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Where is the value? The impacts of sow gestation crate laws on pork supply and consumer value perceptions

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ABSTRACT

Despite varying farm animal welfare laws being enacted and considered, it is unclear and unknown whether they generate sufficient benefits to justify their costs. This study asks how sow gestation crate laws that dictate minimum crate size, a particular farm animal welfare law, impact meat production and the subsequent value that consumers place on meat produced under the laws. An observational analysis leverages the 8 states that have implemented laws regarding gestating sow crates in a difference-in-differences framework using staggered adoption robust estimators. We then perform a consumer preference experiment using a contingent valuation design with treatment effects to separate out the heterogeneous beliefs about animal welfare law effects. The results of the study show that such laws initially reduce gross income and supply, but only gross income recovers. For consumers, they value the corresponding pork products more in accordance with their animal welfare beliefs but not quality. Such information on costs and benefits should be considered in future laws relating to gestation crate bans.

1. Introduction

Increased animal welfare standards, both from private motivations and public regulation, often comes down to the value society places on the animal (Lundmark et al., 2014). However, this perception of value can be confounded by an individual's preferences for improvements in welfare and quality of meat products (Hudson, 2010; Tonsor et al., 2009b). The economic concept of "value" is determined by both sides of a market, which leads to a need to examine both producer and consumer outcomes congruently when it comes to animal welfare regulation.

Yet, there is a sparse literature that considers value from a producer or societal perspective in terms of the costs and benefits of improving animal welfare (Carlier and Treich, 2020; Olynk et al., 2010; Sumner et al., 2010; Tonsor and Wolf, 2010). There is a large literature that focuses on the benefits as measured by consumers' preferences and WTP for products under different animal-friendly practices used in the production process both in the United States and in Europe (Viske et al., 2006; Lusk et al., 2006, 2007; Carlsson et al., 2007; Liljenstolpe, 2008; Tonsor et al., 2009a,b). Some studies have examined the issue of willingness-to-pay and how these estimates compared to costs of production. In a study of Swedish consumers, Liljenstolpe (2008) found that the cost of alternative pig slaughter methods are economically feasible, though their study does not fully capture which consumer beliefs are driving that value. Other work from Europe has found similar results in terms of high consumer value on other types of animal-friendly regulation (Carlsson et al., 2007; Viske et al., 2006). Though most of the work on gestation crates has been around the regulation in the United States (Olynk et al., 2010; Tonsor et al., 2009b).

Thus, this study examines both perspectives of the problem – producer and consumer – and provides a two-sided view of the policy implications. Specifically, we examine the effect of animal welfare regulation on producers and examine what beliefs drive consumer perceptions of value. We hypothesize that consumer value is generally positive but derives from a mix of increases in product quality and desire for increased animal welfare.

From these previous studies we cannot explicitly tell whether consumers care about animal welfare, care only about the quality of products under animal-welfare laws, or care about both (Carlier and Treich, 2020; Hudson, 2010). This stems from the fact that much of the previous literature does not separate consumer beliefs and preferences (Lusk et al., 2014; Neill and Williams, 2016). As such, previous literature only notes that consumers are willing to pay a higher price for animal welfare friendly products without distinguishing where that

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value originates from. These motivations are key to better understanding the origins of value from such practices. As such, we design a consumer experiment to delve deeper into consumer beliefs and preferences and distinguish where the value for such practices originate. In addition, there is a need to better value enhancements in animal welfare from the costs/benefits to producers.

In order to tackle this problem, we utilize causal analysis of the effect of farm animal welfare laws – specifically the ban on sow gestation crates – on production value and stated preference information for pork products to determine the origin of consumer, belief-based changes in market value from animal welfare changes. This dual-sided approach allows for a robust understanding of how animal welfare is valued both under different regulatory scenarios and under different consumer motivations. From this analysis, we are able to assess both costs and benefits of animal welfare regulation within one study. This allows us to address the confounding problem brought forth by Hudson (2010), better separate consumers' preferences and beliefs about animal welfare, and determine the casual effects of welfare regulation on meat producers.

We focus our analysis on hog output (in pounds and head), producer gross income (in dollars), and a related pork product (chops). Specifically, we examine the impacts of several states enacting laws on the minimum size of sow gestation crates to determine the effects on supply side outcomes and use information treatments to determine the core of consumer preferences for pork products produced under such conditions. Our supply side analysis is one of the most unified analyses of sow gestation crate laws to date since it studies the average causal impacts of the law on supply outcomes across all adopting states. In regards to prior work on welfare information, most studies focus on the relationship between label information and consumption decisions (Weaver and Finke, 2003; Chen et al., 2019; Edenbrandt and Lagerkvist, 2021; Lin and Nayga, 2022; Lai et al., 2020). Another branch of research in this area explores the heterogeneity of consumers besides the correlation, and show differential effects of label information on consumers with different characteristics (Zhu et al., 2016; Rimal, 2005; Dörnyei and Gyulavári, 2016; Van Der Merwe et al., 2010). From the consumer side, our paper adds insights of how label information changes consumers' choices to the literature through the analysis of mechanisms.[1]

The results from both sets of analyses indicate that the benefits from the animal welfare laws do not necessarily outweigh the production costs to the producer. This is due to the heterogeneous effects of the laws on producers and the fact that consumers' beliefs about the benefits of gestation crates is a key component in driving willingness-to-pay differences. Moreover, producers, who incur the increased cost of such regulation, do not benefit from the addition of value for consumers. Thus, current and future implications for animal welfare regulation should consider these effects on supply and demand which may not be win–win across both sides of the market.

The remainder of this article is as follows: a discussion of the supply side analysis utilizing a difference-in-differences approach; a section on the consumer demand analysis utilizing a contingent valuation design with information treatments; a section on the policy implications, followed by concluding comments.

2. Supply side analysis

First, this paper estimates how a farm animal welfare (FAW) law, that dictates minimum sizes for gestating sow crates, affects supply and gross income. Data for state-level regulations, in the United States of America, on the size of gestating sow crates comes from the Animal Welfare Institute.[2] Over the sample, the law has been introduced in 8 states which are shown in Fig. 1 and details are given in Table A.1. There is very little variation in the text of the laws, they all dictate the minimum size of gestation crates for sows.

Data on yearly state-level outcomes for hogs comes from the United States Department of Agriculture's (USDA) National Agriculture Statistical Service (NASS). As our first research question is how these laws influence the supply of pork, the first set of dependent variables are pounds sold, gross income, and production dollars.[3] Our next research question asks how consumers value the meat produced under these laws. There is a measurement of value from NASS, the value of intrastate farm home consumption; however, this may not reliably measure willingness to pay, so we report the value results in Section A.2. The lack of observational data on value is an additional motivation for conducting the choice experiment in Section 3.

