Using Matching Estimators to Evaluate the Effect of Unit-Based Pricing on Municipal Solid Waste Disposal: A Case Study of New Hampshire Towns

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Abstract

The provision of municipal services for the collection, transfer, and disposal of household solid waste is often provided by local governmental units; typically at the town or city level. Unit-based pricing, also known as variable-rate-pricing or pay-as-you-throw (PAYT), is a municipal solid waste pricing schedule which requires households to pay a fee per unit of trash disposed. Unit-based pricing represents a significant departure from the common practice of financing solid waste services from property tax. When MSW services are financed solely from property tax revenues the marginal cost to a household for disposing additional units of solid waste is effectively zero. Local governments are motivated to adopt unit-based pricing for the purpose of creating a financial incentive for households to reduce the quantity of solid waste disposed and concurrently increasing the level of recycled materials. The objective of this study is to evaluate the impact of MSW user fees on the level of MWS disposal. A counter-factual model with matching estimation is used to estimate the program effect. The study area is the 234 incorporated towns and cities in the state of New Hampshire. As of 2008, 40 towns had adopted a form of unit-based pricing of household solid waste. Results from propensity score matching suggest the average annual reduction in MSW per household ranges from 823lbs. to 631lbs. for households residing in communities using a form of MSW user fees. This represents a reduction of 53% to 41% from the average of 1530lbs. per household in towns without MSW user fees. Based upon Rosenbaum bounds sensitivity test the results are robust to potential hidden bias from unobserved variables.

Keywords: *Propensity score matching, per unit-based pricing, pay-as-you-throw, municipal solid waste*

Track:

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1. Introduction

Municipal solid waste (MSW) is the combined solid waste discarded by households, businesses, and retailers. Solid waste can be categorized by type of material (paper, food scraps, plastics, metals, glass, wood, rubber, leather, textiles, other) or by major product category such as durable goods (household furniture and appliances, electronic products, cars and car tires) and nondurable goods (packaging materials, food waste, newspapers, junk mail, and beverage and food containers). Municipal solid waste does not include wastes generated by industrial, construction, or demolition activities. Hazardous wastes such as florescent light bulbs and other products containing mercury, liquid wastes such as motor oil, and all hazardous medical and radioactive materials are excluded from the tabulation of MSW.

The words refuse, trash, garbage, and rubbish are commonly used interchangeably to denote households' contribution to the generation of municipal solid waste (MSW). Porter (2002) describes waste as "the stuff we don't want − hence we are willing to pay to get rid of it". Estimates of total annual MSW generated in the United States in 2008 range from 250 million to 389 million tons (USEPA 2009, BioCycle 2010). Household waste generation accounts for two-thirds of this total (USEPA 2008, Porter 2002).

The collection and disposal of municipal solid waste (MSW) is becoming a source of fiscal stress for many cities and towns in the United States. From 1992 to 2008 local government expenditures for MSW services increased nearly 25% in real terms from $57 to $71 per person.[1] This increase in expenditures is attributed to higher levels of per capita waste disposal, increasing populations, and increasing costs to collect, transport, and dispose solid waste. For many communities, solid waste is being transported longer distances to larger regional landfills and incinerators as local and smaller landfills reach capacity or open town dumps which failed to meet environmental standards are closed.

Although the type and level of services vary by municipality, for most public sector service providers, MSW management costs are accounting for an increasing share of local government expenditures. Motivated to reduce the level of solid waste and increase the level of recycled materials diverted from solid waste, local governments are evaluating alternative solid waste management programs and pricing schedules.

An alternative MSW pricing schedule increasing in frequency of use by cities and towns is MSW user fees. MSW user fees are also referred to as variable-rate-pricing or pay-as-you-throw (PAYT) because households pay for each unit of trash disposed. The unit of measurement may be based on either the weight or volume of waste. One popular form of MSW user fee requires households receiving MSW services to purchase and use specially designated plastic garbage bags. The bags are often designated by color or imprinted with a town logo. Another approach is to require residents to purchase designated adhesive stickers to place on commercially sold trash bags.

