

## Happy Hens, sad consumers? The Economic Impact of Restrictions on Farm Animal Housing in California

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New animal welfare policies on the horizon in many states have prompted debates about the cost of achieving happier hens and hogs. A recent policy change in California offers a unique opportunity to measure the economic repercussions of minimum space requirements for egg-laying hens. Using retail scanner data from large California and non-California markets, we use difference-in-difference estimators to identify the effect of minimum space requirements on retail egg prices, quantity sold, and sales value, while also estimating impacts of the policy change on consumer welfare and egg producer net revenue. We estimate that the law increased egg prices by about 22%, decreased quantity sold by around 8%, and increased the value of sales by about 12%; the price and sales effects are statistically significant at conventional levels. In the three California markets included in our data set, consumer welfare losses amount to approximately \$30 million over the 16 weeks immediately following implementation of minimum space requirements while net revenue from eggs sold in the three California markets increased by between 11% and 15% over the same time horizon. Positive impacts on producers may have been short lived, however, and were likely concentrated among producers located in California. Our results are robust to several tests of our identifying assumptions. Overall, our findings indicate that the potential economic costs of mandated improvements in farm animal welfare should be taken seriously when considering such policy changes.

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There are more egg-laying chickens in the United States than there are people. Until recently, virtually all of those hens live in so-called battery cages that provide an amount of space per hen that is 28% smaller than a typical 8.5 by 11 inch sheet of paper. On January 1, 2015, two new laws motivated at least in part by the goal of improving animal welfare (henceforth referred to as the AW laws) went into place requiring that all eggs sold in California come from chickens provided enough room to turn around and fully extend their wings.<sup>2</sup> Other states will soon implement similar measures, and additional federal and state laws are being actively debated.<sup>3</sup> In addition, Wal-Mart has encouraged its food suppliers to adopt more humane housing standards for farm animals, and McDonald's has pledged to phase out the use of eggs produced by hens living in battery cages (Polansek and Nickel 2015, Nassauer 2015). Momentum for more widespread adoption of more animal-friendly housing standards appears to be growing in the private sector and the legislative arena alike, raising questions about the potential economic costs of such restrictions.

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<sup>2</sup> California voters passed Proposition 2 in 2008 banning confinement of farm animals that does not allow them to turn around freely, lie down, stand up, and fully extend their limbs. The measure applied to chicken battery cages, veal crates, and sow gestation crates. However, there is very little veal, pork, or broiler production in California, while California is the 7<sup>th</sup> largest egg producing state in the nation (producing 4.5 billion eggs in 2014 or 5.1% of the nation's total according to USDA-NASS). Therefore the potential impacts are largest for the egg industry. In addition, recognizing that cheaper imports from other states would soon dominate the egg market, the California state legislature passed AB 1437 in 2010, banning the sale of eggs in California produced under conditions that do not comply with Proposition 2. Both laws were implemented on January 1, 2015.

<sup>3</sup> Four other states (Michigan, Ohio, Oregon, and Washington) have already passed laws that will eventually limit the use of battery cages. Similar measures are being considered in many other locales (e.g., Massachusetts has a voter initiative on the ballot in November 2015 that would prohibit the production and sale of eggs using batter cages). In 2012, two common foes, the Humane Society for the United States (HSUS) and the United Egg Producers (UEP) jointly lobbied the US Congress (unsuccessfully) for a federal law that would have required a minimum space per hen. The agreement fell apart when the Farm Bill passed in 2014 without the standards. All of these efforts follow European Union laws that banned battery cages in 2012.

US consumers eat around 250 eggs per person per year on average (USDA-ERS 2015), and as a result housing restrictions for egg-laying hens have the potential to burden consumers with higher food costs. The issue of cost is also at the crux of a paradox surrounding consumer behavior. When California voters went to the polls in 2008 and passed the initiative mandating minimum space requirements for farm animals that would eventually be implemented in 2015, about 90% of California consumers were buying battery cage eggs despite the existence of many alternative, albeit more expensive, options (Chang, Lusk and Norwood 2010). The fact that most consumers were unwilling to pay the higher price of the non-cage alternatives in the grocery store led Allender and Richards (2010) to project over \$100 million in consumer welfare losses as a result of the policy. Because egg demand is relatively inelastic (Brown and Schrader 1990, Okrent and Alston 2011) and the product has few close substitutes, small changes in cost could translate into large price increases, disproportionately impacting low-income consumers (Allender and Richards 2010).

Aside from consumer welfare concerns, others have also suggested California egg producers will suffer economic losses (Sumner et al. 2011). California is a net importer of eggs, and as a result, concern over impacts on egg producers extends beyond California's borders. Seven other states sued the state of California over the prohibition against the sale

of eggs not meeting the standards of the AW laws, arguing that the AW laws violate the Interstate Commerce Clause and harm producers in their states.<sup>4</sup>

This paper aims to identify the causal impact of the California AW laws on market outcomes (price, quantity sold, and value of sales) in the California retail egg market as well as impacts on the welfare of California consumers and producers of eggs sold in California. Using a difference-in-differences approach applied to retail scanner data from three major California markets and three non-California markets for weeks observed before and after January 1, 2015, we estimate that the AW laws increased the average price per dozen eggs by about 22% relative to what the average price per dozen eggs would have been had the AW laws not been implemented. We use estimated price impacts to approximate the equivalent variation of the AW laws for the three California markets in our data. We find that over the 16 weeks following implementation of the AW laws that we observe in our data set, consumers in the three selected California markets would have been willing to pay about \$30 million dollars to avoid the price changes caused by the AW laws. Extrapolating this figure out to an annual figure for California as a whole yields a total annual willingness to pay to avoid the price increases caused by the AW laws of \$105 million.

In addition, we estimate that the AW laws caused the quantity of eggs sold to fall by about 7.9% relative to what would have been observed had the AW laws not been

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<sup>4</sup> A federal court judge dismissed the case in 2014, but the dismissal is currently under appeal.

implemented; however, we cannot reject the null hypothesis of zero effect on quantity sold. We find evidence that supply responses to the AW laws vary by producer location, and that California producers appear to be increasing supply in response to the AW laws while out-of-state producers are reducing supply. Estimated sales impacts indicate that the AW laws lead to an 11.8% increase in the value of egg retail sales in the three California markets. By combining our data with assumptions about the impact of the AW laws on producer costs and the producer share of the retail egg price, we estimate that the AW laws increased net producer revenue by between 11% and 15% over the 16 post-implementation weeks observed in our data; however, nearly all of the increase in net revenue appears to have been driven by the initial price surge associated with implementation of the AW laws.

Given the fact that minimum space requirements for hen housing or outright bans on battery cages may ultimately become policy in other states, the issue has been studied in a number of prior papers. However, nearly all previous efforts to address this issue are ex-ante analyses, relying on simulation models (Sumner, Matthews, et al. 2010; Norwood and Lusk 2011; Allender and Richards 2010), budgetary comparisons of cage and cage-free systems (Matthews and Sumner 2015; Sumner, et al. 2008), or hedonic analyses of retail egg prices (Chang, Lusk and Norwood 2010). While each of these approaches provided useful insights, none provide an ex-post analysis of the actual policy that may ultimately become law elsewhere. Other than the present paper, the only other ex-post analysis of the California AW laws of which we are aware is Malone and Lusk (2015), who rely on (potentially non-representative) reports of wholesale prices to the USDA

Agricultural Marketing Service rather than retail prices, do not have access to quantity or sales data, and do not estimate producer welfare impacts.

Our paper makes a number of significant contributions over previous related work. As far as we know, our paper is the first to provide an ex-post estimate of the impact of farm animal housing restrictions on retail prices, quantities, and sales, as well as effects on producer and consumer welfare. This paper contributes to the relatively nascent literature on the economics of animal welfare (Cowen 2006, Lusk and Norwood 2011). Some animal welfare advocates suggest such policies are inexpensive (costing less than an extra penny per egg) ways to improve animal well-being (Hall 2008). Some animal advocates have even suggested that laws eliminating tight confinement might raise producer profits based on the premise that current conditions are so inhospitable that they lower productivity (Francione and Garner 2010). Aside from productivity effects, such laws might also increase producer profits by reducing the volume of eggs supplied. Indeed, several food retailers have sued large egg producers and their industry organization over allegations of artificial supply constraints enacted, in part, by increasing cage size limits (Harris 2011). Perhaps surprisingly, some animal rights advocates have argued against policies like the California law that improve the well-being of farm animals because such policies may increase consumer demand, leading to greater consumption of animal products (Francione 2008). Our unique data on prices and quantities at the retail level can help provide insights into such debates.

In what follows, we first summarize the data set used in our analysis, and then describe our identification strategy. We then present our estimated impacts on market

outcomes before moving onto our analysis of impacts on consumer and egg producer welfare. Next, we subject our results to robustness checks, estimating placebo impacts on non-California markets and checking the robustness of our results to violations of the common trends assumption underlying difference-in-differences estimation. We end with a brief conclusion.

### **Data**

To conduct our analysis, we use retail scanner data from Nielsen for three California markets (Los Angeles, San Francisco, and San Diego) and three non-California markets (Chicago, Phoenix, and Salt Lake City/Boise). The data set was constructed by aggregating weekly reports from participating retailers. Nationally, Nielsen scanner data include retailers that account for 90% of all grocery sales volume. The geographic scope of each Nielsen market includes the metropolitan area associated with the market name along with surrounding counties. Total population in the three California markets is about 28.5 million (9.6 million households), or 73% of California's total population, while total population in the three control markets is about 15.7 million (7.3 million households). The three California markets were chosen in order to create a data set that is broadly representative of major metropolitan areas in California, while the non-California markets were chosen in order to construct a comparison group that included two moderately sized markets and one large market, yielding a population distribution similar to that of the three California markets; Los Angeles accounts for 62% of total population in the three California markets, while Chicago accounts for 58% of total population in the three control markets.

The data set runs from the first week of April 2013 through the third week of April 2015; the first 89 weeks of data fall immediately prior to implementation of the AW laws, while the final 16 weeks cover the period immediately afterward. The data set consists of 142,170 weekly observations of 798 egg Universal Product Codes (UPC), where a single UPC sold in two different markets during the same week is treated as two separate observations. The data only include shell eggs (liquid eggs, which are used predominantly by the food processing and food services industries, are not included in the data set). Sumner et al. (2008) estimate that about 93% of all eggs produced in California are sold as shell eggs. Each observation includes average price, units sold, and product characteristics (package size, grade, cage free or organic certification, etc.) for a UPC in a given market and week. All UPCs are observed over the entire time horizon covered by the data set.

[TABLE 1 HERE]

Table 1 contains summary statistics calculated using the 89 weeks of data available prior to implementation of the AW laws. Panel A of table 1 gives the average price per dozen, average quantity sold per week (in thousands of dozens), and average weekly sales (in \$100,000) in the selected California and non-California markets for weeks in the data prior to the implementation of the AW laws; prices and sales are in nominal dollars. Average price was estimated by weighting each UPC's weekly price by its share of quantity sold in that week and summing the share-weighted prices. Prices and quantities are higher in California than in control markets for the pre-implementation period, and the differences are significant; this is expected, given the differences in market size and the large number of observations in the data.