This data is well-built for a difference-in-differences approach, since we observe several key variables for a long period and several law changes which occur over the length of this sample. The assembled data is a panel of 50 states from 1988–2020, with descriptive statistics being presented in Table A.2. As shown in Figure A.1, gross income and production dollars are nearly exactly equivalent with an unconditional correlation of 0.9976. The main results use producer's gross income as the main variable and production dollars results are reported in Section A.1.[4]

2.1. Method

The effect of the laws are estimated using two-way fixed effects (TWFE) specified as,

$$y_{st} = \beta_1 Crate\ Law_{st} + \mu_s + \rho_t + e_{st}, \tag{1}$$

in which s stands for state and t stands for time. $Crate\ Law$ is a binary variable that equals 1 if state s has passed the gestation crate law in year t or before year t. The state fixed effects, μ_s, accounts for time-invariant state level factors. The year fixed effects, ρ_t, account for trends in hog meat production and demand. The standard errors are clustered at the state-level to account for errors that are correlated within state and some forms of mis-specification.[5]

The theoretical prediction for β_1 is straightforward for supply. For the supply outcomes, pounds sold, gross income, and production dollars, β_1 is expected to be negative. Gestating sow crates are an input to production and the law dictates using larger crates, assuming the pre-legislation crates do not meet the new legislation. This would require using more inputs for the same amount of output and in a perfectly competitive market this could be seen as shifting the supply inwards due to the increasing price of inputs. For the value outcome, if consumers place higher value on more humanely raised hogs or believe that humanely raised meat is of higher perceived quality, β_1 will be positive. We do not claim that these outcomes span the entire range of effects that the law could have; we understand how these could affect farms, operations, demand, and marketing, but we do not observe these alternative outcomes. We view output and gross income as supply outcomes of first order importance.

[1] Thus, we define 'value' from two perspectives: The output from producers — with particular interest on gross income; and the willingness-to-pay and motivations for purchasing pork products.

[2] See https://awionline.org/.

[3] Gross income and production income are deflated by the consumer price index and are in 2020 dollars.

[4] Gross income is the total values of cash receipts plus the value of home consumption. The value of production is similar and also accounts for differences in inventory.

[5] We account for few treated clusters, 8, using the wild bootstrap.

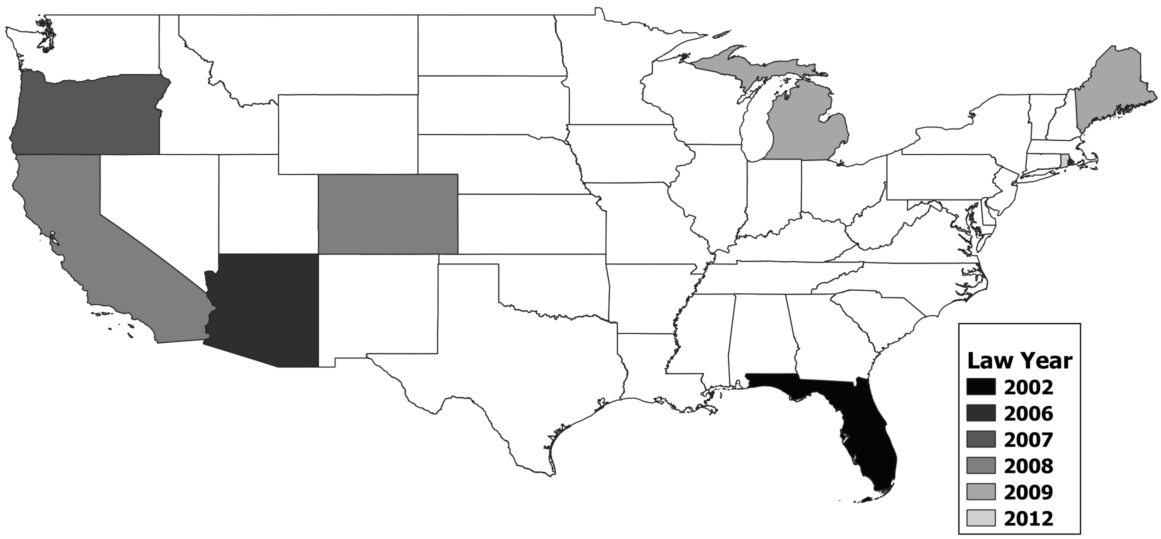


Fig. 1. Adoption of sow gestation crate laws. **Note:** Data comes from the Animal Welfare Institute. Year is the year the law was passed. Details of the laws can be seen in Table A.1.

Identification Attaching a causal interpretation to β_1 requires the parallel trends assumption to hold. In this context, it means that supply in states that introduced these laws would continue trending on parallel paths to states that did not introduce the law in the absence of the passing of the law. We will use dynamic specifications to diagnose whether parallel trends is a plausible assumption. Until recent research uncovered problems that arise due to staggered adoption and heterogeneous effects (Goodman-Bacon, 2021; Sun and Abraham, 2020), dynamic specifications typically followed,

$$y_{st} = \sum_{j=2}^{J} \beta_j + \sum_{k=0}^{K} \beta_k + \mu_s + \rho_t + e_{st}. \quad (2)$$

Due to the omission of the coefficient for 1 year prior to the beginning of the policy, the β_j and β_k coefficients can be interpreted as the difference between the comparison states and the states with the law compared to their difference in the year prior to the adoption of the law.[6] The β_k coefficients represent the dynamic path of the outcomes after the law was passed.

If the β_j coefficients are not distinguishable from zero, it means there are no average differences between treated and comparison states in pre periods which implies that the trends would remain parallel in the post periods in the absence of the intervention. We do not include time-varying covariates, because it is likely that many of the relevant ones, such as number of farms, are affected by the law and are therefore endogenous which is worse than an unconditional approach (Caetano et al., 2022).[7,8] If this identification assumption is met, then the estimates can be interpreted as an average treatment effect on the treated (ATT).[9]

Staggered adoption robust estimators As shown in Fig. 1, the first year a law was passed was in 2002 and the last year one passed was 2012. Since staggered adoption of laws can bias estimates if effects are heterogeneous, several new estimators which address this issue are also used. No time-varying controls are included, but extensive attention is paid to the heterogeneous impacts of the laws across separate cohorts of treated states using different aggregations of the estimates produced by the Callaway-Sant'Anna (CS) estimator (Callaway and Sant'Anna, 2021).[10]

2.2. Static results

As shown by column 1 of Panels A and B of Table 1, when the effect of the law is estimated with TWFE, the estimates are of the expected signs, negative, for supply outcomes of pounds sold (Panel A) and gross income (Panel B). As shown in column 1 of Panel A, the TWFE estimate implies the law reduces the amount of hog sold by 228.91 million pounds. This is a reasonably sized effect which equals 15.4% of the standard deviation of pounds sold. Moreover, the effect is economically significant, representing 38.3% of the average amount of pounds of hog sold. The gestation crate law is also associated with a $92.11 million reduction in producers' gross income.

Both estimates are borderline statistically significant. Using conventional cluster-robust inference at the state level, the estimates for pounds sold is not significant above the 10% threshold. However, once the wild cluster bootstrap is used the estimate obtains statistical significance with a p-value of 0.09. Gross income is not statistically significant above 10% with either conventional cluster robust inference or with bootstrapping. However, it is worth noting that the estimate is large in magnitude at 9.5% of a standard deviation or 22.3% of the average hog gross income (Imbens, 2021).[11]

There are several factors that could be contributing to the estimates in column 1 of Table 1 being relatively imprecise. First, the outcomes

[6] If farmers in neighboring states anticipate the law will be enacted in their state, pre-emptively adjust production, and the effects are the same on average, then this will attenuate the estimates towards 0 since the comparison states will have their outcomes similarly affected as the treatment states, but the method does not account for that.

[7] The other issues with covariates are that farms from NASS do not breakdown by the type of animal making it a noisy measure of hog farms and that there are few states with few observations.

