The Effect of Anti-Price Gouging Law on Post-Disaster Recovery Speed: Evidence from Reconstruction in Virginia and Maryland after Hurricane Sandy

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ABSTRACT

In the wake of a disaster, the price of essential goods and services, including reconstruction materials and labor, sharply increases. Price gouging refers to sellers and supply companies charging exorbitant prices for necessary items to take advantage of spikes in demand. Thirty-seven states out of fifty in the U.S. have legislation regulating price gouging, regarded as an unfair or deceptive trade practice during a disaster or emergency. Consumers, academics, and practitioners have mixed opinions about the effectiveness of this anti-price gouging law. Most existing studies focus on the impact of general price control qualitatively and theoretically. This study aims to empirically examine the effect of the anti-price gouging law on the speed of reconstruction in Virginia and Maryland in the aftermath of Hurricane Sandy. Difference-in-differences (DID) approach was used to estimate the effect of the anti-price gouging law (treatment) on post-disaster

reconstruction speed. This approach allows us to estimate the average treatment effect on the treated group by comparing the pre-to-post changes in the average number of monthly building permits in counties in Virginia (treatment group) with that of counties in Maryland (control group), while at the same time controlling for time-invariant county-specific heterogeneity and some other factors that may affect the monthly building permits for both groups in the absence of treatment. The findings show that the anti-price gouging law decreased the speed of post-disaster reconstruction by 18 units of monthly building permits (additional units in the treatment group due to treatment), indicating that the number of new housing units authorized by monthly building permits in Virginia is 18 units less than that of Maryland. The findings of this research are expected to assist policymakers and decision-makers in understanding the effect of the anti-price gouging law on reconstruction speed and enhancing their post-disaster reconstruction strategies and policies.

INTRODUCTION

Many reconstruction resources are subject to significant price inflation in the aftermath of natural catastrophes (Kim et al., 2022; Olsen & Porter, 2011). The construction material costs increased up to 30 percent after Hurricane Katrina (Khodahemmati & Shahandashti, 2020). This sudden price inflation in the wake of an emergency is often denounced as price gouging (Lee, 2015). Price gouging occurs when a seller sharply increases the prices of necessary goods, services, or commodities beyond the reasonable level that covers increased costs (Zwolinski, 2008). As an example, seventy-two percent of *Washington Post* poll respondents answered that oil companies were price gouging following Hurricane Katrina (Rapp, 2005).

State legislators enacted anti-price gouging laws to stabilize post-disaster price spikes and protect consumers from significantly increased costs (Bae, 2009). Anti-price gouging laws become only in effect during a disaster or emergency upon the disaster declaration by state governors, authorized local officials, or the president of the U.S. (Brewer, 2006). Thirty-eight states, the District of Columbia, the U.S. Virgin Islands, Guam, and Puerto Rico, have laws or regulations against price gouging during a disaster or emergency. However, some states, including Alaska, Arizona, Minnesota, Montana, Nebraska, Nevada, New Hampshire, New Mexico, North Dakota, South Dakota, Washington, and Wyoming, do not have anti-price gouging laws, allowing the free market to handle the post-disaster recovery process. There are controversies over the effects of anti-price gouging laws.

LITERATURE REVIEW

Price gouging during an emergency easily evokes a reactive and emotional outrage from people (Culpepper & Block, 2008). The vast majority of people have often condemned price gouging, arguing that it is unfair, immoral, exploitative, and impermissible (Zwolinski, 2008). Snyder (2009) stated that price gouging undermines the equitability of access to the goods and services essential to minimal human functioning and hits the poorest of a community the hardest.

In the wake of disasters, substantial increases in construction costs can reduce the reconstruction speed in economically marginalized communities (Kim & Shahandashti, 2022; Peacock et al., 2022). Unexpected construction labor cost inflation was found to be negatively correlated with the changes in the number of building permits in economically marginalized communities after disasters (Kim & Shahandashti, 2022). However, the relationship between price-gouging in the construction industry and building permits has not been examined.

