapest silver plan minus a pre-specified "affordable" share of a consumer's income.[16] The fact that insurers are providing additional plans provides an even greater incentive for an insurer to increase the price of its silver plan because the higher subsidy increases demand for the insurer's non-silver plans as well – again by inducing more customers to enter the market.

[13] This is similar to the idea from Werden (1996) that, without assumptions about elasticities away from the equilibrium, one can calculate the marginal cost efficiencies needed to offset the price-increase incentives of a merger.

[14] In addition, the price distortion affects relative premiums and changes the allocation of consumers across plans. But because relative prices may not equal relative marginal costs with imperfect competition (they differ by the difference in markups), it is not clear whether this has a positive or negative effect.

[15] Platinum plans cover 90% of medical costs (comparable to a generous employer plan today); gold covers 80% of costs; silver covers 70% of costs; and bronze covers 60% of costs. Consumers with incomes below 250% of poverty also receive so called "cost-sharing subsidies" that raise the generosity of silver plans.

[16] This subsidy is applied equally to all plans (with a cap ensuring that no premium is pushed below $0), ensuring that at least two silver plans (and likely some bronze plans) cost less than the affordable amount for low-income consumers.

Suppose each firm j offers plans in tiers $l = 1, ..., L$. The insurer maximizes profits:

$$\max_{P_{j1} \dots P_{jL}} \sum_l (P_{jl} - c_{jl}) \, Q_{jl} \left(P^{cons}, M \right),$$

where $P^{cons}_{jl} = P_{jl} - S(P)$ with $S(P) = P_{2nd,Silv} - \text{AffAmt}$. Following the same steps as above, the first-order condition for the silver plan is:

$$\frac{\partial \pi_j}{\partial P_{j\text{silv}}} = Q_{j\text{silv}}(\cdot) + \sum_l (P_{jl} - c_{jl}) \frac{dQ_{jl}}{dP_{j\text{silv}}} = 0.$$

The markup with exogenous subsidies is:

$$Mkup^{\text{Ex}}_{j\text{silv}} = \frac{1}{\eta_{j\text{silv}}} + \frac{1}{-\frac{\partial Q_{j\text{silv}}}{\partial P^{cons}_{j\text{silv}}}} \sum_{l \neq silv} (P_{jl} - c_{jl}) \frac{\partial Q_{jl}}{\partial P^{cons}_{j\text{silv}}}$$

The second term reflects the fact that the insurer captures revenue from consumers who switch to its other plans when it raises the price of its silver plan. This is a standard effect in settings with multi-product firms. However, with the subsidy set based on the second cheapest silver plan, the markup for that subsidy-pivotal plan is:

$$Mkup^{\text{End}}_{j\text{silv}} = \frac{1}{\eta_{j\text{silv}} - \eta_{j\text{silv},M}} + \frac{\sum_{l \neq Silv} (P_{jl} - c_{jl}) \left(\frac{\partial Q_{jl}}{\partial P^{cons}_{j\text{silv}}} + \underbrace{\frac{\partial Q_{jl}}{\partial M}}_{\text{Additional Distortion}} \right)}{-\left(\frac{\partial Q_{j\text{silv}}}{\partial P^{cons}_{j\text{silv}}} + \underbrace{\frac{\partial Q_{j\text{silv}}}{\partial M}}_{\text{Additional Distortion}} \right)}. \tag{7}$$

The fact that other plans offered by the firms also gain some of the consumers driven into the market by the additional subsidy generates an additional distortion.

How much larger the distortion is in the multi-product ACA case is not certain, and we do not have data to credibly estimate its size. We discuss some of the issues in translating our estimates for Massachusetts to the ACA case in Section 3.

2 Data and Estimation

While the theory of the distortion from price-linked subsidies is clear, the question remains whether the distortion is large enough to be of practical importance. To estimate the size of the distortion, we turn to data from the Massachusetts subsidized health insurance exchange (called Commonwealth Care, or "CommCare"). Created in the Massachusetts's 2006 health reform, CommCare facilitates and subsidizes private insurance coverage for individuals earning less than 300% of poverty and without access to insurance through an employer or another government program. The market is quite concentrated, with just four insurers offering plans during the period we study, making it an appropriate setting to study imperfect competition. Importantly, there have been several changes in plan premiums and the mandate penalty over time, allowing us to identify the relevant demand elasticities.

We use administrative data on plan choices and consumer demographics for all enrollees in the CommCare program from 2006 through June 2011.[17] For each month, we observe the set of participating members, their demographics, the plan they chose, and their available plans and premiums. Our main analysis focuses on trends in the number of consumers entering the market ("new enrollees"). Every month, some individuals become newly eligible for CommCare, for instance due to job loss or other income changes. These individuals then choose whether to enroll and which plan to sign up for if they enroll. We study whether changes in the relative cost of not enrolling (the mandate penalty) affect monthly new enrollments in the cheapest plan. (This is the effect needed for the distortion equation; the semi-elasticity of new enrollments in all plans is similar.)

In theory, a higher mandate penalty could also reduce the number of consumers exiting the market, meaning our estimates would understate the true elasticities. However, studying exits was complicated by an income verification program introduced around the same time as our other reforms (late 2007 and early 2008). This forced many enrollees to leave the market, leading to substantial noise and potentially bias in an analysis of exits. Thus, we chose to focus only on new enrollments.

Table 1 shows summary statistics for our sample. The data include 490,368 unique consumers. The population is quite poor, with over half having family income less than the poverty line. There were an average of 11,365 new enrollments per month, giving us a substantial sample to study changes over time in this statistic. Since consumers below poverty do not pay premiums (all plans cost \$0) or a mandate penalty, they act as a control group for our analysis of the population between 100-300% of poverty.

Recall that in the case of single-plan insurers, our formula for the increase in markups is:

$$Mkup_{\underline{j}}^{\text{End}} - Mkup_{\underline{j}}^{\text{Ex}} = \frac{\eta_{\underline{j},M}}{\eta_{\underline{j}}\left(\eta_{\underline{j}} - \eta_{\underline{j},M}\right)}.$$

An estimate of the own price elasticity, $\eta_{\underline{j}}$, is available in the literature (see Section 3), so

[17] This data was obtained under a data use agreement with the Massachusetts Health Connector, the agency that runs CommCare.