[8] While our approach deals with the potentially endogenous assignment of the law, it is beyond the scope of the paper to also deal with endogenous covariates besides the law.

[9] In other words, the estimates are internally valid for the states that did change the law. Establishing external validity requires additional assumptions and this is beyond the scope of this paper.

[10] In particular, all of our dynamic specification estimates will be constructed using the methodology from Callaway and Sant'Anna (2021). Since the dynamic estimates from Callaway and Sant'Anna (2021) come from aggregating across effects that vary by group and time, there is no omitted period. The estimates maintain a very similar interpretation.

[11] As shown in Table A.6, the results for production are very similar to those for gross income. As shown by Table A.7, the coefficient on the law's effect on value of home consumption, excluding inter-state trade, is also of the expected sign when estimated by TWFE, positive.

Table 1
Estimates of effect of gestation crate law on pounds sold and producer gross income.

Panel A: Pounds of hog sold			
	(1)	(2)	(3)
Gestation crate law	−228.91	−232.89*	−142.75*
	(141.69)	(135.36)	(83.77)
	[0.09]		
Observations	1634	1634	1650
TWFE	X	–	–
Imputation method[a]	–	X	–
CS method[b]	–	–	X
Panel B: Gross income			
	(1)	(2)	(3)
Gestation crate law	−92.11	−93.36	−69.38
	(68.20)	(65.47)	(46.21)
	[0.17]		
Observations	1634	1634	1650
TWFE	X	–	–
Imputation method[a]	–	X	–
CS method[b]	–	–	X

Note: *p < 0.1, **p < 0.05, ***p < 0.01. Cluster-robust standard errors, by state, in parentheses. All regressions include county and year fixed effects. Gestation Crate Law is a binary variable equaling 1 if it is in post-adoption period in a state that passes a law dictating minimum gestation crate sizes. In Panel A, the dependent variable is pounds of hog sold. In Panel B, the dependent variable is producer gross income. Column 1 estimates the law's effect using two-way fixed effects OLS.

[a]Column 2 uses the imputation estimator of Borusyak et al. (2021) and Gardner (2022).

[b]Column 3 uses group-time effect aggregation from (Callaway and Sant'Anna, 2021). It uses never treated as comparison units, the doubly robust differences-in-differences estimator based on inverse probability of tilting and weighted least squares (Sant'Anna and Zhao, 2020), and shows the simple aggregation.

are based on survey data and are therefore measured with error which leads to larger standard errors. Second, there is limited power from a limited number of observations overall, 1650, and few states enacting the policy, 8. Finally, there is potential bias that arises from staggered adoption of the law and dynamic or heterogeneous effects. These limitations, particularly the second and third ones, are addressed in the following analyses.

2.3. Alternative estimators

The staggered treatment of welfare laws can lead TWFE to place non-negligible positive weights on inappropriate comparisons. These comparisons can bias the estimates by placing too much weight on comparing late treated to early treated units.[12] As shown in the Goodman-Bacon (GB) Decompositions in Figure A.2, almost all of the weight in the TWFE regressions are put on favorable comparisons (triangle markers) which suggests a limited bias in the estimated effects for most outcomes. Furthermore, most of the estimated effects with substantial weight are near the estimated coefficient for TWFE.[13]

Next, we present the alternative estimators in the second and third columns of Table 1. Each of these estimators, the imputation estimator (Borusyak et al., 2021) and the CS estimator (Callaway and Sant'Anna, 2021), are robust to staggered timing with heterogeneous effects in order to see whether the results are different after accounting for staggered adoption of the law. As shown by columns 2 and 3 of Panel A, using the imputation estimator or the CS estimator leads to similar conclusions of the effect of the law on pounds sold. The imputation estimator has a similar effect size to TWFE, at −232.89, but the CS estimator is smaller at −142.75. Both are statistically significant with 90% confidence. The pattern of the CS estimator producing smaller effects holds for gross income. However, the law does not have a statistically significant impact on gross income when using the other estimators.[14] Despite being statistically insignificant at conventional levels, the magnitude of the estimate is still large and smaller estimates of output effects are paired with smaller impacts on gross income across estimators.

2.3.1. Dynamic effects

Before further investigating the heterogeneity of the treatment effects, we first determine whether there is evidence that these effects can be given a causal interpretation. This requires the parallel trends assumption which we investigate using the dynamic effects specification given in Eq. (2). We use the event-study aggregation from CS (Callaway and Sant'Anna, 2021) and report these event studies separately by treatment cohort to minimize bias due to heterogeneity across cohorts.

As shown in the event studies for pounds sold which are presented in Fig. 2, there are no discernible patterns in the pre-period coefficients. As shown in the event-studies for gross income which are presented in Fig. 3, there are no discernible patterns in the pre-period coefficients although they are more volatile than in the pounds sold event studies.[15] The lack of apparent pre-trends in the figures is consistent with parallel trends holding which allows the estimates to have a causal interpretation. While it is true that states select whether and when to enact these laws, there is no evidence that their hog production or gross income are trending differently in the pre periods which suggests that the model assumptions are adequate to identify causal effects.

While there are some individually significant pre-period coefficients, there are never more than two in a row with the same sign. In contrast, the post-period coefficients exhibit sustained estimated differences that are different than 0 in the same direction. As shown in Fig. 2, the coefficients are negative in every post period for nearly every treatment cohort and increasingly so, suggesting the negative impact on total output grows over time. For gross income (Fig. 3), there is a period where post period coefficients decline, but eventually move back towards 0 which suggests that the harmful impacts of the law on producers' gross income (our measure of producer value) are somewhat, though not completely, mitigated after an adjustment period. The 2012 cohort follows this general pattern, but after a few years the readjustment upwards is large enough to become positive.

There are two plausible explanations for continuing decline in output and an eventually stabilizing gross income. First, farmers could be receiving a higher price for at least some portion of their product which would explain the stabilization of gross income. Second, and this is more speculative due to a lack of panel data on farms, less profitable/innovative farmers could be exiting or acquired which would also raise gross income in spite of declining total output. We are unable to discern between these two possible explanations.

2.3.2. Heterogeneity by cohort

There is likely to be some heterogeneity across treatment cohorts. This is perhaps unsurprising as later adopters can observe early adopters and attempt to mitigate any negative impacts on supply and there are differences in the sizes of the hog industry. Fig. 4 embraces the heterogeneous impacts across treatment cohorts by estimating the

[12] Note that this is not an issue in a 2 period setup or if there are no heterogeneous effects across time or groups (De Chaisemartin and d'Haultfoeuille, 2020; Roth et al., 2022). The comparison that is most desirable is comparing states that are treated to those that are never treated.

[13] One notable exception is the weights on value; as shown in Figure A.2d, the largest weighted comparisons have negative effect sizes, while the TWFE estimate is positive. This is indicative that there may be heterogeneity in the effect of the laws on value across treatment cohorts.

[14] The estimators make different assumptions and which estimator to prefer depends on those assumptions. In this case, the statistical significance is the same and the point estimates are not very different, so it is unnecessary to take a stand on an estimator.

[15] The event-studies for value, presented in Figure A.6, show no noticeable pre-period trends in value.