Reconstruction cost increases are often identified as a significant cause of project delay (Gebrehiwet & Luo, 2017). Cumulative price increases of more than 20 percent over the insurance policy limit following catastrophes delayed post-disaster repairs since the policyholders needed to afford the extra repair costs by themselves (Döhrmann et al., 2017). Kim and Choi (2013) discussed that the increased costs following floods could delay the scheduled project delivery in the vicious cycle of post-disaster rebuild projects. The National Association of Home Builders called on the federal government to protect consumers against the price gouging of lumber since the reliable supply of reasonably priced construction materials is essential for swift disaster recovery (Wallisch, 2017). Rapp (2005) reviewed the existing anti-price gouging legislation and argued that enforcing the anti-price gouging laws can enhance economic efficiency by correcting the failure of the pricing mechanism. The anti-price gouging laws could counteract the gasoline price bubbles that cannot be attributed to market fundamentals after hurricanes (Oladosu, 2022). Warkentin (2021) highlighted the benefits of the anti-price gouging law and insisted that the anti-price gouging law should protect consumers against artificially high predatory pricing in times of crisis and emergency. Chang et al. (2011) discussed that post-disaster price control could stabilize the price of building materials and facilitate reconstruction projects in earthquake-affected regions. However, many economists consider that such price hikes condemned as price gouging following unexpected disasters are a natural and appropriate market response to the shortage of essential goods and services (Wilson, 2014). Price working as the 'invisible hand' in the free market can efficiently and effectively distribute scarce resources in the aftermath of disasters (Culpepper & Block, 2008). Price controls can hinder post-disaster recovery, thwarting the work of the free market and discouraging favorable supply responses to increased demand (Boshoff, 2021; Shannon, 1989). Anti-price gouging law prevented the supply of construction materials such as lumber to the disaster area and subsequently delayed the reconstruction after Hurricane Katrina (McGee, 2008). Chang et al. (2011) pointed out that price regulations can discourage resource supply, leading to resourcing bottlenecks in the post-Wenchuan earthquake housing reconstruction process. Tarrant (2015) investigated that the anti-price gouging laws did not statistically significantly affect wages in the construction industry in the hurricane-affected counties of the United States between 1990 and 2012. The anti-price gouging laws can rather damage the retail markets, especially where the retail prices tend toward fixity (Boshoff, 2021; Richards, 2022; Tarrant, 2015).

Despite the extensive discussion on the effect of price control or the anti-price gouging law, the quantitative empirical evidence on the effect of the anti-price gouging law on post-disaster reconstruction speed is still lacking and controversial (Cabral & Xu, 2021). Therefore, this study aims to examine the effect of the anti-price gouging law enforcement on reconstruction speed in two neighboring states, Virginia and Maryland, damaged similarly by Hurricane Sandy in 2012.

Virginia had a statute to regulate price gouging during Hurricane Sandy, while its neighboring state, Maryland, did not have a regulation for price gouging. Virginia enforced its post-disaster anti-price gouging law in 2004 following severe damages after Hurricane Isabel in 2003 (Rapp, 2005). Virginia's "Post-Disaster Anti-Price Gouging Act" defines price gouging as any price increase beyond the seller's cost increase and allows price escalation if solely incurred by additional costs stemming from an emergency (*Virginia Post-Disaster Anti-Price Gouging Act*, 2004). Virginia's anti-price gouging law is applied to any necessary goods and services, including but not limited to building materials and services, property or services for emergency cleanup, housing, and lodging (*Virginia Post-Disaster Anti-Price Gouging Act*, 2004).

However, Maryland had not regulated price gouging until 2020 because Maryland is one of the states rarely struck by natural disasters (Warkentin, 2021). Recently, Maryland passed an anti-price gouging statute to prevent sellers from profiteering by more than 10 percent during the

COVID-19 emergency declared by the Governor (*Exec. Order No. 20-03-23-03*, 2020; Zumer, 2020). Thus, at the time of Hurricane Sandy in 2012, Maryland did not have an anti-price gouging law, while Virginia regulated the price-gouging in the aftermath.

This study is organized as follows. First, the data and research methodology for measuring the effect of the anti-price gouging law on post-disaster reconstruction speed are elaborated. Then, the empirical results of panel data models with DID technique are presented and discussed. Finally, the implications and caveats of the findings are presented for policymakers, decision-makers, and disaster recovery practitioners to enhance their reconstruction strategies and process. The limitations of this study are also discussed in the conclusions.

