5-2015

Do Footprint-based CAFE Standards Make Car Models Bigger?

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MA Economics Paper
University of New Hampshire
13 May 2015

Abstract

Corporate Average Fuel Economy (CAFE) standards have historically been set equal across all manufacturer fleets of the same type. Concerns about varying costs across firms and safety implications of standards that are set homogeneously across firms and models resulted in a policy shift towards footprint-based standards. Under this type of standard, individual car models face targets based on the size of the area between the wheelbase and wheel track, so that larger models face less stringent standards, and manufacturers who make, on average, larger cars will face a lighter fleet standard. Theoretical models have shown that this type of policy creates an incentive for firms to effectively lighten the standard they face, but no purely empirical study has tested this theoretical conclusion. I use a series of difference-in-difference estimations to test whether firms respond to the policy by increasing the footprint of individual models. I find some statistically significant evidence of an increase in footprint size in response to the policy when the treatment effect is assumed to increase by market share.

I would like to acknowledge the role that guidance from Professor Robert Mohr and Professor Karen Conway play in this paper. Their help was essential in developing and understanding the empirical methodology and theoretical background.

I. Introduction

The burning of fossil fuels exacerbates environmental concerns; in particular, it is linked with climate change and air pollution. It is estimated that the emission of carbon dioxide is the largest cause of climate change and that 80% of the human activity-related emissions of carbon dioxide are from burning fossil fuels (Schmalensee, 1998). The emissions related to transport made up around 27% of the total of emission from the United States in 2013 (EPA, 2015). It follows from these figures that decreasing emissions from the transportation sector is an important piece in decreasing overall emissions, and curbing human-related climate change. One possible avenue to decrease emissions is to increase vehicle efficiency, or the amount of travel possible per a fixed unit of fuel, thereby decreasing the total consumption of fossil fuels. A major policy aimed at increasing efficiency in the United States is the Corporate Average Fuel Economy (CAFE) standards. A recent modification to the way that CAFE standards are set may have an unintended consequence of decreasing some of the efficiency gain from the new, stricter standards. Footprint-based standards give producers a new way of meeting standards, by making cars larger and effectively lightening the standards that they face. It is the purpose of this paper to empirically investigate whether there is evidence that firms respond to footprint-based CAFE standards by making their individual models larger.

The original CAFE standards set fixed standards that all manufacturers, on average, are to meet. There are two major concerns with standards that are homogenous across manufacturers and models: 1) firms face heterogeneous costs in meeting homogenous standards, and 2) homogenous standards by model create pressure to meet standards by decreasing car size, which decreases the safety of vehicles. To address these concerns, the newest standards set individual targets for models based off of the footprint[1] of the model, and create a total manufacturer fleet standard from the sales-weighted mean of the individual model targets. The effect is that smaller cars face more stringent standards than larger cars, and manufacturers that produce, on average, larger cars will face lighter standards. This addresses the concern of heterogeneous costs between firms that make larger and smaller cars, and eliminates at least some of the incentive to meet standards by making cars smaller.

While the intent of footprint-based CAFE standards was to eliminate some of the incentive to make cars smaller, there is some evidence from theoretical models that footprint-based standards might actually create an incentive for manufacturers to *increase* vehicle size. This was not the intent, as the larger cars face less strict standards, and this method of reaching standards could diminish some of the predicted efficiency gain from the increase in standards. This paper uses a difference-in-difference (DD) methodology to test whether manufacturers have actually responded to the footprint-based standards in the way theoretical models predict: by increasing the footprint of models to face lighter standards.

II. Background and Literature Review

Before investigating the effects of footprint-based efficiency standards, it is important to consider the history of CAFE standards and the criticisms lodged against them—and in particular, against standards that are set homogeneously across firms and models. CAFE standards are enforced at the level of a manufacturer's fleet. The CAFE standards break up a manufacturer's total fleet into two separate fleets: a light truck fleet and a passenger vehicle fleet. The National Highway and Transportation and Safety Administration (NHSTA) of the

1. The footprint is the area calculated from the wheelbase and the wheel track.

Department of Transportation (DOT) sets and enforces CAFE standards, and the Environmental Protection Agency (EPA) is responsible for measuring the efficiency levels that are met by each manufacturer's fleet. Manufacturers are fined a fixed amount for every mile over the standard their fleet is on average multiplied by the total number of cars in the fleet (DOT, 2014). In this way, CAFE standards are a restriction placed on the manufacturer which gives incentive for them to make more efficient cars.

The first CAFE standards were introduced under the 1975 Energy Policy and Conservation Act, and were effective for the 1978 model year for cars at 18 MPG and were slowly phased to 27.5 MPG by the 1985 model year. The standards for light trucks were set starting for the 1979 model year at 17.2 MPG and phased up to 20.5 MPG for light trucks by the 1987 model year (Klier and Linn, 2011). The original standards were set at a fixed amount for all firms.

There have been several criticisms of CAFE restrictions. One complaint is that CAFE standards are not the best way to increase car efficiency. In a 2005 paper, West and Williams compare the effects of a gas tax with CAFE standards. They find that both CAFE standards and gas taxes increase efficiency, but that they have different effects on the number of miles traveled. Their study shows that CAFE standards decrease the cost per mile driven and actually increase the miles that car users will drive, this is termed the ‘rebound effect.’ They argue that this increase in driving comes with other externalities such as car fatalities, wear on roads, and traffic congestion; they consider these in their marginal cost calculations for CAFE standards. They found that gas taxes, on the other hand, not only increase efficiency, but also decrease the amount that car users drive. The reason for this is that it increases the value of increased efficiency for consumers, but also the cost of travel per mile. They conclude that the gas tax has a lower marginal cost than CAFE standards and is the preferred policy (West and Williams, 2005). While West and Williams make a compelling argument for the gas tax, there are other things to consider from the literature.