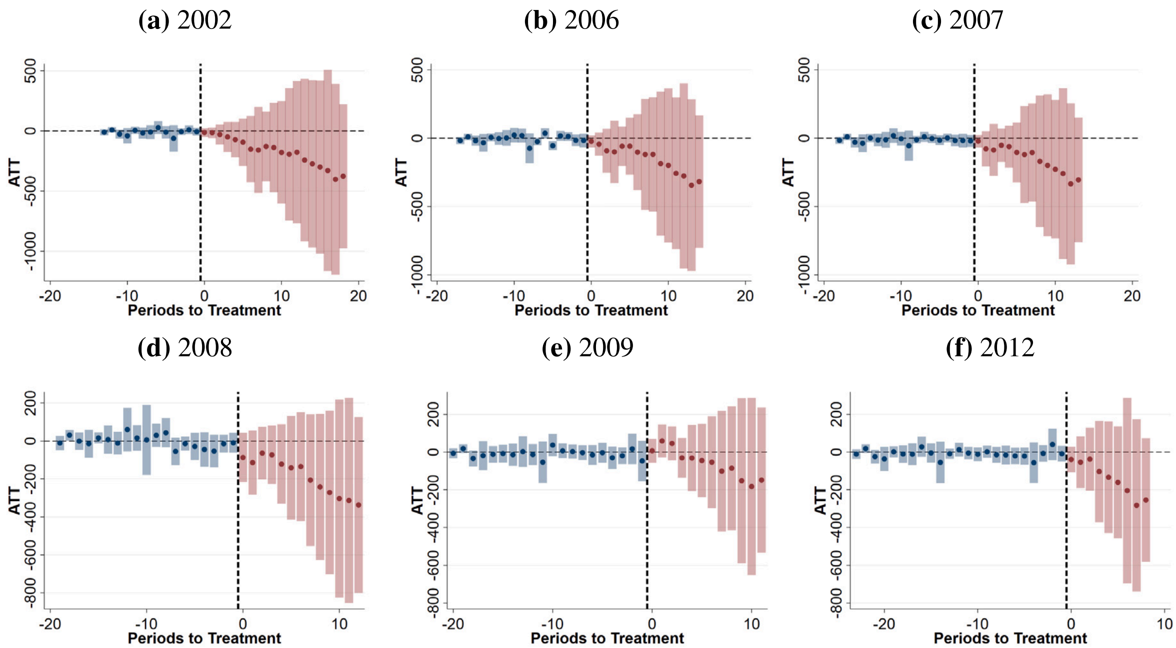


Fig. 2. Event studies by cohort for pounds sold. **Note:** The y-scale and x-scale differs across figures. These figures report coefficients from Eq. (2) for the estimation of dynamic effects of the law change on pounds of hog sold. Each figure represents a different treatment cohort. The coefficients are obtained from the event-study aggregation of Callaway and Sant'Anna (2021). Coefficients represent the change in pounds of hog sold for states that adopt the gestation crate size law relative to non-adopting state in the years before and after the law passes, as compared with the year immediately prior to the law passing. Gestation crate law adoption dates comes from the Animal Welfare Institute and the pounds of hog sold comes from the USDA NASS Quick Stats. The whiskers represent 95% confidence intervals. The standard errors are clustered at the state-level and are calculated using the wild bootstrap.

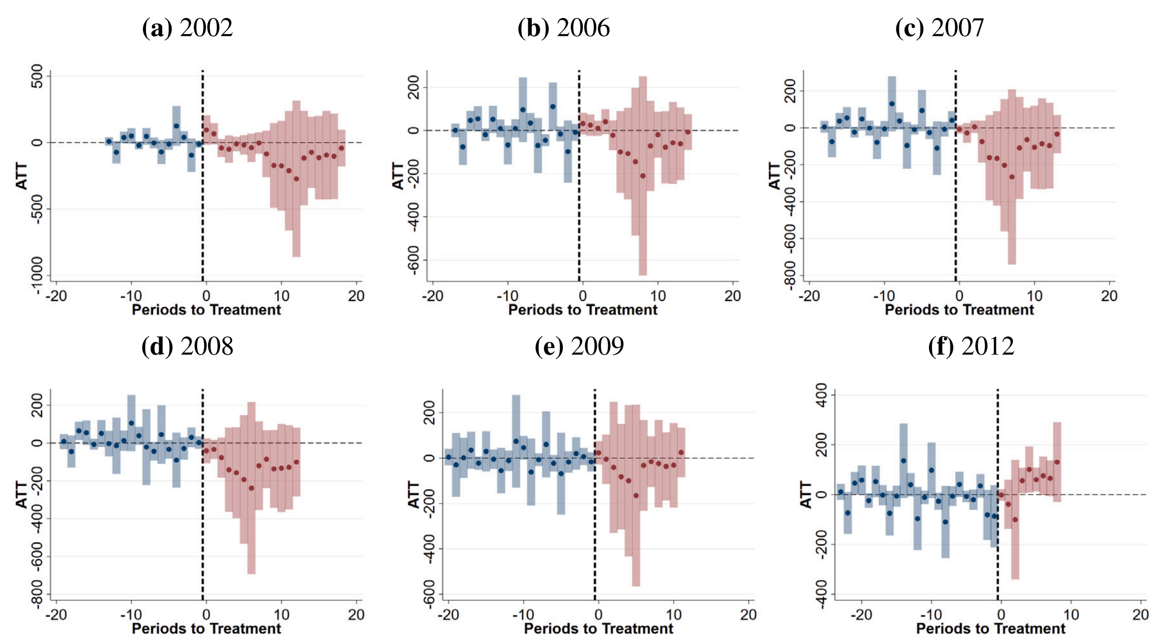


Fig. 3. Event studies by group for gross income. **Note:** The y-scale and x-scale differs across figures. These figures report coefficients from Eq. (2) for the estimation of dynamic effects of the law change on producer gross income. Each figure represents a different treatment cohort. The coefficients are obtained from the event-study aggregation of Callaway and Sant'Anna (2021). Coefficients represent the change in pounds of hog sold for states that adopt the gestation crate size law relative to non-adopting state in the years before and after the law passes, as compared with the year immediately prior to the law passing. Gestation crate law adoption dates comes from the Animal Welfare Institute and producer gross income comes from the USDA NASS Quick Stats. The whiskers represent 95% confidence intervals. The standard errors are clustered at the state-level and are calculated using the wild bootstrap.

effect of the sow gestation crate laws for each separate treatment cohort. As Fig. 4 shows, there are negative impacts that are statistically significant at 10% for pounds sold and these are consistent across all cohorts. However, for gross income and production the laws have negative (and sometimes statistically insignificant) effects on production and gross income for all but the last treatment cohort of 2012.[16]

[16] The effects of the law on value differ the most by cohort. The law increased value for the 2006 and 2012 cohorts and these are both statistically