RESEARCH METHODOLOGY

Data Collection

Building permit data are frequently utilized to estimate the speed of post-disaster reconstruction as local statistics on new privately-owned residential construction (Arneson et al., 2020; Stevenson et al., 2010). Building permits are issued monthly to authorize the new construction of privately-owned housing, counting over 98 percent of all privately-owned residential building constructions (U.S. Census Bureau, 2012). The current study collected the number of total housing units newly constructed and authorized by monthly building permits one year before and after Hurricane Sandy struck Virginia and Maryland counties on October 26, 2012. Table 1 summarizes the data collection used in this study. The determinants of building permits were included in the analysis to control for confounding effects. Population, housing units, median household income, and the percentages of White, Black, and Hispanic populations were considered to monitor the changes in monthly building permits (Lévêque, 2020; Stevenson et al., 2010). The poverty rates were also discussed as a predictor of monthly building permit issuances (Kim & Shahandashti, 2022; Kitchens & Wallace, 2022; Lusugga Kironde, 2006; Peacock et al., 2022).

Table 2 shows the sample design of this research and descriptive statistics of the monthly building permit variable. Hurricane Sandy strongly struck the coastlines of the northeast states including two adjacent states, Virginia and Maryland. The death toll by Hurricane Sandy was at two in both Virginia and Maryland (CNN Wire Staff, 2012). Hurricane Sandy resulted in a similar magnitude of storm surge and sea level rise in Virginia, Maryland, and Delaware (Donovan, 2013; Kang & Xia, 2020). Virginia and Maryland's communities also faced the blizzard conditions induced by Hurricane Sandy (Donovan, 2013). While 122,000 customers in Maryland faced power outages, 55,000 customers stood without power in Virginia (CNN Wire Staff, 2012). The Federal Government declared a major disaster of Hurricane Sandy in Connecticut, Delaware, Maryland, Massachusetts, New Hampshire, New Jersey, New York, Ohio, Pennsylvania, Rhode Island, Virginia, West Virginia, and the District of Columbia (Donovan, 2013). While Virginia received a public assistance grant of only 10 million dollars, Maryland received a grant of 32 million dollars for emergency and permanent work from the Federal Emergency Management Agency (FEMA) (FEMA, 2022). State defense forces were activated to assist in the reconstruction efforts in Maryland and Virginia after Hurricane Sandy (Bucci et al., 2013). Also, both Maryland and Virginia received beach erosion and coastal storm damage risk reduction projects from the U.S. Army Corp’s Hurricane Sandy recovery program (U.S. House, 2013).

Seventy-six counties in Virginia and fourteen counties in Maryland were selected as disaster-affected counties since those whose monthly building permit data are available received federal assistance from FEMA in the aftermath of Hurricane Sandy. The sample size of counties differs between Virginia and Maryland due to the data unavailability and the different number of counties in each state. However, the panel dataset consists of over one thousand observations with over a hundred observations for each state, satisfying the central limit theorem (Hsiao, 2022). Also, the panel dataset is strongly balanced indicating that the variables used in our models are available for all counties and years in the sample. By allowing us to control for county heterogeneity and common factors, panel data models used in this research yield consistent and unbiased estimates of the impact of anti-price gouging law on the number of monthly building permits (Wooldridge, 2021). The monthly building permit data in those counties were collected from November 2011 (one year before Hurricane Sandy) to October 2013 (one year after Hurricane Sandy). The number of total monthly building permit issuances was acquired from U.S. Census Bureau to enumerate newly constructed housing units.

Difference-in-Differences Approach

The difference-in-differences (DID) approach allows us to examine the effect of the intervention on an outcome by comparing the before and after average differences between a treatment group that receives the intervention and the control group that does not (Fredriksson & Oliveira, 2019). In other words, the DID approach quantifies the effect of the treatment on the treated group (e.g., the extra average change in the outcome variable due to the treatment or intervention) (Heckert & Mennis, 2012; Wooldridge, 2021). This DID approach enables a one-step analysis that allows us to control for any other factors that can potentially affect the outcome for both the treatment and control group, assuming that the control and treatment groups are subject to the same trend (Athey & Imbens, 2006; Card & Krueger, 1993; Kiel & McClain, 1995; Papke, 1994). By estimating the pre- and post-difference between the treatment group and control group in the outcome variable (difference-in-differences) and eliminating other factors that can affect the outcome for both groups, this approach allows us to quantify the unbiased and consistent effect of a treatment on the treated group or the additional average change in outcome for the treated group due to the treatment. Figure 1 represents the difference-in-differences (DID) framework to estimate the effect of the anti-price gouging law on the number of monthly building permits.