Panel B of table 1 gives the quantity-weighted average characteristics of eggs sold in California and non-California markets. Product characteristics are very similar across the two groups of markets. In general, any significant differences we observe are small in magnitude. Conventional and white eggs dominate sales volume, with organic and cage-free eggs each accounting for less than 10% of total sales volume (organic and cage-free are not mutually exclusive categories). A total of 466 UPCs are sold in the selected California markets, and 512 are sold in the three non-California markets; 180 UPCs are sold in both sets of markets.

### **Empirical approach**

#### *Identification and inference*

We are interested in estimating the effect of the AW laws on market outcomes as well as consumer and producer welfare. In order to estimate impacts on market outcomes, we estimate the following two regression models for each outcome:

$$(1) \quad y_{imt} = \beta_0 + \beta_1 CA_m + \beta_2 Post_t + \beta_3 CA_m \times Post_t + \beta_4 x_{im} + e_{imt}$$

$$(2) \quad y_{imt} = \lambda_i + \lambda_m + \lambda_t + \alpha_1 CA_m \times Post_t + u_{imt}$$

The outcome variable,  $y_{imt}$ , is average price per dozen, quantity sold, or total sales for UPC  $i$  when sold in market  $m$  in week  $t$ ;  $CA_m$  is a dummy variable equal to one for markets within California and zero otherwise;  $Post_t$  is a dummy variable equal to one for all weeks following the implementation of the AW laws;  $x_{im}$  is a vector of product characteristics and their interactions;  $\lambda_i$ ,  $\lambda_m$ , and  $\lambda_t$  are fixed effects for UPC, market, and

week, and  $e_{imt}$  and  $u_{imt}$  are error terms, each of which is assumed to have a mean of zero conditional on the variables in their respective models. In equation (1),  $\beta_3$  represents the policy effect in a simple difference-in-differences model augmented to include product characteristics, while  $\alpha_1$  in equation (2) represents the policy effect in a difference-in-differences model that replaces  $x_{im}$ ,  $CA_m$ , and  $Post_t$  with fixed effects for UPC, market, and week.

When using average price per dozen as the dependent variable, equations (1) and (2) are estimated by weighted least squares, where the weight for a particular UPC-market-week observation is given by total dozens sold by that UPC in the same market and week. This weighting scheme ensures that in our price regressions,  $\beta_3$  and  $\alpha_1$  are estimators of the impact of the AW laws on the average price per dozen in California, rather than average impacts per UPC. For our quantity and sales models, our estimates of  $\beta_3$  and  $\alpha_1$  are scaled up by the number of UPCs sold in California in order to convert UPC-level impacts into impacts on dozens of eggs sold and total egg sales for the three California markets in our data. Statistical inference is based on two-way cluster-robust standard errors that cluster on UPC and week (Cameron, Gelbach and Miller 2011), and p-values generated by a wild cluster bootstrap percentile-t procedure clustering on market.<sup>56</sup> Each method of inference

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<sup>5</sup> Alternatively, we could have treated California as a single cluster. However, to our knowledge, there are no accurate methods of performing statistical inference on the effect of a discrete policy change affecting a single cluster when the number of control clusters is small. Therefore we opt to treat each of the California markets as a single cluster.

<sup>6</sup> Theory and simulation evidence in MacKinnon and Webb (2015) and Webb (2014) suggests that the wild cluster bootstrap method used in our paper is likely to reject a true null hypothesis less frequently than what

is appropriate under different, but equally reasonable, assumptions about the correlation of regression error terms across observations; we include both as a check on the robustness of our results.

Difference-in-differences will yield unbiased estimates of the impact of the AW laws on market outcomes in California if three assumptions hold: first, that there were no spillover effects on other states in our data, second, that the three California markets and the three non-California markets in our data set would have followed the same trends with respect to price, quantity, and sales in the absence of the AW laws, and third, that there were no effects of the AW laws prior to its implementation.

California is a net importer of eggs, making it possible that new regulations in California could affect egg prices elsewhere in the U.S. However, California imports constitute around 2.4% of total U.S. consumption of eggs, making it unlikely in our view that a shock to supply or demand in California would reverberate throughout egg markets elsewhere (Bell 2013, Promar International 2009); therefore we assume that the assumption of no spillovers holds in the present case. We examine the visual evidence for the assumptions of common trends in California and non-California markets and no pre-implementation effects of the AW laws in figure 1.

[FIGURE 1 HERE]

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is dictated by the chosen significance level when the number of clusters experiencing a policy change is small. Our own simulation evidence suggests that the wild cluster bootstrap procedure is likely to under reject the null hypothesis in the present context as well, i.e., our p-values may be too large. Our simulation results and additional details on our inference procedures are available in the appendix.

The top panel of figure 1 depicts the weekly quantity-weighted average price per dozen for the three California markets and the three non-California markets. The line emanating from the horizontal axis at January 2015 represents the implementation of the AW laws. The two sets of markets are clearly following similar trends until eight weeks prior to the implementation of the AW laws at which point prices in California begin to climb sharply. The average price per dozen reaches a peak of \$4.09 in February 2015 before falling back to \$2.99 per dozen at the end of April, leaving the price per dozen eggs \$0.50 higher at the end of the observed period than the average price before the implementation of the AW laws and \$0.42 higher than the average price per dozen immediately prior to the sharp increase in prices shown in the figure.

The middle panel of figure 1 shows total dozens sold each week for California and non-California markets in the data set. Again, we see similar trends evolving in California and non-California markets, although the co-movement is not as clear as in the case of prices. Total dozens sold in the three California markets begins to fall relative to sales in the three control markets at around the same time we observe the structural break in prices in the top panel. Finally, the bottom panel of figure 1 shows trends for total value of retail sales of eggs in the three California and non-California markets. The vertical distance between the two trends is slightly larger in 2014 than in 2013, but overall, the two trends track each other very closely, exhibiting similar seasonal fluctuations (with upticks in sales from November through January and again in March and April). As in the case of prices, we see what appears to be a structural break in the California trend just prior to implementation of the AW laws, although the jump in sales coincides with a seasonal sales

increase that is also observed in 2014 in both sets of markets. Overall, the trends in California and non-California markets appear to track each other quite closely with respect to all three outcomes until just before implementation of the AW laws.

#### *Anticipation effects*

The fact that we observe departures from common trends prior to the implementation of the AW laws might normally be a reason to doubt the identification strategy employed here; e.g., perhaps the changes in trends that we observe prior to implementing the AW laws would have occurred regardless of whether the AW laws were implemented. However, it seems highly doubtful that the AW laws (passed in 2008 and 2010) could have been written and approved in anticipation of changes in market outcomes predicted several years into the future. It seems far more likely that what we observe in figure 1 is the result of anticipation effects, e.g., producers changing cage configurations and reducing flock size (and egg production) in order to be compliant with the AW laws prior to implementation. That is, the underlying supply shift likely began to occur prior to January 1, 2015. Informal discussions with producers as well as agricultural extension reports indicate that a common strategy for complying with the AW laws is to maintain the battery cage production system while reducing the number of animals per cage (Bell 2013); this is a strategy that could be put into place rapidly and cause a sudden supply reduction that would generate the changes in trends we observe in figure 1.

While anticipation effects on the part of consumers are also possible, we would expect to see increases in quantity sold if consumers were taking action that might drive up prices in anticipation of implementing the AW laws. In fact, we see the opposite. The

most likely scenario would appear to be that actions taken by producers to comply with the AW laws are causing the changes in trends we observe in the weeks leading up to implementation.

Because the point at which the AW laws would have begun to affect producers is endogenously determined, while the implementation of the AW laws is exogenous (if we accept the common trends assumption), we choose to base our difference-in-difference estimator on comparisons of the pre and post-implementation time periods in the data. In other words,  $Post_t$  is set equal to one only for weeks observed after implementation of the AW laws when estimating equations (1) and (2). As a result, our estimated impacts on price, quantity, and sales will be conservative. However, given the length of the pre-implementation time series, the changes in trends observed prior to implementation of the AW laws will have little effect on our estimated impacts for price, quantity, and sales.

### **Average price, quantity, and sales impacts in the post-implementation period**

Table 2 presents our estimates of the impact of the AW laws on the weekly average price per dozen eggs,<sup>7</sup> dozens sold per week, and weekly retail sales of eggs in the three selected California markets.

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<sup>7</sup> We use price rather than log price. Using the log of average price would require first computing the weekly average price in each market and taking logs; this would result in a large loss in variation and sample size. In addition, the common trends assumption appears to hold for price; therefore it should not hold for the log of price, since the latter involves applying a nonlinear transformation to price. Lastly, prices and sales impacts are in nominal dollars. Applying different deflators to California and non-California markets would likely result in violations of the common trends assumption, and given the relatively short time period examined here, inflation is not of great concern.

[TABLE 2 HERE]

Panel A of table 2 gives results based on our estimates of equation (1) for each outcome, while panel B gives results based on estimates of equation (2). Point estimates of the impact of the AW laws as well as their standard errors are not very sensitive to model specification. Impacts on sales and price are statistically significant at conventional levels whether we base inference on the wild cluster bootstrap or the two-way clustered standard errors, whereas quantity impacts are imprecisely estimated.

The estimated price impacts imply that the AW laws raised the price per dozen eggs by around 75 cents (or 6.25 cents per egg) each week on average, or about 22% higher than what the price per dozen would have been in the absence of the AW laws.<sup>8</sup> Our estimated price impacts are lower than what was predicted by some analysts prior to implementation of the AW laws. Sumner et al. (2011), for example, predict a price increase of about 40%, a figure that is outside the 95% confidence interval associated with the price impact shown in table 2. The impact on sales implies that the AW laws raised the value of retail egg sales in California by \$1.06 million per week on average, or 18% higher than what average sales value would have been in the absence of the AW laws. The estimated quantity impacts imply that the AW laws caused total dozens of eggs sold in California to fall by around 132,000 per week, or 7.9% relative to the quantity that would have been sold each week in

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<sup>8</sup> The percent increase is calculated by dividing the estimated price impact by the difference between the average price per dozen in the three California markets after implementation of the AW laws and the estimated price impact, i.e., the price impact is divided by our estimate of the average price in the California markets in the absence of the AW laws.

the absence of the AW laws. However, we cannot reject the null hypothesis of no effect of the AW laws on quantity.

If the AW laws had no impact on demand for eggs and instead operated solely through effects on supply, and demand for eggs can be closely approximated using a linear demand curve in the neighborhood of the price change caused by the AW laws, then the percent impacts on price and quantity imply an elasticity of demand for eggs of -0.36 at the equilibrium price and quantity in the absence of the AW laws. This own-price elasticity estimate is reasonably close to estimates from previous literature.<sup>9</sup> In this scenario, weak consumer response to higher prices caused by the policy change lead to a large increase in prices as well as in the value of sales. However, even if the exclusion restriction ruling out any effect of the AW laws on demand is satisfied, the large standard errors on quantity impacts mean we cannot make precise statements about impacts on quantity or sensitivity of demand for eggs to price changes.