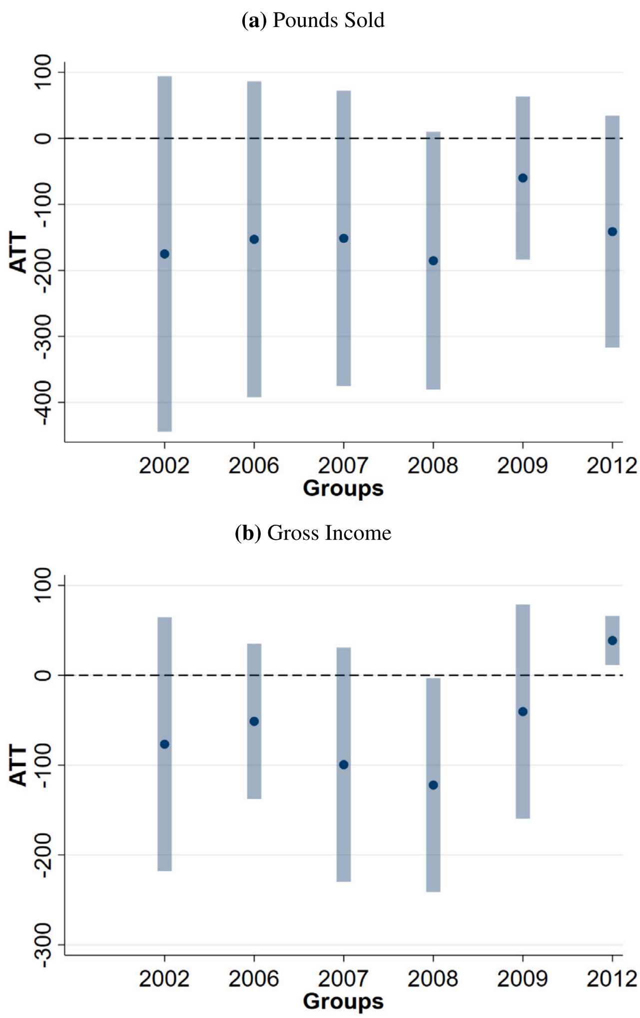


Fig. 4. Cohort level estimates. **Note:** Estimates are group-level aggregates from Callaway and Sant'Anna (2021). Wild bootstrapped standard errors. 95% confidence intervals shown. Pounds sold, production, and gross income are in millions. Value is in thousands.

Ideally, it would be possible to understand the root cause of these heterogeneous impacts of the law. One explanation that is unlikely to generate these differences is the exact text of the laws which all require gestating sows to be able to stand up, lie down, and turn around. Thus, we turn our attention to pre-treatment variables for each of the states. One immediately obvious difference for the 2012 cohort, composed solely of Rhode Island, is that it is a mere fraction of the size of the other cohorts in terms of both amount of land farmed, number of farms, and size of hog industry. An alternative explanation that is consistent with the estimates is that the reduction in pounds sold has gotten less worse for each progressive cohort and the effect on gross income even changed for the final cohort which could hint to adaptation occurring.

2.4. County-level

For several of the analyses presented so far, the results are often more imprecise than would be preferred to make strong conclusions. An alternative way to construct the dataset, which may address this shortcoming, is to perform the analysis at the county-level. This increases the unit of observation from 50 states to over 3000 counties. However, this alternative construction of the dataset comes at several costs. The only outcome available at the county-level is hog production (measured in head), county-level data only exists every 5 years (1997–2017), there is often missing data, and it does not increase the number of states that introduced the law.[17]

Nevertheless, we believe that examining the effect of the law on supply in this alternate way could offer more precise inference which corroborates the robustness of the state-level results. The county-level results are presented in Table 2, in which each panel handles missing outcomes slightly differently. Panel A keeps all missing values as missing, Panel B converts all missing values to 0, and Panel C only reports county-years that were not in the USDA NASS data as zeros with others as missing. As shown in column 1 of Table 2, the TWFE estimators preserve the expected sign and are also statistically significant at 90% confidence regardless of how the missing outcome data are handled. How the zeros are handled also has little effect on the TWFE point estimates with them being between −18,780 and −14,403. As shown in the GB Decomposition in Figure A.3, the TWFE weights are placed nearly exclusively on appropriate comparisons and all point estimates are negative and relatively close to the aggregate point estimate.

In contrast to the TWFE estimate not providing more precise inference, column 2 of Table 2 shows that imputation estimator at the county-level does provide estimates that are significant above 95% confidence. Although the imputation estimator is always statistically significant above 95% confidence, the point estimate does vary more

Table 2
Estimates of effect of gestating sows law on hogs (measured in head) sold, county level .

Panel A: Naïve Missing		
	(1)	(2)
Gestating Sows Law	−18 780.83*	−24 414.04**
	(9745.02)	(9803.96)
	[0.07]	
Observations	11 341	11 552
TWFE	X	–
Imputation method[a]	–	X
Panel B: Naïve Zero		
	(1)	(2)
Gestating Sows Law	−14 403.45*	−14 880.09**
	(7333.56)	(6952.37)
	[0.08]	
Observations	15 726	15 726
TWFE	X	–
Imputation method[a]	–	X
Panel C: Merge Zero		
	(1)	(2)
Gestating Sows Law	−16 602.70*	−21 468.43**
	(8851.37)	(8962.49)
	[0.08]	
Observations	12 656	12 778
TWFE	X	–
Imputation method[a]	–	X

Note: *p < 0.1, **p < 0.05, ***p < 0.01. Cluster-robust standard errors, by state, in parentheses. All regressions include county and year fixed effects. The dependent variable is hogs sold, measured in head. Gestating Sows Law is a binary variable equaling 1 if it is in post-adoption period in a state that passes a law dictating minimum gestation crate sizes. In Panel A, all missing outcomes are left missing. In Panel B, all missing outcomes are replaced with zero. In Panel C, only missing values that arise due to no county-year in the USDA Quick Stats data are counted as zeros. Column 1 estimates the law's effect using two-way fixed effects OLS.
[a]Column 2 uses the imputation estimator of Borusyak et al. (2021) and Gardner (2022).

significant. However, 4 treatment cohorts had increased value and 2 had decreased value. An interesting note is the 2012 cohort reduced pounds sold, but increased production, gross income, and value.

[17] Sumamry statistics are shown in Table A.3.

than TWFE from −24,414 to −14,880. The magnitude, relative to the standard deviation, is slightly smaller than at the state-level. The TWFE estimates are 5.28 to 5.95% of the standard deviation which is a result of going to a more granular geographic unit with higher variation and the different outcome measure (pounds sold at the state-level compared to head at the county-level).[18] Finally, we report the results with standard errors clustered at the county-level in Table A.5 which produces even more forgiving inference, albeit with likely too narrow confidence intervals due to treatment being assigned at the state level, with all coefficients statistically significant above 99% confidence.

3. Consumer analysis

In order to identify the motivation of consumers' WTP, we implemented a between-subject design with different labeling (treatment) information to separate and compare the WTP for animal welfare preferences and the preference of perceived quality that may also be associated with products under gestation crate regulations. We believe the identification of what motivates consumer's WTP is important for policy makers weighing the costs and benefits of related regulations.

We use a double-bounded contingent valuation approach to estimate WTP (Hanemann et al., 1991) in each treatment group. The contingent valuation method (CVM) is a standard approach to elicit people's WTP for non-market goods or services; it has been applied in fields such as environmental, health, food, and marketing (Diener et al., 1998; McCluskey et al., 2003; Venkatachalam, 2004; Neill and Williams, 2016; McFadden and Train, 2017; Neill and Holcomb, 2019). There remains some debate about its validity due mainly to hypothetical response bias, discrepancy between willingness to pay and willingness to accept, and the scope problem (Hausman, 2012; Carson, 2012). This argument is more of a concern for goods and services with no actual market transaction connection — which is not the case in this study, though we still employ cheap talk "Please answer as if you were actually making shopping decisions in the grocery stores" to reduce the amount of hypothetical bias in the experiment (Bishop and Heberlein, 2019).