Unit-based pricing represents a significant departure from the conventional practice of financing solid waste collection using property tax revenues. When waste disposal services are financed from general tax revenues, households incur no direct cost

[1] Calculated using the U.S. Bureau of Census State & Local Government Finance historical data and converted to real value using Bureau of Economic Analysis GDP deflator for 2005 = 100.

for MSW services. The marginal cost for each additional unit of trash disposed is zero. If household are required to use designated trash bags for waste disposal, the marginal cost is the sale price of the trash bag. Prices vary across municipalities and typically range from $1 for a 15 gallon trash bag to $2 for a 30 gallon trash bag with a 30 pound weight limit.

Unit-based pricing is an example where the economist's toolbox of microeconomic theory, economic analysis, and program evaluation can contribute to solid waste management issues (Halstead and Park, 1996). The underlying microeconomic theory associated with unit-based pricing is households respond to economic incentives. Unit-based pricing increases household cost to dispose solid waste and concurrently decreases the opportunity cost of recycling. The total decrease in quantity demanded of MSW services due to a price increase can be partitioned into an income effect and substitution effect.

The income effect results from a decrease in real income due to a price increase, and the substitution effect occurs as consumers substitute away from MSW disposal services and use other means whose relative price has decreased to dispose trash. Examples of practices households substitute for MSW disposal services include reduction of waste generation source such as purchasing products with less packaging and adopting "green" consumption practices, and separate recyclable materials from the solid waste stream, and composting food waste. Other means to reduce use of MSW services is to dispose of trash illegally or to take household waste to a neighboring community which does not use MSW user fees.

Switching from MSW services solely financed from property taxes to a MSW program with user fees will likely be resisted by household who are accustomed to "no-cost" trash disposal service. Unit-based pricing internalizes the cost of waste disposal to the waste generator. An additional benefit associated with unit-based pricing is the issue of equity in financing waste disposal services. When waste disposal is financed with property tax revenues, disposal costs for households with high levels of trash are partially subsidized by households disposing low levels of trash. With unit-based pricing, households who generate less waste will pay less than households who generate more waste. Thus, unit-based pricing may appeal to households who actively practice recycling.

Over the past twenty years a growing number of municipalities are implementing unit-based pricing of household solid waste. The United States Environmental Protection Agency estimates the number of communities using unit-based pricing has increased from 4000 in 1995 to an estimated 7000 in 2006. Approximately 25% of the nations' population disposes their trash using some form of a pay-as-you-throw solid waste management program (Skumatz and Freeman, 2006). When evaluating the benefits and costs of adopting MSW user fees, MSW policy makers, program managers, and citizens want to know what effect the program will have on household waste disposal. The research objective of this paper addresses this question.

2. Economic Literature of Municipal Solid Waste

Beginning in the 1970s, an extensive literature in economics has developed on solid waste management, recycling, and the effect of alternative policies and programs on waste disposal. An initial report by the United States Environmental Protection Agency (USEPA 1979) provides empirical results from five selected cities suggesting the total quantity of waste generated by households may not be sensitive to price. However, the authors attribute this result primarily due to the inadequacy of data. The results find a positive association between household income and solid waste.

In a review of empirical results from studies conducted to evaluate MSW user fees, most studies find an increase in recycling participation. The results regarding the effect of unit-based pricing on waste reduction are mixed (Miranda, Bauer, and Aldy, 1996). Selective findings suggest a portion of the observed reduction in waste disposal levels may be due to increased compaction of garbage, and/or illegal disposal (Fullerton and Kinnaman, 1993). Miranda (1996) characterized the uncertainty over the efficacy of MSW user fees as the most controversial question regarding unit pricing is whether or not it leads to decreases in total waste generation.