The DID approach quantifies the effects of the anti-price gouging law by comparing the pre-period and post-period changes in the average outcome of the treatment and control groups. The anti-price gouging law is the state-level price control only in effect during a declared state of emergency (Davis, 2008; Tarrant, 2015). We hypothesize that the speed of monthly building permit issuances in the disaster-affected counties under the control of the anti-price gouging law would fall relative to the rate in post-disaster counties that are not under its control. The treatment group is the disaster-affected counties in Virginia with the anti-price gouging law enforcement, and the control group is the disaster-affected counties in Maryland without the anti-price gouging law. The treatment effect illustrated in Figure 1 is estimated by the difference between the observed number of monthly building permits and the unobservable counterfactual trend in the treatment group. The unobservable counterfactual trend indicates the number of monthly building permits in the treatment group without the anti-price gouging law.

Non-parametric Approach

DID methods can be implemented using two different approaches: non-parametric and parametric approaches (Callaway & Sant'Anna, 2021; Wooldridge, 2007). The non-parametric approach estimates the treatment effect as the difference in the changes in the outcome (i.e., monthly building permits) from the pre-disaster level to the post-disaster level between the control and treatment groups. The non-parametric approach is expressed in Eq. 1.

$$\tau = (BP_{AT} - BP_{AC}) - (BP_{BT} - BP_{BC}) \quad \text{Eq. 1}$$

where τ is the treatment effect; BP_{AT} is the observed monthly building permits in the treatment group (i.e., disaster-affected counties in Virginia) after the disaster; BP_{AC} is the observed monthly building permits in the control group (i.e., disaster-affected counties in Maryland) after the disaster; BP_{BT} is the observed monthly building permits in the treatment group before the disaster; and BP_{BC} is the observed monthly building permits in the control group before the disaster.

Parametric Approach

The parametric approach assumes a regression model with a response variable (i.e., monthly building permits) and explanatory variables, including dummy variables that indicate the treatment status (Kaneko et al., 2019). Eq. 2 represents the panel data regression model with a DID specification to examine the effect of the anti-price gouging law on the number of monthly building permits accounting for the unobserved time-invariant county-specific effects (α_i). Population, poverty rates, the percentage of the Black population, and the percentage of the Hispanic population were selected as control variables based on the variance inflation factor (VIF) measures to avoid the multicollinearity problem.

$$BP_{it} = \delta_0 + \beta_1 APG_i + \beta_2 DIS_{it} + \beta_3 APG_i DIS_{it} + \beta_4 log(POP)_{it} + \beta_5 POV_{it} + \beta_6 BLK_{it} + \beta_7 HISP_{it} + \alpha_i + \varepsilon_{it} \quad \text{Eq. 2}$$

where BP_{it} is the number of monthly building permits in a county i at time t; APG_i is a dummy variable set to 1 if a county i is located in Virginia with the anti-price gouging law and 0 if a county i is located in Maryland without the anti-price gouging law; DIS_{it} is a dummy variable set to 1 if time t is post-disaster for a county i and 0 if time t is pre-disaster for a county i; APG_iDIS_{it} (i.e., the interaction term defined as APG_i times DIS_{it}) is a dummy variable set to 1 if a county i is in Virginia state and time t is post-disaster and 0 otherwise; $log(POP)_{it}$ is a logarithmic form of the population in county i at time t; POV_{it} is poverty rates in county i at time t; BLK_{it} is the percentage of the Black population in county i at time t; $HISP_{it}$ is the percentage of the Hispanic population in county i at time t; ε_{it} is an error term; α_i is individual effects to account for time-invariant county-specific heterogeneity; and β terms are the coefficients to be estimated by the model.

A significant coefficient of APG_iDIS_{it} (β_3), known as a DID, indicates that the effect of a disaster on the number of monthly building permits is moderated by whether a county i is located in Virginia with the anti-price gouging law or in Maryland without the anti-price gouging law.