Theoretically the gas tax can more accurately account for other costs not considered by CAFE standards, but there may still be reason to keep CAFE standards. A 1990 study by Greene finds that although both gas taxes and CAFE standards affect efficiency, that CAFE standards have a much greater effect. Consumer demand for efficient cars does increase with gasoline prices, but the hard restrictions on the producer side are more effective in the short-run (Greene, 1990). Further, a 2014 study finds that an expected savings of $1 in future gasoline costs is accompanied with a willingness to pay $.76 more in current prices; even after taking into consideration discount rate, it seems that consumers do not value future savings on gasoline enough (Hunt and Wozny, 2014). If the goal is to increase efficiency in the short-run, CAFE standards will likely be more effective than a gas tax because consumers do not value future savings enough. Given the seeming immediacy of the environmental imperative to reduce emissions and develop alternative technologies, these restrictions on producers might be the most effective. That being said, there is no reason that these policies could not also be paired with gasoline taxes which could counteract the ‘rebound effect’ documented by West and Williams.

Anther criticism of CAFE standards is that homogenous standards for manufacturers create heterogeneous costs between manufacturers; that is, standards affect manufacturers unequally. A 2014 working paper shows that while firms do adopt new technologies to meet CAFE standards, a good portion of the increases in efficiency come from decreases in the power and size of vehicles. These are the cheapest ways for firms to meet standards, and these are the methods that are adopted in the medium-run (Klier and Linn, 2014). A 2013 paper shows that Japanese cars, typically smaller and more fuel efficient, historically exceed CAFE standards. European cars, on the other hand, are often luxury cars, and it is often worth it for them to pay the fines and keep their cars more powerful. The companies that were impacted the most were American cars; these companies merely met standards, and faced the majority of costs (Jacobsen, 2013; Knittel, 2012). This cost differential across firms in meeting standards can cut into profits and may damage some firms more than others. A footprint-based policy would address this concern, forcing manufacturers that make smaller cars, on average, to meet harsher standards. This effectively will mean that Japanese manufacturers will face harsher standards than domestic firms under footprint-based standards, but that this may make costs of meeting standards more equal across firms.

There is another concern with the original CAFE standards; some studies have argued that decreases in vehicle size due to CAFE standards have led to an increase in traffic-related deaths of up to 3900 annually (Laffer, 1991). A 2014 working paper uses a quasi-experimental methodology to find a more conservative estimate of an additional 149 fatalities annually due to the ways manufacturers met the original CAFE standards. They also find, using a theoretical model of the effect of footprint-based standards that CAFE standards dependent on the footprint of a model do not contribute to additional motorway fatalities (Jacobsen, 2013b). It seems that reducing or eliminating the incentive to meet standards by decreasing vehicle size can counteract the safety externalities of CAFE standards, in addition to putting American and Japanese manufacturers on more equal footing.

The motivations for footprint-based standards have been outlined; now it is useful to take a detailed look at how and when footprint-based policies were passed. The first footprint-based standards were initiated in 2006 on the 2008-2011 light truck model years. These standards replaced standards set in 2005 for the 2008 models, and manufacturers were given the option to meet either set of standards (NHTSA, 2006). Because manufacturers were given this option, it is not clear which standards manufacturers chose to meet, and it would be difficult to investigate the effect the optional footprint-based policies had on the footprint of models.

The Energy Independence and Security Act signed into effect by George W. Bush in 2007 revised the initial car CAFE standards so that they would increase 40% by 2020. Under Barack Obama the timeline of these standards was pushed forward, so that they were to be met by the 2016 model year (Klier and Linn, 2011). The revision passed on March 23, 2009, established footprint-based standards effective for the 2011 model year. These standards established different targets for individual models from a logistic curve, again, so that larger vehicles would face less stringent standards than smaller vehicles (NHTSA, 2009a). These footprint-based policies had the intent of addressing safety and heterogeneous cost concerns, but theoretical models show that they create an incentive for manufacturers to increase the footprint of car models.

The literature supports the idea that footprint-based standards create an incentive to increase vehicle size. Whitefoot (2011) uses a simulated oligipolistic-equlibrium model from 2006 model years to show that producers that are constrained by CAFE standards and face a tradeoff between power and size have an incentive to increase the footprint of a model for the 2014 footprint-based standards in all cases, except where consumers preference for vehicle size is in the lower bound and the preference for acceleration is in the upper-bound. It is also found that increases in footprint size reduce the predicted efficiency levels from the goals of the NHTSA by 1.4 to 4.1 MPG. This lends to an unintended consequence of larger vehicles and lower increases in efficiency (Whitefoot, 2011). Jacobsen (2013a) performs a similar simulation of the constrained manufacturers to show that footprint-based standards reduce the incentive to shift the mix of a fleet towards smaller cars (Jacobsen, 2013a). While theoretical simulations of the producer problem show that manufacturers have a new incentive to increase footprint and less incentive to shift the composition of their fleets towards smaller cars, there has been little focus on a purely empirical model to test if manufacturers have actually respond to the incentive by increasing the footprints of models.

