The Early Impact of the Affordable Care Act State-By-State*

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Abstract

I examine the impact of state policy decisions on the early impact of the ACA using data through the first half of 2014. I focus on the individual health insurance market, which includes plans purchased through exchanges as well as plans purchased directly from insurers. In this market, at least 13.2 million people were covered in the second quarter of 2014, representing an increase of at least 4.2 million beyond pre-ACA state-level trends. I use data on coverage, premiums, and costs and a model developed by Hackmann, Kolstad, and Kowalski (2013) to calculate changes in selection and markups, which allow me to estimate the welfare impact of the ACA on participants in the individual health insurance market in each state. I then focus on comparisons across groups of states. The estimates from my model imply that market participants in the five "direct enforcement" states that ceded all enforcement of the ACA to the federal government are experiencing welfare losses of approximately $245 per participant on an annualized basis, relative to participants in all other states. They also imply that the impact of setting up a state exchange depends meaningfully on how well it functions. Market participants in the six states that had severe exchange glitches are experiencing welfare losses of approximately $750 per participant on an annualized basis, relative to participants in other states with their own exchanges. Although the national impact of the ACA is likely to change over the course of 2014 as coverage, costs, and premiums evolve, I expect that the differential impacts that we observe across states will persist through the rest of 2014.

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1 Introduction

As part of the implementation of the Affordable Care Act (ACA), all states had their first open enrollment season for coverage through new health insurance exchanges from October 2013 through March 2014. Using data through the first half of 2014, I take an early look at the impact of the ACA on the individual health insurance market. This market includes plans purchased through exchanges as well as plans purchased directly from insurers. Although a small fraction of the national population has historically been enrolled in the individual health insurance market, it is an important market to study because it is the market of last resort for the uninsured, and one focus of the ACA is to expand coverage to the uninsured. In my data, 13.2 million people were enrolled in the individual health insurance market per month of the second quarter of 2014. Had state-level trends persisted from before the implementation of the ACA, 4.2 million fewer people would be enrolled in this market.

I focus on the impact of state policy decisions on the early impact of the ACA. Whether the impact of the ACA differed across states is of central policy-relevance because states made several important decisions regarding the implementation of the ACA. A small number of states decided to cede all enforcement of the ACA to the federal government. The Federal government refers to these states as "direct enforcement" states. Other states took far more responsibility for the implementation of the ACA by setting up their own exchanges and deciding which vendors to use. The Supreme Court gave states authority to decide whether to implement the Medicaid expansion legislated by the ACA, and just over half of the states have elected to so do thus far. Similarly, the White House gave states authority to decide whether to allow the renewal of non-ACA-compliant non-grandfathered plans, and just over half of states have elected to do so.

Furthermore, most pre-ACA regulation of the individual health insurance market was at the state-level. Some states already had two important regulations that could affect the functioning of the individual health insurance market: "community rating" regulations that require all health insurers to charge the same price to all beneficiaries, regardless of observable characteristics, and "guaranteed issue" regulations that prevent insurers from denying coverage to applicants, regardless of their health status. Both of these regulations were enacted nationally with the ACA, and the relevant "community" for the community rating regulations was specified to be the state. Therefore, in those states that already had those regulations, we can attempt to isolate the impact of other provisions of the ACA, the most prominent of which is the individual mandate. Such an exercise sheds light on what the impact of the ACA would have been in the absence of the individual mandate, which would have happened if the Supreme Court had struck down the individual mandate while upholding the other provisions.

Other state policy decisions from before the implementation of the ACA could have lasting impacts. For example, pre-ACA policy decisions could affect the number of insurers in the individual health insurance market, which, in turn, could affect enrollment under the ACA. The number of insurers could also affect markups.

To make comparisons across groups of states, I first examine the impact of the ACA state-by-state. I examine data on coverage, premiums, and costs. Using those data and a model that I developed with Martin Hackmann and Jonathan Kolstad (Hackmann, Kolstad, and Kowalski (2013), hereafter HKK), I estimate how much better or worse off the ACA made participants in the individual health insurance market in each state. In this model, the ACA can make market participants better off if it encourages insurers to decrease "markups" – the difference between the premiums that they charge and the costs that they incur. The ACA can also make market participants better off if it mitigates "adverse selection," meaning that it encourages individuals with lower insured costs to join the pool.

There have been numerous questions in the popular press about whether enough "young and healthy" individuals have signed up for health insurance coverage. These claims imperfectly address whether there was adverse selection by focusing simply on coverage demographics. I assess the presence of adverse selection more systematically using cost data and a model. The main assumption necessitated by the data and the model is that plan generosity did not change with the implementation of the ACA. Plans could have become more or less generous with the implementation of the ACA, since the essential health benefits required by the ACA could have increased plan generosity, but limited network plans offered in exchanges could have decreased plan generosity. By focusing on comparisons across states, I require a weaker assumption regarding changes in plan generosity across states.

The estimates from my model imply that participants in the five "direct enforcement" states that ceded all enforcement of the ACA to the federal government are worse off by approximately

$245 per participant on an annualized basis, relative to participants in all other states. They also imply that the impact of setting up a state exchange depends meaningfully on how well it functions. Market participants in the six states that had severe exchange glitches are worse off by approximately $750 per participant on an annualized basis, relative to participants in other states with their own exchanges. The estimates imply suggestive evidence that participants in states that allowed renewal of non-grandfathered plans are worse off than participants in other states. They also provide inconclusive evidence that participants in states with pre-ACA community rating and guaranteed issue regulations are better off than participants in other states, likely because these regulations contributed to pre-ACA adverse selection. They provide further inconclusive evidence regarding the impact of having more insurers in the pre-ACA state market. Although the national impact of the ACA is likely to change over the course of 2014 as coverage, costs, and premiums evolve, I expect that the differential impacts that we observe across states will persist through the rest of 2014.

In the next section, I present the model, and I describe how I estimate the model in Section 3. I discuss the data in Section 4, I provide summary statistics in Section 5, and I present results in Section 6. I compare my results to existing empirical evidence on selection and conclude in Sections 7 and 8.

2 Model

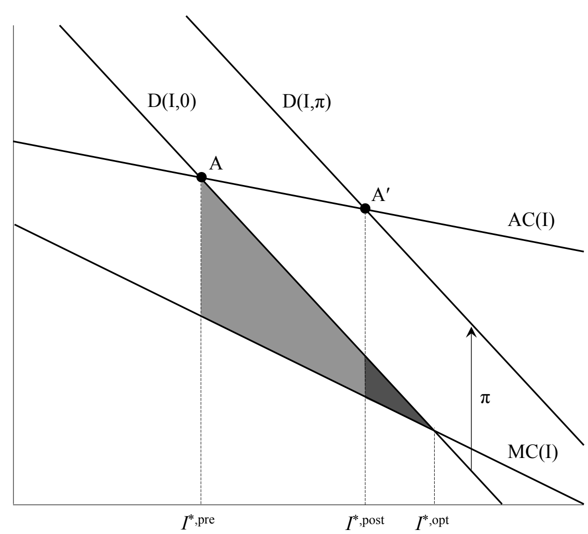
I adapt a simple model from HKK, and I use similar notation to facilitate comparison across papers. In the model, changes in welfare come from changes in selection and from changes in markups. I first present the model with only changes in selection, following previous work by Einav, Finkelstein, and Cullen (2010), hereafter EFC. I then present the full model from HKK, which accounts for changes in markups. EFC and HKK offer micro-foundations that I omit here for brevity.

2.1 Model Without Markups

Assume for now that insurers charge beneficiaries the average cost that they spend to pay medical claims. Because beneficiaries differ in the cost of insuring them, I model the average cost curve

$AC(I)$ as a function of the number of individuals in a given market who have coverage I.[1] If the market is adversely selected, then the sickest individuals are the first to sign up for health insurance coverage at any price. When there is an exogenous increase in the number of insured individuals, the new individuals who sign up for coverage will be healthier than the formerly insured, and insurer per-enrollee costs will decrease. As depicted in Figure 1, a downward-sloping average cost curve indicates the presence of adverse selection. The main assumption required is that plan generosity remains constant for any level of coverage. (If plan generosity decreases, then average costs could go down in the absence of adverse selection.) Assuming constant plan generosity, the downward slope of the AC curve indicates the presence of adverse selection (an upward slope indicates advantageous selection); however, the slope alone is not enough to identify the welfare cost.

Figure 1: Model Without Markups



The welfare cost of adverse selection is determined by the demand curve for insurance as well as the average cost curve. The demand curve $D(I, \pi)$ is a function of enrollment in insurance I, and the penalty that individuals must pay if they do not have health insurance coverage π, which is zero before the implementation of the ACA. As shown in Figure 1, in the presence of adverse

[1] Note that HKK and EFC represent the *fraction* of individuals in a given market who have health insurance coverage with I. I make a different modeling choice since it is so difficult to estimate the potential size of the individual health insurance market, particularly in the first quarter of 2014 (see Abraham et al. [2013]). However, I retain the same notation to emphasize that the formulas for welfare analysis are the same under this definition of I.

selection, pre-reform equilibrium coverage $I^{*,pre}$ occurs at point A, where the average cost curve intersects the demand curve. Insurers must charge enrollees their average costs either because enrollee health cannot be observed or because regulations prevent insurers from pricing based on underlying health. Optimal coverage $I^{*,opt}$ would occur at the intersection of the demand curve and the marginal cost curve $MC(I)$.[2] Because demand exceeds the marginal cost of coverage, but that coverage is not provided in equilibrium, adverse selection induces a welfare loss equal to the entire shaded region (including the lighter area and the darker area) in Figure 1.

Now consider the implementation of the ACA. If individuals must now pay a penalty π if they do not have health insurance coverage, their demand shifts upward by π, and the new equilibrium coverage $I^{*,post}$ occurs at point A. Subsidies behave similarly by shifting the demand curve in the same direction, so we include them in the "penalty" π for expositional simplicity. It is at first counterintuitive that subsidies and penalties shift demand in the same direction in the individual health insurance market. However, since the subsidies are only available in the individual health insurance market, while they decrease demand in other markets, they *increase* demand in the individual health insurance market. In the market for employer-sponsored health insurance, the penalty and the subsidy shift demand in opposite directions, as modeled in Kolstad and Kowalski [2012].

The lighter shaded region in Figure 1 gives the welfare gain that results from the mitigation of adverse selection with the ACA. The penalty depicted is not large enough to eliminate the entire welfare loss from adverse selection. However, if the combination of subsidies and penalties induces optimal coverage, $I^{*,opt}$ then the welfare gain from the implementation of the ACA would also include the darker shaded region.

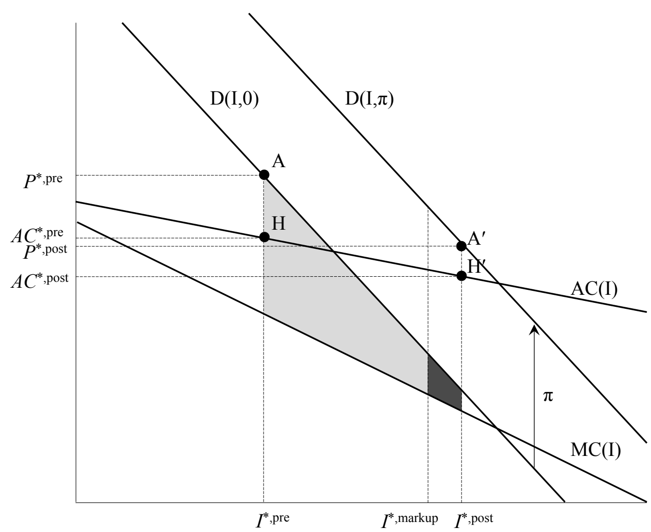
2.2 Model With Markups

HKK extend the model to allow insurers to charge a markup beyond the average cost of paying claims. The "markup" is the difference between the premium and the average cost. It is useful to extend the model to incorporate markups in empirical settings in which is it possible to separately observe the premiums charged to beneficiaries and the average costs paid by insurers.

[2] The average cost curve and the marginal cost curve intersect at zero coverage, but zero coverage is not shown along the horizontal axis so that other phenomena can be observed more easily.

Markups can reflect several factors, including insurer market power and the enrollment predictions of the actuaries that set premiums. Given these factors, we might expect markups to change from before to after the introduction of the ACA. Markups could go down if transparency introduced by the new exchanges decreases market power. Conversely, markups could go up if the actuaries that set premiums attempt to protect their firms from losses that would occur if the new enrollees incur higher than expected costs. State regulations only allow firms to set premiums once per year, well before costs and enrollment from the previous year are realized, so it could take several years for markups to reach equilibrium after the ACA. In the interim, markups set before the implementation of the ACA can induce distortions.

Figure 2: Model with Markups



In the model with markups, equilibrium coverage occurs where average cost plus the markup is equal to demand. In Figure 2, the pre-reform markup is equal to the vertical distance between the pre-reform premium $P^{*,pre}$ at point A and the pre-reform average cost $AC^{*,pre}$ at point H. Analogously, the post-reform markup is equal to the vertical distance between the post-reform premium $P^{*,post}$ at point A' and the post-reform average cost $AC^{*,post}$ at point H'. In this extended model, changes in markups and changes in adverse selection affect welfare. As shown in Figure 2, the full welfare gain from the reduction in adverse selection and the reduction in markups is given by the area in which demand for coverage exceeds the marginal cost of coverage between the initial coverage level $I^{*,pre}$ and the post-reform coverage level $I^{*,post}$. Graphically, in Figure 2, the full welfare gain is the sum of both shaded regions. Algebraically, the full change in welfare from changes in adverse selection and markups is as follows:[3]

$$
\begin{aligned}
\Delta W_{full} &= (P^{*,pre} - AC^{*,pre}) * (I^{*,post} - I^{*,pre}) \\
&- (AC^{*,post} - AC^{*,pre}) * (I^{*,pre} + (I^{*,post} - I^{*,pre})) \\
&+ \frac{1}{2}((P^{*,post} - \pi) - P^{*,pre}) * (I^{*,post} - I^{*,pre}). \qquad (1)
\end{aligned}
$$

From this equation, we see that the welfare impact depends on only seven quantities: pre- and post-reform coverage, premiums, and average costs, as well as the penalty. Stated another way, the welfare impact depends on the slope of the average cost curve as well as the slope of the demand curve. The comparison of point H with point H' identifies the slope of the average cost curve. The comparison of point A with point A', minus the penalty, identifies the slope of the demand curve.