Eq. 2 was examined using pooled ordinary least squares (OLS), fixed effects, or random effects estimators. Pooled OLS estimator does not allow us to control for the unobserved time-invariant county-specific effects or unobservable county-specific heterogeneity (α_i) in the error term that may be correlated with the variables of interest (such as geographical features, institutional quality, and the ability of the local administrators). Not accounting for such heterogeneity will lead to biased and inconsistent estimates. Therefore, panel data models, including fixed-effects and random-effects models, were employed as a parametric DID approach to examine the effect of the anti-price gouging law on post-disaster reconstruction speed in this study. The data were preprocessed to make a balanced sample panel data before establishing fixed effects and random effects models. The fixed effects and random effects models have different assumptions on the county-specific effects (α_i), which are expressed in Eq. 3.

$$\alpha_i = w_i\delta + z_i\lambda \qquad \text{Eq. 3}$$

where w_i is all the unobserved county-specific effects correlated with explanatory variables, z_i is all the unobserved county-specific effects uncorrelated with explanatory variables, and δ and λ are unknown parameters.

The random effects model allows us to control for the unobserved county-specific effects but assumes that they are not correlated with the independent variables in the model (i.e., cov(α_i, X_{it}) = 0). On the other hand, the fixed effects model allows the unobserved county-specific effects to be correlated with independent variables (i.e., cov(α_i, X_{it}) ≠ 0) and thus controls for the potential endogeneity of the independent variables due to these time-invariant county-specific factors.

Model Selection using Breusch-Pagan and Hausman tests

Figure 2 illustrates the framework of the DID parametric model selection process.

We performed two specification tests (Breusch-Pagan and Hausman tests) to identify the appropriate method for our data. These tests help us to assess whether the unobserved time-invariant county-specific effects (α_i) exist and are correlated with the independent variables. In order to determine whether the unobserved time-invariant county-specific effects (α_i) exist, we used the Lagrange multiplier test proposed by Breusch and Pagan (1980). The null hypothesis in this test is that there are no unobserved time-invariant county-specific effects (i.e., var(α_i) = 0). A failure to reject the null hypothesis would support using the OLS regression. Otherwise, we need to conduct the Hausman (1978) test to select between fixed effects and random effects models. The null hypothesis in this Hausman test is that the independent variables and the unobserved time-invariant county-specific effects (α_i) are not correlated. We would choose to use the fixed effects model instead of the random effects model if we reject the null hypothesis. When the unobserved time-invariant county-specific effects (α_i) are correlated with the independent variables, the fixed effects model is preferred as it will yield unbiased and consistent estimates. On the other hand, we prefer to use the random effects model if we fail to reject the null hypothesis. In this case, the random effects will produce both consistent and efficient estimates. Regardless, the random effects estimator allows us to control for the within-county correlation in the error term, and thus yields more efficient estimates (Bell et al., 2019). It also yields consistent estimates if the independent variables are not correlated with the unobserved heterogeneity. However, the results from the random effects estimator suffer from omitted variable bias if the independent variables are correlated with the time-invariant unobservable factors.

EMPIRICAL RESULTS

Both non-parametric and parametric approaches of DID were employed to examine the effect of the anti-price gouging law that regulates the reconstruction market price on monthly building permits in Virginia and Maryland after Hurricane Sandy.

Results of DID Analyses

Results of Non-parametric DID Analysis

Table 3 shows the non-parametric DID analysis results on the anti-price gouging law's effect on post-disaster monthly building permit issuances that can represent the reconstruction speed. Virginia counties issued 25.38 building permits monthly on average, while Maryland counties issued 73.69 permits before Hurricane Sandy. After Hurricane Sandy struck both Virginia and Maryland, the average number of building permits in Virginia counties increased by 5.3 units monthly, while the number in Maryland counties increased by 23.56 units monthly in the aftermath. The treatment effect (τ) of the anti-price gouging law triggered during Hurricane Sandy was calculated as -18.26 units using Eq. 1 and -17.88 units when controlling for the confounding effects. The results of non-parametric DID analysis show that the anti-price gouging law decreased the building permit issuances by 17.88 units monthly during the post-disaster situation. The anti-price gouging law that governs the reconstruction market can negatively affect the speed of post-disaster recovery in Virginia relative to Maryland. This finding is consistent with many economists' expectations that price control under the anti-price gouging law can impede the speed of post-disaster reconstruction (Culpepper & Block, 2008; Giberson, 2011; Shannon, 1989; Wilson, 2014; Zwolinski, 2008).