While no purely empirical methodologies have been employed to test whether firms have increased the footprints of individual models, empirical methodologies have been employed on other attribute-based efficiency standards. Japan has linear-step weight-based efficiency standards, which set less stringent standard for different weight classes of vehicles. Ito and Sallee (2014) show that there is evidence of bunching around the discrete points of the linear-step function. They predict the distribution of weight before weight-based standard and after to show that there is a distortion of the distribution of weight in response to the weight-based standards (Ito and Sallee, 2014). While this gives evidence that size-related attribute-based standards might alter firm behavior, the target function of the footprint-based standards of the United States is very different than the Japanese linear-step target function.

Footprint-based standards address concerns raised against the homogeneous CAFE standards. However, there is theoretical evidence to suggest that they create an incentive for firms to increase the footprint size of models, and that this incentive can offset some of the efficiency gains predicted by the NHTSA. There is limited empirical evidence that tests the simulated models which show that there is this incentive. The only purely empirical evidence is not from the footprint-based standards of the United States, but rather from the weight-based Japanese standards. This paper investigates whether firms changed model footprints in response to footprint-based standards using several different weighted DD models.

III. **Empirical Methodology**

The 2011 footprint-based standards were announced at the end of March in 2009. It is important to note that production of the 2010 model began only a few months later in the summer of 2009. It is unreasonable to think that producers could respond to any incentive to increase footprint for the 2010 model year. For this reason, the 2011 footprint-based policy can be seen as an exogenous shock which cleanly affects the 2011 model year. There may be some argument that adjustments to footprint could happen over more than one year. For this reason the 2012 model year is included in the panel of data. It should be noted that the 2012 model year faced footprint-based standards that are based off of a bounded linear function, and that these standards could also influence the 2012 model year. For this reason, although this year is included in the analysis, the focus of the study is on the 2011 model year.

DD methodology requires that there is a time period before an exogenous treatment, and a time period afterward. As alluded, the data includes a panel of model years from 2009 to 2012. The analysis uses the 2009 model year as the base year and the 2011 and 2012 model years as the model years that could be affected by the footprint-based standards. The 2010 model years were dropped from the panel in case there was any adjustment in the short period after the announcement of the footprint-based standards for 2011 and the beginning of the production of the 2010 model year. As noted above, there is reason to believe that firms could not have made adjustments to the footprint of models in the short time period before the production of 2010 models, and that this was an overly-cautious measure.

Use of DD methodology also necessitates a group of observations which are affected by a policy—a treatment group—and a group which are not—a control group. This allows that the change over time from the control group can be compared to a change over time of the treatment group. Any changes which affect both groups are not due to the treatment. The treatment effect is thus the difference of the change in the treatment group before and after the treatment less the difference in the control group before and after the treatment. This allows that any effect on the dependent variable not due to the treatment can be differenced out, and that the remaining effect is the treatment effect.

A look to the logistic function from the NHTSA (Figure 1)—which is the basis for the target for each mode—can show how a control and treatment group can be constructed from the data. The total variation in the possible model targets is from just above 25 MPG to just below 40 MPG. It can be seen that the logistic function is steepest from 40 to 50 square feet, suggesting that most of the variation of possible targets faced occurs within this range of footprints. In this range, a small increase in footprint could result in a relatively large decrease in the stringency of the target efficiency for the model. However, beyond either bound of this range, the benefit of an increase in the footprint with regards to decrease in the stringency of the model target is relatively small. For this reason, it can be expected that models within the 40 to 50 square feet range will react to the standard shift, while models outside of this range will not.

With this in mind, the control group is constructed from models with footprints above 51 square feet and models with footprints below 39 square feet. The treatment group is constructed from models with footprints between 41 and 49 square feet. This allows that the models with footprints between 39 and 41 square feet, and those with footprints between 49 and 51 square feet are dropped from the sample. This ensures that the treatment and control group are distinct from one another, and removes the possibility that those on the boundaries—with intermediate benefits from increasing footprint—will obscure results.

This framework is the basis for the DD model. The empirical model is given below:

$$\boldsymbol{Footprintit = \beta 1 + \beta 2Xi + \beta 3Treati + \beta 4Aftert + \beta 5Treat * Afterit + \varepsilon it}$$

Here X_i is a vector of control variables which are believed to be related to the footprint of the 2009 model. This vector includes two power measures (torque and horsepower), the efficiency of the model, and the market share of the model. As has been established from the literature, firms often trade-off power and size to increase efficiency. Firms were facing homogenous CAFE standards on the 2009 model year, and may have adjusted either or both of the size and power to meet standards. For this reason, it might be expected that power and footprint would share an inverse relationship. Efficiency is also related to footprint; the literature establishes that one way that firms increase efficiency is to decrease vehicle size; it can be expected that efficiency and footprint are inversely related. These variables thus act as a control for the initial footprint of the 2009 model. Since, this model aims to see the effect of footprint-based standard holding all else constant, the 2009 control variables are used for the 2011 and 2012 models.

The market share is a slightly more complicated control variable. Firms make adjustments by model, but not all adjustments of models provide equal benefit. Producers are limited in their ability to meet standards by consumer demand. For this reason, there may be more incentive for them to adjust those models who have larger market shares. This suggests that there may be a relationship between the initial footprint of the 2009 model and the market share of that model. This relationship could be positive if consumers demand larger cars, or it could be negative if firms reduce the footprint of vehicles with larger market shares to more easily meet standards. Regardless of the direction, there is reason to believe that the 2009 model share may be an important variable in determining the footprint of a model.