Table 1: Summary Statistics

Variable	Full Sample	Income Group (% of Federal Poverty Line)				
		<100%	100-150%	150-200%	200-250%	250-300%
Enrollment						
Total Unique Enrollees	490,368	267,642	95,567	72,657	35,966	18,536
Monthly Enrollment	141,803	72,728	31,173	23,250	12,225	6,338
	[43,968]	[16,922]	[10,596]	[7,741]	[4,457]	[2,375]
Monthly New Enrollees	11,365	6,031	2,346	1,863	942	485
	[3,525]	[2,587]	[1,773]	[635]	[324]	[162]
Enrollment Spell Length (months)	14.5	14.1	15.2	14.6	14.6	14.8
	[12.0]	[11.6]	[12.3]	[12.7]	[12.6]	[12.5]
Share of Enrollment Months	100.0%	51.3%	20.8%	15.5%	8.2%	4.2%
Monthly Prices						
Insurer Price Bid	$390.22	$385.54	$384.18	$392.59	$415.22	$419.71
	[$85.31]	[$83.23]	[$82.13]	[$76.95]	[$101.90]	[$102.89]
Consumer Premium	$22.28	$0.00	$4.90	$48.72	$96.30	$137.83
	[$39.32]	[$0.00]	[$7.80]	[$14.97]	[$21.58]	[$23.19]
Mandate Penalty (starting Jan 2008)	$8.66	$0.00	$0.00	$18.01	$36.37	$54.78
	[$15.25]	[$0.00]	[$0.00]	[$0.88]	[$1.49]	[$2.99]
Demographics						
Age	39.9	37.0	41.4	43.7	44.7	45.7
	[14.2]	[14.2]	[14.1]	[13.1]	[12.9]	[12.7]
Male	47.2%	52.2%	42.0%	40.8%	42.9%	44.4%

NOTE: This table shows means and standard deviations (in brackets) of statistics about CommCare enrollees from November 2006 to June 2011. "Insurer Price Bid" shows the enrollment-weighted average price paid to firms for an enrollee. Premiums are the average (enrollment-weighted) post-subsidy monthly prices consumers pay for plans. The mandate penalty – which is set by law as half of each income group's affordable amount – is its average monthly value from January 2008 (the month the regular penalty started) until June 2011.

we focus on estimating $\eta_{\underline{j},M}$, the effect on demand for the cheapest plan when the mandate penalty is raised. In what follows, we use two natural experiments in CommCare to estimate this key statistic.

2.1 Mandate Penalty Introduction Experiment

Our first strategy uses the mandate penalty's introduction. Under the Massachusetts health reform, a requirement to obtain insurance took effect in July 2007. However, through November 2007 the requirement was not enforced by any financial penalties. Financial penalties began in December 2007. Those earning more than 150% of poverty who were uninsured in December forfeited their 2007 personal exemption on state taxes – a penalty of $219 (see Commonwealth Care, 2008). Starting in January 2008, the mandate penalty was assessed based on monthly uninsurance. The monthly penalties depended on income, ranging from \$17.50 for a person with household income of 150-200% of poverty to \$52.50 for someone between 250-300% of poverty.

There was a spike in new enrollees into CommCare for people above 150% of poverty (the group actually subject to the penalties) exactly concurrent to the introduction of the financial penalties in December 2007 and early 2008. Figure 1 shows this enrollment spike for the cheapest plan, which is proportional to the overall spike; (a fairly constant 60% share of new enrollees chose the cheapest plan.) To make magnitudes comparable for income groups of different size, the figure shows new enrollments as a share of the same plan's total enrollment in that income group in June 2008.[18]

We believe that this enrollment spike was *caused* by the financial penalties for three reasons. First, there were no changes in plan prices or other obvious demand factors for people above 150% of poverty that occurred at the same time. Second, as Figure 1 shows, there was no concurrent spike for people earning less than poverty (who were not subject to the penalties),[19] and there was no enrollment spike for individuals above 150% of poverty in December-March of years other than 2007-08. Finally, Chandra et al. (2011) show evidence that the new enrollees after the penalties were differentially likely to be healthy, consistent with the expected effect of a mandate penalty in reducing adverse selection.

We estimate the semi-elasticity associated with this response using a difference-in-differences specification, analogous to the graph in Figure 1. We estimate excess new enrollments in December 2007-March 2008 relative to the trend in nearby months, using enrollment trends for people earning less than poverty as a control group. We estimate the effect through March 2008 for two reasons. First, the application process for the market takes some time, so people who decided to sign up in January may not have enrolled until March. Second, the mandate rules exempted from penalties individuals with three or fewer months of uninsurance during the year, meaning that individuals who enrolled in March avoided any penalties for 2008. However most of the effect is in December and January, so focusing on those months does not substantially affect our estimates.

We collapse the data to the income group-month level and calculate the new enrollees in the cheapest plan for each group and month, normalized by the same plan's total enrollment

[18]We use June 2008 as a baseline because enrollment, which had been steadily growing since the start of CommCare, stabilizes around June 2008. Therefore, we treat June 2008 enrollment as an estimate of equilibrium market size.

[19]People earning 100-150% of poverty are omitted from this analysis because a large auto-enrollment took place for this group in December 2007, creating a huge spike in new enrollment. But the spike occurred only in December and was completely gone by January, unlike the pattern for the 150-300% poverty groups. This auto-enrollment did not apply to individuals above 150% of poverty (Commonwealth Care (2008)) so it cannot explain the patterns shown in Figure 1.