To separate the welfare impact of the change in adverse selection from the change in markups, HKK perform an accounting exercise to isolate the welfare impact that would have resulted from the change in adverse selection had the pre-reform markup remained unchanged. This selection-induced change in welfare is as follows:

$$
\begin{aligned}
\Delta W_{sel} &= (P^{*,pre} - AC^{*,pre}) * (I^{*,markup} - I^{*,pre}) \\
&- \frac{AC^{*,post} - AC^{*,pre}}{I^{*,post} - I^{*,pre}} * \Big(I^{*,pre} + (I^{*,markup} - I^{*,pre})\Big) * (I^{*,markup} - I^{*,pre}) \\
&+ \frac{1}{2} * \frac{(P^{*,post} - \pi) - P^{*,pre}}{I^{*,post} - I^{*,pre}} * (I^{*,markup} - I^{*,pre})^2 \qquad (2)
\end{aligned}
$$

where the post-reform coverage level under the pre-reform markup, $I^{*,markup}$, is given by:

$$
I^{*,markup} = max\Big(0, min\Big(Pop, I^{*,pre} + \pi \frac{(I^{*,post} - I^{*,pre})}{(AC^{*,post} - AC^{*,pre}) - ((P^{*,post} - \pi) - P^{*,pre})}\Big)\Big),
$$

which accounts for the lower bound of zero coverage and the upper bound of full population coverage Pop. Intuitively, $I^{*,markup}$ equals $I^{*,post}$ if the pre-reform markup equals the post-reform markup.

[3] See HKK for proofs of this equation and the subsequent equations.

In addition to calculating the welfare impact of the reform, HKK also calculate the optimal tax penalty π^* that would induce optimal coverage $I^{*,\,opt}$. Optimal coverage is as follows:

$$\begin{aligned} I^{*,\,opt} &= max\Big(0, min\Big(Pop, I^{*,\,pre} + \frac{(P^{*,\,pre} - AC^{*,\,pre}) * (I^{*,\,post} - I^{*,\,pre})}{2(AC^{*,\,post} - AC^{*,\,pre}) - ((P^{*,\,post} - \pi) - P^{*,\,pre})} \\ &- \frac{(AC^{*,\,post} - AC^{*,\,pre}) * I^{*,\,pre}}{2(AC^{*,\,post} - AC^{*,\,pre}) - ((P^{*,\,post} - \pi) - P^{*,\,pre})}\Big)\Big). \end{aligned}$$

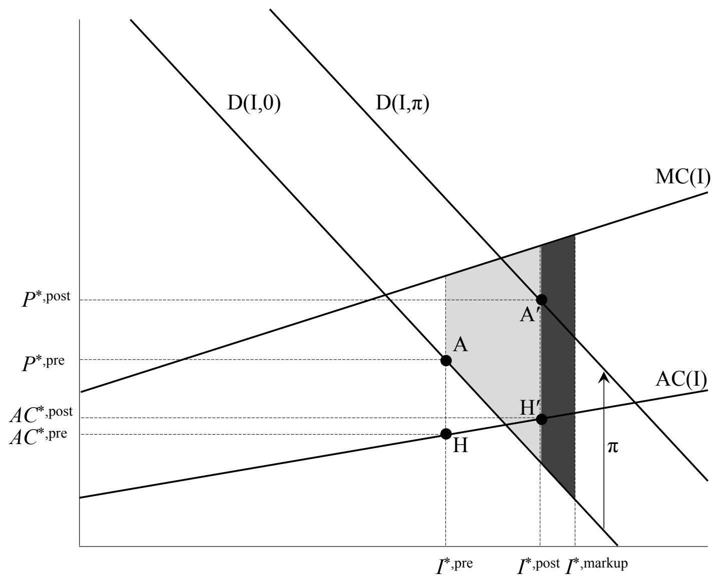
This equation also accounts for the lower bound of zero coverage and the upper bound of full coverage. From optimal coverage, it is possible to calculate the optimal tax penalty π^* as follows:

$$\begin{aligned} \pi^* &= (P^{*,\,post} - P^{*,\,pre}) - (AC^{*,\,post} - AC^{*,\,pre}) \\ &+ \frac{(AC^{*,\,post} - AC^{*,\,pre}) - ((P^{*,\,post} - \pi) - P^{*,\,pre})}{(I^{*,\,post} - I^{*,\,pre})} * (I^{*,\,opt} - I^{*,\,pre}). \end{aligned} \tag{3}$$

We can see from Equation 3 that the optimal tax penalty increases proportionally as the difference between optimal coverage and pre-reform coverage increases. While the optimal tax penalty is sometimes in the range of the actual penalty, when it is not, the assumed linearity of the demand and average cost curves plays a larger role.

As drawn in Figure 2, the market is adversely selected and the post-reform markup is smaller than the pre-reform markup, but Equations 1, 2, and 3 are completely general in the sense that they can also be applied under advantageous selection and increased markups. Figure 3 shows the model under advantageous selection and increased markups. In this scenario, there is a welfare loss from advantageous selection prior to reform because the marginal cost of the last enrollee exceeds her willingness to pay. Therefore, the pre-reform level of coverage $I^{*,\,pre}$ *exceeds* the optimal level of coverage $I^{*,\,opt}$, implying that the optimal penalty is negative. The positive penalty implemented with the reform exacerbates the welfare loss from advantageous selection, and the change in welfare holding markups constant is the sum of both shaded regions. Increased markups mitigate the welfare loss by discouraging some individuals from signing up for coverage, such that the full welfare change from the reform is given by the lighter shaded region. Equation 1 yields the resulting welfare loss.

Figure 3: Model with Markups, Assuming Advantageous Selection and Increased Markups



3 Empirical Implementation of the Model

The natural health insurance market definition is at the state level, so I apply the theoretical model separately within each state. Most pre-ACA insurance regulation was at the state level, and the ACA establishes a separate risk pool for the individual health insurance market in each state (ASPE [2014]).[4] I then compare state-level welfare across states with different policies to isolate the impact of those policies.

3.1 Empirical Implementation By State

As shown above, only seven data moments are needed for identification of the full model, including all welfare-relevant quantities: coverage before the reform $I^{*,pre}$, insurance coverage after the reform $I^{*,post}$, average costs before the reform $C^{*,pre}$, average costs after the reform $C^{*,post}$, premiums before the reform $P^{*,pre}$, premiums after the reform $P^{*,post}$, and the size of the penalty π. With data on these quantities within a state, I could simply plug these data moments into Equations 2, 2, and 3 to obtain the full welfare effect, the net welfare effect, and the optimal penalty.

However, it is likely problematic to do a simple comparison of coverage, premiums, and costs

[4] Risk-adjustment will result in transfers across insurers within a state, so within-insurer analysis would not be relevant to aggregate welfare, motivating our analysis by state.

before and after reform because there are secular and seasonal trends in all of these variables. Therefore, to isolate the impact of reform from secular and seasonal trends, I estimate the impact of reform taking into account seasonal and secular trends. Within each state, I estimate the following equation:

$$Y_t = \alpha^Y (After)_t + \rho_1^Y t + \rho_2^Y (Q1)_t + \rho_3^Y (Q2)_t + \rho_4^Y (Q3)_t + \varepsilon_t^Y \quad (4)$$

where Y_t denotes the respective outcome measure of coverage, average costs or premiums. I estimate a separate regression model for each outcome, obtaining a separate set of coefficients for each outcome, indexed by the corresponding superscript. I use quarterly data from the first quarter of 2008 to the second quarter of 2014. $After$ is a dummy variable equal to one in 2014. I do not include data from the fourth quarter of 2013 in the regression because the open enrollment season had begun but most coverage had not yet begun and the individual mandate had not yet gone into effect. The coefficient of interest for each outcome is α^Y, which denotes the impact of the reform, after taking into account secular and seasonal trends. I account for secular trends with the trend term t and for seasonal trends with the quarterly dummies $Q1$, $Q2$, and $Q3$. Before estimating the regressions, I present graphs that demonstrate the appropriateness of seasonal and secular trends.

Because the 2014 levels of coverage, premiums, and costs are of independent interest without any adjustment for trends, I calculate $Y^{*,post}$ by taking the average of each variable over the first and second quarter of 2014, weighting by average monthly enrollment. I then adjust $Y^{*,pre}$ for seasonal and secular trends as follows:

$$Y^{*,pre} = Y^{*,post} - \widehat{\alpha^Y}, \quad (5)$$

where $\widehat{\alpha^Y}$ is the estimated coefficient from Equation 4. With this transformation of the data, the values of $Y^{*,post}$ are informative summary statistics that capture actual coverage, premiums, and costs in the first half of 2014. The values of $Y^{*,pre}$ are hypothetical values that represent what coverage, premiums, and costs would have been in the first half of 2014 if the ACA had not been implemented.

With this minimal amount of regression adjustment, I can examine whether the pre-reform health insurance market was adversely or advantageously selected, and I can examine whether markups increased or decreased. Assuming that coverage increased, if $C^{*,post} - C^{*,pre} < 0$, then the market was adversely selected, and it was advantageously selected otherwise. Relatedly, markups decreased if $(P^{*,post} - C^{*,post}) - (P^{*,pre} - C^{*,pre}) < 0$, and increased otherwise.

Simply knowing whether the market was adversely or advantageously selected and whether markups increased or decreased can tell us about the sign of the welfare impact of the reform in some specific cases, but in other cases, we need to know the magnitude of the penalty to even know the sign.[5] In all cases, we need to know the magnitude of the penalty to estimate the welfare impact.

To conduct welfare analysis, I choose a baseline value of \$1,500 for π, and I examine robustness to the plausible range of penalties and subsidies based on their statutory values.[6] There is substantial heterogeneity in subsidies and penalties across individuals, so the assumption of a single penalty is arguably a strong one. With individual-level data, I could potentially extend the model to account for heterogeneity in the statutory penalties and subsidies. However, as I discuss below, I do not have individual-level data. Furthermore, given that there is heterogeneity in the penalties and subsidies for the same individuals over time, I would still need an assumption about whether the individuals respond to the contemporaneous penalty or to future penalties. Finally, the behavioral response to the same penalty could differ across individuals based on the perceived penalty and the cost of navigating the individual health insurance market. It is likely that even individuals that are technically exempt from the penalty could respond to it, given the nuance involved in determining who is exempt. Behavioral responses would be difficult to isolate empirically, so I

[5] For example, assume that demand is downward sloping and that coverage increases following reform. First consider the case that HKK found with respect to Massachusetts reform, as depicted in Figure 2. The pre-reform market was adversely selected, and markups decreased, so the full welfare impact was unambiguously positive. However, if the pre-reform market had been adversely selected but the markups had increased, then the full welfare impact would have been ambiguous without further calculation. Similarly, if the pre-reform market had been advantageously selected and markups had increased, then the full welfare impact would have been positive. However, if the pre-reform market had been advantageously selected and markups had increased, as shown in Figure 3, then the full welfare impact would have been ambiguous.

[6] According to CBO [2014], "Beginning in 2014, the ACA requires most legal residents of the United States to obtain health insurance or pay a penalty. People who do not obtain coverage will pay the greater of two amounts: either a flat dollar penalty per adult in a family, rising from \$95 in 2014 to \$695 in 2016 and indexed to inflation thereafter (the penalty for a child is half the amount, and an overall cap will apply to family payments); or a percentage of a household's adjusted gross income in excess of the income threshold for mandatory tax-filing - a share that will rise from 1.0 percent in 2014 to 2.5 percent in 2016 and subsequent years (also subject to a cap)." Subsidies, which are based on income, are benchmarked to the cost of the second-lowest-cost silver plan in the exchanges. According to CBO [2014], "CBO and JCT estimate that the average cost of individual policies for the second-lowest-cost silver plan in the exchanges - the benchmark for determining exchange subsidies - is about \$3,800 in 2014. That estimate represents a national average, and it reflects CBO and JCTs projections of the age, sex, health status, and geographic distribution of those who will obtain coverage through the exchanges in 2014."

proceed by examining robustness to the calibrated penalty. With a calibrated value for the penalty as well as the empirical moments by state, I use Equations 1, 2, and 3 to obtain the full welfare effect, the net welfare effect, and the optimal penalty.

3.2 Empirical Implementation By State Policy Groupings

To make comparisons across states, I first separately calculate the change in welfare within each state, and then I regress state-level change in welfare on indicators for state policies. It would be tempting to simply compare decreases in average costs in one state to decreases in average costs in another state and to claim that the state that experienced greater decreases in average costs was more adversely selected prior to reform. However, if the slope of the demand curve differed across states, this comparison alone would not be sufficient to identify the welfare impact of reform. Thus, it is more informative to compare changes in welfare across states because changes in welfare allow the demand curve to have a different slope in each state.[7]

One drawback of comparing changes in welfare across states is that the model arguably fits less well in some states than in others. For example, one institutional feature that is outside the model is that potential market participants could be excluded from purchasing health insurance before the ACA, especially in states without guaranteed issue and community regulations before the ACA. In those states, the assumption of a single demand curve for all market participants is likely a much stronger assumption than it is in other states. If there are indeed two demand curves pre-reform, one for participants excluded from the market and one for participants included in the market, then the welfare estimates will be biased in a way that is difficult to assess empirically. However, applying the same model to every state imposes a level of discipline. Rather than altering the model for each state or group of states, I use a single model, but I divide states into groups based on their policies, such as community rating and guaranteed issue regulations. I also show changes in coverage, premiums, and costs separately for each state and group of states to show which changes in these variable drive the reported changes in welfare.