Kinnaman (2003) provides a collection of theoretical and empirical papers on residential solid waste management. Reviews of the MSW economics literature are presented in Miranda, Bauer, and Aldy (1996), Choe and Fraser (1998), and the Organization for Economic Co-operation and Development (2008).

The OECD reviews the environmental economics literature on environmental policy designed to alter human behavior. Seventeen empirical studies of household solid waste disposal are examined during the period 1993 to 2003. Five of the studies use community-level data, eight use household level data, and the remaining four use either aggregated data or simulation. All the studies use a form of regression analysis for conducting program evaluation. Based upon their review of the MSW economics literature, the OECD concludes that MSW user fees are effective at reducing waste and increasing recycling, and further more finds litter empirical evidence that unit pricing does not yield the benefits stipulated on economic theoretical grounds (OECD 2008).

The effect of pricing on household behavior varies depending on type of pricing schedule (subscription fees, block fees, unit-based fee), and research results suggest a mix of different outcomes on recycling, waste generation, and composting. In a discussion of future MSW empirical research Kinnaman (2009) characterizes the recent empirical findings as suggestive that household responsiveness to unit-based pricing is smaller than previously estimated, and he encourages additional research to understand the robustness of current empirical results.

The predominate econometric method used to estimate the effect of MSW user fees on levels of solid waste disposal has been some form of linear regression. Two MSW issues challenge the assumptions of regression model to estimate program effects 1) policy endogeneity and 2) heterogenous treatment effects.

Recognizing that policy adoption of MSW user fees may be motivated by different factors, such as the level of MSW generated or the level of community support for conservation programs, selected empirical studies have tested for endogeneity and/or employed alternative specification of regression models, such as two-stage least squares (2SLS) for the purposing of controlling for policy endogeneity.

In a regression model to explain the residential waste disposal with a volume based user fee as an explanatory variable Jenkins (1993) may represent the first paper to test for endogeneity of the price variable. Using a Hausman test, the null hypothesis that the price variable is predetermined could not be rejected. One explanation for a weak causal link from level of solid waste to MSW user fees is municipalities are less responsive to setting price based upon demand for services, and are more sensitive to changes in collection and disposal costs. Jenkins did not report the 2SLS results.

Kinnaman and Fullerton (2000) compiled a cross-sectional data of 959 towns located in six states; 148 of the towns used some form of MSW user fees. The two variables controlled for possible endogeneity are presence of curbside recycling collection and MSW user fee. After substituting the predicted values of MSW user fees from the Tobit model estimation into the OLS model to controlling for endogeneity, the coefficient for MSW user fee increases 50% from a reduction of 247 pounds per person to 373 pounds per person. However, based upon a Hausman test, the null hypothesis of no correlation between the price variable and the error term could not be rejected. This outcome of this hypothesis test is similar to the result reported by Jenkins (1993). The authors note that the study uses a dataset with the largest number of observations with MSW user fees studied to date. The coefficient of determination value of 0.09 indicates the regression model explain a relatively small fraction of the total variation in waste disposal compared to an R^2 of 0.30 for the model used to estimate the level of recycling.

Huang, Halstead, and Saunders (2010) compare treatment effects of MSW user fees using ordinary least squares (OLS) with no correction for policy endogeneity and 2SLS to correct for policy endogeneity. To model policy endogeneity, a Probit model is use to estimated predicted values of policy adoption. The predicted values are used in a OLS model to estimate policy impact on MSW level. Results of 2SLS suggest use of MSW user fees is associated with a 70 percent reduction in per capita MSW disposal. This value is 65% higher than the OLS estimated coefficient of 43% and suggests that the standard analytical methods used to evaluate MSW user fees without modeling policy endogeneity may underestimate the program effect.

Dijkgraaf and Gradus (2004, 2009) control for self selection of policy adoption by controlling to environmental activism at the municipal level. If municipalities which implement MSW user fees also are more supportive of conservation and recycling practice relative to municipalities without MSW user fees, the effect of MSW user fees may be confounded with community support for environmental activism. When controlling for environmental activism results in a 31% reduction in MSW using a bag-based pricing schedule. Environmental activism accounts for a 6% reduction in MSW disposal. Without modeling for differences in municipal environmental activism, standard linear regression methods may overestimate the program effect.