*Impacts of the AW laws on price, quantity, and sales, by week*

Figure 2 shows the impacts of the AW laws in the post implementation period by week; impacts were estimated using a model similar to equation (2), replacing  $CA_m \times Post_t$  with interactions between  $CA_m \times Post_t$  and dummy variables for each of the post-

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<sup>9</sup> Okrent and Alston (2011) estimate an elasticity of demand for eggs of -0.22 when using monthly data from the Bureau of Labor Statistics and -0.73 when using annual data from the Bureau of Economic Analysis. When they review previous literature, they find an average elasticity estimates of -0.17 (for studies that do not distinguish between food at home and food away from home) and -0.18 (for studies that did make this distinction).

implementation weeks in our data; confidence intervals were computed using two-way clustered standard errors.

**[FIGURE 2 HERE]**

In the top panel of figure 2, we can see that the AW laws causes price to jump by around \$0.60 per dozen immediately after implementation, with the impact reaching a peak of just over \$1.20 five weeks after the start of the new law. The price impact then falls over the next two months before settling at around \$0.40 per dozen. In the middle panel, we can see that the impact on quantity sold varies between -200,000 and -400,000 dozens for the first six weeks before moving closer to zero and switching sign in the final weeks of the observed time horizon; however, the quantity impact cannot be statistically distinguished from zero in all but four weeks. As we might expect given that impacts on price and quantity are moving in opposite directions, the impact on the total value of sales does not exhibit any obvious trends. Instead, the impact of the AW laws on sales moves between \$500,000 and \$1.5 million over the 16 post-implementation weeks in the data.

*Domestic and out-of-state producer supply response to the AW laws*

Adopting the standards of the AW laws will increase production costs for California egg producers and out-of-state producers exporting to California. However, the effect of the AW laws on production costs may negatively impact the competitive position of egg producers not just in the California market, but in other markets as well. California is a net importer of eggs, and therefore the implications of the AW laws for sales of eggs produced in California to other states may be negligible. But out-of-state producers who are not

completely specialized in selling to the California market may find themselves unable to compete in their home states or other markets following adoption of the new standards, should they choose to adopt. Therefore we might expect to see sales of imported eggs fall relative to sales domestic eggs in California, as out-of-state producers choose to adopt the new standards of the AW laws to a limited degree or not at all. A reduction of imports into California will push the California price up further, creating a greater incentive for California producers to expand production.

If we are willing to assume that UPCs only sold in California are primarily sourced from California egg producers while UPCs sold in California and control markets may be sourced from producers in a variety of locations, and that the impact of the AW laws on demand does not vary by where eggs are produced (conditional on product characteristics), then examining impacts by where a UPC is sold can provide evidence for differential supply response based on producer location. We examine this possibility by estimating separate price, quantity, and sales models for UPCs sold strictly in California or both in California and other states, according to what we observe in our data set. Results are shown in table 3.

[TABLE 3 HERE]

Panel A of table 3 presents estimated impacts of the AW laws on UPCs sold in California and control markets, while panel B shows impacts on UPCs only sold in California (where UPCs only sold in non-California markets serve as controls). Regression-adjusted impacts on prices are very similar for both groups of UPCs.

Unadjusted growth in prices for UPCs sold only in California is substantially higher than that of UPCs sold in both sets of markets, with the average price per dozen for the former group of UPCs growing by \$1.25 (from \$2.57 to \$3.82) while the average price per dozen for the latter grew by 66 cents (from \$2.37 to \$3.03). It would seem that most of the difference in the growth of prices by where a UPC is sold can be attributed to differences in product characteristics.

The estimated impacts on quantity sold differ sharply for UPCs sold in both sets of markets and UPCs sold just in California. However, the 95% confidence intervals (based on the two-way clustered standard errors) for quantity impacts on the two groups of UPCs overlap one another, and neither point estimate is statistically significant at conventional levels when we base inference on the wild cluster bootstrap. Therefore we hesitate to say that the results in column (2) of table 3 constitute strong evidence for supply responses that vary by producer location.

Examining the results of our estimated sales models in column (3) of table 3 leads to a similar conclusion. The point estimates suggest that impacts on sales were substantially higher for UPCs sold strictly in California; the weekly per-UPC impact of the AW laws on sales is seven times higher for UPCs sold just in California than it is for UPCs sold in both sets of markets, and UPCs sold just in California increased their sales by 26% relative to what they would have enjoyed in the absence of the AW laws. However, we cannot reject the hypothesis that the two effects are equal to one another, regardless of whether we test the total impacts on net revenue or the per UPC impacts. In addition, inference on each

point estimate is sensitive to whether we base inference on the two-way clustered standard errors or the bootstrap.

[FIGURE 3 HERE]

Figure 3 shows the impact of the AW laws on quantity sold by week for both sets of UPCs in the period following implementation. The impact on quantity for UPCs sold in both sets of markets varies between 2.5 million and 1.5 million fewer dozens sold per week, whereas the impact on UPCs sold just in California is initially at about 1.25 million fewer dozens sold per week before following an upward trend and eventually reaching a positive impact of about 5 million more dozens sold per week. Figure 3 is consistent with the hypothesis that after an initial adjustment to the standards put in place by the AW laws, California producers expanded supply in response to higher prices, while the adjustment made by out-of-state producers was to reduce supply. Figure 3 is also consistent with a demand response that varies by where eggs are produced. However, the fact that the estimated price impact on each type of UPC is very similar suggests that UPCs sold strictly in California and UPCs sold in both sets of markets are close to perfect substitutes, and that differences in quantity and sales impacts by where UPCs are sold are the result of varying supply responses.

We cautiously interpret the results in table 3 and figure 3 as being indicative of a supply response that varies by producer location, and recognize that we cannot draw a stronger conclusion without the ability to statistically distinguish impacts on the two groups of UPCs or completely rule out a demand response as a driver of impact heterogeneity. Of

course, it should also be noted that if there are time-varying, unobserved characteristics of UPCs that are correlated with whether a UPC is sold only in California or in both sets of markets, then differences in the impact estimates in panel A and panel B of table 3 could be caused by selection bias. It is reassuring, then, that price impacts in table 3 are very similar across both sets of UPCs, and also quite similar to the price impacts for the full sample as shown in table 2; estimated price impacts provide the basis of our consumer welfare analysis.

### **Welfare effects of the AW laws**

#### *Impact of the AW laws on California consumers*

To examine the consumer welfare impacts of the AW laws, we follow the approach suggested by Chetty (2009) and derive a “sufficient statistic” for welfare analysis that can be estimated using the reduced-form econometric approach employed above, rather than requiring that we specify a complete model of behavior. Suppose that consumers have preferences over different states of the world, where state  $s$  faced by a consumer  $i$  can be completely characterized by her income,  $y_i^s$ , a vector of prices for all market goods,  $\mathbf{P}^s$ , and characteristics of market goods,  $\mathbf{C}^s$ . Each good  $j$  consists of a set of characteristics, where  $c_{jk}$  is the amount of characteristic  $k$  contained by good  $j$ , and each good can be completely described by a vector of characteristics, e.g.  $\mathbf{c}_j = [c_{j1}, \dots, c_{jK}]$ . Given her income and prices, the consumer selects a bundle of goods and its associated set of characteristics in order to maximize her utility; maximized utility in state  $s$  is denoted as  $u_i^s$ . Let the

minimum expenditure needed for consumer  $i$  to reach utility  $u_i^s$  given prices and product characteristics be given by the consumer's expenditure function,  $e(\mathbf{P}^s, \mathbf{C}^s, u_i^s)$ .

In addition the price impacts described above, the AW laws may have affected the perceived characteristics of eggs sold in California. Suppose that the AW laws affected the level of animal welfare associated with eggs as perceived by consumers. For simplicity, we drop product subscripts, and let eggs be product  $\mathbf{c}$  with price  $p$ , with the “animal welfare” characteristic of eggs represented by  $c_1$ . The AW laws cause the animal welfare characteristic of eggs to shift from  $c_1^0$  to  $c_1^1$ . Suppose that the impact of the AW laws on egg prices in California causes a shift from  $p^0$  to  $p^1$ . The result of the price and product characteristic changes brought about by the AW laws is a change in utility from  $u^0$  to  $u^1$ .

Dropping the  $i$  subscript, the consumer's equivalent variation (EV) associated with the AW laws can be written as:

$$(1) \quad EV = e(\mathbf{P}^1, \mathbf{C}^1, u^1) - e(\mathbf{P}^0, \mathbf{C}^0, u^1)$$

The EV gives the maximum amount the consumer would be willing to pay to avoid passage of the AW laws. We can sum the EV over California consumers and obtain an aggregate measure of the consumer welfare impact of the AW laws. Note that rather than picking a functional form for the expenditure function, the EV can be written using a first order approximation (Deaton and Muellbauer 1980):

$$\begin{aligned}
e(\mathbf{P}^0, \mathbf{C}^0, u^1) - e(\mathbf{P}^1, \mathbf{C}^1, u^1) &\approx \frac{\partial e(\mathbf{P}^1, \mathbf{C}^1, u^1)}{\partial p} (p^0 - p^1) + \frac{\partial e(\mathbf{P}^1, \mathbf{C}^1, u^1)}{\partial c_1} (c^0 - c^1) \\
(2) \quad \text{EV} &\approx \frac{\partial e(\mathbf{P}^1, \mathbf{C}^1, u^1)}{\partial p} (p^1 - p^0) + \frac{\partial e(\mathbf{P}^1, \mathbf{C}^1, u^1)}{\partial c_1} (c^1 - c^0) \\
\text{EV} &\approx q^1 (p^1 - p^0) + \frac{\partial e(\mathbf{P}^1, \mathbf{C}^1, u^1)}{\partial c_1} (c^1 - c^0)
\end{aligned}$$

where  $q^1$  represents quantity of eggs purchased in the state of nature with the AW laws; the third line of equation (2) follows from Shephard's Lemma.

The first term on the right hand side of equation (2) is quantity of eggs demanded multiplied by the price impact of the law change; this term is an approximation to the consumer's willingness to pay to avoid the price increase caused by the AW laws. Summing the first term on the right hand side of equation (2) over all consumers in a given market would result in a measure of aggregate willingness to pay to avoid price increases caused by the AW laws.

If we are willing to assume perfect competition so that all consumers in each market pay the same price for eggs, we obtain the following measure of aggregate willingness to pay to avoid the price increase caused by the AW laws:

$$(3) \quad \sum_{m=1}^M \sum_{t=1}^T \sum_{i=1}^{N_m} q_{imt}^1 (p_{mt}^1 - p_{mt}^0) = \sum_{m=1}^M \sum_{t=1}^T q_{mt}^1 (p_{mt}^1 - p_{mt}^0)$$

where  $m$  indexes markets,  $N_m$  is the number of consumers in market  $m$ ,  $q_{imt}^1$  is total dozens of eggs purchased by consumer  $i$  in market  $m$  in week  $t$ ,  $q_{mt}^1$  is total dozens of eggs purchased in market  $m$  in week  $t$ ,  $(p_{mt}^1 - p_{mt}^0)$  is the impact of the AW laws on the price per

dozen eggs for market  $m$  in week  $t$ , and  $T$  is the number of weeks in which the AW laws affect the price of eggs.