[7] Although the slope of the demand curve differs across states, the model assumes that the demand curve shifts according to a constant penalty/subsidy π that does not differ across states. This assumption makes sense given that the penalties and subsidies are set nationally. However, to the extent that state policies themselves shift demand, the model will attribute these shifts to changes in the slope, potentially biasing the welfare results.

4 Data

I use data collected by the National Association of Insurance Commissioners (NAIC) and compiled by SNL Financial. The data include filings from all insurers in the comprehensive individual health insurance line of business, excluding life insurers in all states and Health Maintenance Organizations (HMOs) in the state of California. These data are more comprehensive than data from the health insurance exchanges because they include policies sold outside of the exchanges. Under the ACA, health insurers can sell policies inside and outside of the exchanges, but all policies must be included in the same risk pool (ASPE [2014]). I compare my enrollment estimates to enrollment estimates from the exchanges and survey data in Section 6.

I focus on the most recently-available data from the second quarter of 2014 and back through the first quarter of 2008. Each insurer files quarterly and annual filings with the NAIC, which include enrollment in member months, total premiums collected, and total costs paid. There are 393 insurers that have populated values for member months, costs, and premiums during at least one of our quarters of interest.

Even though much of the regulation of the individual health insurance market is at the state level, the NAIC requires quarterly and annual filings at the insurer level, and some insurers operate in several states. Annual filings are broken down at the insurer-year-state of coverage level, but quarterly filings are only broken down at the insurer-quarter level. Because I am interested in examining the early impact of the Affordable Care Act at the state level without waiting for the annual data, I use quarterly data from the first and second quarters of 2014.

Because I am using quarterly data, I need to make assumptions to allocate the data at the insurer-quarter-state level. I predominantly infer state of coverage by using the corresponding annual filings. For 2014, I use the percentages from the 2013 annual filing, since the 2014 annual filing will not be available until the end of the year. In rare instances, I use supplemental quarterly Schedule T filings to allocate the data by state. Of the 6,727 insurer-quarter observations (393 insurers operating in at least one of 26 quarters), I can uniquely allocate 5,728 to states because the annual data only report coverage in a single state. These observations account for nearly 80% of enrollment in member months, total premiums collected, and total costs paid. In such instances, I allocate all insurer-quarter observations within that given year to the unique state.

For the remaining observations, I make assumptions to allocate the data by state using annual filings and supplemental quarterly Schedule T filings if the annual data are not available. I detail these assumptions in Appendix B. These procedures allow all insurer-quarter observations to be allocated across the 50 states and the District of Columbia.

Before allocating data by state, I take several steps to clean the data, which I detail in Appendix A. The ultimate effect of the data cleaning is rather minor, and as I show in the Online Appendix, the main results are robust to the usage of the raw data instead of the clean data. It is not surprising that the results are robust because I do not do anything to clean data from 2014. The 2014 data are the main basis for the results, and the data from earlier years are just used to estimate pre-trends.[8] I prefer the clean data, which imputes anomalous insurer-quarter observations instead of dropping insurers from all quarters, because dropping insurers would make state totals less meaningful.

Even after data cleaning, the data from California and New Jersey do not appear to be complete. SNL acknowledges that California HMO plans have different NAICS filing requirements, so those data are not complete. The data from New Jersey are also incomplete.[9] I report state-level statistics for California and New Jersey in the interest of transparency, but I exclude them from comparisons across groups of states to prevent data anomalies from driving the comparisons.

5 Summary Statistics

I present state-level summary statistics that are informative in their own right because they paint a picture of the individual health insurance market in the first half of 2014. Furthermore, with only six statistics for each state - coverage, premiums, and average costs before and after reform – I can calculate the state-level impact of the implementation of the ACA on welfare. Simple comparisons of the summary statistics within a state provide an intuitive basis for the welfare impact.

Coverage The first two columns of Table 1 depict average monthly enrollment I, in thousands, by state. $I^{*,post}$ gives average monthly enrollment in the first half of 2014.[10] $I^{*,pre}$ gives an estimate

[8] I include graphs of the data by state using both the raw and the imputed data in the Online Appendix so that the interested reader can examine state trends and the impact of my imputation technique.

[9] New Jersey does not require quarterly filings from insurers that only write business in the state of New Jersey. Accordingly, Triad Healtcare of NJ, which is the largest insurer in New Jersey during the majority of our period, does not report quarterly data during our period of interest.

[10] The data report quarterly enrollment in member months. To obtain average monthly enrollment in the first half of 2014, I sum member months across both quarters of 2014 and divide by 6.

Table 1: Summary Statistics

	Coverage (Monthly Average, Thousands of Persons)		Premium (Monthly Average, $)		Average Cost (Monthly Average $)		Adverse Selection?	Markup Increase?	Exchange Enrollment as % of Post Enrollment	Post Enrollment as % Percent of Population
	$I^{*,pre}$	$I^{*,post}$	$P^{*,pre}$	$P^{*,post}$	$AC^{*,pre}$	$AC^{*,post}$				
AK	10	65	387	346	242	187	1	1	16	10.8
AL	183	174	185	278	159	218	1	1	50	4.0
AR	100	193	179	271	145	181	0	1	19	7.7
AZ	128	179	234	254	183	192	0	1	59	3.1
CA*	871	226	218	243	175	256	1	0	792	0.5
CO	218	257	220	260	184	195	0	1	44	5.4
CT	58	100	335	403	285	250	1	1	66	3.3
DC	13	29	285	291	304	251	1	1	30	5.5
DE	14	21	368	346	265	279	0	0	53	2.9
FL	849	1,204	196	272	157	189	0	1	65	7.8
GA	411	557	188	261	150	167	0	1	48	6.5
HI	33	28	236	242	220	230	1	0	44	1.4
IA	140	180	241	265	206	236	0	0	14	6.7
ID	93	110	202	242	156	200	0	0	60	7.8
IL	330	524	260	326	221	267	0	1	36	4.7
IN	97	225	277	364	222	241	0	1	50	4.0
KS	79	139	153	200	117	166	0	0	37	5.3
KY	140	193	251	290	254	205	1	1	43	4.4
LA	164	227	243	292	174	198	0	1	39	5.6
MA*	326	210	438	479	400	413	1	1	16	3.0
MD	122	236	221	224	175	164	1	1	24	4.8
ME	17	45	413	411	410	247	1	1	78	4.3
MI	246	375	214	281	202	204	0	1	62	4.5
MN	218	274	230	256	192	259	0	0	17	5.3
MO	212	223	223	266	160	201	0	1	64	3.9
MS	49	69	216	265	166	177	0	1	84	2.4
MT	11	21	226	415	224	295	0	1	69	5.2
NC	395	554	240	310	203	212	0	1	56	6.5
ND	43	46	284	310	272	269	1	1	23	6.5
NE	79	89	253	277	208	233	0	0	50	4.6
NH	31	39	333	341	200	192	1	1	71	4.3
NJ*	28	79	530	331	550	298	1	1	190	1.0
NM	63	69	195	318	188	243	0	1	37	4.1
NV	50	106	222	209	187	163	1	1	40	4.1
NY	212	303	354	371	350	238	1	1	101	1.9
OH	289	301	213	280	149	207	0	1	46	2.9
OK	87	135	191	263	156	205	0	1	42	4.3
OR	150	199	235	290	200	271	0	0	30	5.7
PA	484	632	231	284	229	266	0	1	43	5.7
RI	17	32	367	363	315	283	1	1	74	3.7
SC	86	143	224	285	169	180	0	1	68	3.6
SD	63	72	249	274	215	250	0	0	17	9.0
TN	213	270	203	244	165	190	0	1	50	4.7
TX	737	1,037	167	243	153	187	0	1	61	4.5
UT	121	193	179	228	140	175	0	1	41	7.1
VA	278	343	254	280	198	203	0	1	55	4.7
VT	23	27	403	404	379	360	1	1	132	4.6
WA	284	237	285	337	218	259	1	1	69	3.4
WI	109	393	219	225	181	175	1	1	24	10.3
WV	15	27	271	378	270	306	0	1	58	1.9
WY	14	21	329	389	239	286	0	1	49	4.2
Summary*	7,776	10,914	224	280	189	212	16	39	50	4.9
Summary	9,001	11,429	232	282	196	215	19	41	61	4.2

* States with data anomalies omitted from state-level welfare regression analysis. MA is also omitted.

Source: Author's calculations from SNL with exchange enrollment from ASPE and population from Census. Post values are averages from 2014Q1 and 2014Q2, weighted by average monthly enrollment. Pre values are an estimate of what the post value would have been absent the implementation of the ACA. They are obtained by estimating a seasonally-adjusted trend regression for each series from 2008Q1 to 2014Q2, omitting 2013Q4 and allowing for a separate intercept for 2014. The pre value reflects the post value minus the 2014 intercept. See text for more details.

of what enrollment would have been in the first half of 2014 absent the implementation of the ACA, calculated according to equation 5. Therefore, $I^{*,post} - I^{*,pre}$ yields an estimate of the change in individual health insurance market coverage attributable to the implementation of the ACA. In most states, the coverage increase attributable to the ACA is substantial in percentage and level terms. Indeed, only 5 states, including California and Massachusetts, which we omit from our state policy groupings, experienced coverage *decreases* attributable to the ACA.[11] To be clear, those states could have still experienced coverage increases in level terms from 2013 to 2014, but they would not count as coverage increases attributable to the ACA unless they exceeded coverage predicted given pre-reform seasonally-adjusted trends.

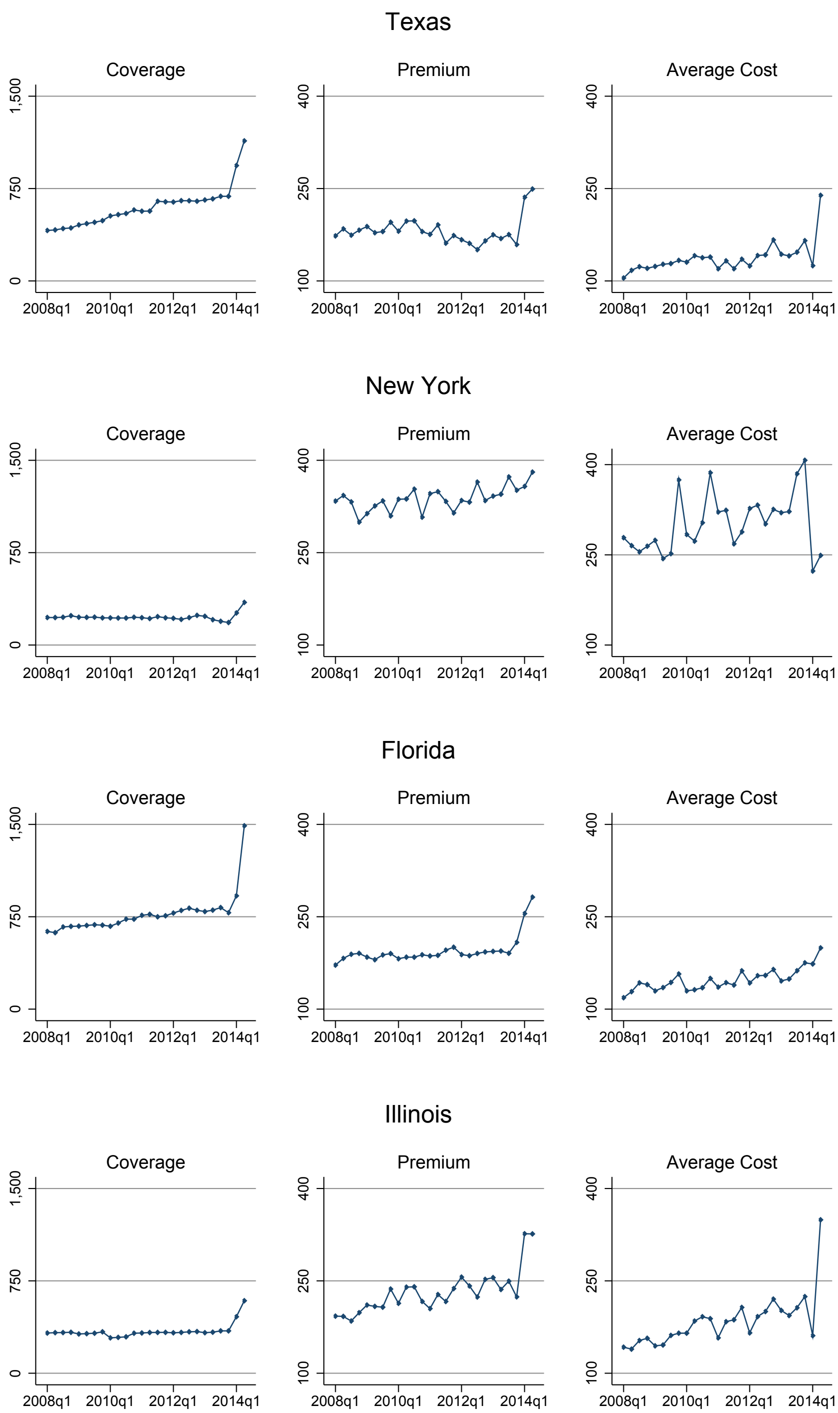
Figure 4 illustrates the importance of taking into account seasonally-adjusted trends by showing quarterly trends in coverage in the four most populous states - Texas, New York, Florida, and Illinois. The subfigures in the left column depict unadjusted coverage trends by quarter from the first quarter of 2008 through the second quarter of 2014. In all four states, there is a striking increase in coverage in the first quarter of 2014 followed by another large coverage increase in the second quarter of 2014. We present unadjusted quarterly data for every state analogous to that in Figure 4 in the Online Appendix. Almost all states show striking increases in coverage in 2014.

Some increase in coverage in the second quarter of 2014 likely reflects new coverage relative to the first quarter, but some is likely an artifact of the aggregation of the data by quarter. Since many people enrolled in coverage just before the open enrollment deadline of March 31, they were covered on March 31, but their average monthly enrollment over the course of the first quarter of 2014 was low. Second quarter average monthly enrollment therefore likely gives a more accurate picture of enrollment at the end of the first quarter.