The standard analytical approach to modeling the effect of MSW user fees is to use a form of linear regression with the level of MSW as the dependent variable and an indicator variable for presence or absence of policy adoption. Huang, Halstead, and

Saunders (2010) use the natural log transformation of MSW as the dependent variable based upon results from a Box-Cox test for functional form.

Recent empirical studies have recognized that policy adoption is not exogenous. Economic theory suggests municipalities will be motivated to implement MSW user fees if net benefits are positive. There are likely to be differences between municipalities which adopt and do not adopt MSW user fees which must be controlled for to isolate the effect of MSW. Municipalities self-select to implement MSW user fees. This self selection process suggests that the benefits of MSW user fees may differ for municipalities which choose to adopt MSW user fees and municipalities which do not implement MSW user fees. The occurrence of self-selection suggests the effect of MSW user fees is heterogeneous across municipalities.

Linear regression models assume the effect of MSW user fees is the same for program participants and nonparticipants. To model for heterogeneous policy effects an alternative estimation method is necessary. Matching estimation methods can be used as an alternative to linear regression when policy effects are heterogeneous.

The occurrence of self-selection to adopt MSW user fees is partially attributed to the legislative provisions of the federal Resource Conservation and Recovery Act of 1976 (RCRA). Although RCRA establishes federal oversight of solid waste management, primacy is delegated to the states to enforce regulations prohibiting the then common practice of disposing solid waste in open municipal dumps. States are responsible for enforcement of pollution abatement controls to reduce air and groundwater contamination from sanitary landfill and MSW incinerators.

Under RCRA, local governmental units are responsible for collection and disposal of solid waste. The type and frequency of MSW collection and disposal services varies across local governmental units. Municipalities self-select whether to implement a user fee for household solid waste. Given the lead role and flexibility of local governments in managing municipal solid waste, policy efforts to reduce MSW disposal are asymmetric across the nation (Callan and Thomas 2006).

For MSW policies in which municipalities self select to implement MSW user fees, the benefit of MSW user fees will likely differ across municipalities with and without MSW user fees. Matching estimation is an alternative to linear regression and is used when program effects are assumed to be heterogeneous across participants.
In the economics literature matching was initially used to estimate the impact of job training programs (Heckman, Ichimura, and Todd 1997, Dehejia and Wahba 1998, Smith and Todd 2005). Selected examples of empirical studies from the environmental and resource economics literature include evaluation of the Clean Air Act (List 2004, Greenstone 2004), Endangered Species Act (Ferraro, McIntosh, and Ospina 2007), open space and agricultural land protection programs (Towe 2010, Lynch, Gray, and Geoghegan 2007, Liu and Lynch 2011), and agricultural research, farm programs, and forest management (Jumbe and Angelsen 2006, Pufahl and Weiss 2008, Liebenehm, Affognon, and Waibel 2009). Matching methods have been extensively refined in the recent evaluation literature and are now a valuable part of the evaluation toolbox (Blundell and Dias 2002).

3. Theoretical Framework of the Counterfactual Model

This proposed study evaluates the impact of unit-based pricing on the level of municipal solid waste disposal. The fundamental problem associated with estimating the impact of a program on an outcome can be characterized as one of a missing data problem (Holland 1986). In the context of this study, evaluating the impact of MSW user fees on a town's generation of MSW requires knowing two concurrent potential outcome variables; 1) the level of MSW generation without MSW user fee and 2) the level of MSW generation with MSW user fee.

Only one of these potential outcomes can observed at any one time period. The missing observation is referred to as the counterfactual. Constructing a value for the counterfactual serves as the underlying motivation for using a matching estimation method to conduct program evaluation.