Beyond the control variables, the After variable is an indicator variable which equals 1, for all 2011 and/or 2012 models, and equal 0 for all 2009 models. Likewise, the Treatment variable is an indicator variables which equals 1 if the 2009 year of a model has a footprint between 41 and 49 square feet, and a value of 0 if the 2009 year of a model has a footprint above 51 square feet or below 39 square feet. The interaction variables of After and Treatment takes on a value of 1 only if the model is in the treatment group and is after the policy is effective. Thus, the value of β_5 is the treatment effect of the footprint-based policy on the size of the footprint for an individual model.

In addition to this simple DD methodology, a model which allows the dose of the treatment to vary by market share and a model which allows the dose of the treatment to vary by the distance the 2009 model year's efficiency is below the 2011 standard. The idea is that models with larger market shares are more likely to experience an increase in footprint in response to the policy, because this will increase the overall standard of a firm more than a model with smaller market share. Further, models which are farther away from the 2011 target in 2009 are more likely to experience an increase in footprint to decrease their target efficiencies. This assumes that firms will try to meet the target for a model, and may not bear out if firms react on the average, and not for every model. The dosed DD model is given below:

$$\boldsymbol{Footprintit = \beta 1 + \beta 2Xi + \beta 3Treati + \beta 4Aftert + \beta 5Treat * Dose * Afterit + \varepsilon it}$$

As can be seen, the weight is only applied to the treatment effect term. The reason for this is that these factors should not affect the control group after the policy is enacted because the control group should not react to the policy at all. Weighting this term would indicate that models were somewhere between the before and after period, which is not the intent. Further, the treatment group should not be affected by the policy at all in 2009, and is not somewhere between the control and treatment group. For this reason, it is only the treatment effect that is seem to vary by either the market share of a model or the distance of the 2009 efficiency below the 2011 target.

IV. **Data**

As has been established, the data is a panel from 2009-2012, but the control variables are held constant for each model for all years at the 2009 values. The panel is of 81 models which were sold continuously between 2009 and 2012. The data does not include models that were either discontinued or added to a fleet during this time period. Of the data set, 14 models are within the control group bounds, 53 are within the treatment group bounds, and 14 models are within the section that are dropped from the model. Figure 1 shows the distribution of the models in the sample along the 2011 model year logistic curve based on their footprints. Here it can be seen that there is decent variation among the footprints of the car models in the sample.

The data source for the 2009 market share, used both as in independent variable and a weight in the weighted DD estimation, is Automotive News. The rest of the 2009 model characteristics and the 2009-2012 model footprints are collected from Motortrend.com[2]. The

2. I would like to thank Kevin Vansylyvong for his help in collecting this data.

distance of the 2009 model below the 2011 standard was calculated from the NHTSA target logistic curve and the efficiency from Motortrend.com. There were three models which had targets below their 2009 efficiency level in the treatment group. For the distance short of the target, weighted DD model these three models were treated as control models. This is consistent with the idea that the distance below the standard determines the dose of the treatment effect.

Summary statistics of the dependent variables based on the treatment and control, and before (2009) and after (2011) variables give a preliminary glance into the expected sign of the treatment effect on footprint size. Table 1 shows that the average footprint of the treatment group before the treatment is 45.04 square feet, and after the treatment is 45.64 square feet. This makes the difference in the treatment group after the treatment 0.6 square feet. The average footprint of the treatment group in 2009 is 45.32 square feet, and in 2011 is 45.33 square feet. Thus, the difference in the control group after the treatment is 0.1 square feet. This makes the difference in the difference from the treatment group and the control group 0.5 square feet, and suggests that the average footprint of the treatment group did increase more than the average footprint of the control group as the model predicts.

V. **Results**

Results from the OLS DD regressions (Table 2) are not as strong as the preliminary look from the mean footprint of the four groups. The simple 2011 model DD regression shows a treatment effect of 0.583, suggesting that the treatment results in an increase of the footprint of the vehicle by 0.583 square feet. While this result is close to the 0.5 square feet estimate from the difference-in-difference of the means of the four groups, it is not statistically significant. It can also be seen that a 10% increase in market share results in a 0.16 increasing in the footprint, suggesting that cars with larger footprints make up a larger market share, on average and holding all else constant. This result is not statistically significant. It can also be seen that horsepower has a statistically significant negative relationship with footprint, so that an increase of 1 hp. is associated with a 0.035 square foot decrease in the footprint. Similarly, efficiency has a negative, statistically significant relationship with footprint, as expected. A 1 MPG increase in efficiency, is associated with a 0.693 decrease in footprint. Surprisingly, torque is found to have a positive and statistically significant relationship with footprint, so that a 1 ft. lb. increase in torque is associated with a 0.037 square foot increase in footprint. Although it was expected that torque would be negatively related with footprint—since both are tradeoffs a firm could make to meet standards—more torque is required for a larger car to experience the same acceleration as a smaller car. This fact likely explains the positive relationship between torque and footprint.

The results from the simple DD regression using the 2011 model as the treatment group and the bounds outlined in the empirical methodology are not statistically significant. The share-weighted DD regression also yields positive and statistically insignificant results. The share-dosed treatment effect is 4.81, which suggests—considering the independent share variables—that a 10% increase in market share results in a 0.442 square foot increase in the footprint of a model. It is interesting to note that share has a negative and statistically insignificant effect on the footprint of models not in the treatment group after the policy. All other control variables from the 2009 model remain similar in magnitude and significance as they were in the simple DD regression.