For our welfare analysis, we aggregate the data across the entire first half of 2014. In this market, the calendar year is the welfare-relevant unit of time because premiums are only set once per calendar year, and individuals purchase coverage through the end of the calendar year. Because data for the full 2014 calendar year are not yet available, we present data from the first half of 2014 in Table 1. However, since it is of independent interest to report national enrollment estimates

[11] As discussed above, we omit California because the SNL data do not include HMO enrollment, which likely increased with reform. We omit Massachusetts because it had a similar reform to the ACA, but the ACA required some changes in Massachusetts, making it difficult to compare Massachusetts to other states. Although the difference between $I^{*,pre}$ and $I^{*,post}$ in Massachusetts indicates that enrollment in Massachusetts declined relative to a Massachusetts-specific seasonally-adjusted trend, enrollment in Massachusetts also declined in absolute terms. Decreases in enrollment in Massachusetts likely reflect problems with the redesign of its state-based exchange.

Figure 4: Trends by State for the Four Most Populous States



that are as up-to-date as possible, we report a table analogous to Table 1 that only uses data from the second quarter of 2014 in the Online Appendix. In those data, we re-estimate the seasonally-adjusted trends so that $I^{*,post}$ takes on slightly different values.

Aggregating $I^{*,post}$ from the first half of 2014 across all states, we find that 11.4 million people were covered in the individual health insurance market, on average in each month for the first six months of 2014. This number understates true coverage in the individual health insurance market because the data do not report enrollment in HMO plans in California and enrollment for one very large insurer is not reported in New Jersey. It also understates true coverage at the end of June 2014 because coverage increased over time - 9.9 million people were covered per month in the first quarter of 2014, and 12.9 million people were covered per month in the second quarter of 2014.

Because not all people enrolled for all three months of the second quarter of 2014, the actual number of people enrolled at many points throughout the second quarter of 2014 was higher than 12.9 million. Although we prefer to use coverage in member months for our main analysis because premiums and costs are monthly, we can obtain a separate quarterly enrollment series from the SNL data. We present state-level statistics from the enrollment series in the Online Appendix. According to that series, there were 13.2 million people enrolled in the second quarter of 2014.

From our summary statistics, we can obtain total enrollment in the individual health insurance market attributable to the implementation of the ACA as the sum of $I^{*,post} - I^{*,pre}$ across all states. Averaged across the first six months of 2014, we find that the coverage increase in the individual health insurance market attributable to the implementation of the ACA was 2.4 million people. Using the quarterly enrollment series, of the 13.2 million people covered in the second quarter of 2014, we attribute 4.2 million to the implementation of the ACA. Stated another way, from before the reform to the second quarter of 2014, national enrollment in the individual health insurance market increased by 32% beyond what it would have had it simply followed state-level seasonally-adjusted trends. We note that enrollment in the individual health insurance market that we attribute to the implementation of the ACA does not necessarily represent new coverage for individuals who were previously uninsured – it could also represent new coverage for individuals who previously had a different type of insurance.

These national estimates complement existing estimates of health insurance enrollment under the ACA. A widely-cited report from the Office of the Assistant Secretary for Planning and Evaluation (ASPE) at the Department of Health and Human Services finds that 8 million people enrolled in health insurance exchanges through March 31, including individuals who enrolled during the additional special enrollment period that was put in place through April 19 for individuals who had attempted to enroll by March 31, the last day of the open season (ASPE [2014]).[12] Our estimate of 13.2 million people covered per month in the second quarter of 2014 is larger for two main reasons: it uses more recent data, and it includes individual health insurance enrollment outside of the exchanges. One strength of my data over the ASPE data is that they allow for the calculation of pre-trends that I can use to isolate the impact of the ACA on enrollment in the individual health insurance market. The ASPE data necessarily do not include enrollment from before 2014 because most of the exchanges began providing coverage in 2014. While all exchange coverage was "new," in some sense, my analysis of pre-trends suggests that only 4.2 million enrollees can be attributable to the ACA nationally. One limitation of my data relative to the ASPE data is that I cannot directly separate exchange coverage from other coverage.

To get a sense of what fraction of coverage in my data is purchased on exchanges, I present ASPE exchange enrollment as a percentage of the SNL quarterly enrollment series in Table 1. Nationally, the ASPE report accounts for approximately 70% of enrollment observed in my data. However, ASPE exchange enrollment as a fraction of enrollment in my data varies dramatically by state, from a low of 14% in Iowa. In some states the fraction exceeds 100%. This occurs most prominently in California and New Jersey, states subject to severe under-reporting of enrollment in my data. In other states, exchange enrollment can exceed enrollment in my data because I allocate total enrollment by state with some error, as discussed in Section 4. This measurement error does not affect my national enrollment estimates.

Beyond the widely-cited figures from ASPE, which are based on administrative data like my own, I can also compare my national enrollment estimates to estimates from other sources. Based on a variety of sources, the CBO projects 6 million people will enrolled on the exchanges over the full course of 2014, which is broadly in line with the ASPE report and my data. Survey estimates differ more substantially. Based on the RAND Health Reform Opinion Study (HROS), Carman and Eibner [2014b] find a much lower estimate of 3.9 million enrolled in exchange plans nationally

[12] HHS Secretary Sylvia Matthews Burwell [2014] announced in September 2014 that 7.3 million people were enrolled in the exchanges and had paid their premiums. The earlier enrollment of 8 million included those who had signed up without yet paying their premiums.

as of March 28, 2014. This estimate is likely low because many interviews took place early in March before the surge in enrollment at the end of the month. The Urban Institute Health Reform Monitoring Survey showed that 5.4 million previously uninsured people gained coverage between September 2013 and March 31, 2014 (Long et al. [2014]). This estimate is not directly comparable to the other estimates because it accounts for marketplace and Medicaid enrollment and it focuses on the previously uninsured. This estimate also does not capture the surge of late March 2014, as most of the data were collected by March 6. McKinsey and Gallup conducted surveys about health insurance coverage in 2014, but I am not aware of any national enrollment estimates based on their results (Bhardwaj et al. [2014], Gallup [2014]). Estimates from often-used national surveys such as the American Community Survey (ACS), the Current Population Survey (CPS), the Behavioral Risk Factor Surveillance System (BRFSS), the Survey of Income and Program Participation (SIPP), the National Health Interview Survey (NHIS), and the Medical Expenditure Panel Survey (MEPS) are not yet available.

To put total enrollment in my data into a context that facilitates better comparison with survey data, I divide total quarterly enrollment in the second quarter of 2014 by 2013 U.S. Census population estimates in the last column of Table 1. I see that Alaska is the state with the largest enrollment in percentage terms, with 10.8% of the population enrolled. Nationally, only 3% of the population is enrolled in the individual health insurance market monthly in the first half of 2013. Given the small fraction of the population enrolled in the market, it will be very difficult to obtain accurate estimates of the impact of national reform on enrollment in the individual health insurance market using survey data unless the survey is very large or very focused. The 4.2 million person individual health insurance market coverage increase that I attribute to the ACA using data from the second quarter of 2014 is only a 1.3 percentage point coverage increase nationally.

Premium In the column labeled $P^{*,post}$, in Table 1, I show that in the first half of 2014, there was wide variation in average monthly premiums paid by state, with insurers in Kansas collecting average premiums per enrollee of \$200 per month and insurers in several other states collecting average premiums per enrollee in excess of \$400 per month.[13] In the vast majority of

[13]The data report total premiums collected separately by quarter for the first two quarters of 2014. To obtain average premiums collected in the first half of 2014, I sum premiums collected in both quarters, and I divide by the sum of enrollment in member months in both quarters such that my statistic is weighted by average monthly enrollment. Movements in premiums over time within a year reflect changes in enrollment into and across plans as

states, premiums went up relative to state seasonally-adjusted trends in the first quarter of 2014. Health insurance premiums almost always go up, but it is striking that they went up so much relative to trend. As shown in Figure 4, premiums in all four of the most populous states increased relative to seasonally-adjusted trends in the first half of 2014.[14] Across all states, from before the reform to the first half of 2014, enrollment-weighted premiums in the individual health insurance market increased by 24.4% beyond what they would have had they simply followed state-level seasonally-adjusted trends.[15]

The premium increase that we observe reflects unsubsidized premiums. Insurers receive the full premiums each month, regardless of whether they are paid by the individual or the federal government [IRS, 2014]. Thus, though our data reflect premiums received by insurers, individuals likely faced smaller changes in premiums after taking the subsidy into account.[16]

An article in Forbes magazine also examines changes in unsubsidized premiums from before to after the ACA by scraping the Internet for premiums for a standardized plan in select counties in 2013 and 2014 [Roy, 2014]. It concludes that the ACA increased individual health insurance market premiums by an average of 49%. This estimate is even higher than my estimate, likely because it is not enrollment-weighted, and individuals in areas with high premiums likely selected cheaper plans.

Aside from the Forbes article, I am not aware of any other sources that estimate premium changes from before to after the ACA. ASPE [2013] examines premium trends before the ACA and Cox et al. [2014] examines premium trends from select cities from 2014 to 2015, finding a widely-cited estimate that unsubsidized premiums will decrease by an average of -0.8% from 2014 to 2015, but these studies do not address premium changes from before to after the ACA. Before the passage of the ACA in 2009, the CBO predicted that the average enrollment-weighted individual health insurance premium would be 10 to 13 percent higher in 2016 under the ACA relative to current law, and the CBO revised their estimate downward by 15% in April of 2014. On the whole, the CBO estimates are in the same ballbark as the estimates borne out in my data. One reason why

premiums for a given plan do not generally change within a year.

[14]The increase in New York was less pronounced, but it started from a much higher level. As we discuss below, New York had a different regulatory environment than the other three states before the implementation of the national reform.

[15]I obtained this number by calculating the percentage change in the monthly enrollment-weighted national average premium, $(P^{*post}_{national} - P^{*pre}_{national})/P^{*pre}_{national}$, excluding Massachusetts, California, and New Jersey.

[16]Discussions with NAIC and SNL confirm that we cannot separately observe subsidizes in our data.

the CBO predicts lower premium increases relative to trend is that it estimated trends prior to the national slowdown in health spending (see Chandra et al. [2013]).

Average Cost In the column labeled $AC^{*,post}$ in Table 1, I report average costs incurred by insurers in the first half of 2014. Average cost decreases are particularly striking in the states where they occurred because just as health insurance premiums almost always go up, average costs do too. In many states, average cost not only went *down* relative to trend, but also average cost went down in absolute terms. Average costs decreased relative to trend in 19 states and increased relative to trend in all others. Nationally, I find that from before the reform to the first half of 2014, average costs in the individual health insurance market increased by 11% relative to state-level seasonally-adjusted trends.[17]

Assuming that plan generosity remained constant, coverage increases combined with decreases in average costs indicates that the pre-reform market was adversely selected (lower-cost people gained coverage after reform). However, a small number of states experienced coverage decreases, so in those states, an increase in average costs indicates adverse selection (because as the market shrunk, healthier people exited). Taking into account reported $(I^{*,post} - I^{*,pre})$ as well as $(AC^{*,post} - AC^{*,pre})$, I indicate those states that exhibit adverse selection with a dummy variable in the column labeled "*Adverse Selection*?". Other states exhibit advantageous selection.

I can compare my estimates of cost changes and adverse vs. advantageous selection at the state level to state-level predictions made in a report by the Society of Actuaries in 2013 for the state of the individual health insurance market in 2017. Relying on survey data from the MEPS and the CPS, the report simulates changes in coverage and costs for each state and the District of Columbia. The report predicts increases in coverage and costs in most states, which are borne out in my data. At the national level, the report predicts a 32% increase in costs as a result of the ACA; however, there is wide variability across states, with cost changes ranging from a decrease of 14% to an increase of 81%. My data also show a great deal of variability in average cost changes, but I estimate a much smaller national cost increase of 11%. Combining the Society of Actuaries predictions for coverage and costs and assuming no change in plan generosity, their predictions imply that five states – Massachusetts, New Jersey, New York, Rhode Island, and

[17] I obtained this number by calculating the percentage change in the monthly enrollment-weighted national average average cost, $(AC^{*post}_{national} - AC^{*pre}_{national})/AC^{*pre}_{national}$, excluding Massachusetts, California, and New Jersey.

Vermont – exhibited pre-reform adverse selection. My data imply adverse selection in all of these states except Massachusetts, which I exclude from analysis for aforementioned reasons.

It is important to note that findings of adverse selection within states are subject to change over time. Because individuals pay their premiums first and then incur costs, average costs could be artificially low relative to premiums in the start of 2014. Indeed, when we infer adverse selection based on data from the first quarter of 2014 alone, as shown in the Online Appendix, we find that a much larger number of states - 32 states - were adversely selected prior to reform. Figure 4 shows that although there was an initial striking decline in average costs in the first quarter of 2014, there was a subsequent, even more striking increase in average costs in Texas and Illinois. However, average costs in New York decreased in the first quarter of 2014 and remained below trend in the second quarter, perhaps due to the influence of its differential pre-reform regulatory environment, which could have exacerbated adverse selection. While average costs patterns are likely to change over time for several reasons, including pent up demand among the newly covered, the relative changes across groups of states with different policies are likely to be more robust. Therefore, we focus on comparing welfare across states rather than within states.

Taking welfare within states at face value for now, we see some evidence that the coverage expansions experienced under the ACA improved welfare by reducing adverse selection in the individual health insurance market. Even given the evidence on average costs, to know the sign of the full welfare impact of the ACA as defined by the model, we also need to show the impact of the reform on markups. Even in the states with pre-reform adverse selection, increased markups could lessen or reverse the welfare gains from reform. The column labeled "$Markup\ Increase$?" reports a dummy variable that is equal to one if $(P^{*,post} - C^{*,post}) - (P^{*,pre} - C^{*,pre}) > 0$, indicating that markups increased. Markups increased in 41 states. As shown in Figure 4, markups increased dramatically in Florida without a corresponding increase in average costs. These changes in markups could reflect uncertainty on the behalf of the actuaries that had to set premiums without knowing the health status of the individuals likely to enroll. If these increases in markups persist, they could result in the ACA having an overall negative welfare impact in the individual health insurance market.