The purpose of program evaluation is to measure the causal effect of program participation on an outcome variable. The units of observations can be individuals, households, markets, firms, governmental units, states or countries. In the evaluation literature participation or exposure to the program is referred to as a treatment. Treatments can be widely interpreted such as job training or educational program, laws, environmental regulations, or new technologies. In the context of this study, cities and towns are the observational units, treatment is enforcement of MSW user fees, and the outcome variable used to evaluate the treatment effect is the change in quantity of MSW generation.

The mathematical model of causal inference based on the counterfactual account of a casual relation was pioneered by Fisher (1935) for randomized treatment assignment and extended by Rubin (1978) to nonexperimental or observational data with nonrandom treatment assignment. Over the past twenty years, the counterfactual model has become a received estimation model in the statistics and econometrics literature. Two advantages of the counterfactual model are: 1. its use for estimating treatments effects when effects are hypothesized to be heterogeneous across groups and, 2. the estimators can be defined without specifying a particular form of the statistical model (Imbens and Wooldridge 2009).

A standard conceptual framework and notation for the counterfactual model has developed over the past 20 years (Heckman, Ichimura, and Todd 1997, Blundell and Dias 2002, Wooldridge 2002, Imbens 2004, Rosenbaum 1995, Rubin 2006).The following description follows (Caliendo and Kopeinig 2005, Morgan and Harding 2006, Guo and Fraser 2010).

Adoption of MSW user fee is the treatment. The notation used for the counterfactual model in the context of this study is:

Let i index the towns in the study area, with i = 1, 2, 3…N

Yi = mean annual town MSW per house measured in pounds.

Di = (0, 1) is an indicator variable of the treatment received by unit i

Di = 0 if town i does not have MSW user fee (non-participant)

Di = 1 if town i uses MSW user fee (participant)

The evaluation problem can be represented as:

Y_{i0} = outcome of town i if non-participant (i.e. no MSW user fee).

Y_{i1} = outcome of town i if participant (i.e. with MSW user fee).

The treatment effect for unit i is defined as the difference between two theoretical random outcomes with and without treatment:

$$\tau_i = Yi_1 - Y_{i0} \tag{1}$$

The actual outcome, Y_i , is expressed by the counterfactual model as:

$$Y_i = DY_{i1} + (1 - D)Y_{i0} \tag{2}$$

Where, Y_i is the actual observed outcome of unit i. Only one of the two potential outcomes Y_{i1} or Y_{i0} is observed; the missing variable is the counterfactual which must be estimated. Because only one potential outcome is observed per unit, the counterfactual model is estimated using the average outcome of the units in the treatment group, and the average outcome of the units in the nontreatment group. These averages can be expressed as:

Population mean of control group: $E[Y_0| D=0]$

Population mean of treatment group: $E[Y_1 | D=1]$

The unconditional average treatment effect (ATE) of the population is:

$$ATE \equiv E[\tau] = E[Y_1 | D=1] - E[Y_0 | D=0] \tag{3}$$

The average treatment effect is the outcome if assignment to treatment is random and is an estimate of the effect if a unit is randomly drawn from the population which consists of both treated and nontreated units. There are two missing counterfactuals associated with the ATE. One is the outcome of treated units if they had not received treatment denoted as $E[Y_0| D=1]$, and the other is the nonparticipant's outcome if they had received treatment denoted as $E[Y_1 | D = 0]$. The process of random treatment assignment results in equality between the observed outcome and missing counterfactual $E[Y_0 | D=0] = E[Y_0 | D = 1]$ and the observed outcome and missing counterfactual $E[Y_1 | D=1] =E[Y_1 | D=0]$ and the population parameter ATE can be estimated as a difference in sample means for the treatment and nontreatment groups.

When assignment to treatment is nonrandom $E[Y_0 | D=0] \neq E[Y_0 | D = 1]$and $E[Y_1 | D=1] \neq E[Y_1 | D=0]$ and the two counterfactuals must be constructed.