The main purpose of our experiment is to compare the WTP across different treatment groups, instead of the absolute value of the WTP. Compared to studies which elicit the absolute value of WTP, the combination of CVM and a between-subject design to some extent mitigates the main issues of using CVM mentioned above in our experiment. The between-subject design also allows us to test mechanisms about how the treatment information affects the WTP, while being able to distinguish preferences from beliefs.

3.1. Design

The product of interest in our experiment are pork chops, a common cut of pork, with 4 price point variations ($4.0, $4.5, $5.0, $5.5).[19] In each two-stage choice question, subjects were first asked to select between pork chops produced under the Gestating Crate Law (GCL) and pork chops which are not produced under the GCL. The price of pork chops under GCL is randomly picked from the 4 price variations; the price of pork chops which are not produced under the GCL is always $4.50 per pound. If the subject chooses the GCL pork chops in the first stage, the price of the GCL pork chops increases by $0.50 in the second stage; if the subject chose the non-GCL pork chops, the price of the GCL pork chops decreases by $0.50 in the second stage. The price of non-GCL pork chops remains at $4.50 in the second stage. In each question, we also presented an introduction of gestation crates and the GCL before subjects make selections in both the first and second stage.

The experiment was conducted online through Qualtrics.[20] Subjects were randomly assigned to one of the four groups (one baseline & three treatment groups). Each group only varies in the extra information provided in the choice question about the increase in the quality of products and the increase in animal welfare due to the law.

Table 3 shows the information summary of each treatment group. In the Baseline group, there was no extra information about the quality of products or animal welfare. In the "Quality & Welfare" group, we presented the information "Research has found that the such Gestation Crate Laws increase the quality of pork products, and increases the welfare of hogs/sows". In the "Quality" and "Welfare" group, the information provided was only about the quality of products and the welfare, respectively. Fig. 5 is a screenshot of the choice question in the first stage in the "Quality" group. The information in the second stage only differs in the price changed based on subjects' choices in the first stage.

At the end of the experiment, we asked participants' about their beliefs on animal welfare, welfare laws, and other benefits and costs of gestating sow laws. Demographic information including the state of residence was also elicited at the beginning of the experiment (for detailed instructions of the experiment, please refer to Appendix B).[21] In total, 1039 observations were collected. As shown in Table 4, the average demographic characteristics are similar and not statistically different across groups suggesting randomization was successful in terms of identifying effects of the information treatments.

3.2. Methods

A double-bounded regression model, controlling for demographic characteristics and beliefs, is used to estimate the effects of the information treatments on WTP. The price in the first stage is p_1 and the price in the second stage is p_2. There are four possible outcomes in our methodology: (a) the individual chooses GCL products in the first stage and chooses non-GCL products in the second stage, $p_2 > p_1$; (b) the individual chooses GCL products in both stages, $p_2 > p_1$; (c) the individual chooses non-GCL products in the first stage and GCL products in the second stage, $p_2 < p_1$; (d) the individual chooses non-GCL products in both stages, $p_2 < p_1$. We can infer that in case (a), the individual's $p_1 < WTP < p_2$; in case (b), $p_2 < WTP < \infty$; in case (c), $p_2 < WTP < p_1$; in case (d), $0 < WTP < p_2$.

The double-bounded model by Lopez-Feldman (2012) assumes the WTP can be modeled as a linear function:

$$WTP_i(z_i; u_i) = z_i\beta + u_i \tag{3}$$

where z_i is a vector of explanatory variables, β is a vector of parameters and u_i is an error term $u_i \sim N(0, \sigma^2)$. The estimation of the double-bounded model captures the probability of individuals answering yes or no in the first and second question for the four cases, and uses maximum likelihood estimation to get estimates of β and σ for the WTP in Eq. (3). The detailed estimation process, following Lopez-Feldman (2012), is shown in Appendix C.

Besides the demographic information, we include beliefs as explanatory variables in the model. We have a set of 11 belief questions at the end of the experiment, and the order of the 11 questions

[18] The CS estimator is reported in Table A.4 and the results are never significant. Since we are doing this as a robustness check for precision and the imputation estimator is more efficient (Roth et al., 2022), it is appropriate to focus on the imputation estimator over the CS estimate.

[19] The market price of pork chops was $4.36 per pond at the time we conducted the survey. Thus, we chose $4.5 as the anchor price, and added two prices above it and one price below it to elicit WTP for different products.

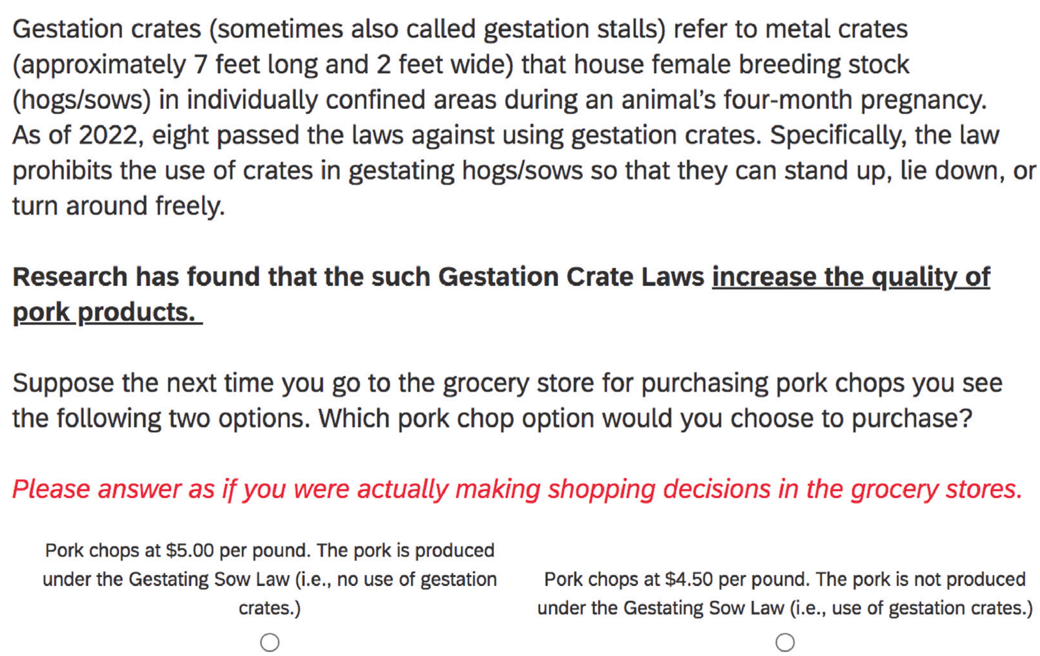
[20] In the survey, we included an attention-check question so that we could make sure that the experiment was not answered by a bot.

[21] We included two screening questions at the beginning of the survey "How often do you purchase pork products?" and "What percentage of your household's purchasing decisions (e.g. groceries) do you make?". Those whose answers were "never" to the first question and "less than 50%" to the second question were screened out.

Table 3
Summary of information given to treatment groups.

	Introduction of gestation crates and the law	Extra information	No. observation
Baseline	Yes	N/A	268
Quality & Welfare	Yes	"Research has found that the such Gestating Crate Laws increase the quality of pork products, and increases the welfare of hogs/sows."	258
Quality	Yes	"Research has found that the such Gestation crate laws increase the quality of pork products."	243
Welfare	Yes	"Research has found that the such Gestation Crate Laws increase the welfare of hogs/sows."	270

Notes: Each subject was randomly assigned to one of the groups.