The results from the distance below the 2011 standard as the dose of the treatment show conflicting results. The distance falling short treatment effect has a statistically significant negative relationship with the footprint of a model, so that a 1 MPG increase in the gap of the 2009 efficiency below the 2011 standard decreases the footprint of a model by 0.39 square foot, on average. In this model, market share has a positive and statistically insignificant effect, and all control variables have coefficients with similar signs, magnitudes and significances as the simple DD regression.

The negative result of the treatment suggests that the further a model's efficiency in 2009 from the 2011 standard, the more the footprint of the model will decrease. This was not the expected outcome, but has several potential explanations. The first is that this treatment is not the correct one to capture the effect of footprint-based CAFE standards on the footprint; perhaps firms do not respond to the incentive to increase footprint based on the distance a model is from the standard. They may react on the average and not care about the distance a single model is from the standard. Since standards are enforced and firms are fined at the fleet level, perhaps this dose effect is not the one which would capture the incentive to increase footprint.

Another thing to consider is that decreasing size does increase efficiency. Firms may gain more efficiency by decreasing the size than the standard is tightened in response to the footprint decrease. In this case, it may make sense for them to decrease the footprint to meet standards. While this is true, the footprint-based standards should still decrease the incentive to meet standards by decreasing the footprint. One way to test this assumption would be to compare the effect of homogeneous CAFE standards with the effect of footprint-based standards. It may still be that firms reduce footprint size in response to footprint-based standards, but less so than in response to homogeneous standards. This is an avenue to be explored in future studies.

While the simple and the share-dosed DD regressions showed positive and statistically insignificant results of the policy on the treatment group for the footprint of the 2011 model years, a graph of the residuals of the regressions on the footprint suggest the potential for a systematic relationship between the error terms and the independent variable. Using robust standard errors increases the significant slightly, but the treatment effect remains insignificant. Robust standard errors are reported in all result tables to correct for any heteroskedasticity. Future analysis could also considered clustered standard errors based on the manufacturer.

It was noted that the effects of the 2011 policy may not have been fully observed on the 2012 model. For this reason, a model considering the 2012 model as the treatment, and one considering the 2011 and 2012 models pooled as the treatment are considered. The model using the 2012 model year (Table 3) shows a slightly larger treatment effect of 0.772, suggesting that the policy increases the average footprint of the treatment group by 0.774 square foot. Although this result is slightly larger, it remains statistically insignificant. Share independently also has a slightly larger, but statistically significant positive effect. All other control variables retain similar coefficients and significance levels.

The share-dosed DD estimation also has a larger estimated treatment effect, which is now marginally statistically significant. In this regression, a 10% increase in market share results in a 0.544 square foot increase in the footprint the treated models. All other variables remain similar in magnitude and significance as in the 2011 control share-dosed DD estimation. The distance falling short dose-treatment shows results similar in signs and magnitude as the 2011 control, distance falling short dose-treatment estimation.

The 2012 control group estimations show results that are similar to the 2011 control group estimations. The major difference is both the simple and the share-dosed treatment effects have slightly larger magnitudes, and the share-dosed treatment effect becomes marginally statistically significant. This may suggest that some of the effects of the policy may not be seen in the 2011 model. It may be that the results are not effective until the 2012 model year. While this is a likely explanation, it is also important to note that 2012 model years had their own footprint-based standards which were based off of a bounded linear function. These standards should be considered in any detailed analysis of the 2012 model year footprints in response to the footprint-based policies.

Estimations from the DD regressions using 2011 and 2012 models as a pooled control group show largely similar results in the simple DD estimation and the distance falling short dosed-treatment estimation. However, as Figure 4 shows, the results for the pooled control group, share-dose estimation are slightly larger and statistically significant. In this estimation, a 10% increase in market share is associated with a 0.511 square foot increase in the footprint of treated models relative to untreated models. One possible explanation for this is the increase in observations by pooling the two years as the control group. This mere increase in sample size may be the driver in the increase in efficiency of the estimations. While the increase in sample size may be driving the increase in significance, adding a year dummy variable may decrease the new-found level of significance. For this reason, without further analysis, these results should not be considered final.

One final robustness check was considered. Estimations using all model year after groups and DD treatments were run using discrete boundaries for the control and treatment groups. In these estimations, models with footprints over 50 and under 40 were in the control group, and models with footprints between 40 and 50 were in the treatment group. Tables (5-7) report the results from these estimations. The magnitude of the simple DD treatment effect increased under all definitions of after groups, but remained statistically insignificant. The share-dosed treatment effect remained similar in magnitude, but gained marginal statistical significance for the 2011 and 2012 single-year after groups. Again, the increase in observations may drive the increase in statistical significance. The coefficients were only marginally insignificant in the single-year after periods with the more distinct control and treatment group. The increase of 28 observations could have significantly decreased the standard errors.