The second term on the right hand side of equation (2) is an approximation to the consumer's willingness to pay to avoid any changes in animal welfare attributed to the AW laws. Assuming that most consumers prefer more animal welfare to less and view the AW laws as leading to better treatment of chickens, the animal welfare term in equation (2) is likely to be negative; i.e., improvements in animal welfare decrease how much the consumer must spend to reach utility level  $u^1$ . Summing the animal welfare term over consumers and multiplying by negative 1 would yield a measure of aggregate willingness to pay to implement animal welfare improvements perceived as resulting from the AW laws, holding any price impacts of the AW laws constant.

Our data do not allow us to estimate the animal welfare term of the aggregate EV, potentially resulting in an estimated EV that is too large. The evidence that is available in the literature suggests that average willingness to pay for improvements in housing for egg-laying hens is low. For example, Chang et al. (2010) perform a hedonic analysis of egg prices using retail scanner data and find that average willingness to pay for hen housing improvements cannot be distinguished from zero, while the results of a meta-analysis by Lagerkvist and Hess (2011) indicate that willingness to pay for more humane hen housing is significantly decreased when consumers are asked if such improvements ought to be legislatively required. It would seem that any error introduced into our EV approximation by failing to include willingness to pay for welfare improvements for chickens will be small. In addition, any overestimation of the aggregate EV will be offset by the

approximation error of the first order expansion used to approximate the EV for an individual consumer, which understates the true value of the portion of the EV caused by the price change; graphically, the size of the error is given by the area bounded by the compensated demand curve for utility level  $u^1$ , a horizontal line emanating from the price axis at  $p^0$ , and a vertical line emanating from the quantity axis at  $q^1$ .

In order to compute the approximate EV, we first estimate the following equation by weighted least squares:

$$(4) \quad price_{imt} = \lambda_i + \lambda_m + \lambda_t + \sum_{t=90}^{105} (\alpha_{LA,t} LA_i + \alpha_{SF,t} SF_i + \alpha_{SD,t} SD_i) + u_{imt}$$

where the weight for each observation is given by its quantity sold in a given market and week. The  $LA_i$ ,  $SF_i$ , and  $SD_i$  terms are dummy variables equal to one for sales in Los Angeles, San Francisco, and San Diego, respectively. The post-implementation period begins in week 90 and runs through week 105 (the last week in the data). The models yields a set of price impact coefficients that vary by week and market. The estimated price impact coefficients are then substituted into the first order approximation to the EV:

$$(5) \quad \hat{EV} = \sum_{t=90}^{105} (\hat{\alpha}_{LA,t} q_{LA,t} + \hat{\alpha}_{SF,t} q_{SD,t} + \hat{\alpha}_{SD,t} q_{SD,t})$$

where  $q_{LA,t}$  is total dozens sold in the Los Angeles market in week  $t$ , and other quantity terms are similarly defined.

Equation (5) is a valid approach to estimating the approximate EV under the assumption of perfect competition within each California market in the data set and zero

covariance between quantity sold and the price impact of the AW laws within a given week. If either of these assumptions were violated, equation (5) would have to be adjusted to allow for price impacts and quantities that vary at a finer level than market and week. However, the retail egg market is likely to be highly competitive within the three California markets in our data set, and the existence of a high degree of covariance between quantities sold and price impacts that is not already captured by allowing impacts to vary by week seems unrealistic.

In addition to estimating the EV for all 16 post-implementation weeks, we also estimate the EV for the last four weeks in the data set. As can be seen in figure 1 and figure 2, egg prices and price impacts were particularly high in the first month following the implementation of the AW laws, suggesting that the consumer welfare impacts of the AW laws in the last four weeks of data may be more representative of what we can expect in the future. Results are presented in table 4.

[TABLE 4 HERE]

Columns (1) and (2) of table 4 show the estimated EV for all three markets and the average weekly EV per household over all 16 post-implementation weeks. The results in columns (1) and (2) suggest that over the sixteen post-implementation weeks in our data set, consumers in the Los Angeles, San Francisco, and San Diego markets would have been willing to pay a maximum of about \$30 million to avoid the price increase caused by Prop 2, or an average of 19.5 cents per household per week. In columns (3) and (4), we estimate the EV for the last four weeks observed in our data set, under the assumption that price

impacts in these last four weeks may be more representative of what we can expect to observe going forward. The weekly EV per household falls to 15 cents. Both sets of estimates of the EV are quite precisely estimated.

The results in columns (3) and (4) suggest that on average the households in the three California markets included in our data set would be willing to pay an annual lump sum tax of \$7.98 in lieu of the increase in the price per dozen eggs caused by Prop 2. Scaling the per household average annual EV by the number of households in California (according to projected 2014 population figures from the US Census Bureau) yields a figure of \$105 million; this is our best estimate of the annual lump sum that California consumers would be willing to pay in lieu of the price impacts of Prop 2.<sup>10</sup> This figure is very similar to the ex-ante consumer welfare impact of the AW laws estimated by Allender and Richards (2010), who assume a 20% increase in the retail price of eggs and calculate the compensating variation (CV) of the AW laws to be approximately \$106 million at 2007-2008 prices. Since the EV is necessarily lower than the CV, and our estimate of the EV likely understates the welfare impact of the AW laws for the reasons cited above, our results suggest that the consumer welfare impact of the AW laws was larger than anticipated. However, our consumer welfare impacts were generated using estimates of short-run price effects of the AW laws. Whether our estimates are representative of long-

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<sup>10</sup> As already mentioned, Nielsen scanner data include about 90% of all retail grocery sales nationally. If we inflate our EV estimates by 0.9, we conclude that households would be willing to pay \$8.86 per year in lieu of the price impacts of Prop 2, equivalent to an annual figure for all of California of \$117 million.

run consumer welfare effects will depend in large part on how the long-run and short-run elasticities of demand for eggs compare.

*Impacts on net revenue for egg producers*

Our analysis of the impacts of the AW laws on producer welfare focuses on impacts on net revenue earned by egg producers. In the short run, impacts of the AW laws on gross revenue and variable costs could result in egg producers either earning higher net revenue or suffering losses as a result of the new policy. The impact of the AW laws on net revenue at time  $t$  for a single egg producer is given by:

$$(6) \quad NR_{it}^1 - NR_{it}^0 = (\tilde{p}_t^1 q_{it}^1 - \tilde{p}_t^0 q_{it}^0) - (q_{it}^1 AVC_{it}^1 - q_{it}^0 AVC_{it}^0)$$

where  $\tilde{p}_t^1$  is the price per dozen eggs paid to producer  $i$  at time  $t$  with the AW laws in place and  $\tilde{p}_t^0$  is the price that would have been received by the same producer in week  $t$  had the AW laws not been passed.  $AVC_{it}^1$  and  $AVC_{it}^0$  are average variable cost of production for producer  $i$  at time  $t$  with and without the AW laws, respectively. The first term on the right hand side of equation (6) is the impact of the AW laws on gross revenue for producer  $i$  at time  $t$ , and the second term is the impact on total variable cost.<sup>11</sup>

The limitations of our data set make it impossible to directly estimate impacts on net revenue for egg producers. Instead, we combine information from other sources with additional assumptions to obtain what we believe is a reasonable estimate of the impact of

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<sup>11</sup> We refer to variable cost rather than total cost because we are discussing short-run impacts.

the AW laws on egg producer net revenue. First, in order to convert observed retail sales into gross revenue for egg producers, we compare the monthly average retail price for grade A large eggs in the US as reported by BLS with the monthly price received by US egg producers over the same period as reported to the USDA National Agricultural Statistical Service to obtain the producer share of the egg retail price for each month from April 2013 through April 2015. These data indicate that the farmer share of the US retail egg price averaged 61% over time period covered by our data (60% in the post-implementation period). We assume that the producer share of the egg retail price is the same in California as is it on average nationally, and we estimate gross revenue earned each week by egg producers from sales of each UPC in states included in our data set using the product of average price (i.e., the weekly price given in the scanner data), quantity sold, and the producer share of the retail price.

Next, since we do not observe producer costs, we instead use projected impacts of the AW laws on producer costs as well as pre-implementation costs of production for battery cage and cage-free production systems in California from Sumner et al. (2011). The cost data and projected cost impacts were generated using data from a small sample of California egg producers that produce eggs using both cage and non-cage production systems. Costs consist primarily of pullets (young chickens), feed, housing, and labor.<sup>12</sup>

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<sup>12</sup> It should be noted that under Prop 2, producers are not obligated to switch to non-cage production systems, and as mentioned earlier in this article, there is anecdotal evidence that producers are maintaining battery cages while reducing flock density. This suggest that the cost impact estimates of Sumner et al. (2011) may be biased upwards, and as a results we may understate the benefits of Prop 2 for egg producers. The degree of uncertainty over the impacts of Prop 2 on production costs should be taken into account when interpreting our estimates of impacts on producer net revenue.

For conventional egg production systems prior to the implementation of the AW laws, we assume that the average cost of producing a dozen eggs is \$0.745, while cage-free systems have an average cost of production equal to \$1.05; these are the midpoints of the range of production costs for each production system as reported by Sumner et al. (2011).

Once the AW laws are implemented, we allow for two different cost impact scenarios: a high cost impact scenario in which the average variable cost of production for conventionally-produced UPCs increases by 40 cents, and a low cost impact scenario in which the average variable cost of production for conventional UPCs increases by 30.5 cents; these figures correspond to the high and low cost impact scenarios considered by Sumner et al. (2011). We assume that production costs for cage free and organic UPCs do not change. Because eggs sold outside of California will be used to represent the counterfactual “no AW laws” scenario, we hold their production costs fixed at pre-implementation California levels as reported by Sumner et al. (2011). For conventionally-produced UPCs sold in California, production costs increase following implementation of the AW laws as described above. The total cost of producing eggs sold by each UPC each week is obtained by multiplying average variable cost by quantity sold, and net revenue is calculated by subtracting total variable cost from our gross revenue estimate.

Prior to estimating the impact of the AW laws on net revenue, we provide visual evidence that the identifying assumptions of difference-in-differences are satisfied. Figure 4 shows trends for our measure of net revenue in the California markets and control markets in our data.

[FIGURE 4 HERE]

As in the case of the other outcomes examined in our data set, trends in California and control markets are very similar until immediately prior to implementation of the AW laws. Only the high cost impact scenario is shown in figure 4, but the graph of the low cost impact scenario is very similar to what we see in the figure. Figure 4 also suggests that our estimated impacts on net revenue are likely to be conservative.