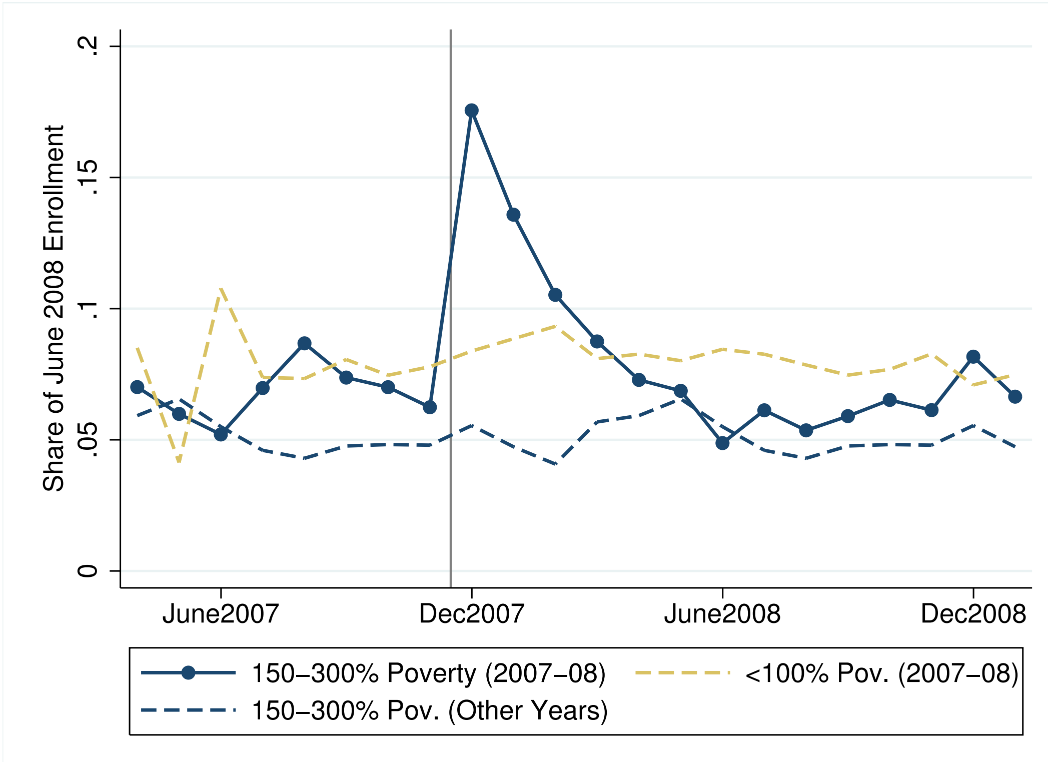


Figure 1: New Enrollees in Cheapest Plan by Month

NOTE: This figure shows for two income groups the monthly number of new enrollees into CommCare who get the cheapest plan. The vertical line is drawn just before the introduction of the mandate penalty, which applied to people 150-300% poverty but not those below 100% poverty. The "150-300% Poverty (Other Years)" series shows average new enrollments in each calendar month in all years in our data *except* July 2007–June 2008. Each income group's numbers are normalized by the group's total enrollment (in the same plan) in June 2008, so units can be interpreted as fractional changes in enrollment. "New enrollments" include both individuals enrolling in CommCare for the first time and individuals re-enrolling after a break in coverage, since both groups select a new plan. For the 150-300% poverty group, the cheapest plan is defined as the lowest-premium plan (or plans if there is a tie) in each individual's choice set (which can vary by region and income group). For the below 100% poverty group for whom all plans are free, the cheapest plan is defined as the lowest-premium plan for 150-200% poverty enrollees in the same region.

for the income group in June 2008 (as discussed above). The difference-in-differences model we estimate is:

$$NewEnroll_{g,t} = \sum_g \alpha_g \cdot 1_g + \beta \cdot Treat_t + \sum_g \gamma_g \cdot 1_g \cdot Treat_t + \sum_{g,t} \delta_{g,t} \cdot 1_g \cdot X_t + \varepsilon_{g,t}, \quad (8)$$

where 1_g is an indicator for income group g, $Treat_t$ is an indicator for t being in December 2007 through March 2008, and X_t is a vector of time polynomials and CommCare-year dummies, the coefficients of which we allow to vary across groups. The difference-in-difference coefficient of interest is γ_g for the groups above 150% of poverty subject to the penalties.

Table 2: Introduction of the Mandate Penalty.

Dependent Var: New Enrollees in Cheapest Plan / June 2008 Enrollment

Variable	(1)	(2)	(3)
Sum of Dec2007 - Mar2008 coefficients (below)	**0.253*** (0.017)**	**0.237*** (0.023)**	**0.225*** (0.024)**
150-300% Poverty x Dec2007	0.112***	0.110***	0.103***
	(0.003)	(0.006)	(0.007)
x Jan2008	0.073***	0.067***	0.069***
	(0.004)	(0.006)	(0.006)
x Feb2008	0.043***	0.033***	0.033***
	(0.005)	(0.006)	(0.006)
x Mar2008	0.025***	0.027***	0.020***
	(0.006)	(0.006)	(0.007)
Control Group (<100% Poverty)		X	X
Triple Difference (dummies for Dec-March)			X
Observations	51	102	102
R-Squarec	0.969	0.923	0.925

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

NOTE: This table performs the difference-in-difference regressions analogous to the graphs in Figure 1. The dependent variable is the number of new CommCare enrollees who choose the cheapest plan in each month in an income group, scaled by total group enrollment in that plan in June 2008. There is one observation per income group (the 150-300% poverty treatment group, plus the <100% poverty control group in columns (2) and (3)) and month (from April 2007 to June 2011). All specifications include CommCare-year dummy variables and fifth-order time polynomials, separately for the treatment and control group, to control for underlying enrollment trends. (The CommCare-year starts in July, so these dummies will not conflict with the treatment months of December to March.) Specification (3) also includes dummy variables for all calendar months of December-March for the treatment group, to perform the triple-difference. See the note to Figure 1 for the definition of new enrollees and the cheapest plan.

Table 2 presents the regression results. Column (1) starts with a baseline single-difference specification that estimates based only on enrollment for the 150-300% poverty group in December 2007-March 2008 relative to the surrounding months. Column (2) then adds the <100% poverty group as a control group, to form the difference-in-difference estimates. Finally, Column (3) adds dummies for December-March in all years, forming the triple difference that nets out general trends for those months in other years. All of these specifications use data from March 2007 to June 2011 and also include CommCare-year dummies and time polynomials separately for the treatment and control groups.[20] Despite the small number

[20]CommCare plan years run from July to June, so the year dummies are stable over the treatment period

of group-month observations, all the relevant coefficients are highly significant. Summing across treatment months, the total excess enrollment after the mandate penalty introduction was between 22-25% of equilibrium market size.

Table 3: Introduction of the Mandate Penalty, by Income Group.

Dependent Var: New Enrollees in Cheapest Plan / June 2008 Enrollment

Variable	**Income Group (% of Poverty Line)**		
	150-200%	**200-250%**	**250-300%**
Sum of Dec2007 - Mar2008 coefficients (below)	**0.208*** (0.026)**	**0.263*** (0.022)**	**0.250*** (0.024)**
150-300% Poverty x Dec2007	0.101***	0.113***	0.091***
	(0.007)	(0.007)	(0.008)
x Jan2008	0.061***	0.085***	0.086***
	(0.007)	(0.007)	(0.007)
x Feb2008	0.026***	0.045***	0.051***
	(0.007)	(0.006)	(0.006)
x Mar2008	0.020**	0.020***	0.022***
	(0.008)	(0.006)	(0.007)
Observations	102	102	102
R-Squared	0.927	0.922	0.920