Results of Parametric DID Analysis

Table 4 summarizes the results of parametric DID analyses using fixed effects and random effects models. The treatment effect was measured to be negative by the parameter of APG_iDIS_{it}. The effect of the anti-price gouging law was estimated as 18 units decrease monthly in the number of building permits in post-disaster situations according to the results of both the fixed effects and random effects models. This indicates that the monthly building permits decreased by 18 units in Virginia counties where the anti-price gouging law was triggered in the wake of Hurricane Sandy compared to Maryland counties without the anti-price gouging law in the post-disaster recovery process.

The disaster shows a statistically significant positive effect on the number of monthly building permits regardless of the existence of the anti-price gouging law. The disaster occurrence increases the number of monthly building permits by approximately 15 units. This result seems plausible because housing reconstruction and repair projects are largely and quickly undertaken in the aftermath of a disaster (Dikmen & Elias-Ozkan, 2016). The number of monthly building permits increases as the population increase. This positive relationship between monthly building permits and the population is consistent with the findings in the previous studies (Carlucci et al., 2018; McDonald & McMillen, 2000; McGibany, 1991).

Results of the Breusch-Pagan Tests

The null hypothesis of no individual effects was rejected according to the results of the Breusch-Pagan tests. In other words, statistically significant individual heterogeneity exists among the county-level monthly building permit data. Table 5 summarizes the results of the Breusch-Pagan test to select between the pooled OLS regression and the fixed effects model. The null hypothesis was rejected at the 1% significance level, indicating no individual fixed effects. Therefore, the fixed effects model is more appropriate to control for the county-specific effects than the pooled OLS regression.

Table 6 shows the results of the Breusch-Pagan test to choose between the pooled OLS regression and the random effects model. The null hypothesis of no individual random effects was rejected at the 1% significance level. Therefore, the random effects model is more appropriate to control for the county-specific effects than the pooled OLS regression. Both results of the Breusch-Pagan tests in Tables 5 and 6 indicate that county-level heterogeneity exists, and thus the results from pooled OLS will be biased and inconsistent.

Results of the Hausman Test

The Hausman test failed to reject the null hypothesis that the independent variables and fixed effects (α_i) are not correlated. Given the test results reported in Table 7, we failed to reject the null hypothesis of the Hausman test at the 5% significance level, indicating that the random effects model is likely more appropriate than the fixed effects model for our data. However, we report the results from both random and fixed effects models.

DISCUSSIONS OF RESULTS

The anti-price gouging law triggered by the declaration of a state of emergency or disaster enforces civil or criminal penalties for price gouging violations that happened during a disaster. The effect of the anti-price gouging law on post-disaster reconstruction speed was estimated using panel data models (fixed effects and random effects) with a DID specification. The reconstruction speed was quantified by the number of monthly building permits that authorize the new construction of housing units. The number of monthly building permits was compared between Virginia counties with the anti-price gouging law enforcement and Maryland counties without the anti-price gouging law enforcement to examine the effect of the anti-price gouging law in the aftermath of Hurricane Sandy using the DID approach. The DID estimators present evidence that the number of building permits that authorize new housing construction decreases by 18 units monthly in Virginia counties where the anti-price gouging law was triggered relative to Maryland counties without anti-price gouging law in the aftermath of Hurricane Sandy. It can be implied that construction cost inflation, often denounced as price gouging in the construction industry, is a natural market response to a post-disaster imbalance between supply and demand and can address the market imbalance, facilitate reconstruction works, and increase the number of monthly building permits.

The change in the number of monthly building permits in both Virginia and Maryland counties after Hurricane Sandy is a fifteen-unit increase in new housing units. Hurricane Sandy increased the monthly number of new housing units authorized by monthly building permits by 15 units in both Virginia and Maryland. This result is consistent with the findings of existing disaster studies that reconstruction activities largely increase following a disaster (Celentano et al., 2019; Dikmen & Elias-Ozkan, 2016).

The results of the Breusch-Pagan tests show unobserved time-invariant county-specific effects (α_i) exist in the monthly building permit data. Therefore, panel data models, including fixed effects and random effects models, are recommended to include and control for those county-specific effects (α_i). Then, the Hausman test was conducted to choose between fixed effects and random effects models. Since the null hypothesis of the Hausman test was not rejected at the 5% significance level, the random effects model was preferred as it produces both consistent and efficient estimates. The random effects estimator enables us to control for the within-county correlation in the error term and thus yields more efficient estimates. The random effects estimator also yields consistent estimates if the independent variables are not correlated with the unobserved heterogeneity.