To estimate the impact of the AW laws on net revenue, we estimate the following regression:

$$(7) \quad NR_{imt} = \varphi_i + \varphi_m + \varphi_t + \psi_1 CA_m \times Post_t + \psi_2 CA_m \times I(t \geq 102) + w_{imt}$$

where  $NR_{imt}$  is net revenue earned by producers from sales of UPC  $i$  in market  $m$  in week  $t$ .<sup>13</sup> The  $I(t \geq 102)$  term is a dummy variable equal to one for all observations occurring in the last four weeks of our data set (i.e., in week 102 or later), and it allows us to obtain a separate estimate of the impact of net revenue for the last four weeks of data. The average impact of the AW laws in the post-implementation period is given by  $\psi_1$  plus the product of  $\psi_2$  and the average of  $I(t \geq 102)$  in the post-implementation period, while the average impact over the last four weeks in the data is given by  $\psi_1 + \psi_2$ . Average impacts over weeks and UPCs are transformed into total impacts on net revenue by multiplying by the

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<sup>13</sup> Since we use California production costs for all UPCs, this definition of the dependent variable in equation (7) is not quite correct. A better name for the variable might be “California net revenue,” with sales of UPCs in other states providing us with the counterfactual “no AW laws” growth in net revenue that would have been earned from sales of eggs in California in the absence of the AW laws.

relevant total number of UPCs (depending on whether we are estimating impacts on all California UPCs, UPCs sold just in California, or UPCs sold in both sets of markets) and weeks (either 16 or four). Estimation results are presented in table 5.

[TABLE 5 HERE]

Table 5 gives our estimates of the impact of the AW laws on net revenue under the high cost impact scenario. In column (1), we see that the total impact of the AW laws on net revenue is estimated to be about \$2.8 million, an increase of 11% relative to what would have been earned through sales of eggs in California in the absence of the AW laws; the impact is somewhat imprecisely estimated when basing inference on the two-way clustered standard errors. In columns (2) and (3) it is apparent that nearly all the increase in net revenue was realized through impacts on UPCs sold only in California; the per UPC impact for UPCs sold strictly in California is 2.3 times the per-UPC impact for UPCs sold in both sets of markets. However, regardless of whether we compare total impacts on net revenue or impacts per UPC, the confidence intervals for UPCs sold strictly in California and UPCs sold in both sets of markets overlap. In columns (4), (5), and (6) of panel A, we see that impacts on net revenue over the last four weeks were negative for all UPCs sold in California as well as the two subsets of UPCs. In general, estimated impacts on net revenue over the final four weeks in the data are estimated imprecisely.

Panel B tells a story similar to that of panel A. The lower assumed impact on the average cost of production pushes up the estimated impacts on net revenue in all columns relative to what we see in panel A; the total impact on net revenue over the 16 post-

implementation weeks in our data is \$3.7 million (or a 14.5% increase over what producers would have earned in the absence of the AW laws) under the low cost impact scenario. Impacts on UPCs sold in both sets of markets are still quite small relative to those of UPCs sold only in California, and impacts measured over the final four weeks in our data set cannot be statistically distinguished from zero at a 5% significance level, regardless of how we divide UPCs.

The overall conclusion gleaned from examining table 5 is that positive welfare impacts of the AW laws on egg producers were short lived, and nearly all benefits flowed to producers whose output was marketed under UPCs sold only in California. However, the results in table 5 were estimated under the assumption that the impacts of the AW laws on price, quantity sold, and production costs were first realized at the moment that implementation of the new housing standards became legally required. In figure 5 we examine the sensitivity of the estimated effect of the AW laws on net revenue under the high cost impact scenario by varying the week in which production costs were first increased as a result of the AW laws.

[FIGURE 5 HERE]

The horizontal axis of figure 5 gives the number of weeks prior to implementation of the AW laws in which producers selling eggs to California first adjusted production practices and experienced higher costs of production as a result. In order to isolate the effect of assumptions about the producer adjustment process on our results, we maintain the assumption that impacts on gross revenue and quantity sold were not realized until after

implementation of the AW laws. As we move the date at which producers began to experience higher costs further back in time (i.e., moving from left to right on the horizontal axis of figure 5), the total impact of the AW laws on net revenue over the 16 post-implementation weeks grows from under \$3 million to nearly \$5 million.

Figure 5 suggests that the magnitude of the impact on net revenue is sensitive to assumptions about the timing of changes in production costs, but the effect remains rather small. Although not shown here, when we repeat this exercise for the impact on net revenue over the last four weeks of our data set and for both the high and low cost impact scenarios, we find that the impact over the last four weeks of our data set is positive only when producers experience higher costs at least 17 weeks prior to implementation of the AW laws and only for the low cost impact scenario; in all cases, the 95% confidence interval around the net revenue impact over the last four weeks of the data includes zero.

### **Robustness checks**

#### *Placebo impacts on control markets*

While we cannot directly test our identifying assumptions, we can conduct robustness checks that provide evidence for or against the validity of our methods. Firstly, we estimate “placebo” impacts on price, quantity sold, and value of sales for eggs in each of the markets used in our control group. That is, we drop California from the sample and estimate equation (2) after replacing the  $CA_m$  dummy with a dummy variable for observations in a given control market. Results are presented in table 6.

[TABLE 6 HERE]

As shown in column (1) of table 6, placebo price impacts are very small in magnitude when estimated using each of the control markets in turn as the treatment market. The estimated impact on the average price per dozen eggs is significant at the 10% level when using Chicago as the treatment market and basing inference on two-way clustered standard errors, but the magnitude of the effect is so small that we do not interpret the Chicago result as evidence against our identifying assumptions. Estimated impacts on quantity are imprecisely estimated for each of the control markets, just as they were for the case of California. Estimated impacts on the value of sales are small in magnitude and imprecise. Overall, the placebo impact lend support to our identification strategy.

*Robustness to violations of the common trends assumption*

Next, we check the robustness of our results to violations of the common trends assumption. Specifically, we estimate models of the following form:

$$(8) \quad y_{imt} = \lambda_i + \lambda_m + \lambda_t + \delta CA_m \times t + \alpha_1 CA_m \times Post_t + u_{imt}$$

$$(9) \quad y_{imt} = \lambda_i + \lambda_m + \lambda_t + \delta_1 CA_m \times t + \delta_2 CA_m \times t \times t + \alpha_1 CA_m \times Post_t + u_{imt}$$

Equations (8) and (9) are estimated for price, quantity, value of sales, and egg producer net revenue. In addition, we estimate a price model that allows impacts to vary by market or week, and use the resulting coefficients to estimate the approximate EV.

If the common trends assumption is valid, then allowing for California-specific trends should have no effect on estimated impacts of the AW laws on price, quantity, and sales. Since we know that trends in outcomes began to diverge shortly before

implementation of the law, equation (8) and equation (9) will provide an especially harsh test of the common trends assumption; the structural breaks that we can see in figure 1 will be incorporated into the California-specific trends used to predict outcomes in the absence of the AW laws, resulting in smaller estimated impacts of the AW laws. Results are given in table 7.

[TABLE 7 HERE]

Panel A of table 7 presents estimated impacts on price, quantity, sales, the approximate EV, and impacts on net revenue based on equation (8). The impact on the average price per dozen falls from 75 cents to 58 cents; the magnitude of the price impact is still quite large and precisely estimated. The quantity impact falls in absolute value by about 30,000 dozens per week, and inference is unchanged relative to our regression models without a California-specific trend. The impact on sales falls from about \$1 million per week to \$740,000 per week, and is still estimated precisely.

The approximate EV in panel A is about \$22 million (a drop of \$8 million from our earlier estimate), while the precision of the estimated EV is hardly affected. Although it is not shown in the table, the estimated EV for the final four weeks in our data falls from \$5.9 million to \$3.6 million, implying that California household would be willing to pay \$2.80 annually in lieu of the price changes caused by Prop 2; the approximate annual EV for California as a whole would be about \$64 million under this scenario. Lastly, the impact on egg producers drops from a net revenue increase of \$2.8 million to a loss in net revenue

of \$2.6 million; we cannot reject the null hypothesis that the impact of Prop 2 on egg producer net revenue is zero when allowing for a California-specific linear trend.

In panel B, we allow for a quadratic California-specific trend in each outcome equation, as in equation (9). The results are similar to those of panel A, although the magnitude of effects has fallen across the board. Overall, our takeaway from table 7 is that consumer welfare effects remain large when we change the functional form used to produce our main results, while producer welfare effects are sensitive to assumptions about trends but remain relatively small regardless of specification. More generally, we acknowledge that our data are better suited to examining impacts of Prop 2 on California consumers, and that our results for estimated impacts on net revenue should be interpreted somewhat cautiously.

## **Conclusion**

In this paper, we used retail scanner data from three major California markets and three non-California markets to estimate the impact of legislation meant to guarantee more humane housing conditions for egg-laying hens. Over the 16 post-implementation weeks observed in our data set, we find that the AW laws increased prices by about 22%, decreased quantity sold by around 8%, and increased the value of sales by 12%; the price and sales impacts are precisely estimated, whereas we cannot statistically distinguish the quantity impact from zero. We re-estimate impacts on price, quantity, and sales after segregating UPCs by whether they are observed as being sold only in the California markets in our data or in California markets as well as in our non-California control markets. The results suggest that California producers increased supply in response to the

higher prices generated by the AW laws while out-of-state producers sharply reduced exports to California.

In addition, we estimate that consumers in the three California markets included in our data would have paid \$30 million to avoid the price increases caused by the AW laws over the 16 post-implementation weeks observed in our data set. Extrapolating this figure up to an annual figure for the state of California as a whole yields a total of \$105 million. Although impacts on egg producer net revenue are positive and precisely estimated, they appear to have faded once the initial price spike following implementation of the AW laws had passed.

While we examine a relatively short post-implementation time horizon in this paper, in one sense this is an advantage. An outbreak of bird flu shortly after the period of time observed in our data set may have sharply increased egg prices, and any conclusions drawn about the impacts of the AW laws in the context of a bird flu outbreak might not be representative of the impacts of the AW laws going forward. However, an analysis of the impact of the AW laws that examined long-run impacts would of course be useful. In the long run, we might expect both demand and supply of eggs in the California retail egg market to be more elastic than they were over the 16 post-implementation weeks observed in our data set. Our back-of-the-envelope estimate of the own price elasticity of demand for eggs is  $-0.36$ , which is consistent with previous literature suggesting that demand for eggs is highly inelastic (Okrent and Alston 2011). Therefore whether negative consumer welfare effects of the law can be mitigated going forward may depend in large part on the

elasticity of the long-run supply curve, particularly for California producers if out-of-state producers have indeed begun an exodus from the California market.

Regardless, it should be noted that our methods are likely to underestimate the size of consumer welfare losses associated with the AW laws over the time horizon considered in this paper. While consumer welfare impacts may be mitigated in the long run by greater sensitivity to price changes, the fact that we estimate large consumer welfare losses over a short period of time should be reason enough to give anyone pause before suggesting that laws such as those passed in California are an inexpensive way to improve farm animal welfare.