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

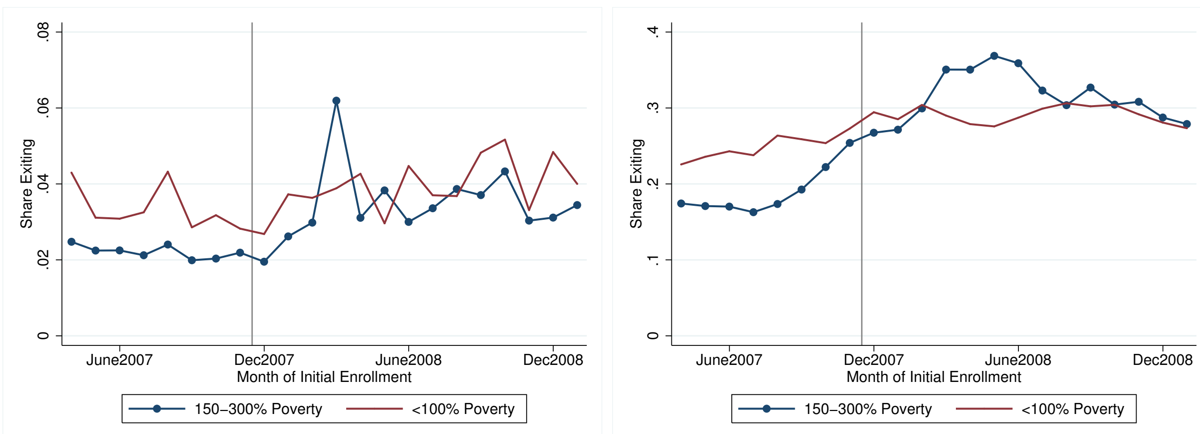
NOTE: This table performs the triple-difference regression specifications in Column (3) of Table 2 separately for each 50% of poverty income group (the level at which the mandate penalty varies). The dependent variable is the number of new CommCare enrollees who chose the cheapest plan in each month in an income group (the 50% of poverty group specified in the column headings), scaled by total group enrollment in that plan in June 2008, with the below 100% poverty as the control group. See the note to Table 2 for further discussion of variable definitions.

Table 3 takes the final, triple-difference specification from Table 2 and breaks the analysis down into narrower income groups (by 50% of poverty interval, the narrowest we have). The coefficients are a little larger for the higher income groups – about 25% instead of 21% – who faced higher mandate penalties. Given the mandate penalties of $17.50-$52.50, the coefficients imply that each $1 increase in the mandate penalty raised demand by between 1.2% for the 150-200% of poverty group to 0.48% for the 250-300% of poverty group, with a weighted average of 0.97%.

We would like to interpret the increases in enrollment as being the result of the $17.50–$52.50 monthly mandate penalty that went into effect in January 2008. However, the 2007

from December 2007 to March 2008. We use data only up to June 2011 because of significant changes in the prices and availability of the cheapest plans that took effect in July 2011.

uninsurance penalty of $219 was assessed based on coverage status in December 2007,[21] making that month's effective penalty much larger. If consumers were only buying insurance because of the larger December penalty, we would expect many of them to leave the market soon after the monthly penalty dropped to the lower level. To assess this story, Figures 2a and 2b plot the probability of market exit within 1 and 6 months, respectively, for new enrollees. Each point represents a distinct group of new enrollees in the corresponding month shown on the x-axis, and the y-axis value is the share who exited within 1 or 6 months. While there is a general upward trend over time, there is no jump in either series for people who joined in December 2007. The large spike for people enrolling in March 2008 is due to an unrelated income verification program.[22] This analysis suggests that consumers were not enrolling for just December to avoid the $219 penalty and leaving soon after because of the lower penalty.



(a) One Month

(b) Six Months

Figure 2: Share of New Enrollees Exiting Within the Specified Number of Months.

NOTE: These graphs show the rate of exiting CommCare coverage within one month (left figure) and six months (right) of initial enrollment among people newly enrolling CommCare in a given month. If individuals had enrolled at the end of 2007 to avoid the one-time $219 penalty and quickly dropped coverage thereafter, we would expect to see a spike for the 150-300% poverty series in December 2007 and/or January 2008. The absence of such a spike in exits suggests that the monthly penalties starting in January 2008 were sufficient to induce individuals to remain in the program. The spike that does occur among new enrollees in March 2008 reflects the start of an income-verification program for the 150-300% poverty group in April 2008. See the note to Figure 1 for the definition of new enrollees and the cheapest plan.

[21] In addition, an exemption was given for individuals who applied for CommCare in 2007 and enrolled on January 1, 2008. However, December 31, 2007, was the main advertised date for assessing coverage status.

[22] The income verification program took effect in April 2008 for individuals above 150% of poverty (but earlier on for people $<100\%$ poverty). Prior to April, income group at enrollment was based partly on self-reporting, and changes in income over time were also supposed to be self-reported. The verification program uncovered a large number of ineligible people, who were dis-enrolled in April 2008 and subsequent months. This event can also explain the upward trend in exits within 6 months leading up to April 2008.

2.2 Affordable Amount Decrease Experiment

Our second strategy for identifying the effect of the price of the outside option on the cheapest plan's demand uses changes in the "affordable amount." Recall that CommCare sets subsidies so that the post-subsidy premium for the cheapest plan equals the affordable amount. Therefore, for a fixed set of pre-subsidy prices, a \$1 decrease in the affordable amount raises the subsidy and lowers the premium of all plans by \$1. In our model, this has an equivalent effect as a \$1 increase in the mandate penalty (holding plan premiums fixed), so we can use changes in the affordable amount to estimate $\eta_{\underline{j},M}$.

This approach addresses a concern with our first method: that the *introduction* of a mandate penalty may have a larger effect (per dollar of penalty) than a *marginal increase* in penalties.[23] Some individuals may obtain coverage to avoid the stigma of paying a penalty, but this stigma might not change when mandate penalties increase.[24] Our second approach using the affordable amount avoids this problem and also allows us to obtain estimates for the 100-150% poverty group, who faced a \$0 mandate penalty.

The most significant changes in the affordable amount occurred in July 2007 for consumers between 100-150% of poverty.[25] For the first half of 2007, their affordable amount was \$18 and premiums ranged from \$18 for the cheapest plan to \$74.22 for the most expensive. In July 2007, CommCare eliminated premiums for this group, so all plans became free. We can think of this as the combination of two effects: (1) The affordable amount was lowered from \$18 to zero, and (2) the premium of all plans besides the cheapest one were differentially lowered to equal the cheapest premium (now \$0). The second change should unambiguously lower enrollment in what was the cheapest plan, since the relative price of all other plans falls. Therefore, if we use the actual policy change to estimate the effect on new enrollment in the (formerly) cheapest plan, this will be a lower bound on the effect of just lowering the affordable amount (the effect we want to estimate).

As a control group, we use the 200-300% poverty group, whose affordable amounts were essentially unchanged in July 2007.[26] We exclude other incomes from our controls for several

[23] In principle, we could also study several marginal changes in the mandate penalty after 2008. However, these changes were small: a \$0.50 decrease for 150-200% poverty in January 2009, and a \$2-6 increase, depending on income, in January 2010. Their small size makes it difficult to distinguish enrollment changes at these times from underlying noise.

[24] An argument against the stigma explanation is that the legal mandate to obtain insurance had been in place since July 2007, though without financial enforcement. In addition, individuals below 100% of poverty were also required to obtain insurance (again without financial enforcement). However to the extent there is a stigma specifically from paying a fine for non-coverage, this concern is valid.

[25] Prices in CommCare usually change in July, but in the first year of the program, prices were held fixed from November 2006 to June 2008, so firms' price-bids did not change at the same time as this change in the affordable amount.