The random effects estimator can be helpful when the entities are randomly assigned to the treatment and control groups. In this case, the correlation between the independent variables and the unobserved time-invariant variables is likely insignificant, validating the use of random effects. This is likely relevant to disaster treatment in the current study. Tofighi et al. (2016) reported that the occurrence of a disaster followed an inherently random process. Note also that the fixed effects model eliminates the cross-section variation in the explanatory variables, and only uses the within-county variation over time, thus relying on enough within-county variation in the variables. The results from both fixed effects and random effects estimators are consistent. There is a significantly negative effect of the anti-price gouging law on monthly building permits regardless of the methods used.

We note some caveats for policymakers, decision-makers, and disaster recovery practitioners. First, we found empirical evidence suggesting that the free market be allowed to accelerate reconstruction speed via the invisible hand without price control. It can be implied that people's emotional denunciation and legal accusations against the post-disaster price escalation, often referred to as price gouging, did not help to expedite the reconstruction process in the aftermath of a disaster but rather decelerated the speed of reconstruction. It is also implied for policymakers and practitioners that providing incentives to support reconstruction resource supply and procurement can more effectively enhance post-disaster reconstruction speed and strategies rather than controlling post-disaster market price inflation stemming from the large-scale post-disaster reconstruction demand and supply chain disruption.

Policymakers and practitioners should provide market-driven post-disaster reconstruction strategies by incentivizing the suppliers to ensure resource availability for housing reconstruction projects instead of restricting the prices. Post-disaster reconstruction strategies and plans are expected to increase accessibility to available resources, satisfying the large-scale reconstruction demand and facilitating reconstruction work.

Second, because of the nonnegligible individual county-specific heterogeneity in the housing reconstruction process, it is recommended to implement panel data models to include and control for these county-specific effects on the post-disaster reconstruction process. Last but not least, we found that the unobservable county-specific heterogeneity is neither related to the enforcement of anti-price gouging law nor the occurrence of Hurricane Sandy according to the results of the Hausman test. This seems plausible because the anti-price gouging law is a state-level price control that does not rely on county-specific factors but affects all the counties in the state equally. The occurrence of a disaster is considered to follow an inherently random process (Tofighi et al., 2016) and is unrelated to county-specific heterogeneity.

CONCLUSIONS

Thirty-eight states out of fifty states in the U.S. have anti-price gouging laws or regulations to control the increased price in the aftermath of a disaster. The anti-price gouging laws enforce civil or criminal penalties for price gouging violations. However, the effect of the anti-price gouging laws on the post-disaster reconstruction process has been surrounded by controversy.

In this paper, we investigated the effect of the anti-price gouging law on post-disaster reconstruction speed. There is evidence that the anti-price gouging law triggered in the wake of a disaster decreased the number of new housing constructions authorized by monthly building permits. We employed a DID technique, including non-parametric and parametric approaches to estimate the effect of the anti-price gouging law on post-disaster reconstruction speed. All the DID estimators yield a consistent result that the presence of the anti-price gouging law decreased the number of new housing constructions by 18 units in Virginia counties relative to Maryland counties that were not subject to the anti-price gouging law during Hurricane Sandy.

The results of the Breusch-Pagan tests found the existence of time-invariant county-specific heterogeneity (α_i) and suggested that panel data models be implemented to control for such heterogeneity. According to the results of the Hausman test, the random effects model was preferred because the random effects model yields both efficient and consistent estimates.