## Appendix

### *Statistical inference: multiway clustered standard errors*

To estimate the two-cluster robust covariance matrix, we follow the procedure outlined by Cameron, Gelbach, and Miller (2011). Let  $t$  index weeks and let  $i$  index UPCs. Consider estimating the following regression:

$$(A.10) \quad y_{it} = \mathbf{x}'_{it} \boldsymbol{\beta} + u_{it}$$

Under two-way clustering, we make the following assumption:

$$(A.11) \quad E[u_{it} u_{i't'} | \mathbf{x}_{it}, \mathbf{x}_{i't'}] = 0, \text{ unless } i = i', \text{ or } t = t'$$

Observations that share the same week or UPC are allowed to be arbitrarily correlated. Let  $V^i(\hat{\boldsymbol{\beta}})$  be the cluster-robust covariance matrix for the estimated regression parameters that results when clustering on UPC, let  $V^t(\hat{\boldsymbol{\beta}})$  be the cluster robust covariance

matrix when clustering on week, and let  $V^{iwt}(\hat{\beta})$  be the cluster-robust covariance matrix when clustering on UPC-week (e.g., two observations that share the same UPC and week of observation would be members of the same cluster). In this case, Cameron, Gelbach, and Miller (2011) show that the multiway cluster-robust covariance matrix,  $V(\hat{\beta})$ , can be written as:

$$(A.12) \quad V(\hat{\beta}) = V^i(\hat{\beta}) + V^t(\hat{\beta}) - V^{iwt}(\hat{\beta})$$

Each of the three right hand side components of equation (A.12) can be estimated as a separate cluster-robust covariance matrix, and the results can be combined to form the two-way cluster robust covariance matrix. All results were estimated in Stata, and each of the three cluster-robust covariance matrices in equation (A.12) was multiplied by the default Stata degrees of freedom correction for cluster-robust covariance matrix estimation:  $[G / (G - 1)][(N - 1) / (N - K)]$ , where  $G$  is the number of clusters (the number of weeks, UPCs, or UPC-weeks, as the case may be),  $N$  is the total sample size, and  $K$  is the number of regression parameters.

*Statistical inference: wild cluster bootstrap*

Wild cluster bootstrap p-values were generated by closely following the procedure outlined by Webb (2014). For impacts on prices, quantities, and sales, we used the following algorithm:

1. Estimate the regression model, clustering standard errors by market (Los Angeles, San Francisco, San Diego, Chicago, Phoenix, Salt Lake City/Boise).

2. Calculate the t-statistic for the main parameter of interest (or function of parameters, in the case of our consumer and producer welfare measures), and save it.
3. Re-estimate the regression model, imposing the null hypothesis of no effect on the parameter or function of parameters of interest, as recommended by Davidson and MacKinnon (1999).
4. Save the fitted values for the dependent variable and the residual generated by the previous step.

Next, we begin our bootstrap replications. For each replication, we do the following:

5. Generate the transformed outcome variable,  $\tilde{y}_{imt}$ , as:

$$(A.13) \quad \tilde{y}_{imt} = \hat{y}_{imt} + w_m \hat{e}_{imt}$$

where  $\hat{y}_{imt}$  and  $\hat{e}_{imt}$  are the fitted value and residual generated by estimating the regression model of interest after imposing the null hypothesis, and  $w_m$  is a weight that can take on one of six values:

$$(A.14) \quad w_m = -\sqrt{1.5}, -1, 0, 1, \sqrt{1.5}, \text{ each with probability } \frac{1}{6}$$

Details on the theory behind these weights can be found in Webb (2014). For each bootstrap replication, all observations from the same state share the same value of  $w_m$ .

6. Next, we re-estimate our main regression model, this time using our transformed dependent variable, clustering our standard errors by market.
7. Lastly, we calculate and save the t-statistic for the coefficient on  $CA_m \times Post_t$ , estimated in the previous step.

For each model, we use 1,999 bootstrap replications. The p-values reported in the main text were calculated as:

$$(A.15) \quad p = \frac{1}{(B+1)} \left( 1 + \sum_{b=1}^B I(|t_b^*| > |\hat{t}|) \right)$$

where  $\hat{t}$  is the t-statistic for the parameter (or function of regression parameters) of interest from step 1,  $b$  indexes bootstrap replications,  $B$  is the total number of replications,  $t_b^*$  is a bootstrapped t-statistic, and  $I(\cdot)$  is an indicator function taking a value of one when its argument is true and zero otherwise.

In step 3, imposing the null hypothesis is straightforward for our models of impacts on price, quantity sold, and sales values. For those outcomes, imposing the null hypothesis amounts to estimating the following regression:

$$(A.16) \quad y_{imt} = \lambda_i + \lambda_m + \lambda_t + u_{imt}$$

That is, we re-estimate equation (2) assuming that  $\beta_3 = 0$ . For the hypothesis of no effect, this amounts to re-estimating the model without  $CA_m \times Post_t$ . For the approximate EV, which is a function of multiple regression parameters, we impose the null hypothesis by estimating equation (4) from the main text while imposing a linear constraint that forces

equation (5) to zero, or forces the EV over the last four weeks in the data to zero, depending on the hypothesis being tested. For net revenue, we impose the null hypothesis in the same way, estimating (7) while imposing a linear constraint that either sets the net revenue impact to zero, or sets the net revenue impact over the last four weeks observed in the data set to zero, depending on the hypothesis being tested.

*Simulation evidence: wild cluster bootstrap*

The theory and simulations presented in MacKinnon and Webb (2015) suggest that the bootstrap procedure described here is likely to reject the null hypothesis less often than the chosen size of the test would indicate. However, MacKinnon and Webb (2015) examine scenarios with small numbers of clusters experiencing a policy change and a larger number of clusters serving as controls, rather than the even split between treatment and control exhibited in our paper. Therefore we conducted a small Monte Carlo experiment to provide evidence for the suitability of our wild cluster bootstrap procedure as an inference tool in the present context. We ran 2,000 simulations designed to simulate the hypothesis testing process for our price regressions under the assumption that the null hypothesis of no effect is true. Each simulation consisted of the following steps:

1. Create a data set with 83,765 observations distributed among six clusters as in our scanner data set.
2. Create a binary indicator equal to one if a cluster experiences a policy change and zero otherwise; this indicator is set equal to one in our three simulated California

clusters for the same number of observations for which  $CA_m \times Post_t$  equals one in our data.

3. Generate the following random variable:

$$(A.17) \quad y_{ig} = 1 + \varepsilon_g + u_{ig}$$

where  $i$  indexes individual observations,  $g$  indexes clusters, and  $y_{ig} \sim N(0,1)$ ,  $\varepsilon_g \sim N(0, \sigma^2)$ ,  $u_{ig} \sim N(0, \omega^2)$ ,  $Cov(\varepsilon_g, u_{ig}) = 0$ ; i.e.,  $y_{ig}$  is a standard normal random variable equal to the sum of an intercept, a cluster-level shock, and an idiosyncratic shock. To calibrate  $\sigma^2$  and  $\omega^2$ , we used the Stata *loneway* command to estimate the intracluster correlation coefficient for *price* when clustering on market; we set  $\sigma^2$  equal to the resulting estimate (approximately 0.08) in all simulations.

4. Regress  $y_{ig}$  on the binary policy indicator and an intercept.
5. Calculate the cluster-robust t-statistic for the coefficient on the policy indicator.
6. Perform 399 wild cluster bootstrap replications using the exact same procedure described in the previous subsection.
7. Test the null hypothesis of no effect at a 5% significance level by comparing the absolute value of the t-statistic from step 5 to the distribution of the absolute value of the t-statistic generated by the bootstrap procedure; we reject the null if:

$$(A.18) \quad \frac{1}{(B+1)} \left( 1 + \sum_{b=1}^B I(|\hat{t}_b^*| > |\hat{t}|) \right) < 0.05$$

We reject the null in 2.75% of our simulations; the 95% confidence interval around this estimated rejection rate is [0.020, 0.035], where the interval is calculated as  $0.0275 \pm 1.96\sqrt{(0.0275(1-0.0275))/(1,999)}$ . We interpret this as evidence that our wild cluster bootstrap procedure is likely to yield somewhat conservative results with respect to hypothesis testing.

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**Table 1. Summary Statistics, Prior to Implementation of AW Laws**

	California markets		Non-California markets		Difference in means	p-value
	Mean	Standard deviation	Mean	Standard deviation		
<i>A. Average price, quantity, and egg sales by week</i>						
Price per dozen	2.49	0.26	1.87	0.14	0.63	0.000
Dozens sold (1,000 dozens)	2,691	209	2,073	202	618	0.000
Sales (\$100,000)	65.66	6.60	37.97	4.77	27.69	0.000
Total weeks	89		89			
<i>B. Product attributes, by UPC</i>						
<i>Production system</i>						
Conventional	0.89	0.32	0.95	0.22	-0.06	0.009
Cage free	0.09	0.28	0.04	0.21	0.04	0.027
Organic	0.07	0.25	0.02	0.15	0.05	0.010
<i>Color</i>						
White Egg	0.86	0.35	0.92	0.27	-0.06	0.026
Brown Egg	0.14	0.35	0.08	0.27	0.06	0.027
<i>Grade</i>						
AA	0.67	0.47	0.59	0.49	0.07	0.390
A	0.32	0.47	0.39	0.49	-0.08	0.360
<i>Packaging</i>						
Molded Tray	0.77	0.42	0.61	0.49	0.16	0.048
Molded Tray In Wrap	0.09	0.28	0.05	0.21	0.04	0.337
Box	0.07	0.25	0.28	0.45	-0.21	0.003
Tray In Wrap	0.05	0.22	0.03	0.17	0.02	0.307
Total UPCs	466		512			

*Notes:* Price per dozen and UPC summary statistics calculated weighting each UPC by total dozens sold each week.

**Table 2. Impact of the AW Laws on Egg Retail Price, Quantity, and Sales Value**

	Weekly average price per dozen	Dozens sold per week (1,000 dozens)	Value of sales per week (\$100,000)
	(1)	(2)	(3)
<i>A. Models with covariates</i>			
Impact of the AW laws	0.753	-131.99	10.59
Two-way clustered standard error	[0.102]	[211.34]	[4.74]
Two-way clustered p- value	(0.000)	(0.534)	(0.028)
Wild cluster bootstrap p-value	(0.004)	(0.313)	(0.049)
Observations	83,765	142,170	142,170
<i>B. Models with fixed effects for UPC, week, and market</i>			
Impact of the AW laws	0.746	-131.99	10.59
Two-way clustered standard error	[0.074]	[221.51]	[4.93]
Two-way clustered p- value	(0.000)	(0.554)	(0.034)
Wild cluster bootstrap p-value	(0.001)	(0.327)	(0.064)
Observations	83,765	142,170	142,170

Notes: Price models in column (1) have fewer observations because UPCs with no units sold in a given week have no price. Models in column (1) estimated by weighted least squares, with weights given by quantity sold each week. Models in columns (2) and (3) estimated by OLS. Two-way clustered standard errors cluster on week and UPC. Wild cluster bootstrap allows for clustering by market. See appendix for additional details of inference procedures.