[26] The affordable amount for consumers 200-250% poverty was unchanged at \$70, while the amount for

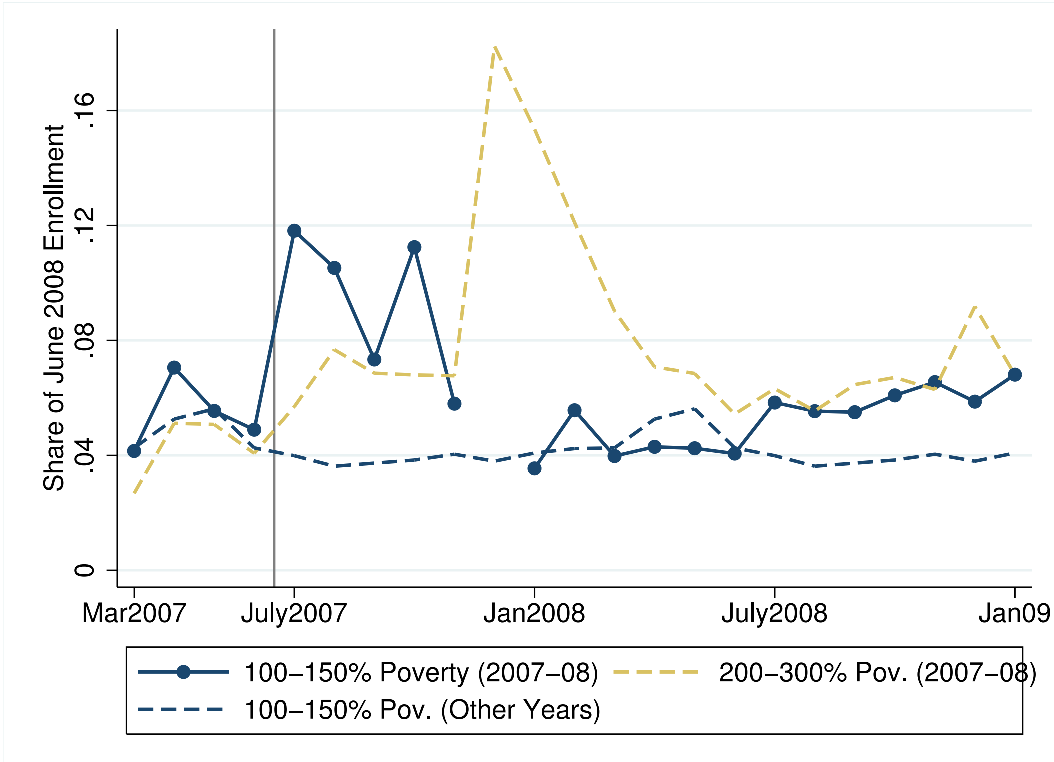


Figure 3: New Enrollees in Cheapest Plan by Month, around the Change in the Affordable Amount

NOTE: This figure shows for two income groups the monthly number of new enrollees into CommCare who chose the cheapest plan, scaled by total group enrollment in that plan in June 2008. The vertical line is drawn just before the decrease in the affordable amount (the consumer premium for the cheapest plan) from \$18 to \$0 for the 100-150% poverty group. During this period, the affordable amounts for the 200-300% poverty groups were essentially unchanged (they were constant at \$70 for people 200-250% poverty and fell by \$1 from \$106 to \$105 for people 250-300% poverty). The "100-150% Poverty (Other Years)" series shows average new enrollments in the corresponding calendar month in all other years in our data *after* June 2008. We start the graph from March 2007, the first month with significant CommCare enrollment for these income groups. We exclude December 2007 for the 100-150% poverty group because a one-time auto-enrollment caused a sharp spike in new enrollees (to over 30% of June 2008 enrollment), and showing this point makes it difficult to see the other points in the graph. See the note to Figure 1 for the definition of new enrollees and the cheapest plan.

reasons. First, we do not use the below 100% poverty group because of its somewhat different enrollment history and trends. Whereas the groups above poverty only started joining CommCare in February 2007, the below 100% poverty group became eligible in November 2006 and had a large influx in early 2007 due to an auto-enrollment. Second, we exclude the 150-200% of poverty group from the controls because their affordable amount also fell

250-300% poverty was lowered by just \$1 from \$106 to \$105. To the extent this slightly increased enrollment for the control group, it would only bias our estimates downward.

non-trivially (from $40 to $35) in July 2007. While this smaller change does not produce as dramatic of an enrollment spike, we show in the Appendix that their enrollment increase was consistent with semi-elasticity calculated from the the mandate penalty introduction results presented in Table 3.

Table 4: Decrease in the Affordable Amount.

Dependent Var: New Enrollees in Cheapest Plan / June 2008 Enrollment

Variable	(1)	(2)	(3)
Sum of July - Oct 2007 coefficients (below)	**0.200*** (0.021)**	**0.156*** (0.031)**	**0.169*** (0.032)**
100-150% Poverty x July2007	0.064***	0.060***	0.063***
	(0.005)	(0.008)	(0.008)
x Aug2007	0.052***	0.031***	0.035***
	(0.005)	(0.008)	(0.009)
x Sep2007	0.022***	0.011	0.016*
	(0.005)	(0.008)	(0.008)
x Oct2007	0.062***	0.054***	0.055***
	(0.005)	(0.008)	(0.008)
Control Group (200-300% poverty)		X	X
Triple Difference (dummies for July - October)			X
Observations	52	104	104
R-Squarec	0.988	0.981	0.981

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

NOTE: This table performs the difference-in-difference regressions analogous to the graphs in Figure 3, with specifications analogous to those in Table 2. The dependent variable is the number of new CommCare enrollees who chose the cheapest plan in each month in an income group, scaled by total group enrollment in that plan in June 2008. There is one observation per income group (the 100-150% poverty treatment group, and the 200-300% poverty control group in columns (2) and (3)) and month (from March 2007 to June 2011). All specifications include CommCare-year dummy variables and fifth-order time polynomials, separately for the treatment and control group, to control for underlying enrollment trends. (The first CommCare year ends in June 2008, so there is no conflict between the CommCare-year dummies and the treatment months of July to October 2007.) Specification (3) also includes dummy variables for all calendar months of July-October for the treatment group, to perform the triple-difference. Where applicable, specifications also include dummy variables to control for two unrelated enrollment changes: (a) for 100-150% poverty in December 2007, when there was a large auto-enrollment spike, and (b) for 200-300% poverty in each month from December 2007 to March 2008, when there was a spike due to the introduction of the mandate penalty. See the note to Figure 1 for the definition of new enrollees and the cheapest plan.