It is important to list the limitations of this study and suggest a promising avenue for future research. To begin with, the current study only examines the impact of the anti-price gouging law on the number of monthly building permits, which may not fully capture the complexity of post-disaster reconstruction. Since the monthly building permits are used to authorize new privately-owned residential constructions, the effect of anti-price gouging law on the post-disaster repairs, restorations, or non-residential constructions was not examined in this study. It would be an important avenue for future research to investigate the effect of anti-price gouging law on other non-residential construction markets and price gouging practices in the post-disaster reconstruction industry. Secondly, our results are based on the reconstruction process after Hurricane Sandy in Virginia and Maryland counties. Due to data unavailability, we used only 76 counties in Virginia and 14 counties in Maryland. However, additional county-level data in different states need to be examined in future research to examine if the findings of this research can be generalized or robust. Different findings can be found for other states and time periods due to their distinct market structures, population characteristics, and other factors. It would be interesting to investigate whether the findings of this research can still hold in other post-disaster scenarios. Also, other explanatory variables, such as spatial closeness to the disaster-affected communities, can be incorporated into future analyses. In future research, the spatial DID approach modeling the geographical locations can be utilized to examine the spatial interactions among communities in the post-disaster reconstruction process. Further research on post-disaster policy or legal interventions can add insightful value to this line of study, providing crucial implications for policymakers and decision-makers in enhancing post-disaster reconstruction strategies and processes.

DATA AVAILABILITY STATEMENT

All data, models, or codes that support the findings of this study are available from the corresponding author upon reasonable request.

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Table 1. Data Collection

Data	Frequency	Level	Period	Source
Dependent variable				
Building Permits	Monthly	County-level	Nov 2011 – Oct 2013	Census Bureau
Control variables				
Population	Yearly	County-level	2011 - 2013	Census Bureau
Poverty Rates	Yearly	County-level	2011 - 2013	Census Bureau
Housing Units	Yearly	County-level	2011 - 2013	Census Bureau
Median Income	Yearly	County-level	2011 - 2013	Census Bureau
%White Population	Yearly	County-level	2011 - 2013	Census Bureau
%Black Population	Yearly	County-level	2011 - 2013	Census Bureau
%Hispanic Population	Yearly	County-level	2011 - 2013	Census Bureau

Table 2. Sample Design and Descriptive Statistics

	All	VA	MD
Number of counties in the sample data	90	76	14
Number of pre-disaster sample data for 12 months	1,080	912	168
Number of post-disaster sample data for 12 months	1,080	912	168
Mean (Units):			
Pre-disaster monthly building permit counts	32.9	25.38	73.70
Post-disaster monthly building permit counts	41.04	30.69	97.26

Table 3. Results of the Non-Parametric DID Analysis

Monthly Building Permits	All	Before Sandy	After Sandy	DID	DID with controls
VA (Treatment)	28.04	25.38	30.68	5.3 (3.01)	5.25[a] (1.89)
MD (Control)	85.48	73.69	97.26	23.56[b] (11.01)	21.9[b] (8.65)
Change in monthly BP (τ)	-57.44	-48.31	-66.58	-18.26[b] (8.47)	-17.88[b] (8.94)

Notes: Standard errors are given in parentheses.
[a]Rejection of the null hypothesis at the 1% significance level
[b]Rejection of the null hypothesis at the 5% significance level

Table 4. Results of the Parametric DID analyses

Data	Monthly Building Permits (Units)	
Variables	FE (Fixed effects)	RE (Random effects)
APG_i	-	2.505 (13.45)
DIS_{it}	15.36[b] (6.416)	15.65[b] (6.384)
APG_iDIS_{it}	-18.04[a] (5.76)	-18.05[a] (5.73)
$log(POP)_{it}$	442.8[b] (172.7)	28.60[a] (3.945)
POV_{it}	0.777 (1.334)	-0.924 (0.739)
BLK_{it}	622.0 (770.8)	3.619 (29.36)
$HISP_{it}$	-327.7 (973.0)	123.2[c] (73.69)
Intercept	-	-283.5[a] (48.9)
Time dummy	Yes	Yes
Observations	2,160	2,160

Notes: Robust standard errors are given in parentheses.
[a]Rejection of the null hypothesis at the 1% significance level
[b]Rejection of the null hypothesis at the 5% significance level

Table 5. Results of the Breusch-Pagan Test (Pooled OLS vs. Fixed Effects)

Monthly Building Permits	F-statistic	df1	df2	*p*-value
F-test for individual effects	15.248	88	2042	0.00

Notes: df1 and df2 represent a degree of freedom.

Table 6. Results of the Breusch-Pagan Test (Pooled OLS vs. Random Effects)

Monthly Building Permits	Chi-Square Statistic	Degree of Freedom	*p*-value
Lagrange Multiplier test for balanced panels	3346.1	1	0.00

Table 7. Results of the Hausman Test

Hausman Test	Chi-Square Statistic	*p*-value
Fixed Effects vs. Random Effects	9.969	0.126

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