Another treatment effects estimator for policy evaluation is the average treatment effect on the treated (ATET). The ATET estimator is:

$$ATET \equiv E[\tau | D=1] = E[Y_{1i} | D=1] - E[Y_{0i}| D=1] \qquad (4)$$

Estimation of this population parameter requires weaker assumptions than the ATE because only one counterfactual, $E[Y_{0i}| D=1]$, must be constructed. For this study, ATET is an estimate of the benefit to towns with MSW user fees compared to what they would have experienced had they not implemented MSW user fees. Heckman (1997) describes this estimator as the gross gain to units who choose to participate in the programs.

For policy consideration of extending the treatment program to nonparticipants the relevant treatments effects estimator is the average treatment effect on the untreated (ATEU). The ATEU estimator is:

$$ATEU \equiv E[\tau | D=0] = E[Y_{1i} | D=0] - E[Y_{0i}| D=0] \qquad (5)$$

The missing data problem of the counterfactual model is that only Yi_1 or Y_{i0} is observed for each town but not both; one cannot observe the no-program effect for towns that adopt unit based pricing, and one cannot observe the program effect for towns that do not adopt unit based pricing. $E[Y_{1i} | D=1]$ and $E[Y_{0i} | D=0]$ is observed, $E[Y_{i0} | D= 1]$ and $E[Y_{1i} |D=0]$ is not.

When applied to the ATET, if $E[Y_{0i} | D=0]$ is substituted for $E[Y_{0i} | D = 1]$, the estimator is:

$$E[Y_{1i} | D=1] - E[Y_{0i}| D=0] = ATET + E[Y_{0i}| D=1] – E[Y_{0i} | D=0] \quad (6)$$

A condition for the estimator to be unbiased is:

$$E(Y_{i0} | D = 1) - E(Y_{i0} | D=0). = 0 \quad (7)$$

As a thought experiment, if towns were randomly assigned to implement MSW user fees, then the outcome variables, Y_{1i} and Y_{0i} , are independent of assignment to treatment, W_i. This is denoted as:

$$Y_{i1} , Y_{i0} \perp\!\!\!\perp W_i$$

$$\text{Implies } E(Y_{i0} | D_i = 0) = E(Y_{i0} | D_i = 1) = E(Y_i | D_i = 0)$$

$$\text{ATET bias} \equiv E(Y_{i0} | D = 1) - E(Y_{i0} | D=0) = 0 \quad (8)$$

In randomized experiments, use of the observed outcome for non-participants, $E(Y_{i0} | D_i = 0)$,as an estimate of the counterfactual for treated units, $E(Y_{i0} | D_i = 1)$ does not introduce bias in the estimator. When units are randomly assigned to a treatment group and a nontreatment group (i.e. control), there is a high probability each group will have the same average characteristics for both observable and unobservable attributes. Random assignment to treatment constructs two groups comparable in terms of all observed and unobserved covariates (Rosenbaum 1995). This is analogous to stating the difference in mean values of group attributes is not statistically significant.

Random assignment solves the evaluation problem by direct construction of the unobserved counterfactual. If towns could be randomly assigned to enforce MSW user fees, then the non-participants' solid waste per household could be substituted for the participants' unobserved "outcome had they not participated" without introducing bias.

Matching addresses the evaluation problem by assuming that the choice of participation (D_i =1 or D_i = 0) is independent of the non-participant outcome when the outcome is conditioned on a set of observable variables X (Smith 2006) The assumption is referred to as conditional independence assumption (CIA) or "ignorable treatment assignment" (Dahajia 1998, Rosenbaum and Rubin 1983). The assumption is expressed as:

$$Y_{i1}, Y_{i0} \perp\!\!\!\perp Di \mid X_i, \text{ for all i.} \tag{9}$$

Where X is a vector of observable town characteristics which simultaneously influence the decision to adopt MSW user fees and the outcome variable, MSW generation, and is unaffected by the outcome variable. The two potential outcomes are independent of assignment to treatment when conditioned on a set of attributes X.
The term matching is used because a treated town's characteristics and the characteristics of an untreated town are matched to identify towns with similar characteristics which influence the choice to implement MSW user fees and influences the level of MSW generated.