6 Results

6.1 Welfare Results by State

Using only summary statistics presented in the first six columns of Table 1, and three different calibrated values of the annual penalty of $1,000, $1,500, and $2,000, I calculate changes in welfare for each state. For each value of the penalty for each state, I calculate the full change in welfare due to changes in selection and changes in markups according to Equation 1, and I calculate the change in welfare due to changes in selection assuming that changes in markups remained constant according to Equation 2. To make the welfare impacts easier to compare across states, I divide the welfare effects by post-reform enrollment and report $W_{sel}/I^{*,post}$ and $W_{full}/I^{*,post}$ in Table A1. In that table, I also present the optimal tax penalty calculated according to Equation 3. As discussed above, I place more emphasis on comparisons across states than I do on changes in welfare within a state since coverage, premiums, and average costs are still evolving for 2014.

Nonetheless, taking changes in welfare from before to after the ACA within each state at face value, my results show that the reform increased welfare in 11-18 states, depending on the calibrated value of the annual penalty. These welfare increases generally occurred in states in which average costs decreased but increases in markups did not outweigh the welfare gains from reductions in adverse selection.[18] Among the states that we include in the state groupings, at a penalty of $1,500, Maine saw the largest welfare gain. The results indicate a welfare gain of $126 per month per market participant over the first six months of 2014. If this welfare gain persists throughout 2014, it will translate into an annual welfare gain of $1,512 (=126*12) per market participant. In contrast, among the states that we include in the state groupings, Oregon saw the largest decrease in welfare at the same penalty value - a decrease of $66 per market participant, which will translate into $792 annually.

Given the observed full change in welfare, I report the optimal annual penalty $12\pi^*$, for each calibrated value of the annual penalty 12π, for each state. As most states experienced welfare decreases, it is not surprising that I find that the penalty is too large. In most states, I find that

[18]The calculated changes in welfare are still valid under other conditions, but they are more subtle to interpret. For example, the welfare calculation is still valid when demand is upward-sloping, but it is unlikely that demand is actually upward-sloping. In 46 states, demand is downward-sloping for all calibrated values of the penalty. The data for Massachusetts and California suggest upward-sloping demand, giving further credence to our decision to eliminate those states from state groupings.

the optimal penalty is smaller than the calibrated penalty because those states exhibit advantageous selection, so optimal coverage should be lower than observed coverage. Again, I expect the calculated optimal penalty to change with time.

Finally, I report per-enrollee changes in welfare due to changes in selection $W_{sel}/I^{*,post}$. Because changes in markups were so pronounced, it is non-trivial to hold markups constant to calculate the change in welfare due to changes in adverse selection, using Equation 2, leading to nonsensical values in some states. Furthermore, given the observed changes in markups, markup changes could have such important real welfare impacts that it would not make sense to focus exclusively on selection. Therefore, in the analysis that follows by state groupings, we only compare the full welfare impact across states.

6.2 Welfare Results by State Policy Groupings

I compare per-enrollee changes in welfare in the individual health insurance market $W_{full}/I^{*,post}$ across states along eight policy dimensions. As discussed above, the only states that I exclude are California, Massachusetts, and New Jersey, which are denoted with asterisks in the tables. I include the District of Columbia as a "state." I consider the effect of each policy on the state-level welfare impact of the ACA on the individual health insurance market, alone and controlling for other policies.

Direct Enforcement I first categorize states into five mutually-exclusive groups, based on their involvement in the implementation of the ACA. On one end of the spectrum are the five states that ceded all authority to implement the ACA to the Federal government. The federal government refers to these states as the "direct enforcement" states (CMS [2014]). Table 2 and identifies the five direct enforcement states as Alabama, Missouri, Oklahoma, Texas and Wyoming. Figure 5 depicts the direct enforcement states on a map that divides states according to my implementation spectrum. Since support for the ACA is low in direct enforcement states, it is likely that outreach efforts to increase enrollment are less targeted in these states, resulting in lower enrollment of healthy individuals.

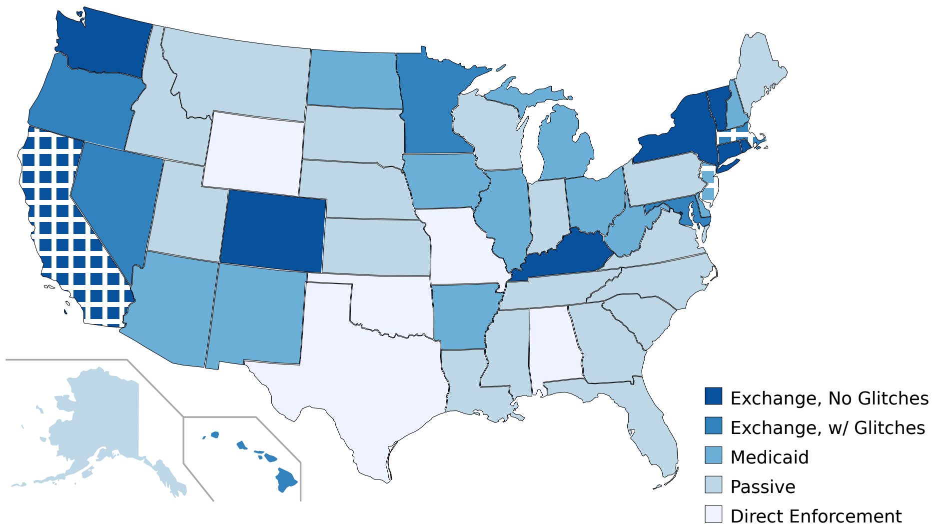
The first row of Figure 6 shows trends in total coverage for groups of states with and without direct enforcement, weighted by enrollment. As shown in the left subfigure, states with direct

Table 2: State Policies

	Direct Enforce-ment	State Exchange	Exchange Glitches	Medicaid Expansion	Non-Grand-fathered Plans	Com-munity Rating	Guranteed Issue	Number of Insurers
AK	0	0	0	0	0	0	0	3
AL	1	0	0	0	1	0	0	1
AR	0	0	0	1	0	0	0	4
AZ	0	0	0	1	0	0	0	6
CA*	0	1	0	1	0	0	0	1
CO	0	1	0	1	0	0	0	13
CT	0	1	0	1	0	0	0	5
DC	0	1	0	1	0	1	0	7
DE	0	0	0	1	0	0	0	4
FL	0	0	0	0	1	0	0	18
GA	0	0	0	0	1	0	0	12
HI	0	1	1	1	1	0	0	3
IA	0	0	0	1	1	1	0	4
ID	0	0	0	0	1	1	1	6
IL	0	0	0	1	1	0	0	9
IN	0	0	0	0	0	0	0	5
KS	0	0	0	0	1	0	0	7
KY	0	1	0	1	1	1	0	7
LA	0	0	0	0	1	1	0	6
MA*	0	1	1	1	0	1	1	12
MD	0	1	1	1	0	0	0	8
ME	0	0	0	0	1	1	1	5
MI	0	0	0	1	1	0	1	23
MN	0	1	1	1	0	1	0	9
MO	1	0	0	0	1	0	0	11
MS	0	0	0	0	0	0	0	2
MT	0	0	0	0	1	0	0	2
NC	0	0	0	0	1	0	0	5
ND	0	0	0	1	1	1	0	4
NE	0	0	0	0	0	0	0	5
NH	0	0	0	1	1	1	0	3
NJ*	0	0	0	1	1	1	1	8
NM	0	0	0	1	0	1	0	3
NV	0	1	1	1	0	1	0	9
NY	0	1	0	1	0	1	1	17
OH	0	0	0	1	1	0	1	15
OK	1	0	0	0	0	0	0	8
OR	0	1	1	1	0	1	1	9
PA	0	0	0	0	1	0	0	22
RI	0	1	0	1	0	0	1	2
SC	0	0	0	0	1	0	0	6
SD	0	0	0	0	1	1	0	6
TN	0	0	0	0	1	0	0	5
TX	1	0	0	0	1	0	0	18
UT	0	0	0	0	1	1	1	6
VA	0	0	0	0	0	0	0	10
VT	0	1	0	1	0	1	1	3
WA	0	1	0	1	0	1	1	11
WI	0	0	0	0	1	0	0	15
WV	0	0	0	1	0	0	1	4
WY	1	0	0	0	1	0	0	3
Summary*	5	13	5	24	26	17	11	8
Summary	5	15	6	27	27	19	13	8

*States with data anomalies omitted from state-level welfare regression analysis. MA is also omitted.
Source: Various, see text for more details.

Figure 5: ACA Implementation Spectrum

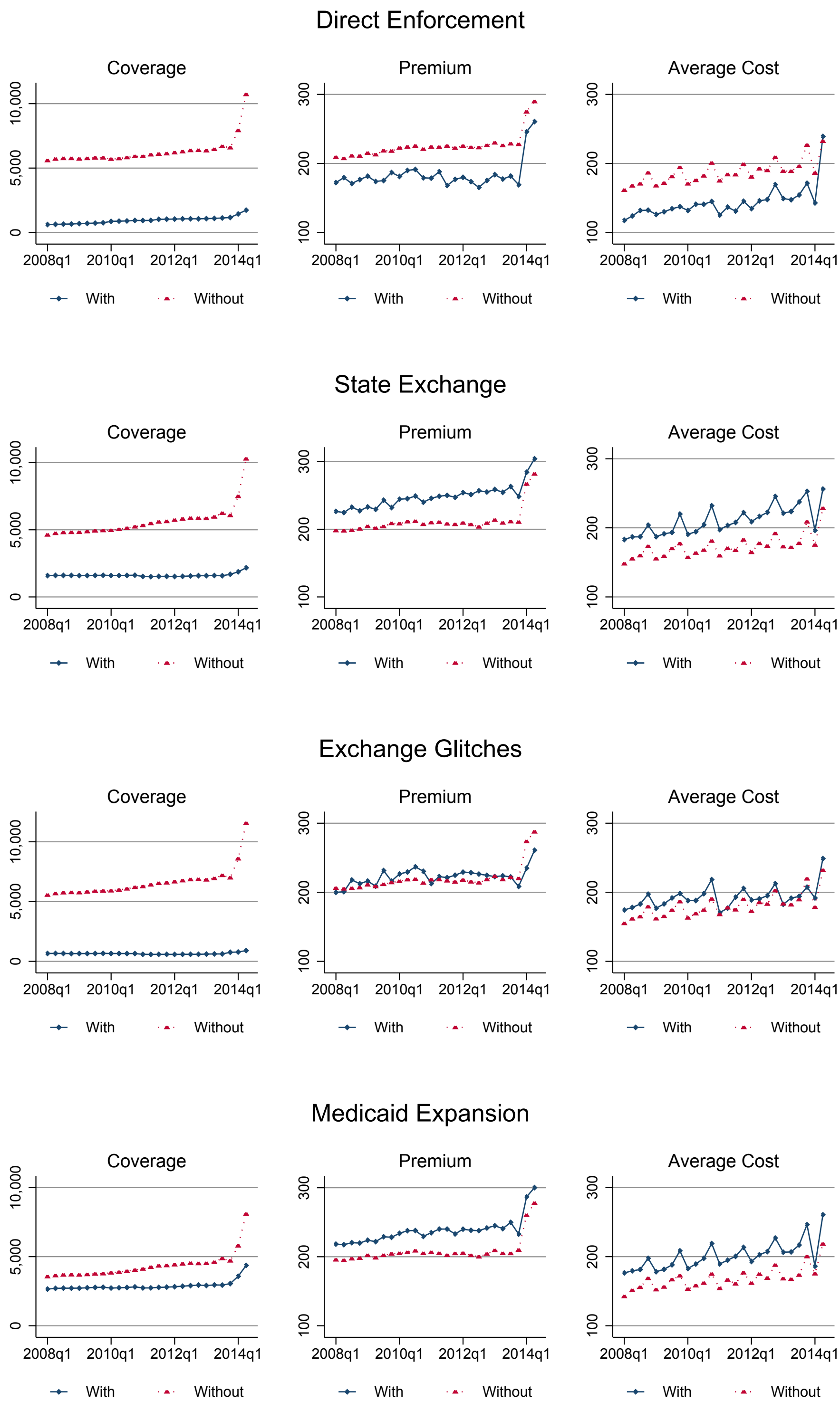


enforcement made up a small share of total coverage before the introduction of the ACA. Although we observe slight coverage increases in states with direct enforcement in the first and second quarters of 2014, increases in coverage were dramatically higher in states without direct enforcement.

The middle subfigure of Figure 6 show trends in enrollment-weighted premiums. Premiums in direct enforcement states began lower than premiums in other states, but they almost caught up in the first two quarters of 2014. As shown in the right subfigure, which shows enrollment-weighted average costs on the same scale, the increase in premiums in direct enforcement states appears necessary to cover the observed increases in average costs. Although average costs in direct enforcement states started out much lower than average costs in other states, they surpassed average costs in other states in the second quarter of 2014. Assuming that plan generosity remained constant, the increase in average costs observed in direct enforcement states indicates that sicker people enrolled in coverage after reform. However, as discussed above, we cannot make solid claims about the welfare impact of ACA due to changes in selection by making comparisons across groups of states without using the model.

In the top panel of Table 3, we present results from a regression in which we regress state-level changes in welfare per enrollee attributable to the ACA, $W_{full}/I^{*,post}$, on a dummy variable for direct enforcement and a constant. In each of the three columns, the underlying data reflect a

Figure 6: Trends by State Policy Groupings



different value of the calibrated penalty π. In the second column, reflecting an annual penalty of \$1,500, we see that enrollees in the individual health insurance market in direct enforcement states are \$23 worse off per month than enrollees in other states. If these losses persist, which I expect they will, at least until the end of 2014, then the annual welfare loss for enrollees in direct enforcement states relative to enrollees in other states will be approximately \$275 (obtained by multiplying the monthly coefficient by 12). Controlling for other state policies that we discuss below in the multivariate regression results shown in the second panel of Table 3, the comparable welfare loss is \$245 per year. Varying the magnitude of the calibrated annual penalty by \$500 around the baseline penalty has a small impact on the estimated losses. In all regression results, this loss is statistically different from zero at the 1% level, according to confidence intervals block-bootstrapped by state.[19]

State Exchange We next compare states based on whether they implemented their own exchanges, following Kaiser Family Foundation [2014b]. As shown in Table 2, 15 states implemented their own exchanges.[20] As shown in Figure 5, states that implemented their own exchanges fall on the opposite end of the implementation spectrum from the direct enforcement states. States that set up their own exchanges were generally states that had stronger enthusiasm for the ACA and thus, they might have solicited enrollment with more enthusiasm. The one countervailing factor, which we consider immediately below, is that several state-based exchanges had high profile implementation glitches which could have affected enrollment.