Gestation crates (sometimes also called gestation stalls) refer to metal crates (approximately 7 feet long and 2 feet wide) that house female breeding stock (hogs/sows) in individually confined areas during an animal's four-month pregnancy. As of 2022, eight passed the laws against using gestation crates. Specifically, the law prohibits the use of crates in gestating hogs/sows so that they can stand up, lie down, or turn around freely.

Research has found that the such Gestation Crate Laws increase the quality of pork products.

Suppose the next time you go to the grocery store for purchasing pork chops you see the following two options. Which pork chop option would you choose to purchase?

Please answer as if you were actually making shopping decisions in the grocery stores.

Pork chops at $5.00 per pound. The pork is produced under the Gestating Sow Law (i.e., no use of gestation crates.)

Pork chops at $4.50 per pound. The pork is not produced under the Gestating Sow Law (i.e., use of gestation crates.)

Fig. 5. Screen shot of choice question in the first stage of "Quality" group.

Table 4
Summary of demographic characteristics by treatment group.

	Baseline	Quality & Welfare	Quality	Welfare
Age (median)	43	41	43	42
Gender	51.9% (F) 48.1% (M)	50.8% (F) 49.2% (M)	51.0% (F) 49.0% (M)	52.2% (F) 47.8% (M)
Education (median)	2 year/Associates degree	2 year/Associates degree	2 year/Associates degree	2 year/Associates degree
Household income (median)	$60,001–$65,000	$60,001–$65,000	$60,001–$65,000	$50,001–$55,000
Household size (mean)	3	3	3	3
Pork products consumption (weekly)	2.5	2.5	2.5	2.4

Notes: Two-sided Mann–Whitney U-Tests were used to test the difference in Age, Household Income, Household Size, and Pork consumption between each two of the groups. χ^2 tests were used to test the difference in Gender and Education. We did not find any significant differences in any comparisons.

are randomized. Subjects used a 5-point Likert scale to address the belief questions (from "strongly disagree" to "strongly agree"). In order to reduce the issue of multicollinearity/dimensionality and create a more parsimonious model, we use principal component analysis with varimax rotation to conduct a factor analysis of responses to the belief questions before the estimation of the double-bounded model (Boxall and Adamowicz, 2002; Tonsor et al., 2009b). Based on loadings of indicators by the factor analysis, we suggest two factors that capture the major associations individuals make with the gestation crate laws. The first factor is labeled "pork products quality and animal welfare". The second factor is referred to as "benefits of gestation crates to consumers". Table 5 shows factor loadings of each belief question, and each question is assigned to the specific factor which has higher loadings. Then, a score of each factor is calculated for each individual and is used in the double-bounded model estimation as explanatory variables.

3.3. Results

We first show the estimation results of each treatment group separately in Table 6. The significant positive coefficient of Factor 1 for each group suggest that consumers perceiving the GCL with higher quality of products and higher animal welfare are willing to pay a higher premium for pork chops produced under the GCL. Individuals who believe the gestation crates generate benefits to consumers such as the lower price are WTP lower premium for pork chops produced under the GCL, as indicated by the negative coefficient of Factor 2 for each group.

Table 5
Factor analysis of beliefs.

Statement (belief question)	Factor1 loadings: pork products quality and animal welfare	Factor2 loadings: benefits of gestation crates to consumers
I believe a ban of gestation crates increases the quality of pork products.	[0.7210]	0.0127
I believe pork products generated using gestation crates are subject to more food safety risks.	[0.6891]	0.0257
Animals should not reside in confined areas like gestation crates.	[0.7107]	−0.1753
I believe a ban on gestation crates increases the welfare of hogs.	[0.7401]	−0.0151
I believe that natural pork is from animals not raised in gestation crates.	[0.6746]	0.0187
I rarely think about the use of gestation crates when purchasing hog products.	−0.0291	[0.5582]
I would be less likely to purchase pork products which are generated using gestation crates.	[0.7391]	−0.1110
I would likely pay more if pork products were generated under the ban of gestation crates.	[0.7152]	−0.0101
The use of gestation crates by farmers reduces pork prices.	0.1962	[0.6563]
The use of gestation crates increases productivity of hog operations.	−0.0043	[0.7863]
All things considered, the use of gestation crates by farmers is good for consumers.	−0.2783	[0.7310]

Notes: The brackets indicate the specific factor each belief item assigned to.

Table 6
Censored regression results of each treatment group.

	Baseline	Quality & Welfare	Quality	Welfare
Constant	5.622*** (0.866)	7.425*** (0.868)	5.724*** (0.927)	6.312*** (0.999)
Age	−0.001(0.008)	−0.015** (0.007)	−0.006 (0.008)	0.004 (0.008)
Male	0.309 (0.245)	−0.191 (0.234)	−0.256 (0.253)	0.113 (0.253)
Household income	0.009 (0.018)	0.007 (0.016)	0.030* (0.017)	0.018 (0.017)
Education	0.054 (0.120)	0.070 (0.109)	−0.061 (0.120)	−0.069 (0.118)
Children in the household	0.094 (0.310)	−0.245 (0.279)	0.124 (0.319)	−0.140 (0.347)
Household size	−0.184*(0.106)	−0.208** (0.097)	−0.006 (0.116)	−0.073 (0.126)
Weekly pork consumption	0.041 (0.112)	0.011 (0.117)	0.034 (0.127)	−0.024 (0.112)
Resident of the GCL state	0.029 (0.256)	−0.043 (0.231)	−0.239 (0.258)	−0.057 (0.267)
Factor 1	0.495*** (0.123)	0.517*** (0.101)	0.454*** (0.118)	0.615*** (0.123)
Factor 2	−0.312*** (0.119)	−0.161 (0.114)	−0.347*** (0.122)	−0.214* (0.126)
Mean WTP	$5.764 (0.131)	$5.978 (0.141)	$5.584 (0.126)	$6.010 (0.144)
95% WTP Conf. interval	[$5.506, $6.022]	[$5.702, $6.253]	[$5.337, $5.831]	[$5.728, $6.291]
Log-likelihood	−297.935	−243.681	−281.985	−282.627
No. observation	268	258	243	270

Notes: Factor 1: Pork products quality and animal welfare. Factor 2: Benefits of gestation crates to consumers. Standard errors are presented in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

We compare the mean WTP in Table 6 between each treatment group. We find the WTP of the “Quality” group is significantly lower than the WTP of the “Quality & Welfare” group and the “Welfare” group ($p = 0.039$, $p = 0.028$). There are no significant differences in the mean WTP between the baseline and the other groups, nor between the “Quality & Welfare” group and the “Welfare” group.

We further robustly compare the WTP of each group by setting treatment groups as independent variables in pooled models. Table 7 contains estimation results of pooled models with and without controls. We treat the Baseline and the Quality group as the base group, respectively. In both results, the “Quality & Welfare” and the “Welfare” treatments significantly increase consumers’ WTP compared to the Baseline or the “Quality” group. We do not find any significant effects of the “Quality” treatment on WTP compared to the baseline. In pooled models, the coefficients of “household income” and “household size” become significant due to the increase in power. The results of the pooled model analysis are in line with our comparisons in Table 6. We argue that the increase in the WTP of pork products produced under the GCL is mainly from consumers’ considerations about the animal welfare, instead of the quality of products, which is in line with the intention of the law.