The treatment effect for the pooled after years remain similar in magnitude, but actually decreased in statistical significance. This result is not unexpected. The logistic curve has a gradual decrease in steepness in the area included under the discrete control and treatment groups. In these parts of the curve, it is expected that the treatment effect should be smaller. Including some areas with smaller treatment effects unrelated to market share will increase the variance of this treatment effect, and the standard errors. This should decrease the significance of the estimated coefficients. While this is true for the pooled after year estimations, it is not for the single year estimations. A potential explanation for the increase in significance of the single-year estimations is due to an increase in the number of observations, but the pooled-year after groups also experience an increase in sample size. In this case, it is important to note that increases in sample size have diminishing increases in the efficiency of estimations, and that the pooled model already contains more observations. It may be that the increase in variance offsets any gain in the increase of efficiency due to the added observations. While this may be a potential explanation of the surprising shifts in statistical significance, more analysis should be performed before this explanation is taken to be sufficient.

As can be seen, the positive results for the policy on the footprint of models do not vary much in magnitude depending on which control and after groups are chosen. What does vary is the statistical significance of these estimates. Under all specifications, the simple DD methodology shows a positive and statistically insignificant treatment effect. When the treatment is dosed more heavily for models with larger market shares, the effects of the treatment on footprint remain positive, but are only marginally significant in the pooled after year of the bounded control and treatment specification and in all of the discrete control and treatment group specifications. In all of these specifications, there is an increase in the number of observations in the analysis which could be driving the increase in significance. For the DD estimation which allows treatment dose to increase with the distance a 2009 model falls short of the 2011 standard, the effects are negative and statistically significant under all specifications. There are several explanations for the negative results; it may be that this is the wrong treatment specification—manufacturers may gain more in efficiency than they lighten the standard by decreasing footprints for these models. Also, they may be only reacting to the incentive to decrease standards for models which make up a significant market share. In any case, it is reasonable to think that manufacturers may not care how far an individual model is from the target efficiency, and may only try to lighten the target for those models with larger market shares which will significantly decrease the overall standard they face.

VI. **Conclusions**

Theoretical models have shown that car manufacturers have a new incentive under footprint-based standards to increase the size of vehicles to lighten the standards they face. This methodology looks at models that are sold continuously throughout 2009-2012 and uses DD methodology to test whether manufacturers increase the footprint of cars which fall within a footprint range that allows a small increase in footprint to result in a larger decrease in model target relative to models that fall within a footprint range with smaller decreases in model target with the same increase in footprint size. Under all specifications, a simple DD methodology shows a positive and statistically insignificant treatment effect. A DD methodology which allows treatment to increase with market share shows marginally statistically significant treatment effects of around 0.5 square foot under specifications which allow for more observations. DD methodology which allows the dose to increase with the distance the 2009 is below the 2011 standard, shows statistically significant decreases of around 0.4 square foot under all specifications. It may be that manufacturers have more to gain in efficiency on models farther below the standard than they would by lightening the standard. A comparison of this effect under footprint-based standards with homogeneous standards could show whether this treatment effect declines under the footprint-based policy.

While some results do not support the theoretical conclusions that manufacturers increase footprint size in response to footprint-based policies, there is some statistically significant positive evidence from the share-dosed specifications that they do. This treatment dose is likely to be the ones that model footprint sizes would react to because changing footprint takes some investment. This investment will pay off more for vehicles that make up a larger share, since the total fleet standard is determined by the average model target, and models with larger shares will have greater pull in affecting the total standard for the fleet. It is also important to note that this methodology only detects changes within model footprints. Firms can also add and drop models from fleets and shift the composition of their fleets towards larger cars; this methodology does not detect shifts towards larger cars through these means. Further analysis of these potential ways that footprint-based policies might distort footprint sizes will be studied in future studies. Due to these limitations to this analysis, the estimates considering within model variation *only* are likely to be underestimates of the total distortion of fleet footprint under footprint-based policies.