**Table 3. Impacts by Where UPCs are Sold**

	Weekly average price per dozen	Dozens sold per week (1,000 dozens)	Value of sales per week (\$100,000)
	(1)	(2)	(3)
<i>A. Impacts on UPCs sold in California and in control markets (180 UPCs)</i>			
Impact of the AW laws	0.760	-222.73	1.01
Two-way clustered standard error	[0.074]	[121.20]	[2.47]
Two-way clustered p- value	(0.000)	(0.069)	(0.684)
Wild cluster bootstrap p-value	(0.001)	(0.456)	(0.147)
Observations	40,380	66,675	66,675
<i>B. Impacts on UPCs only sold in California (286 UPCs)</i>			
Impact of the AW laws	0.704	191.82	11.303
Two-way clustered standard error	[0.078]	[187.39]	[4.52]
Two-way clustered p- value	(0.000)	(0.308)	(0.014)
Wild cluster bootstrap p-value	(0.001)	(0.205)	(0.705)
Observations	43,385	75,495	75,495

Notes: Price models in column (1) have fewer observations because UPCs with no units sold in a given week are treated as missing data. Models in column (1) estimated by weighted least squares, with weights given by quantity sold each week. Models in columns (2) and (3) estimated by OLS. Two-way clustered standard errors cluster on week and UPC. Wild cluster bootstrap allows for clustering by market. See appendix for additional details of inference procedures.

**Table 4. Consumer Welfare Impact of Egg Price Increase**

	All 16 post-implementation weeks		Last four post-implementation weeks	
	Total (millions of dollars)	Average per household per week (dollars)	Total (millions of dollars)	Average per household per week (dollars)
	(1)	(2)	(3)	(4)
Approximate equivalent variation	30.04	0.195	5.91	0.153
Two-way clustered standard error	[1.47]		[0.59]	
Two-way clustered p-value	(0.000)		(0.000)	
Wild cluster bootstrap p-value	(0.001)		(0.012)	

Notes: Household averages computed using population figures for market areas provided by Nielsen. Two-way clustered standard errors cluster on week and UPC. Wild cluster bootstrap allows for clustering by market. See appendix for additional details of inference procedures.

**Table 5. Impacts on Egg Producer Net Revenue**

	All 16 post-implementation weeks			Last four post-implementation weeks		
	All UPCs sold in California	UPCs sold only in California	UPCs sold in all markets	All UPCs sold in California	UPCs sold only in California	UPCs sold in all markets
	(1)	(2)	(3)	(4)	(5)	(6)
<i>A. High cost increase scenario (average cost of producing a dozen eggs increases by 40 cents)</i>						
Impact on net revenue (millions of dollars)	2.80	2.34	0.64	-0.66	-0.26	-0.37
Two-way clustered standard error	[1.57]	[1.22]	[0.84]	[0.47]	[0.33]	[0.20]
Two-way clustered p-value	(0.077)	(0.057)	(0.446)	(0.163)	(0.427)	(0.064)
Wild cluster bootstrap p-value	(0.007)	(0.014)	(0.351)	(0.121)	(0.656)	(0.007)
Total UPCs	466	286	180	466	286	180
<i>B. Low cost increase scenario (average cost of producing a dozen eggs increases by 30.5 cents)</i>						
Impact on net revenue (millions of dollars)	3.71	2.97	0.95	-0.40	-0.60	-0.29
Two-way clustered standard error	[1.63]	[1.32]	[0.86]	[0.45]	[0.31]	[0.19]
Two-way clustered p-value	(0.025)	(0.026)	(0.270)	(0.375)	(0.847)	(0.117)
Wild cluster bootstrap p-value	(0.001)	(0.001)	(0.012)	(0.148)	(0.804)	(0.049)
Total UPCs	466	286	180	466	286	180

Notes: All models include fixed effects for UPC, market, and week. Two-way clustered standard errors cluster on week and UPC. Wild cluster bootstrap allows for clustering by market. See appendix for additional details of inference procedures.

**Table 6. Placebo Impacts on Control Markets**

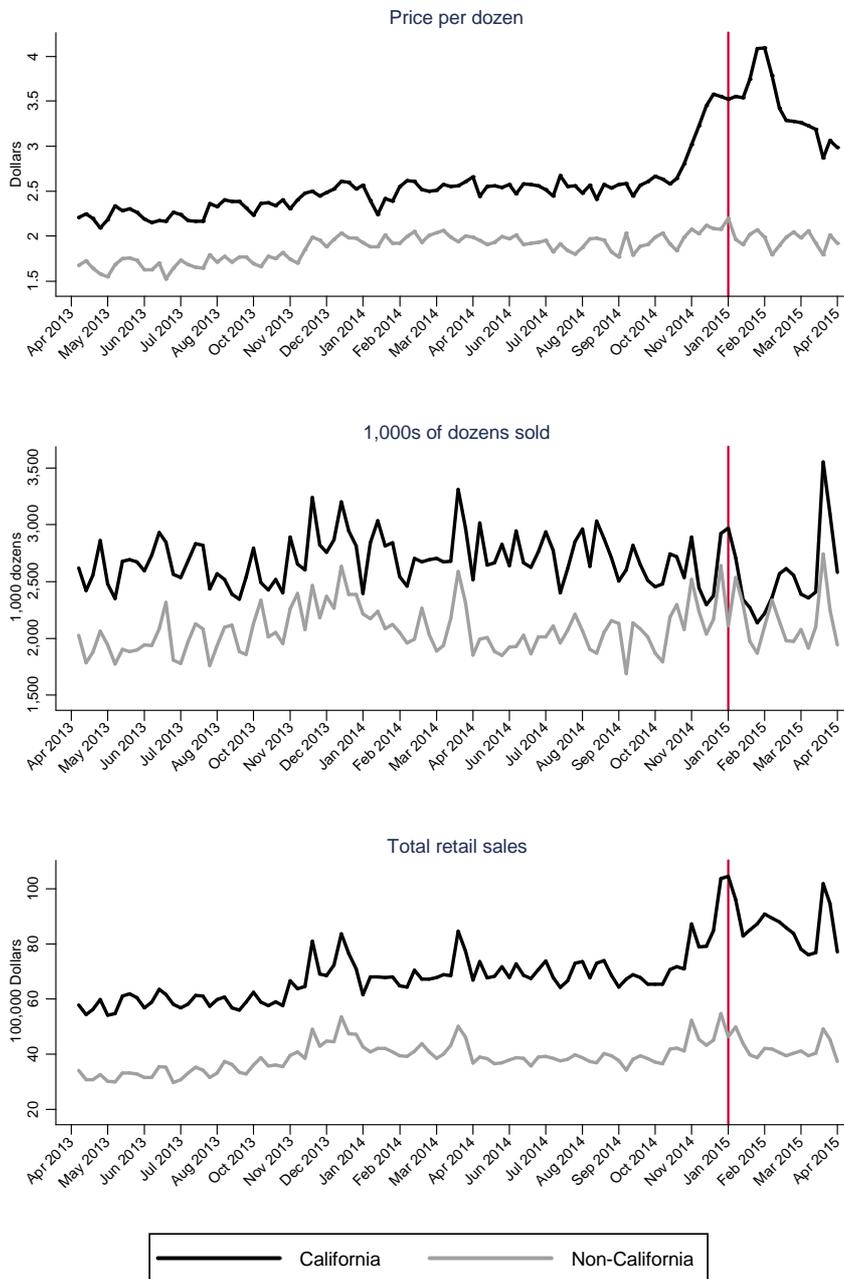
	Weekly average price per dozen (1)	Dozens sold per week (1,000 dozens) (2)	Value of sales per week (\$100,000) (3)
<i>A. Chicago as treatment market</i>			
Impact of the AW laws	0.092	-147.02	-1.23
Two-way clustered standard error	[0.041]	[95.76]	[1.34]
Two-way clustered p- value	(0.028)	(0.128)	(0.363)
Wild cluster bootstrap p-value	(0.044)	(0.312)	(0.760)
Observations	40,139	66,570	66,570
<i>B. Phoenix as treatment market</i>			
Impact of the AW laws	-0.057	93.56	1.33
Two-way clustered standard error	[0.053]	[91.96]	[0.98]
Two-way clustered p- value	(0.281)	(0.311)	(0.178)
Wild cluster bootstrap p-value	(0.698)	(0.858)	(0.461)
Observations	40,139	66,570	66,570
<i>C. Salt Lake City/Boise as treatment market</i>			
Impact of the AW laws	-0.023	33.914	-0.296
Two-way clustered standard error	[0.039]	[59.268]	[0.788]
Two-way clustered p- value	(0.553)	(0.567)	(0.707)
Wild cluster bootstrap p-value	(0.799)	(0.901)	(0.919)
Observations	40,139	66,570	66,570

*Notes:* All models include fixed effects for UPC, market, and week. Models in column (1) estimated by weighted least squares, with weights given by quantity sold each week. Models in columns (2) and (3) estimated by OLS. Two-way clustered standard errors cluster on week and UPC. Wild cluster bootstrap allows for clustering by market. See appendix for additional details of inference procedures.

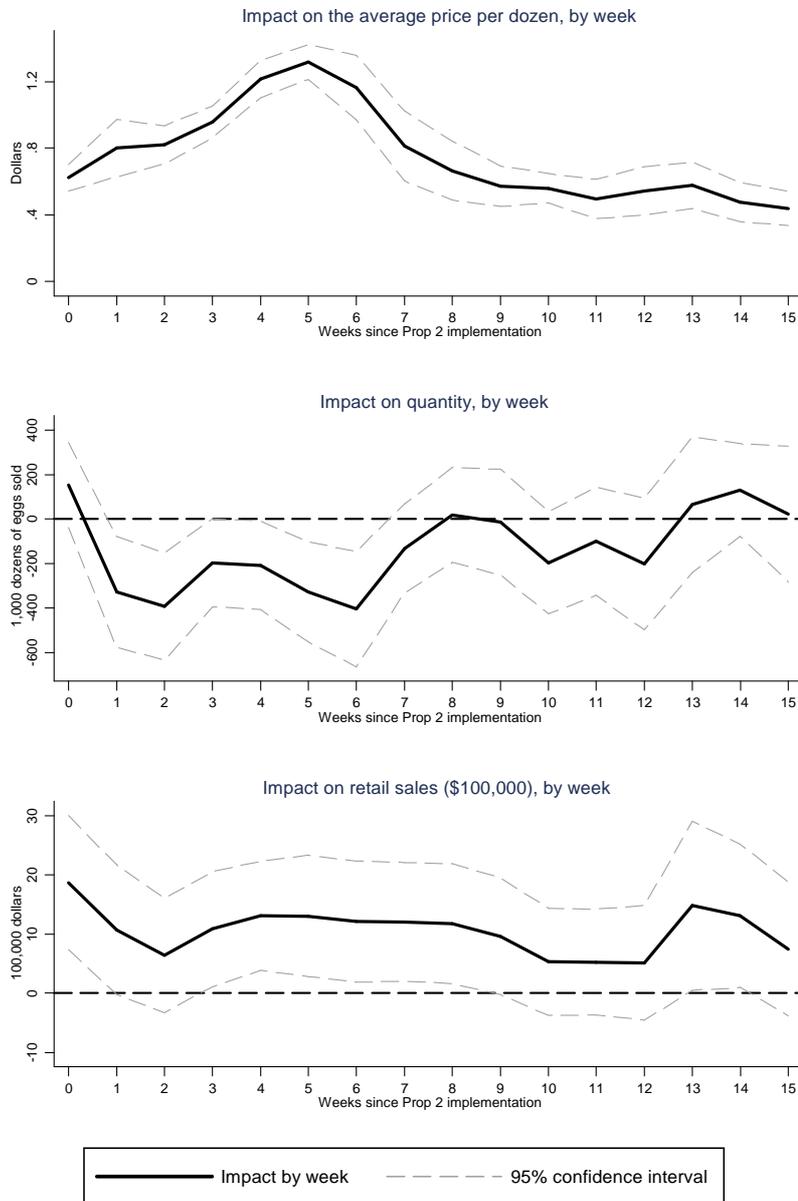
**Table 7. Robustness Checks for the Common Trends Assumption**