Figure 6 shows that average costs decreased substantially relative to trend in the first quarter of 2014 in states that implemented their own exchanges, indicating that lower-cost individuals selected into the pool, if we assume that plan generosity remained constant. However, costs picked back up again in second quarter of 2014. Averaging across the first two quarters of 2014, costs were below trend, indicating that there could have been some welfare gains from reductions in adverse selection. However, premiums grew markedly in these states, suggesting potential welfare losses

[19] The block-bootstrapping by state does not account for the prediction of $I^{*,pre}$, $P^{*,pre}$, or $AC^{*,pre}$ in the underlying state-level welfare estimates because those predictions take place *within* states. Block-bootstrapping the data generating process and regressions by state-quarter would account for the prediction of $I^{*,pre}$, $P^{*,pre}$, or $AC^{*,pre}$, but the relevant unit of analysis for our regression is the state and not the state-quarter. The same issues apply to clustering by state. We prefer block bootstrapping to clustering on theoretical grounds because it requires fewer parametric assumptions. In practice, both results yield very similar confidence intervals, and we do not lose statistical significance for any of our estimated parameters if we instead cluster by state.

[20] Idaho and New Mexico have been approved to implement their own exchanges, but they used the federal exchange in 2014, so we consider Idaho and New Mexico to be non-exchange states.

Table 3: Impact of State Policies on Welfare by State

	Calibrated Annual Penalty ($)		
	12π = 1000	**12π = 1500**	**12π = 2000**
	Univariate Regression Results		
Direct Enforcement	-24.64	-23.12	-21.61
	[-47.67,-13.14]***	[-41.26,-11.84]***	[-37.92,-10.75]***
State Exchange	22.26	23.49	24.73
	[-15.55,68.16]	[-15.45,63.47]	[-11.33,66.44]
Exchange Glitches	-17.33	-18.07	-18.81
	[-51.77,31.79]	[-50.71,28.03]	[-52.7,26.44]
Medicaid Expansion	7.45	8.32	9.18
	[-16.51,33.21]	[-15.58,32.54]	[-11.38,35.79]
Non-Grandfathered Plans	-18.51	-18.45	-18.39
	[-49.03,8.4]	[-45.22,5.68]	[-47.2,6.37]
Community Rating	10.13	11.85	13.57
	[-22.37,50]	[-20.29,47.85]	[-16.88,49.45]
Guaranteed Issue	9.41	11.45	13.48
	[-29.96,62.94]	[-26.57,60.01]	[-24.55,59.07]
Number of Insurers	-0.52	-0.52	-0.52
	[-1.96,1.83]	[-1.93,1.49]	[-1.83,1.69]
	Multivariate Regression Results		
Direct Enforcement	-22.72	-20.39	-18.07
	[-57.44,-8.74]***	[-50.38,-6.74]***	[-47.43,-5.73]***
State Exchange	46.73	48.67	50.60
	[-2.52,101.03]*	[-0.72,99.50]*	[3.22,98.17]**
Exchange Glitches	-60.45	-62.94	-65.43
	[-129.97,6.39]*	[-123.82,6.73]*	[-125.60,4.63]*
Medicaid Expansion	-13.60	-13.15	-12.70
	[-39.07,11.32]	[-35.55,9.23]	[-35.67,11.21]
Non-Grandfathered Plans	-11.54	-10.62	-9.70
	[-37.12,13.59]	[-33.11,13.44]	[-29.74,16.35]
Community Rating	-3.78	-2.68	-1.59
	[-32.14,32.48]	[-28.39,29.07]	[-29.46,27.70]
Guaranteed Issue	1.44	2.87	4.31
	[-38.93,39.35]	[-32.01,39.44]	[-27.49,44.18]
Number of Insurers	-0.31	-0.34	-0.37
	[-2.48,2.22]	[-2.31,2.05]	[-2.28,1.69]
Constant	1.63	-5.81	-13.25
	[-19.79,32.55]	[-28.74,20.73]	[-33.39,12.03]

Each column of the multivariate regression results reports all coefficients from a single state-level regression of the welfare impact of the ACA for a given calibrated annual penalty on state policy variables and a constant. Each cell of the univariate regression results reports the coefficient from a separate regression on each policy variable and a constant (coefficient not reported). See text for more details.

*, **, *** indicate significance at the 10%, 5%, 1% levels, respectively, block-bootstrapped by state.

from increased markups.

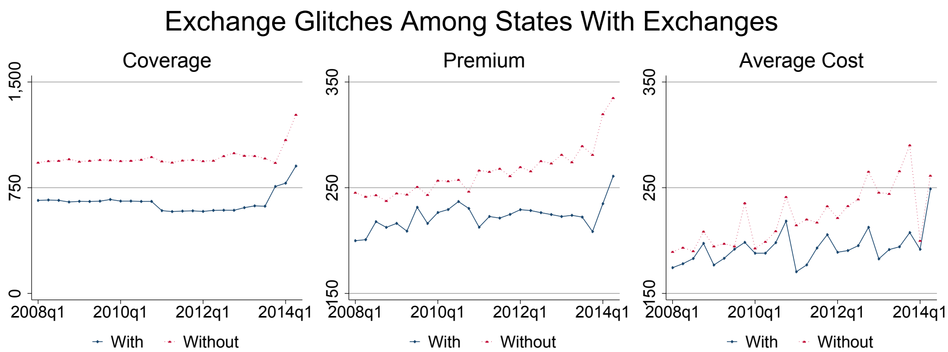
In Table 3, the regression results show that if we do not control for any other state-level policies, at the baseline penalty value, enrollees in states that set up their own exchanges were better off by about \$23.50 per month, which will translate into \$282 (=23.49*12) annually. This coefficient is not statistically different from zero, but it doubles and becomes statistically different from zero at the 10% level when we control for whether the exchange had an implementation glitch and whether the state expanded Medicaid, among other policies. We defer interpretation of the latter results until we have considered these two other policies.

Exchange Glitches Exchange glitches reflect states policies in the sense that states that set up exchanges were responsible for the selection of a vendor. In our characterization of state policies, only states that set up their own exchanges had implementation glitches, even though the federal exchange had its own difficulties. Following Dash and Thomas [2014] and other widespread media reports, six states with their own exchanges - Hawaii, Maryland, Massachusetts, Minnesota, Nevada, and Oregon - had severe technology problems. Several sources have questioned whether these technology problems could have lasting affects on the welfare impact of the ACA, including research by Scheuer and Smetters [2014]. If those snags deterred lower-cost individuals from navigating the system to purchase coverage before the open season ended, then the reductions in adverse selection expected with the implementation of the ACA might not have been as great. Furthermore, the high future premiums necessitated by current adverse selection could deter future enrollees.

In the third panel of Figure 6, we compare states with exchange glitches to all other states, and we do not notice any remarkable patterns. However, the impact of glitches is more salient when we restrict our focus to states with state exchanges in Figure 7. In this figure, there is no clearly visible hindrance to enrollment in states with glitches. In fact, enrollment began increasing in states with glitches in the fourth quarter of 2013, sooner than it increased in other states. Furthermore, states with and without glitches experienced similar changes in premiums, which is to be expected given that actuaries would not have known in advance which states would experience glitches.

Though enrollment and premium trends are similar, there is a clearly visible difference in average costs. In the states with well-functioning state exchanges, average costs decreased substantially in the first quarter of 2014 while remaining in line with trends in the comparison states. This decline is particularly striking because it seems intuitive that states without glitches would have to start paying claims sooner because their beneficiaries could enroll sooner. While average costs increased in the second quarter of 2014 in states with well-functioning state exchanges, they remained below trend, suggesting that those states succeeded in enrolling healthier individuals if we assume constant plan generosity. In contrast, states with exchange glitches saw marked increases in average costs in the second quarter of 2014.

Figure 7: Trends by State Policy Groupings

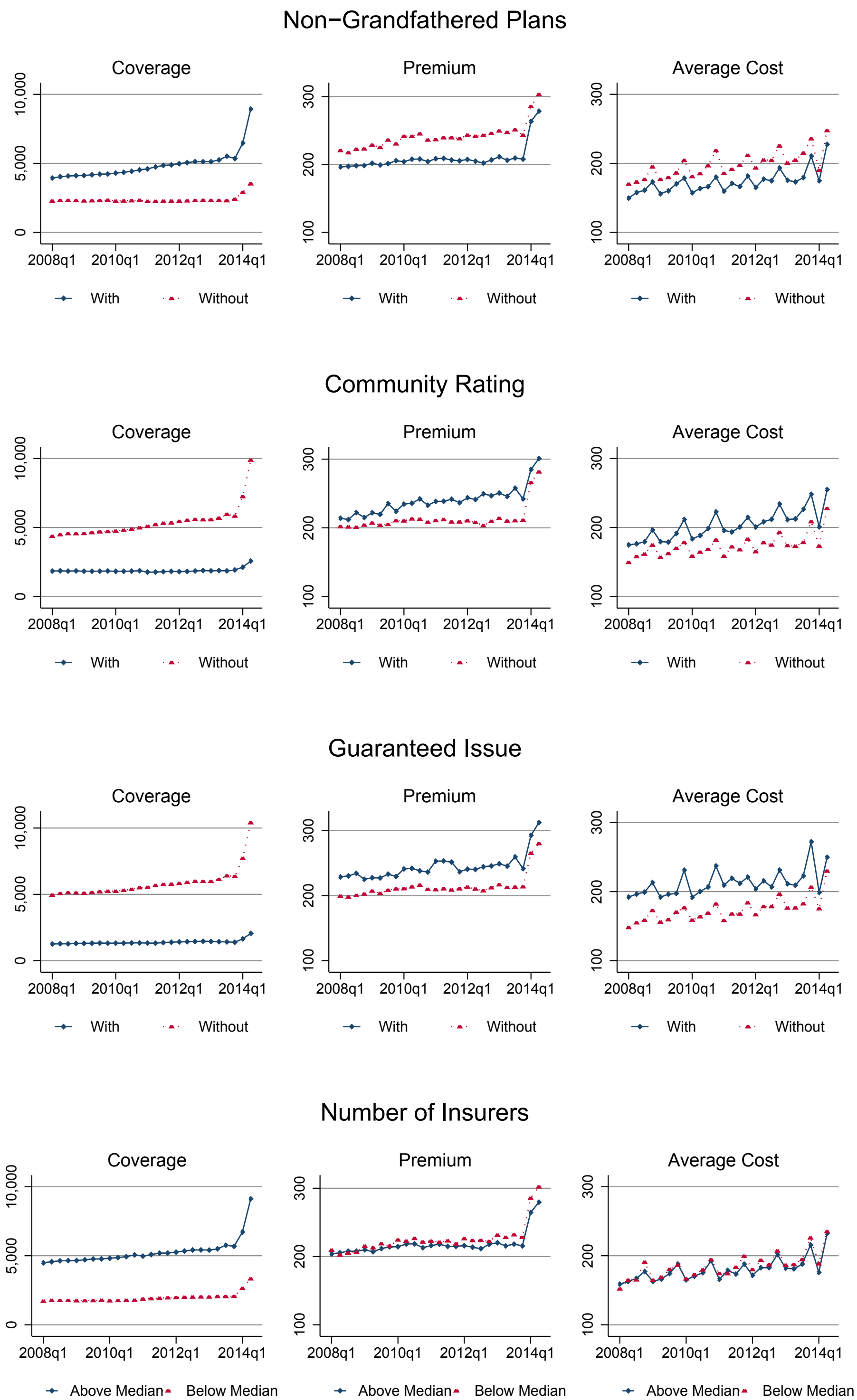


Given this visual evidence, the regression evidence shown in Table 3 is not surprising. The coefficients show that states with exchange glitches are worse off than other states. The difference is not statistically significant in the univariate regression, but it triples in magnitude and becomes statistically significant at the 10% level in the regression that controls for other state policies, including whether states expanded Medicaid. We defer the interpretation of the magnitude until we have considered state policy on the Medicaid expansion.

Medicaid Expansion The Supreme Court gave the states the power to decide whether to expand Medicaid as legislated by the ACA. 27 states are currently implementing the Medicaid expansion, and Pennsylvania is set to implement it starting in 2015 (Kaiser Family Foundation [2014b]). In states that implemented the Medicaid expansion, fewer individuals might have turned to the individual health insurance market for coverage because Medicaid was available to them. In that case, the impact on adverse selection depends on whether the Medicaid-eligibles are higher or lower cost than other participants in the individual health insurance market.

Using Figure 6, we can examine trends in states that implemented the ACA Medicaid expansion

Figure 8: Trends by State Policy Groupings



vs. those that did not. In the bottom left subfigure, we see that states that did *not* implement the Medicaid expansion saw greater increases in individual health insurance market coverage than other states. It could be the case that more individuals turned to the individual health insurance market for new coverage in states in which they were not eligible for Medicaid. It could also be the case that individuals who had individual health insurance market coverage exited it for Medicaid coverage. Whatever the mechanism, if the new Medicaid eligibles were sicker than the rest of the population and the Medicaid expansion crowded them out of the individual health insurance market, then I expect a differential decrease in average costs in those states that implemented the expansion. Indeed, such a decrease is visible in the first quarter of 2014, as shown in Figure 6. However, average costs increased dramatically in the second quarter of 2014 such that average costs over the first half of 2014 were only slightly lower than predicted.