Figure 3 shows monthly new enrollments in the cheapest plan (again normalized by each group's enrollment in June 2008) for the 100-150% poverty treatment group and 200-300%

poverty control group around the July 2007 change, denoted by the vertical gray line. As an additional control, the figure shows the 100-150% of poverty group in the same calendar months in other years. Though the series are noisy there is a clear jump in the new enrollment for the 100-150% of poverty group in July 2007 and subsequent months relative to control groups. The large spike in 200-300% poverty enrollment in December 2007 reflects the mandate penalty introduction, which we used for our first identification strategy.

Table 4 presents the regression results corresponding to Figure 3. Again, in Column (1) we just look at the enrollment difference for the 100-150% poverty treatment group relative to trend, captured by CommCare-year dummies and time polynomials. Column (2) then adds the 200-300% poverty control group to form the difference-in-difference estimates. The last column does a triple-difference, further netting out changes in July-October of other years. The coefficients change a bit more between specifications, but they all imply significant enrollment increases of at least 15 percentage points. The triple-difference, our preferred specification, suggests that the decrease in the affordable amount increased enrollment in the cheapest plan by 16.9% points, implying a 0.94% effect for each \$1 decrease in the affordable amount. This semi-elasticity is very similar the one we estimated from the mandate penalty introduction.

3 Pricing Distortion Calculation

We use the coefficients we have estimated to calculate the semi-elasticity with respect to the mandate penalty for each income group and for the market overall. These, combined with the own-price elasticity, allow us to calculate the price distortion due to the price-linked subsidy. Table 5 combines the estimated coefficients from Table 3 and 4 with the corresponding changes in the mandate penalty or affordable amount. Since the dependent variables in our regressions are normalized by the plan's steady-state enrollment (taken to be June 2008), a coefficient of 0.20 implies an increase of 20% of the steady-state enrollment. To find semi-elasticities, we divide this by the dollar amount of the change in the mandate penalty or affordable amount. This results in semi-elasticities that are generally decreasing in income, as one would expect. The exception is the semi-elasticity for 100-150% poverty (0.0094), which is lower than that for 150-200% poverty (0.0119), but we consider the 100-150% poverty estimate to be a lower bound because of the other price changes that happened at the same time (see discussion above).

The statistic entering our pricing distortion formula (6) is the overall mandate penalty semi-elasticity across all consumers, which equals a share-weighted average of each income group's semi-elasticity. Using June 2008 enrollment shares, we calculate an average semi-

Table 5: Calculation of Semi-Elasticities and Distortion.

Statistic	Income Group (% of Poverty)			
	Aff. Amt. Change	*Mandate Penalty Introduction*		
	100-150%	150-200%	200-250%	250-300%
Enrollment % Increase	16.9%	20.8%	26.3%	25.0%
Mandate/Affordable Amount Change	$18.00	$17.50	$35.00	$52.50
Mandate Semi-Elasticity	0.0094	0.0119	0.0075	0.0048
Enrollment Shares (June 2008)	*46.1%*	*31.3%*	*15.1%*	*7.5%*
Pooled Population Calculation				
Average Mandate Semi-Elasticity	0.0095			
Own Price Semi-Elasticity*	0.0197			
Increase in Markup ($/month)	$47.54			
(% of avg. price = $383)	(12.4%)			

* Based on coefficient from Chan and Gruber (2010).

NOTE: This table puts together the estimated semi-elasticities for different groups into one overall "mandate semi-elasticity." We combine this with the estimate for in-market own price semi-elasticity from Chan and Gruber (2010) to calculate the dollar change in markup due to the endogenous subsidy, as illustrated in Equation (9).

elasticity of 0.0095. Thus, each $1 increase in the monthly mandate penalty increases overall enrollment in the cheapest plan by 0.95%.

The final statistic in our distortion formula is the own-price semi-elasticity of demand, $\eta_{\underline{j}}$. Because they have already analyzed this market, we use the estimate from Chan and Gruber (2010). They report an own-price semi-elasticity of demand (of 0.0154), but because their model does not account for the outside option of uninsurance, we need to adjust for that. For new enrollees, they find a price coefficient of -0.027. Using the average market share (including the outside option) for the cheapest plan of 27%,[27] the logit model implies an own-price semi-elasticity of

$$\eta_{\underline{j}} = -\alpha \cdot (1 - s_{\underline{j}}) = 0.027 \cdot (1 - 0.27) = 0.0197.$$

Allowing for out-of-market substitution mechanically makes this elasticity larger than that reported by Chan and Gruber (2010), though the estimated distortion would be larger if we

[27] During this time period, the in-market share for the cheapest plan was 47.3%. Using the 2008 American Community Survey, we estimate that there were 66,219 Massachusetts uninsured with incomes between 100-300% poverty and citizenship status making them eligible for CommCare, relative to 2008 average monthly CommCare enrollment of 87,491 among those 100-300% poverty. This implies that 43% of potential the market do not buy insurance, so the overall market share for the cheapest plan is 0.473*(1-0.43)=0.27.

used their number. Plugging the elasticities into Equation (6) gives us

$$Mkup_{\underline{j}}^{\text{End}} - Mkup_{\underline{j}}^{\text{Ex}} = \frac{\eta_{\underline{j},M}}{\eta_{\underline{j}}\left(\eta_{\underline{j}} - \eta_{\underline{j},M}\right)} = \frac{0.0095}{0.0197(0.0197 - 0.0095)} \approx \$48. \tag{9}$$

Thus, based on demand parameters from the Massachusetts exchange, the upward distortion in the pivotal plan's markup (and therefore the subsidy) due to price-linked subsidies is about \$48 per member-month. This is substantial: it is 12% of the average price of \$383 in 2007-2009 for people 100-300% poverty.

Modeling counter-factual pricing without the distortion requires making assumptions about demand away from the observed equilibrium – hence our assumption of a constant semi-elasticity of demand. This assumption is not necessary if we formulate the distortion in terms of the equivalent increase in per-member cost, instead of an increase in price. Our estimation indicates that if the distortionary incentive were removed and per-member costs for the cheapest plan were \$48 per month higher, there would be no change in the pricing equilibrium.

It is important to apply several caveats to this estimate. First, our results are not intended to be a perfect estimate of the historical distortion in Massachusetts. The Massachusetts market was relatively new in the period we study, so prices had probably not converged to equilibrium, and there was likely uncertainty about which plan would be subsidy pivotal. Also, we have made several conservative assumptions that may understate the size of the distortion in Massachusetts. And while our model assumes a single plan price and subsidy (as is true in the ACA), the actual pricing rules in Massachusetts at the time were a bit more complicated.[28]

Applying the estimates out-of-sample to the ACA exchanges requires even more caution. The ACA has different institutions in several dimensions: it includes higher income groups (including unsubsidized enrollees above 400% poverty); it has plans across four generosity tiers; and its subsidies are based on the second-lowest price silver plan, not the cheapest plan as in Massachusetts. Also, some ACA exchange consumers will be unsubsidized (about 20%, according to CBO projections). These consumers may help mitigate the distortion, though analysis of the Massachusetts experience (Ericson and Starc, 2012) suggests that most of these consumers will enroll in bronze plans, providing little discipline to silver plan prices.