	Weekly average price per dozen	Dozens sold per week (1,000 dozens)	Value of sales per week (\$100,000)	Approximate equivalent variation (millions of dollars)	Egg producer net revenue, high cost scenario (millions of dollars)
	(1)	(2)	(3)	(4)	(5)
<i>A. Allowing California-specific linear trend</i>					
Impact of the AW laws	0.577	-104.70	7.41	22.6	-2.56
Two-way clustered standard error	[0.088]	[177.71]	[3.67]	[2.35]	[1.84]
Two-way clustered p- value	(0.000)	(0.557)	(0.046)	(0.000)	(0.167)
Wild cluster bootstrap p-value	(0.001)	(0.262)	(0.021)	(0.001)	(0.163)
Observations	83,765	142,170	142,170		
<i>B. Allowing California-specific quadratic trend</i>					
Impact of the AW laws	0.461	-93.83	5.66	13.1	-6.87
Two-way clustered standard error	[0.130]	[99.61]	[1.62]	[4.42]	[2.69]
Two-way clustered p- value	(0.001)	(0.348)	(0.001)	(0.004)	(0.012)
Wild cluster bootstrap p-value	(0.001)	(0.142)	(0.112)	(0.009)	(0.003)
Observations	83,765	142,170	142,170		

Notes: All regressions include fixed effects for UPC, market, and week. Price models in column (1) have fewer observations because UPCs with no units sold in a given week have no price. Models in column (1) estimated by weighted least squares, with weights given by quantity sold each week. Models in columns (2) and (3) estimated by OLS. Two-way clustered standard errors cluster on week and UPC. See the appendix for details on the two-way clustered standard errors and the wild cluster bootstrap procedure used to generate the results in the table.

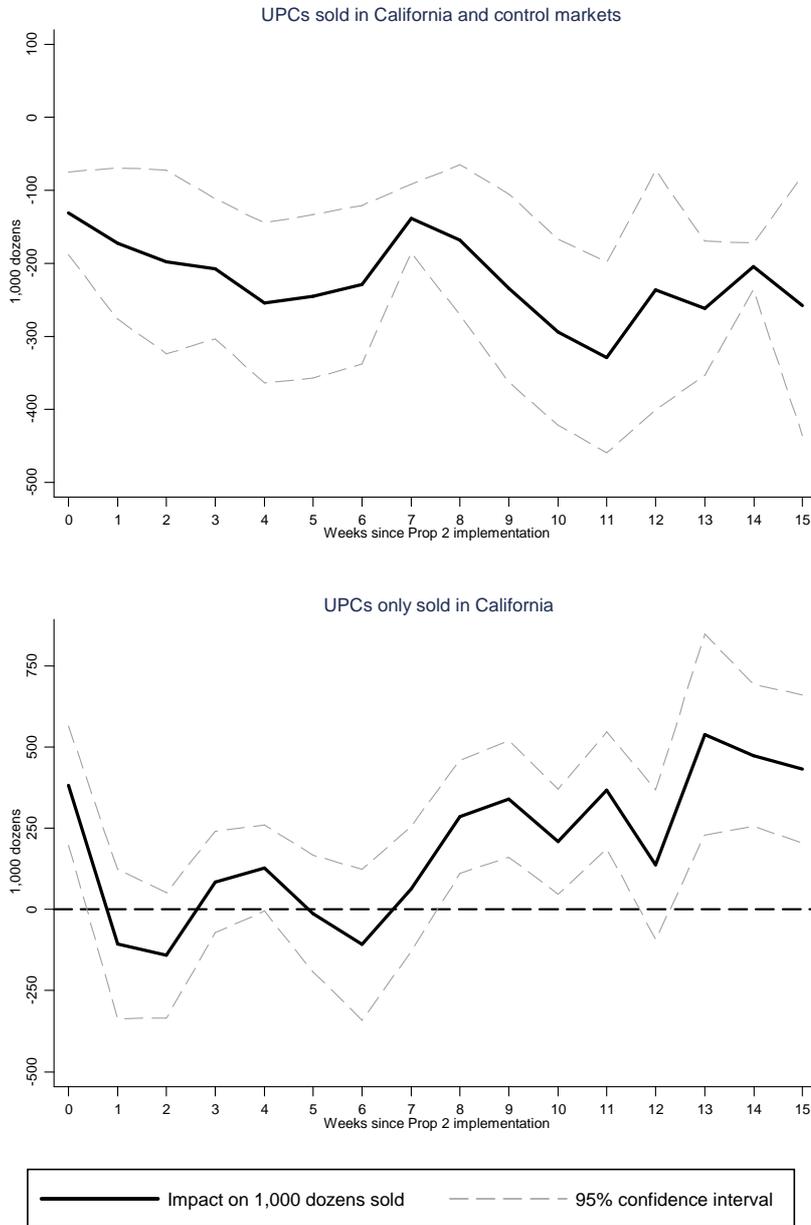


**Figure 1. Retail Price, Quantity, and Sales Trends for Eggs in Selected California and Non-California Markets**



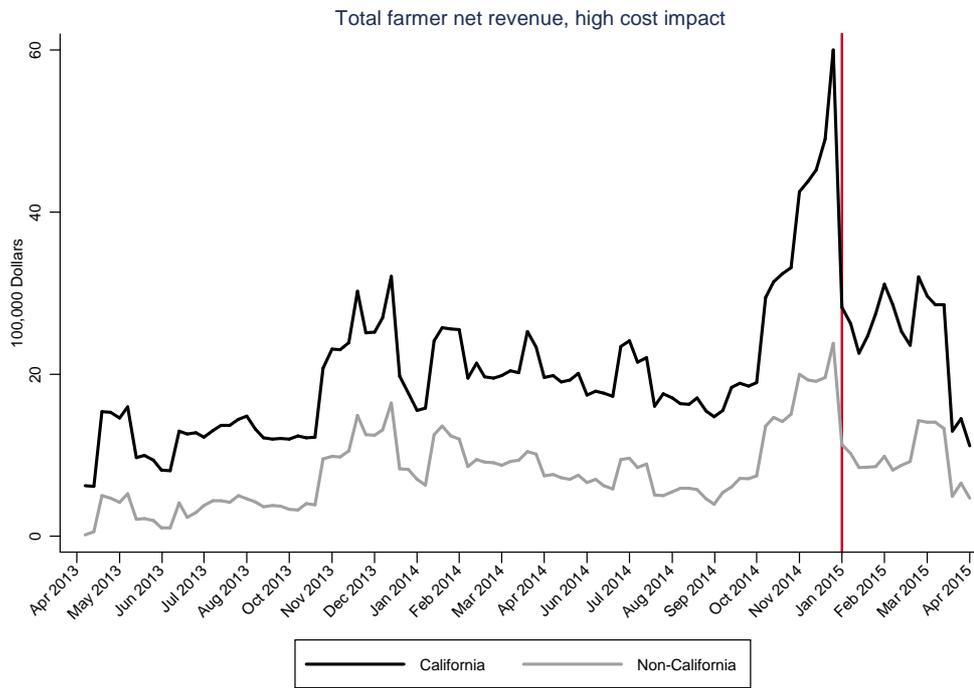
**Figure 2. Impact of the AW Laws by Week**

Notes: Weekly price impacts estimated using a model that included fixed effects for market, week, and UPC. 95% confidence interval computed using standard errors that clustered on week and UPC.

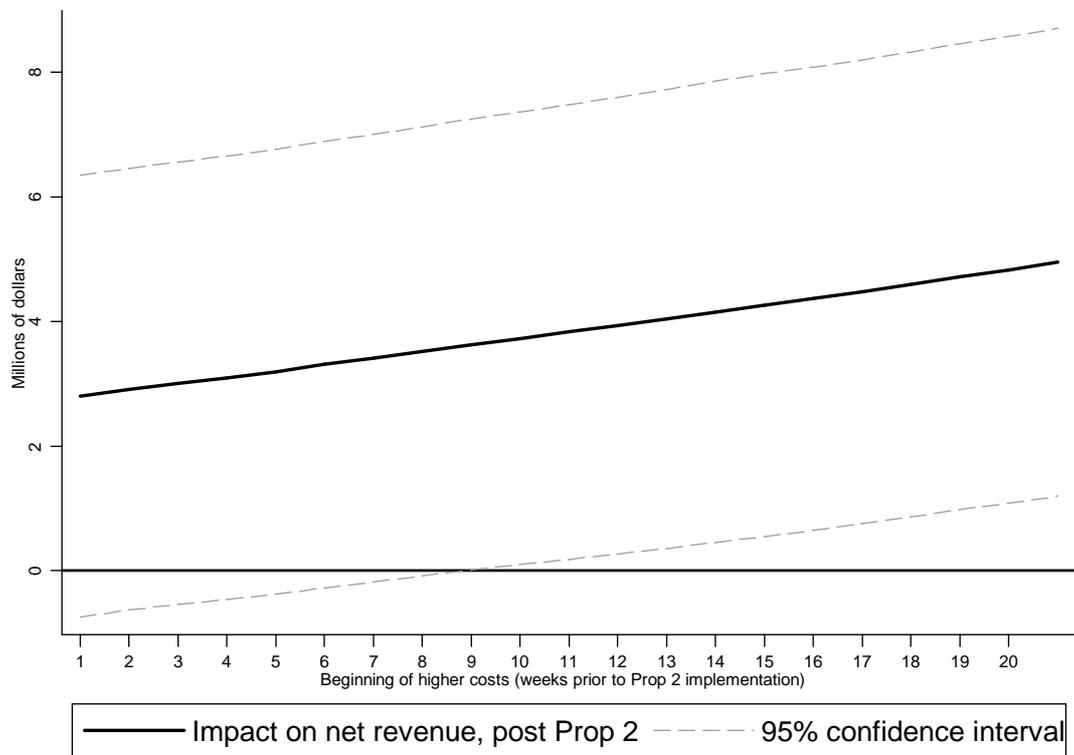


**Figure 3. Impacts on Quantity by Where a UPC is Sold**

Notes: Weekly quantity impacts estimated using a model that included fixed effects for market, week, and UPC. 95% confidence interval computed using standard errors that clustered on week and UPC.



**Figure 4. Net Revenue Trends, California and Control Markets**



**Figure 5. Relationship between Timing of Change in Production Costs and the Impact of the AW Laws on Net Revenue**

Notes: Net revenue impact estimated using a model that included fixed effects for market, week, and UPC. 95% confidence interval computed using standard errors that cluster on week and UPC.