Indeed, the univariate regression results in the bottom of Table 3 show suggestive evidence that states that adopted the Medicaid expansion were better off than all other states by approximately $100 (approximately 8.32*12) per year, but this difference is not statistically distinguishable from zero. At first glance, the multivariate regression results appear to show a different story. However, the Medicaid expansion is highly correlated the three other policies that we have discussed, so we must consider them all simultaneously.

As shown in Table 2, all 15 states that set up their own exchanges also implemented the Medicaid expansion. Another group of states took less of an active role by implementing the Medicaid expansion but not setting up an exchange. Therefore, we can fill in the middle of the ACA implementation spectrum shown in Figure 5 with the 12 states that implemented the Medicaid expansion but did not set up their own exchanges. The final group of "passive" states were not so extreme as to leave direct enforcement to the federal government, but they did not set up a state exchange, and they did not implement the Medicaid expansion—these states are in the category omitted from the first four state policies presented in the multivariate regression results. (The next four included state policies included in the multivariate regression do not fit neatly onto this spectrum).

Therefore, the coefficient on "Medicaid expansion" in the multivariate regression results gives the welfare impact of deciding to expand Medicaid, among states that did not opt for direct enforcement on one side of the spectrum or for a state exchange on the other side of the spectrum.

To recover the welfare impact of setting up a state exchange with a glitch relative to the "passive" states, we add together the coefficients on "State Exchange," "Exchange Glitches," and "Medicaid expansion." We find the participants in the individual health insurance market in states that set up exchanges with glitches were worse off than they would have been had their states been "passive" - at the baseline value of the calibrated penalty, they were worse off by \$27 (=48.67-62.94-13.15) per month or \$330 (=27.42*12) annually. In contrast, participants in states that set up well-functioning exchanges were better off than they would have been had their states been "passive," by \$426 (=(48.67-13.15)*12) annually. Therefore, the impact of having an exchange glitch far outweighs the impact of the other policy decisions that we have considered thus far. Market participants in the six states that had severe exchange glitches are worse off by approximately \$750 (approximately 330+426) per participant on an annualized basis, relative to participants in other states with their own exchanges.

Non-Grandfathered Plans Next, I compare states on the basis of whether they allowed beneficiaries to renew non-grandfathered plans that did not meet the standards for coverage required by the ACA. As shown in Table 2, just over half of states allow renewal of non-grandfathered plans (NCSL Health Reform Task Force [2013]). The decision to allow non-grandfathered plans appears to be separate from other state decisions, as direct enforcement states, state exchange states, and Medicaid expansion states do not have uniform policies on non-grandfathered plans.

If the beneficiaries enrolled in non-grandfathered plans are lower-cost than other beneficiaries, then the general individual market health insurance pool might have experienced smaller reductions in adverse selection with the implementation of the ACA. The Congressional Budget Office (CBO), the Joint Committee on Taxation (JCT), a former Centers for Medicare and Medicaid Services (CMS) senior actuary, and researchers from the Rand Corporation all predict that healthier individuals will remain in non-ACA-compliant plans, but they differ in their assessment of whether the market-level impact will be large or small (See CBO [2014], Bertko [2014], and Saltzman and Eibner [2014]). I address this question empirically.

Figure 8 shows trends in states that allow renewal of non-grandfathered plans relative to trends in all other states. As shown in the top left subfigure, states with non-grandfathered plans clearly experienced greater coverage increases in absolute terms and relative to trend than other states.

Perhaps allowing individuals to keep their plans encouraged them to remain in the individual health insurance market instead of seeking other forms of coverage. As shown in the next two subfigures, though premiums increased more in states with non-grandfathered plans, it is difficult to discern any differential movements in average costs.

In the top panel of Table 3, the coefficients on "Non-Grandfathered plans" gives suggestive evidence that participants in the individual health insurance market in states that allow renewal of non-grandfathered plans are worse off from the implementation of the ACA by approximately \$18 per month, \$221 (=18.45*12) annually. However, this difference is not statistically different from zero. If we control for other state policies, we find results that are half as large, and they are still not statistically different from zero. Therefore, we see suggestive evidence that the allowed renewal of non-ACA complaint plans has a negative impact on the individual health insurance market, but time will tell if this evidence is conclusive.

Community Rating and Guaranteed Issue We next compare states on the basis of two individual health insurance market regulations that are often implemented jointly. First, we compare states on the basis of "community rating" regulations that require all health insurers to charge the same price to all beneficiaries, regardless of observable characteristics. As shown in Table 2, 19 states had such restrictions before the implementation of the ACA (Kaiser Family Foundation [2014a]). These regulations could exacerbate adverse selection by increasing asymmetric information between insurers and beneficiaries: if insurers cannot charge lower prices to healthy beneficiaries and must instead charge the average price to all beneficiaries, only sick beneficiaries will find it worthwhile to enroll and the community rated price will be the average price for the sick.

Second, we compare states on the basis of "guaranteed issue" regulations that prevent insurers from denying coverage to applicants, regardless of their health status. As shown in Table 2, 13 states had such restrictions before the implementation of the ACA, 4 of which did not have accompanying community rating regulations.[21] These regulations alone need not induce adverse selection. However, they could exacerbate adverse selection in the presence of community rating regulations because in the presence of both regulations, insurers must accept all comers and charge them the same price.

[21]We define the guaranteed-issue states as states in which all insurers must issue all or some products, either periodically or continuously (Kaiser Family Foundation [2013].

The ACA establishes community rating and guaranteed issue regulations nationally in 2014. These regulations have been some of the most popular provisions of the ACA because people like the idea of being able to purchase health insurance regardless of health status at a uniform price. However, in the absence of the individual mandate, one of the least popular provisions of the ACA, these regulations could exacerbate adverse selection. By comparing states with these regulations before and after the implementation of the ACA, I isolate the impact of these regulations from the individual mandate.

As shown in the middle rows of Figure 8, states with community rating and guaranteed issue regulations experienced coverage increases smaller than those experienced in other states. Given particular interest in the welfare cost of adverse selection imposed by community rating and guaranteed issue regulations, we are especially interested in differential changes in average costs before and after the ACA for states that already had those regulations relative to states that implemented them with the ACA. As discussed above, we expect more adverse selection in states with community rating and guaranteed issue regulations. Therefore, holding the slope of the demand curve and its shift constant, we expect a greater decline in average costs in states with these regulations. Such a pattern could be there, but it is difficult to discern graphically.

Turning to the regression results in Table 3, we see that when we examine community rating and guaranteed issue regulations individually, not controlling for any other state policies, the signs of the coefficients suggest that states with these policies had higher welfare gains from the establishment of the ACA than other states. Multiplying the community rating or guaranteed issue coefficient from the middle column by 12 suggests that individuals in states with either one of these regulations will better off from the implementation of the ACA by approximately \$140 annually, possibly because these regulations induced, or exacerbated, adverse selection in the pre-ACA market.

These estimated welfare gains for states with community rating/guaranteed issue regulations under the ACA are less than half as large than the annual welfare gains of \$442 per person experienced in Massachusetts following its reform, as calculated by HKK. Massachusetts had community rating and guaranteed issue regulations that could have exacerbated adverse selection before its reform. However, HKK shows that Massachusetts experienced welfare gains from reductions in adverse selection *and* from decreases in markups, whereas most states seem to have experienced increases in markups under the ACA. HKK use annual SNL filings, as opposed quarterly filings, so comparison to Massachusetts will be more conclusive when the 2014 annual filings become available.

Although it is interesting to analyze the magnitudes of the community rating and guaranteed issue coefficients, they are not statistically different from zero. Furthermore, the community rating coefficient changes sign in the multivariate regression. Therefore, these results are inconclusive as of the second quarter of 2014.

Number of Insurers Finally, I compare states on the basis of how many insurers were operating in their individual health insurance markets in the third quarter of 2013, just before ACA open enrollment began. Although the number of insurers in the market is not technically a state policy, it could reflect other state policy decisions. I obtain the number of insurers directly from the SNL data. As shown in the last column of Table 2, there was widespread variation in the number of insurers by state before the implementation of the ACA, from three or fewer in 11 states to 9 or more in 17 states.[22] We might expect the individual health insurance market to function better in states with more insurers (see Dafny [2010], Haislmaier [2013], and Dafny et al. [2014]). Therefore, to the extent that the individual health insurance market was already functioning well in states with more insurers, the welfare impact of the ACA might not be as positive in those states. Conversely, if states with more insurers have better-functioning markets, then the implementation of the ACA might also go more smoothly, leading to higher welfare.

In the last row of Figure 8, we compare states with an above-median number of insurers to states with a below-median number of insurers, and we see that states with more insurers saw greater increases in coverage under the ACA. Differential patterns in premiums and average costs are difficult to discern. Turning to the regression results in the top panel of Table 3, we see that for each additional ten insurers in the market before the reform, state-level welfare from the implementation of the ACA was lower by \$5.20 per participant on a monthly basis and \$62 (=5.2*12) on an annual basis. However, the coefficient is not statistically different from zero, and its magnitude decreases by a third when we control for other state policies. Therefore, we see no statistical relationship between the number of insurers in a state before the reform and the welfare impact of the ACA.

[22] Not all insurers operating in a given state offer coverage statewide. Furthermore, we overstate the number of insurers in some sense because "insurers" in our data can be carriers under the same parent company. However, comparisons of the total number across states should still be informative.

6.3 Robustness

As discussed, even though the premium and average cost data are measured at the same time, they could contain differential information because actuaries must set premiums and individuals must pay them before incurring any costs. To exploit the differential informational content of each data series, we can conduct two separate exercises: we can use premium data to measure premiums *and* average costs in the model, or we can use average cost data to measure average costs and *premiums* in the model. To interpret the results, we can assume that markups are zero but only selection changes.

By using premium data to measure average costs, we can get a sense of what the actuaries expected to happen to average costs in each state before anyone enrolled (keeping in mind that the premium data do contain some information on enrollment insofar as that the weighted average premiums reflect the generosity of the selected plans). For each exercise, we present results analogous to Table 3 and Table A1 in the Online Appendix. The signs and magnitudes of the univariate regression results in the analog to Table 3 suggest that actuaries generally expected selection to vary across state policy groupings along the lines that we observe using the full data. However, using the premium data alone yields a large and statistically significant positive coefficient on welfare in states with exchange glitches, which stands in contrast to the smaller and less significant negative coefficient that we obtain with the full data. This finding gives credibility to our results because it suggests that the selection that we observe was real and foreseen by the actuaries, apart from the exchange glitches, which actuaries would not have foreseen. Turning to other coefficients, using the premium data alone yields a larger and more statistically significant coefficient on "community rating," suggesting that the actuaries expected even greater changes in selection in states with previous community rating regulations than we observe using the full data. Perhaps the actuaries overestimated the impact of existing community rating regulations, or perhaps the observed impact of those regulations will sharpen as time passes.

Next, by using average cost data to measure premiums, we can get a sense of what the longer run market equilibrium might look like if the markups that we currently observe return to pre-reform levels. In this exercise, measured changes in welfare reflect changes in selection, but they do not account for changes in markups. Comparing the univariate regression results from the analog of Table 3 to those in Table 3, we see that the signs and magnitudes of the welfare impacts are similar. This comparison suggests that even though we see large changes in markups in many states, changes in selection drive the reported differences in changes in welfare.

7 Comparison to Existing Empirical Evidence on Selection

This paper contributes empirical evidence to a growing literature on the welfare impact of adverse selection. Adverse selection is a key market failure from a theoretical perspective (Akerlof [1970]; Rothschild and Stiglitz [1976]), but there is little work on its magnitude from an empirical perspective. The early empirical literature focused on testing for the presence of adverse selection, but it did not establish whether the welfare cost of selection was large or small (Chiappori and Salanie [2000]; Finkelstein and Poterba [2006]). Accordingly, the large existing empirical literature on community rating and guaranteed issue regulations suggests they lead to adverse selection, but it does not quantify the welfare cost (see, for example, Ericson and Starc [2012] and my own previous joint work: Kowalski et al. [2008]).

The more recent empirical literature has established how to measure the welfare cost of adverse selection (EFC; Einav et al. [2010b]; Bundorf et al. [2012]), and it provides empirical estimates. However, it generally has focused on empirical contexts in which adverse selection is likely to have a small welfare cost. These contexts focus on intensive margin (across insurance plan) selection for individuals with access to employer-sponsored health insurance. However, there is reason to expect that the extensive margin (insured vs. uninsured) selection among individuals without access to employer-sponsored health insurance could be larger, if only because the individual mandate is intended to address this type of selection.

Hackmann et al. [2012] and HKK examine extensive margin selection using variation induced by the implementation of the Massachusetts health reform of 2006. The results show that the Massachusetts individual health insurance market was adversely selected before the reform and that markups decreased after the reform. The total welfare gain in Massachusetts was large - around 8.4% of insurer expenditures, or $442 per person annually - which is roughly twice as large as the welfare cost of intensive-margin selection found by EFC. However, it is unclear if the Massachusetts results will generalize to other states.

Using data from the first quarter of 2014, we see that most states experienced welfare gains from decreases in adverse selection, as Massachusetts did. However, data through the first half of 2014 show advantageous selection in most states. Massachusetts experienced decreases in markups, but data from the first quarter and first half of 2014 show markup increases in most states. We cannot say conclusively if the Massachusetts experience will generalize to other states because the data on coverage, premiums, and costs are still evolving. However, the current finding that higher-cost individuals entered the pool in most states stands in stark contrast to the more established finding that lower-cost individuals entered the Massachusetts pool after its reform. One potential driver of the difference is that individuals who obtained subsidized coverage in Massachusetts had to purchase it through the "CommCare" exchange, which was separate from the unsubsidized exchange and excluded from the HKK analysis. In contrast, under national reform, individuals who obtain subsidized coverage must obtain it through the same exchange that offers unsubsidized coverage. If individuals who are eligible for subsidized coverage have higher costs than other individuals, then they could drive the increases in average costs observed in most states in the first half of 2014, but they would not have appeared in the Massachusetts pool.