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Tables and Figures

Figure 1: The Efficiency Target by Footprint for the 2012 Model Year

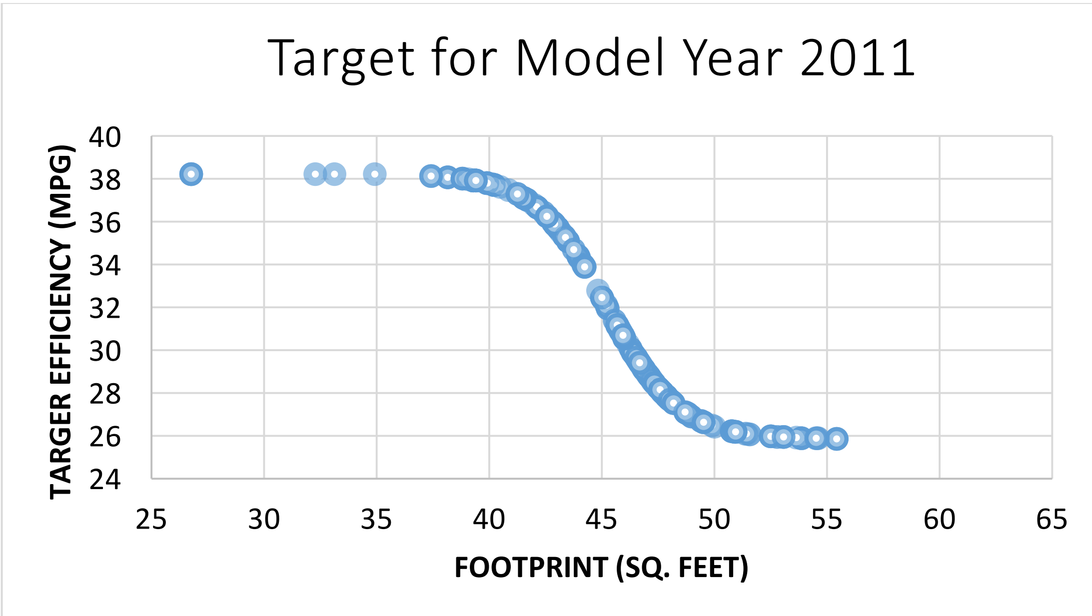

Figure 2: The Distance below the 2011 Standard by the Footprint

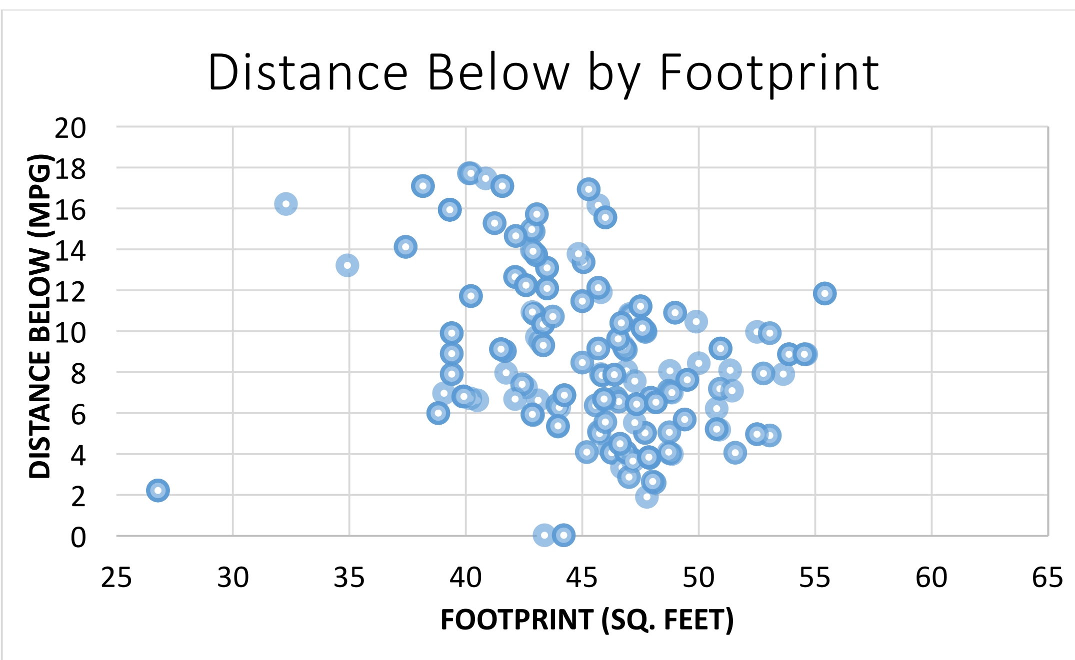

Table 1: Summary Statistics and Description of Variables

Variable	Description	Mean	Std. Dev.	Min.	Max.
Footprint	Footprint in square feet	45.28	4.53	26.80 (Smart ForTwo)	55.45 (Lincoln Navigator)
Share	Model share of manufacturer fleet	.15	.16	.0029 (Volkswagen R8)	.68 (Mazda3)
Std	The standard set for 2011 model	31.96	4.40	25.82 (Lincoln Navigator)	38.20 (Smart ForTwo)
Below	Distance of 2009 model below the standard	9.02	4.19	0 (Toyota Prius)	17.7 (SLK Chrysler)
MPG	Efficiency of 2009 model	23.08	5.03	14 (Lincoln Navigator)	46 (Toyota Prius)
HP	Horsepower	221.22	97.01	70 (Smart ForTwo)	600 (Dodge Charger)
Torque	Torque in ft. lb.	216.13	94.35	68 (Smart ForTwo)	560 (Dodge Charger)
After	=0, if 2009 model =1, otherwise	.666		0	1
Treat	=0, if in control group =1, if in treatment group	.727		0	1
*Treat*After*	=1, if after=1 and treat=1 =0, otherwise	.483		0	1

Table 2: The Difference-in-Difference of the Mean Footprint by Group

	Treatment	Control	Difference in Difference
Before	45.07	45.32	
After	45.67	45.33	
Difference	0.6	0.1	0.5

Table 3: Results from 2011 with deleted observations

Dose	No Dose	No Dose	Share	Distance below
*Treat*Dose* After*	.599 (1.93)	.599 (1.54)	4.83 (3.67)	.071 (.081)
Treat	-.179 (1.36)	.657 (1.10)	.670 (.815)	-.826 (.656)
After	.009 (1.73)	.009 (1.39)	-.172 (.791)	-.461 (.720)
MPG	N/A	-.386** (.154)	-.383*** (.087)	-.741*** (.070)
Horsepower	N/A	-.028** (.012)	-.027*** (.010)	-.009 (.007)
Torque	N/A	.037*** (.012)	.037*** (.009)	.015* (.008)
Share	N/A	N/A	-1.80 (2.55)	N/A
Below	N/A	N/A	N/A	-.753*** (.071)
R-squared	.004	.375	.383	.691

Note: 134 observations

OLS regressions with robust standard errors

****   Signifies a 1% significance level***

*****   Signifies a 5% significance level***

****** Signifies a 10% significance level***

Models with footprints between 39 and 41 square feet and 49 and 51 square feet deleted