4. Conclusions and discussion

Analyzing the observational data in a causal framework allows for estimating both the costs (to production) and benefits (to value of a prominent animal welfare law. From previous literature, it is clear that the impacts of farm animal regulation are positive over the long term and can benefit consumers of all types (Olynk et al., 2010). Our findings that gross income does recover from initial declines support this previous finding.

The results from the consumer preference study help us to understand where the change in pork pounds sold and revenue is originating from in the causal inference analysis, and how a change in perceptions might change the value to consumers. While other literature has found that voting initiatives for gestation crate bans are supported by consumers’ desire for increases in pork quality (Tonsor et al., 2009b), we find that this motivation for product quality is overshadowed by core desires for increased animal welfare. Moreover, it is important to note that the vote-buy gap has consistently been estimated to be critical to understanding the impacts of animal welfare laws. Social desirability bias is likely present in our hypothetical experiment (as noted by Lai et al. (2022)), but we moderate this effect by further examining consumer beliefs and using that in our estimations of preferences.

This work has implications for the larger understanding of changes in value for animal welfare regulations based on consumer beliefs/perceptions. Moreover, this work can inform other states’ policymakers on the potential changes in costs and benefits based on consumer perceptions before the implementation of such policies. This study addresses the sparse literature that examines both costs and benefits from animal welfare regulations across a large country with substantial pork production and extensive policy experimentation across space.

We take into account consumer beliefs and uncover that welfare information is the most effective for increasing WTP for pork products. Yet, animal welfare laws have heterogeneous effects on producer value and income, but most importantly have negative impacts on quantity of hog sales. Further work should examine disaggregated effects of such policies and determine if the benefits truly outweigh the costs of the regulation.

Our study has some limitations worth noting. First, instead of observing only revenue or quantity sold, we would ideally have a more

Table 7
Censored regression results of pooled models.

	vs. Baseline	vs. Baseline	vs. Quality group	vs. Quality group
Q&W group	0.295* (0.158)	0.376** (0.154)	0.444*** (0.161)	0.454*** (0.156)
Welfare group	0.241 (0.154)	0.282* (0.150)	0.390** (0.157)	0.360** (0.153)
Quality group	−0.149 (0.155)	−0.078 (0.150)		
Baseline			0.149 (0.155)	0.078 (0.150)
Constant	5.705*** (1.114)	6.153*** (0.462)	5.556*** (1.116)	6.075*** (0.464)
Age		−0.005 (0.004)		−0.005 (0.004)
Male		−0.004 (0.123)		−0.004 (0.123)
Household income		0.017** (0.008)		0.017** (0.008)
Education		−0.003 (0.058)		−0.003 (0.058)
Children in the household		−0.058 (0.155)		−0.058 (0.155)
Household size		−0.117** (0.055)		−0.117** (0.055)
Weekly pork consumption		0.016 (0.057)		0.016 (0.057)
Resident of the GSL state		−0.099 (0.126)		−0.099 (0.126)
Factor1		0.514*** (0.058)		0.514*** (0.058)
Factor2		−0.276*** (0.060)		−0.276*** (0.060)
Controls	No	Yes	No	Yes
Log-likelihood	−1177.065	−1116.243	−1177.065	−1116.243
No. observation	1039	1039	1039	1039

Notes: Standard errors are presented in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

rich set of output and price outcomes before and after the implementation of the regulation. This would be the ideal measure, since it could be used to compute profits. This could simultaneously account for net changes in value that arise from increased costs of the regulation.

In addition, the hog industry has undergone considerable consolidation both horizontally and vertically. This likely induced economies of scale effects and forced some producers to exit. The industrial organization of agriculture will remain an important topic. Lastly, though there has been much policy experimentation in the United States compared to other countries, there are still few regulatory changes, which leads to moderately imprecise estimates.

On the consumer side, we limited our study to a stated preference experiment. From these limitations; however, there are a number of opportunities for future research. Analyzing the issue on a farm level where income and costs are more likely to be captured would allow for more detailed analysis. Furthermore, a multinational study across European countries and the United States would address the issue of small sample size while also broadening the impacts to the global hog industry. A multinational study would also allow for comparisons across countries and provide more nuance to the effects of such FAW regulation. Future research should also be keen on performing more revealed preference studies that include belief elicitation and test other motivating factors beyond product quality and animal welfare beliefs.

In addition, an important aspect to consider simultaneously with the producer/consumer effects are the social welfare aspects. As noted by Blackorby and Donaldson (1992) and Carlier and Treich (2020), socially optimal levels of animal welfare would also impact the populations of farm animals in a way that may have spillover effects into societal welfare. While this is difficult to quantify, following a similar approach presented in this study that combines experimental methods with causal approaches may provide a fruitful avenue.

4.1. Policy implications

Following the two-sided view of the costs and benefits of FAW regulations, we also examine the policy implications from this vantage point. In terms of costs, our analysis finds that such policies impact production in terms of fewer pounds of pork being produced. While producer income is heterogeneously affected across states and time, future regulation changes are likely to induce similar costs to production pounds and temporary losses in producer income. Considering several states have FAW level regulations on the docket or are phasing in implementation in the next decade, preparing the hog industry for temporary income reductions is critical. Similar conclusions have been found in farm animal industries such as egg-laying hens, which suggests that producer losses from FAW policies need to be addressed (Sumner et al., 2010).

In terms of benefits, average consumer WTP for pork products produced has increased under these FAW regulations. This positive result is indicative for future policy changes in other states and potentially for other farm animal industries such as poultry and egg production. As such, marketing efforts around these products are likely effective in inducing increased purchasing behavior (Ortega and Wolf, 2018). However, these regulations only specify that hogs raised in a state with these laws must meet these requirements, but meat products sold within that state could be produced under either situation if it is imported from another state. This contributes to the ambiguous benefits of the animal welfare improvements. Because of data limitations, we cannot separate the effects of other state's practices on the value of production in states that have enacted the regulation.

Policymakers can reduce the costs of enacting FAW regulations through subsidies, or increase marketing efforts through state agricultural marketing programs. Coupling FAW policies with subsidy programs to offset producer losses is not new, but one that has been suggested for other farm animal commodities (Bennett, 1997). Subsidies alongside FAW policies are expected to increase consumer choice while also achieving increased societal welfare (Mishan, 1993). However, using taxpayer funds for such efforts could be politically difficult. As such, using established state agricultural marketing programs is another option. As noted by Neill et al. (2020), even with beggar-thy-neighbor effects, state's have an opportunity to appeal to consumers who desire local/regional foods and those that desire increased animal welfare standards. In both policy scenarios, the goal would be to create a win–win situation rather than having producers or consumers pay for the externality of FAW regulation.

CRediT authorship contribution statement

Benjamin Blemings: Conceptualization, Methodology, Software, Formal analysis, Investigation, Data curation, Writing – original draft, Writing – review & editing, Visualization, Supervision, Project administration. **Peilu Zhang:** Methodology, Software, Formal analysis, Investigation, Data curation, Writing – original draft, Writing – review & editing, Visualization. **Clinton L. Neill:** Conceptualization, Resources, Writing – original draft, Writing – review & editing, Supervision, Project administration.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.foodpol.2023.102447.

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