In Massachusetts, existing participants in the individual health insurance market did not have to cross-subsidize new subsidized participants through higher premiums after its reform because there were two separate exchanges, but the results suggest that participants in some states had to do so after national reform because there was only a single exchange. To the extent that existing participants in the individual health insurance market were already a vulnerable group in the sense that they did not have employer-sponsored coverage, which is generally cheaper and more generous than individual market coverage, it is undesirable that this population would have to cross-subsidize new subsidized enrollees through higher health insurance premiums *and* higher tax payments, whereas individuals with employer-sponsored coverage would only cross-subsidize new subsidized enrollees through higher tax payments. As individual-level data become available, it will be interesting to investigate whether the newly subsidized individuals do indeed have higher costs than previous participants.

For the purposes of this paper, we cannot use Massachusetts as a reliable control group for other states. Massachusetts is different from other states in many ways, but the main reason why we cannot use Massachusetts as a reliable control group here is that empirically, it experienced anomalous decreases in enrollment after the ACA. These enrollment decreases were likely due to substantial changes that Massachusetts made to its exchange. Even though there is no reliable control state that did not experience the implementation of the ACA, by focusing on comparisons between groups of states instead of comparisons within states, we can better control for national trends and for changes in data reporting after the influx in coverage. Furthermore, we can examine the welfare impact of some state policies as well as the impact of the ACA.

Several policies that potentially affect the individual health insurance market do not vary by state, and this analysis holds them constant. For example, tax subsidies for employer-sponsored health insurance could affect selection into the individual health insurance market. The availability of bankruptcy as a backstop for medical bills in the absence of insurance could also affect selection into the individual health insurance market (see Mahoney [2012]). However, in this paper, instead of being totally agnostic about potential sources of adverse selection, the state-level comparisons allow us to isolate the impact of some state-level policies.

8 Conclusion

I examine the impact of state policy decisions on the early effects of the ACA, focusing only on the individual health insurance market. This is an important market to study because many of the uninsured turn to this market for coverage. The overall impact of the ACA will depend on impacts on several other markets, so findings that imply that individual health insurance market participants in some states were "better off" or "worse off" do not capture the overall impact of the ACA. Even in the states where I find that participants in the individual health insurance market were "worse off," the overall impact of the ACA could be positive.

Using a model developed by Hackmann, Kolstad, and Kowalski (2013), I examine the impact of the ACA on adverse selection and markups in the individual health insurance market state-by-state. Estimates from my model imply that market participants in the five "direct enforcement" states that ceded all enforcement of the ACA to the federal government are worse off by approximately $245 per participant on an annualized basis, relative to participants in all other states. They also imply that the impact of setting up a state exchange depends meaningfully on how well it functions. Market participants in the six states that had severe exchange glitches are worse off by approximately $750 per participant on an annualized basis, relative to participants in other states with their own exchanges. My estimates provide suggestive evidence that participants in states that allowed renewal of non-grandfathered plans are worse off than participants in other states. They also provide inconclusive evidence that participants in states with pre-ACA community rating and guaranteed issue regulations are better off than participants in other states. The estimates imply further inconclusive evidence regarding the impact of having more insurers in the pre-ACA state market.

This paper relies on data from the first half of 2014, and the national experience might evolve over time. Given that the open season for coverage on the exchanges ended at the end of the first quarter, enrollment is unlikely to change dramatically in the short term. However, it might be the case that even though newly-insured individuals paid their premiums in the first half of 2014, they will use their coverage with a lag, resulting in smaller markups as the year progresses. As long as the cost lag does not vary along the same dimension as other state policies (and we have no reason to expect that it will), what we have learned by comparing states with different policies should be more robust than what we have learned nationally. The differential impact of state policies is likely to be stable in the short term, at least until the next open season for coverage and likely until those policies are changed.

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Appendices

A Data Cleaning

The underlying SNL data at the insurer-quarter level display several anomalies, such as missing, negative, or extreme values for enrollment, coverage, premiums, and costs. My discussions with SNL suggest that these anomalies persist because the NAIC does not have regulatory authority over the insurers that submit the filings. To address these anomalies, I perform several cleaning techniques before allocating the data by state.

I begin the data cleaning process by first identifying the periods of time for which each firm is active in the market. I define the active period as the period that begins when a firm first appears with non-zero, positive enrollment, premiums, and costs and ends when the firm no longer appears with non-zero, positive enrollment, premiums, and costs. This definition assumes that there are no firms that enter the market, exit the market, and then re-enter the market at a later period.[23] Once I have identified the non-defunct periods of operation for firms, I drop the defunct insurer-quarter observations from the sample.

One relatively common data anomaly appears to be that insurers file annual numbers in a single end-of-year filing rather than in quarterly reports throughout the year.[24] In the case of this data anomaly, I allocate the values reported in the fourth quarter across the entire year, in proportion to an estimated seasonally-adjusted trend for the given firm from the first quarter of 2008 through the third quarter of 2013. (I do not include later data, which could be influenced by health reform). I apply this treatment only to the larger firms that are capable of having a substantial impact on the state-level analysis. Nevertheless, it should be clear that this method of imputation is a clear improvement over using the raw data. Fortunately, this data anomaly does not seem to be a major concern for the 2014 data. Throughout the period from Q1 2008 through Q4 2013, the prevalence and severity of this data anomaly decrease substantially. In 2008–2010, this type of data error affected firms accounting for nearly 6% of enrollment in terms of member months. By 2013, however, the comparable figure drops to 0.2% of coverage. In addition, firms that appear in the data during 2013 but not in 2014 (some of which may be legitimate examples of firm exit) account for less than 1% of enrollment in terms of member months, suggesting that reporting is only rarely an issue with respect to our 2014 data.

Finally, for each remaining firm, I identify and address remaining data anomalies using regression techniques at the firm level. For each firm, for each of the three variables of interest, I first run a seasonally-adjusted trend regression from the first quarter of 2008 through the third quarter

[23]There are several cases for which a firm reports numbers for enrollment, premiums, and costs that are negligible relative to other numbers in their active periods. In order to properly perform firm-level imputation, I exclude these insurer-quarter observations from the non-defunct period and flag them for later imputation. Specifically, I flag such observations as those for which the enrollment, premiums, or costs are less than one-tenth the median value.

[24]When this particular error occurs, the data reported by the firm in Q4 are roughly four times as large as the data reported by the firm in quarters of other, non-anomalous years.

of 2013. These seasonally-adjusted trend regressions exclude observations with a reported value of 0. Using fitted values from these regressions, I identify outlier observations by predicting the studentized residual for each observation and flagging those observations for which this statistic is greater than 2. I then re-run each seasonally-adjusted trend regression, this time also excluding the flagged outlier observations, and replace those observations, as well as observations with reported values of zero (or less than zero), with the fitted value from the second-stage of estimation. I assess the effect of my imputation procedure by comparing my imputed data to the raw data.[25]

All-in-all, though the data cleaning process requires many steps, it affects a very small number of observations. For enrollment in terms of member months, 7.3% of observations, accounting for less than 7% of aggregate member months, are imputed; for premiums, 8.6% of observations, accounting for less than 7% of aggregate premiums are imputed; for costs, 10.0% of observations, accounting for less than 8% of aggregate costs are imputed. Furthermore, for of the variables of interest—enrollment, premiums, and costs—the coefficient of correlation between in the raw data and clean data is in excess of 0.97. As I show in the Online Appendix, the state graphs constructed using the imputed data are noticeably "cleaner" than those constructed using the raw data; however, our corrections to these apparent outliers have no material impact upon our results and conclusions. Therefore, we are confident that the imputations we have made are, at worst, benign and likely present the analysis more transparently.

B Data Allocation by State

After cleaning the data at the insurer-quarter level, I allocate the data to the insurer-quarter-state level. Allocation by state is trivial if the annual or Schedule T filings indicate a unique state. If the filings do not indicate that the insurer operates in a unique state, I use the filings to inform state allocation.

I first allocate the data by state according to the corresponding annual filing. For the 2014 quarters, I use the percentages from the 2013 annual filing, since the 2014 annual filing will not be available until the end of the year. From the corresponding annual filing, I calculate the percentage of aggregate enrollment in member months, total premiums collected, and total costs paid by state,

[25] For some firms, I have identified instances where analysis of firm-level time series patterns suggests that imputation was unnecessary. In these cases, I have replaced the imputed data back with the raw data.

and I apply that percentage to aggregate coverage, premiums, and costs by state, respectively, from the quarterly filing. This allocation methodology ensures that the aggregate amounts of enrollment, premiums, and total costs (when summed across all states) are preserved for each insurer-quarter observation.

For insurer-quarter observations for which a corresponding annual filing is not available, I allocate the data using supplemental Schedule T filings. The Schedule T filings are reported quarterly, but they aggregate the individual health insurance line of business with other lines of business, including "accident & health", "life & annuity", and "property/casualty." Furthermore, they only include premiums, and not enrollment or coverage, leading me to prefer the annual filings. My allocation methodology using the Schedule T filings is as follows: I calculate the percentage of total premiums attributable to each state for the insurer-quarter, and I apply those percentages to the insurer-quarter data from the individual health insurance line of business.

C Table of Welfare Results by State

Table A1: Welfare Results by State

	Calibrated Annual Penalty ($) $12\pi = 1{,}000$			Calibrated Annual Penalty ($) $12\pi = 1{,}500$			Calibrated Annual Penalty ($) $12\pi = 2{,}000$		
	Full Monthly Welfare Change Per Enrollee ($)	**Monthly Welfare Change from Selection Per Enrollee ($)**	**Optimal Annual Penalty ($)**	**Full Monthly Welfare Change Per Enrollee ($)**	**Monthly Welfare Change from Selection Per Enrollee ($)**	**Optimal Annual Penalty ($)**	**Full Monthly Welfare Change Per Enrollee ($)**	**Monthly Welfare Change from Selection Per Enrollee ($)**	**Optimal Annual Penalty ($)**
	$W_{full}/ I^{*,post}$	$W_{sel}/ I^{*,post}$	$12\pi^*$	$W_{full}/ I^{*,post}$	$W_{sel}/ I^{*,post}$	$12\pi^*$	$W_{full}/ I^{*,post}$	$W_{sel}/ I^{*,post}$	$12\pi^*$
AK	125	147	8,727	107	117	3,824	90	94	3,067
AL	-61	-100	7,015	-60	-80	9,221	-59	-72	10,468
AR	-18	-145	644	-28	-86	633	-38	-83	628
AZ	-3	-6	438	-9	-12	451	-15	-18	457
CA*	-119	-147	479	-60	-122	586	-1	-82	663
CO	-9	-17	108	-12	-19	89	-15	-21	81
CT	53	-35	1,574	44	338	890	35	68	3,882
DC	21	150	492	10	13	2,313	-2	-15	1,291
DE	3	7	389	-4	2	411	-11	-3	425
FL	-22	-70	260	-28	-57	184	-35	-58	148
GA	-9	-62	590	-14	-42	567	-20	-42	558
HI	-6	-6	799	-2	-2	828	2	2	843
IA	-29	-26	-714	-33	-31	-771	-38	-36	-804
ID	-40	-37	-1,525	-43	-41	-1,702	-46	-45	-1,810
IL	-34	-55	-22	-42	-57	-77	-50	-63	-108
IN	13	-179	1,031	1	-55	1,181	-11	-58	1,224
KS	-41	-39	-241	-50	-48	-270	-59	-58	-287
KY	42	-41	1,177	36	134	-145	31	55	5,085
LA	-10	-21	356	-16	-25	363	-21	-30	366
MA*	-23	-20	1,067	-11	-3	1,132	0	11	1,162
MD	14	13	976	3	1	925	-7	-10	904
ME	139	-12	2,339	126	-17	2,159	113	41,581	1,887
MI	0	-62	893	-7	-41	898	-14	-43	899
MN	-65	-39	-2,216	-70	-48	-2,383	-74	-55	-2,500
MO	-38	-40	-5,407	-39	-40	-6,108	-40	-41	-6,523
MS	-2	-15	666	-8	-20	688	-14	-25	697
MT	-44	28	1,878	-54	-7,813	1,350	-64	-466	1,066
NC	-1	-49	841	-7	-33	868	-13	-35	875
ND	2	2	1,084	1	0	1,066	-1	-2	1,059
NE	-24	-23	-1,536	-26	-26	-1,664	-28	-28	-1,736
NH	29	34	2,429	25	26	2,313	20	20	2,265
NJ*	149	754	441	135	309	157	122	209	-121
NM	-53	-364	-561	-55	-134	-2,385	-57	-104	-3,224
NV	17	18	1,162	7	5	1,003	-4	-7	945
NY	104	-67	2,472	97	-91	1,658	91	574	493
OH	-55	-63	-8,781	-56	-61	-10,451	-57	-61	-11,472
OK	-38	-64	-73	-46	-64	-153	-53	-69	-199
OR	-66	-52	-1,467	-71	-60	-1,645	-76	-67	-1,765
PA	-40	-53	-716	-45	-54	-859	-50	-58	-939
RI	36	50	2,755	26	27	1,869	16	13	1,673
SC	6	-18	937	-2	-23	993	-10	-30	1,013
SD	-34	-30	-2,094	-36	-33	-2,278	-39	-36	-2,389
TN	-22	-29	-308	-26	-32	-365	-30	-36	-394
TX	-31	-85	38	-37	-69	-98	-43	-68	-163
UT	-27	-37	6	-35	-43	-17	-42	-50	-30
VA	0	-2	654	-4	-7	665	-7	-11	671
VT	16	20	2,484	13	14	2,167	10	10	2,056
WA	-52	-58	2,553	-48	-51	2,925	-44	-45	3,141
WI	6	2	683	-9	-15	665	-24	-31	658
WV	-31	-607	713	-40	-153	534	-49	-124	468
WY	-19	-27	201	-27	-33	211	-34	-40	216
Summary*	-15	-53		-21	-53		-27	143	
Sumary	-16	-48		-20	-51		-25	137	

*States with data anomalies omitted from state-level welfare regression analysis. MA is also omitted.