[28]Specifically, the complications in plan years 2007-08 were: (1) It allowed for separate pre-subsidy prices for the 100-200% poverty and 200-300% poverty groups, so these were separate markets with different distortions; (2) the exchange had minimum prices (imposed under federal Medicaid rules), which appear to have been binding in some cases, meaning that the cheapest plan could not have been priced lower even without the distortion. Neither of these complications apply under the ACA, so we have not included them in our model.

The ACA will also have medical loss ratio (MLR) requirements, which prohibit a plan from increasing its administrative costs and profits beyond an allowed share of revenues (20% in the exchanges). If MLR restrictions are effective and binding, they could limit the distortion.

Working in the other direction is the fact that the ACA has multi-plan insurers (which theoretically should exacerbate the distortion; see Section 1) and initial data suggests that many markets may be uncompetitive. According to a *New York Times* analysis of states served by the federal exchange, 58% of markets (which are counties) have two or fewer insurers, and about 20% of markets have just one insurer (Abelson et al. (2013)). These areas are disproportionately small and rural, but the distortions there are potentially sizable. While normally the distortion is capped by the price of the third-cheapest silver plan, counties with one or two insurers may not have a third-cheapest plan (or it may be controlled by the same insurer as the second-cheapest). Along with the usual benefits of competition, our model suggests that having at least three (and preferably more) insurers in ACA exchanges is important for mitigating the distortions from price-linked subsidies.[29] While, we cannot estimate what the size of the distortion will be in the ACA exchanges, our estimates suggest that it has the potential to be substantial and thus is an important consideration for policy makers designing the exchanges.

4 Discussion

In this section, we discuss the policy implications of our results. So far, we have discussed two alternatives for subsidy design in an insurance exchange: exogenous and price-linked subsidies. We have argued that, relative to exogenous subsidies, linking subsidies to prices increase the incentive for insurers to raise prices. We now propose a third subsidy alternative that would eliminate the distortion while maintaining price-linked subsidies with their advantages. However, this alternate policy has its own weaknesses, and we next compare the advantages and disadvantages under all three possible subsidy designs. Finally, we discuss the implications of our findings for insurance programs other than the ACA.

[29] Single-insurer markets are even more problematic. In theory, the lone insurer could turn the subsidy rules into a money machine by raising price arbitrarily without losing subsidized consumers. Although regulations like minimum loss ratio requirements will certainly mitigate this extreme outcome, single-insurer markets are particularly worrying in light of our analysis.

4.1 Correcting the Distortion while Maintaining Price-Linked Subsidies

Our model suggests a simple alternative to exogenous subsidies that would fully correct the distortion while keeping price-linked subsidies: apply the same subsidy to the mandate penalty. Specifically, set an exogenous base mandate penalty amount M_0 and reduce this by the subsidy, for a final penalty of

$$M = M_0 - S(P).$$

The base amount could be set so that, based on the government's expectations about prices and the subsidy, the final penalty would be similar to the penalty specified by current law.[30] This adjustment would remove the pricing distortions discussed above. If the cheapest plan raised its price bid by \$1, this would leave its premium unaffected (since the subsidy would rise) but lower by \$1 the price of all other plans *and* the cost of uninsurance. The effect on own demand would be

$$\frac{dQ_{\underline{j}}}{dP_{\underline{j}}} = \sum_{k \neq \underline{j}} \left(-\frac{\partial Q_{\underline{j}}}{\partial P_k^{cons}} \right) - \frac{\partial Q_{\underline{j}}}{\partial M} = \frac{\partial Q_{\underline{j}}}{\partial P_{\underline{j}}^{cons}}$$

The price has only its direct effect on the demand for each plan, so even if insurers offer multiple plans (the ACA case), the subsidy does not distort incentives. Therefore, optimal prices would be identical to the benchmark condition with exogenous subsidies.

A concern with this approach is that it creates uncertainty about the mandate penalty. If prices (and therefore the subsidy) are higher than expected, the final mandate penalty would be lower than the target, likely leading to an increase in the uninsurance rate. This could be desirable: since the cost of insurance has risen, it may be optimal for fewer people to be insured. Alternatively, it would be undesirable if the price increase also signified an increase in the social cost of *uninsurance* – for instance, through a general health care cost increase that also increases the externality of uncompensated care (Mahoney, 2012). While we have abstracted away from adverse selection, it is a potential concern in these markets and the endogenous mandate penalty could exacerbate it: unexpectedly higher prices would lower the mandate penalty, which could lead healthier consumers to exit, further driving up prices. Spatial variation in prediction error would also imply variation in the final mandate penalty, which could be seen as inequitable.

[30] In particular, policy makers would want to increase M_0 over time with their estimate of medical cost growth, to avoid having the mandate penalty decline as costs and therefore prices and subsidies increase.

4.2 Comparing Subsidy Structures

We have so far discussed three subsidy alternatives: (1) exogenous subsidies, (2) price-linked subsidies with an exogenous mandate penalty, and (3) price-linked subsidies also applied to the mandate penalty. If insurers always priced at cost, option (2) (the current ACA policy) would have desirable properties: it would allow the government to take on price risk, guarantee affordability for consumers, and ensure a certain mandate penalty. Unfortunately, in imperfectly competitive markets, this policy distorts pricing incentives – and our results suggest the distortion is substantial.

The other two alternatives do not distort firm pricing, but each has its own disadvantage. Exogenous subsidies shift price risk onto poor enrollees and may, if they grow too large, push post-subsidy prices to their lower bound of zero. Price-linked subsidies applied to the mandate penalty create certainty about consumer prices but shift the volatility onto the mandate penalty. Both of these policies involve the government making a prediction for prices at the market (county) level to properly set exogenous subsidies or the base mandate penalty (M_0). If such predictions can be made reasonably well, then these disadvantages will be mitigated. Unfortunately, experience with Medicare Advantage's exogenous subsidies suggests that this process can be challenging.

Thus, none of the three policies is perfect. The optimal policy could involve a mixture of two or more of them. For instance, a 50/50 mixture between exogenous subsidies and our policy suggestion would increase subsidies by 50 cents for each $1 increase in prices (passing 50 cents onto consumers) and reduce the mandate penalty by 50 cents. Because the net insurance subsidy, $S + M$, remains constant, the distortion would be fully corrected. Alternatively, a 50/50 mixture between ACA policy and our policy would raise subsidies by the full $1 and reduce the mandate penalty by 50 cents for each $1 increase in prices. This would not eliminate the distortion, but would reduce it.