The conditional average treatment effect on the treated using a matching estimator is:

$$\text{ATET} \equiv E[\tau \mid D{=}1, X_i] = E[Y_{i1} \mid D{=}1, X_i] - E[Y_{i0} \mid D{=}0, X_i] \qquad (10)$$

For the estimator to be unbiased requires that $E(Y_{i0} \mid D{=}1, X_i) = E(Y_{i0} \mid D{=}0, X_i)$. The interpretation of the above expression is that an unbiased estimate of the conditional ATET can be calculated by substituting the observed outcome for a non-participant for the unobserved outcome of the participant's missing counter-factual.

In application, what is proposed is if town A adopts unit based pricing, only the town's outcome as a participant is observed ($Y_{i1}|D{=}1, X$). To estimate town i's outcome had the town not participated ($Y_{i0} |D_i =1, X$), one wants to identify a non-participant town, j ,with outcome designated as ($Y_{j0} |D_j = 0, X$), which possesses all attributes as nearly identical as possible as town i that influence the decision to adopt MSW user fees and the generation of MSW.

By conditioning on a set of town attributes, the conditional independence assumption implies that the observed outcomes are independent of assignment to treatment, which is the same as assignment to treatment being effectively random for the two groups, participant, and non-participant (Borland 2005). The objective of matching is to select a set of observable town attributes such that any two towns with the same attribute values will display no systematic difference in treatment effect.

Exact matching on attribute values quickly becomes problematic as the number of variables increases. For three binary variables the number of cells for matching is $2^3 = 8$. As the number of variables is increased, the number of attribute combinations increases exponentially, $2^5 = 32$. It becomes increasingly difficult to matches with similar combination of attributes; this occurrence is referred to as the curse of dimensionality.

Rosenbaum (1982) derived the theoretical proof that if potential outcomes are independent of treatment conditioned on a vector of covariates, then the outcome variables are also independent of treatment conditioned on a balancing score. The balancing score is defined as the probability a unit will participate in the treatment program given the observed covariates. This balancing score came to be referred to as the propensity score given it is the likelihood of a unit electing to participate in the treatment program, and matching based on the propensity score became referred to as propensity score matching. The propensity score is expressed as:

$$e(X) = P(D_i = 1 | X = x_i) \qquad (11)$$

Matching on the propensity score required the assumption that units with the same propensity score have a positive probability of being both participants and non-participants. The assumption is expressed as:

$$0 < P(D = 1 | X) < 1 \qquad (12)$$

The ATET estimator using matching on propensity score is :

$$ATET_{PSM} \equiv E[Y_1 | D=1, e(X)] - E[Y_0 | D=0, e(X)] = E[Y_1 - Y_0 | e(X)] \qquad (13)$$

All matching estimators (ATE, ATET, and ATEU) have the following general form

$$: \quad E[\tau_{Matching}] = 1/n_1 \sum i \; I_1 \; S_p \; [Y_{1i} \; - E(Y_{0i}| \; D_i = 1, P_i)] \qquad (14)$$

$$\text{Where } E(Y_{1i}| \; D_i=1, P_i) = \sum_{j \; I0} w(i, j)Y_{0j}$$

and where I_1 is the set of program participants, I_0 is the set of non-participants, S_p is the common support region defined over a range of the propensity scores, n_1 is the number of participants in the set $I_1 \cap S_p$. The match for each participant i, i $I_1 \cap S_p$ is constructed as a weighted average over the outcomes of non-participants, where the weights w(i, j) depend on the distance between P_i and P_j (Smith and Todd 2004).