Table 4: Results from 2012 with deleted observations

Dose	No Dose	No Dose	Share	Distance below
*Treat*Dose* After*	.765 (1.94)	.771 (1.54)	5.64* (3.29)	.048 (.081)
Treat	-.179 (1.38)	.660 (1.10)	.700 (.810)	-.787 (.642)
After	.009 (1.74)	.009 (1.38)	-.133 (.787)	-.397 (.703)
MPG	N/A	-.395** (.156)	-.394** (.156)	-.750*** (.069)
Horsepower	N/A	-.029** (.009)	-.028** (.011)	-.009 (.007)
Torque	N/A	.037*** (.009)	.037*** (.011)	.015* (.008)
Share	N/A	N/A	-1.75 (2.53)	N/A
Below	N/A	N/A	N/A	-.766*** (.069)
R-squared	.007	.385	.395	.707

Note: 134 observations

OLS regressions with robust standard errors

****   Signifies a 1% significance level***

*****   Signifies a 5% significance level***

****** Signifies a 10% significance level***

Models with footprints between 39 and 41 square feet and 49 and 51 square feet deleted

Table 5: Results from Pooled 2011 and 2012 with deleted observations

Dose	No Dose	No Dose	Share	Distance below
*Treat*Dose* After*	.682 (1.67)	.684 (1.33)	6.62** (2.95)	.073 (.066)
Treat	-.179 (1.36)	.677 (1.09)	.596 (.672)	-.862 (.588)
After	-.058 (1.55)	-.059 (1.23)	-.418 (.725)	-.489 (.638)
Year12	.134 (.766)	.137 (.607)	.148 (.602)	-.081 (.427)
MPG	N/A	-.386*** (.087)	-.373*** (.702)	-.738*** (.057)
Horsepower	N/A	-.028*** (.010)	-.024*** (.008)	-.008 (.006)
Torque	N/A	.037*** (.009)	.035*** (.008)	.015** (.006)
Share	N/A	N/A	-2.96 (2.30)	N/A
Below	N/A	N/A	N/A	-.777*** (.061)
R-squared	.005	.384	.399	.698

Note: 202 observations

OLS regressions with robust standard errors

**** Signifies a 1% significance level***

***** Signifies a 5% significance level***

****** Signifies a 10% significance level***

Models with footprints between 39 and 41 square feet and 49 and 51 square feet deleted.

Table 6: Results from 2011 including all observations

Dose	No Dose	No Dose	Share	Distance below
*Treat*Dose* After*	.851 (1.60)	.851 (1.24)	5.27* (3.18)	-.010 (.065)
Treat	.715 (1.13)	.669 (.880)	.747 (.656)	.399 (.507)
After	-.294 (1.37)	-.294 (1.06)	-.325 (.676)	.090 (.552)
MPG	N/A	-.386*** (.083)	-.385*** (.083)	-.742*** (.060)
Horsepower	N/A	-.029*** (.009)	-.028*** (.009)	-.009 (.006)
Torque	N/A	.041*** (.009)	.040*** (.009)	.016*** (.006)
Share	N/A	N/A	-1.35 (2.24)	N/A
Below	N/A	N/A	N/A	-.748*** (.056)
R-Squared	.016	.421	.430	.754

Note: 162 observations

OLS regressions with robust standard errors

** Signifies a 1% significance level*

*** Signifies a 5% significance level*

**** Signifies a 10% significance level*

Table 7: Results from 2012 including all observations

Dose	No Dose	No Dose	Share	Distance below
*Treat*Dose* After*	.817 (1.61)	.818 (1.24)	5.49* (3.21)	-.037 (.065)
Treat	.715 (1.13)	.665 (.880)	.713 (.655)	.383 (.494)
After	-.056 (1.37)	-.056 (1.06)	-.128 (.675)	.236 (.539)
MPG	N/A	-.391*** (.083)	-.391*** (.083)	-.747*** (.058)
Horsepower	N/A	-.029*** (.009)	-.029*** (.009)	-.009 (.006)
Torque	N/A	.040*** (.009)	.040*** (.009)	.016*** (.006)
Share	N/A	N/A	-1.33 (2.24)	N/A
Below	N/A	N/A	N/A	-.748*** (.056)
R-squared	.017	.424	.434	.766

Note: 162 Observations

OLS regressions with robust standard errors

****   Signifies a 1% significance level***

*****   Signifies a 5% significance level***

****** Signifies a 10% significance level***

Table 8: Results from Pooled 2011 and 2012 including all observations

Dose	No Dose	No Dose	Share	Distance below
*Treat*Dose* After*	.834 (1.38)	.834 (1.07)	6.84*** (2.60)	.059 (.052)
Treat	.715 (1.13)	.687 (.873)	.678 (.543)	-.634 (.460)
After	-.282 (1.24)	-.282 (.951)	-.518 (.629)	-.370 (.491)
Year12	.213 (.713)	.213 (.550)	.223 (.543)	-.006 (.357)
MPG	N/A	-.369*** (.068)	-.366*** (.067)	-.754*** (.050)
Horsepower	N/A	-.027*** (.007)	-.025*** (.007)	-.008* (.005)
Torque	N/A	.040*** (.007)	.038*** (.007)	.015*** (.005)
Share	N/A	N/A	-2.77 (2.05)	N/A
Below	N/A	N/A	N/A	-.800*** (.050)
R-squared	.020	.425	.441	.760

Note: 244 Observations

OLS regressions with robust standard errors

****   Signifies a 1% significance level***

*****   Signifies a 5% significance level***

****** Signifies a 10% significance level***