4.3 Broader Relevance for Health Insurance Programs

Although our empirical estimates are from a specific setting (the subsidized Massachusetts exchange in 2007-2008), we believe that our theoretical point about distortions with price-linked subsidies is relevant more broadly.

The applicability of our theory to a market depends on two factors: (1) There must be some substitution to an unsubsidized outside option, and the distortion is larger the greater is this substitutability. (2) Insurers must have some market power,[31] and the distortion

[31] The presence of market power is not a restrictive condition; it merely requires that a $1 price increase does not cause a plan's demand to fall to zero (as would be the case in perfect competition).

is larger with greater market power. Price-linked subsidies work by exacerbating existing market power – by effectively removing the competition of the outside option – so are more severe where baseline market power is greater.

We have discussed the close link with the ACA exchanges, but the theory also applies to Medicare Advantage, Medicare Part D, and employer-sponsored insurance programs. In the Medicare Advantage market, which uses exogenous baseline subsidies,[32] our results suggest that switching to endogenous subsidies would create a pricing distortion unless the subsidy was also applied to traditional Medicare (as in "premium support" proposals). As long as the government does not have a preference for whether consumers use traditional Medicare or private plans, the volatility of the price of the outside option is not an issue in this case. In order to avoid the potential for negative prices for traditional Medicare (if the Medicare Advantage bids are high), a premium support system would need to count the cost of traditional Medicare as another bid in the market.

Medicare Part D uses flat, price-linked subsidies (as in the ACA), but the distortion for the main subsidy is probably relatively small (though the low-income subsidy is likely larger; see Decarolis (2013)). The subsidy is based on a national enrollment-weighted average of plan price bids. Because all plans' prices affect the subsidy through this average, our theoretical distortion applies to all plans – not just a subset of potentially pivotal silver plans as in the ACA – but the distortion for each plan is smaller. It is approximately proportional to the national market share of the plan's parent insurer, the largest of which is United Health Group with 28% in 2011 (see Decarolis, 2013).

Employers who subsidize insurance also need to consider the trade-off between making prices predictable for employees and distorting pricing incentives. Employers typically pick a small menu of options for their employees and set subsidies based on prices (either implicitly or explicitly). To the extent that an employer's chosen insurer(s) have market power (for evidence of insurer market power, see Dafny (2010)), this can lead to the same pricing distortions we discuss. Because under tax rules employers cannot subsidize employees' outside options to the same extent (e.g., obtaining coverage through a spouse), they would need to use exogenous subsidies to avoid this distortion.

5 Conclusion

This paper considers the distortion of pricing incentives generated by price-linked subsidies in health insurance exchanges, an important topic for economists analyzing these markets

[32] However, after applying this baseline subsidy, Medicare reduces the subsidy when a plan reduces its price below the benchmark. This creates a different kind of pricing distortion, which may be significant.

and policy makers designing and regulating them. We highlight this distortion in a simple theoretical model and derive sufficient statistics for estimating its size. We then use two natural experiments in the Massachusetts exchange to confirm that insurance demand responds to the relative price of the outside option. Using the Massachusetts estimates to calibrate our model and assuming constant semi-elasticities, we find an upward distortion of the subsidy-pivotal cheapest plan's price of $48 per member-month, or 12% of the average price of insurance. The potential budgetary effect is substantial: Massachusetts had about 2 million member-months of subsidized coverage in fiscal 2009, so a $48 increase in monthly subsidies would translate to $96 million per year in government costs. For the ACA, using the CBO projection of 20 million subsidized enrollees annually, a $48 per month subsidy increase would translate to $11.5 billion per year in federal spending.

This estimate suggests the importance of price-linked subsidies in affecting equilibrium pricing incentives and subsidies in a health insurance exchange. Nonetheless, we do not view it as a perfect estimate of either the historical distortion in Massachusetts or the expected distortion in the ACA exchanges. Rather, we think that our calibration implies that the pricing distortion we identify in theory is of practical concern to policy makers deciding on subsidy rules.

How policy should respond depends on balancing several goals: preventing the pricing distortion, guaranteeing "affordable" post-subsidy premiums, and ensuring a target mandate penalty. The current ACA price-linked subsidies guarantee affordability and a fixed mandate penalty but involve pricing distortions. Exogenous subsidies prevent distortions and allow for a fixed mandate penalty but shift risk to poor consumers and cannot guarantee that post-subsidy premiums will be affordable. We present an alternate policy that guarantees affordability and eliminates the pricing distortion but does so by reducing the mandate penalty when prices are higher than expected. This policy carries the risk that unexpectedly high prices in an area would increase uninsurance. Thus, none of the alternatives is perfect, and optimal policy could involve a mixture between two or more of them. Nonetheless, the potentially large cost of the pricing distortion under current ACA policy makes reform of subsidy design worthy of consideration.

While our theory and empirics are focused on the ACA, we think the point that price-linked subsidies distort pricing incentives is applicable more generally. It applies to any program that uses price-linked subsidies and has an outside option. In addition to analyzing the ACA markets as data becomes available, future research could seek to measure the relevant elasticities in order to assess the quantitative importance of pricing distortions in Medicare Advantage, Medicare Part D, and employer-sponsored insurance programs.

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Appendix

We use the smaller changes in the affordable amount that occurred for the 150-200% poverty group in July 2007 as a check on the semi-elasticity estimated for this group. Table 6 shows the results. Our preferred triple-difference estimate from column (3), indicates that the \$5 affordable amount decrease translated into a 6.3% increase in demand for the cheapest plan. This implies a semi-elasticity of 0.0126, quite similar to the semi-elasticity of 0.0119 that we found for this group using the mandate penalty introduction (see Table 5).

Table 6: Decrease in the Affordable Amount: 150-200% Poverty

Dependent Var: New Enrollees in Cheapest Plan / June 2008 Enrollment

Variable	**(1)**	**(2)**	**(3)**
Sum of July - Oct 2007 coefficients (below)	**0.063*** (0.019)**	**0.052** (0.025)**	**0.063** (0.027)**
150-200% Poverty x July2007	0.013***	0.017**	0.018**
	(0.005)	(0.007)	(0.008)
x Aug2007	0.029***	0.016**	0.022***
	(0.005)	(0.007)	(0.008)
x Sep2007	0.013**	0.011*	0.015**
	(0.005)	(0.006)	(0.007)
x Oct2007	0.008	0.009	0.009
	(0.005)	(0.006)	(0.006)
Control Group (200-300% poverty)		X	X
Triple Difference (dummies for July - October)			X
Observations	52	104	104
R-Squarec	0.960	0.957	0.958

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

NOTE: This table performs the difference-in-difference regressions analogous to those in Table 4 (see the note to that table for additional information), but with a treatment group of enrollees 150-200% poverty, whose affordable amount dropped from \$40 to \$35 in June 2008. The control group continues to be enrollees 200-300% poverty, whose affordable amounts were essentially unchanged at that time.