A challenge confronting all analytical methods to evaluate policy effects is the choice of explanatory variables. For matching methods the selection criteria is motivated by the conditional independence assumption (CIA). For matching estimators to be unbiased requires a set of variables such that the outcome variable is independent of treatment assignment when conditioned on the propensity score.

For this study the explanatory variables are related to a municipalities decision to use MSW user fees and related to the generation of MSW, and in turn are unaffected by policy adoption and the outcome variable MSW generation. Choice of explanatory variables is influenced by economic theory, previous empirical results, and knowledge about the institutional setting of the program ((Caliendo and Kopeinig 2005).

Callan and Thomas (1999) and Huang, Halstead, and Saunders (2010) estimate the determinants of MSW user fee adoption. Callan uses a cross-sectional data set of 351 Massachusetts cities and towns with 79 communities using a form of MSW unit pricing in 1995. Huang et. al. use a cross-sectional data set of 200 towns in New Hampshire in 2008 with 31 towns using a form of MSW user fees. Common regressors include property tax rate, education level, income, population, and indicator for curbside trash collection and indicator for curbside recycling collection. Explanatory variables which are statistically significant at conventional levels in both studies and different model specifications include income and property tax. Neither model found the presence of curbside collection of trash or recyclables to be a significant determinant of policy adoption. Callan and Thomas find housing density, number of single-family homes, indicator if landfill located in town, education, and median value of single-family housing to be statistically significant predictors of policy adoption. Huang et. al. find per capita solid waste expenditures to be a statistically significant predictor of policy adoption.

This study builds upon these results and uses the following logit model to estimate the propensity score:

$$P(D=1|X) = \exp[\mathbf{X\beta}] \,/\, [\,1 + \exp[\mathbf{X\beta}]] \tag{15}$$

This nonlinear model can be transformed to a linear functional form by taking the log of the odds of policy adoption and estimated using maximum likelihood:

$$\ln[\,P_i / 1\text{-}P_i] = \beta_0 + \beta_1 Inc + \beta_2 Inc^2 + \beta_3 T + \beta_4 T^2 + \beta_5 HV + \beta_6 HV^2 + \beta_7 Mem + \beta_8 FixEffect \tag{16}$$

Where:

P_i is probability town *i* adopts MSW user fee,
Inc is median household income measured in 1000s dollars,
T is property tax rate assessed on residential property,
HV is median housing value measured in 1000s dollars,
Mem is membership per 100 households in private nonprofit statewide conservation organization, and
FixEffect is indicator variable if town is located in Sullivan, Grafton, or Coos county along the Massachusetts and Vermont state boarder.

4. Study Area and MSW Data

The study area consists of 13 cities and 221 incorporated towns in the State of New Hampshire. Figure 1 illustrates community adoption of MSW user fees beginning in 1984 with the town of Lebanon. Forty towns (17%) were using a form of MSW user fees as of 2008. and serving approximately 15% of the state population. In 2009 the city of Concord, which is the state capital and third largest city in population implemented MSW user fees, increasing the percent of population served to 18%. The frequency of unit-based pricing adoption over the period 1984 – 2008 is illustrated in Figure 2.

The town of Dover implemented MSW user fees in 1991 and subsequently was recognized by the U.S. EPA in 1999 as one of ten cities in the nation for record setting waste reduction after implementing MSW user fees. Dover increased its rate of recycling from 3% in 1990 to 53% in 1996. During this time period the number of households receiving municipal MSW services increased by 10%, and total solid waste disposal declined from 10,496 tons per year to 4,541 tons per year.

In 1999 the New Hampshire Governor created a Solid Waste Task Force charged with developing policy recommendations in response to a series of solid waste management issues confronting local governmental units. Cities and towns providing MSW services were experiencing problems related to landfill closures, rising disposal costs, increasing industry concentration in waste disposal services, and increasing levels of imported out-of-state waste disposal.

Included in the Task Force report was a recommendation for the N.H.Department of Environmental Services (DES) to promote adoption of MSW unit-based pricing programs to local municipalities. as an effective means to reduce MSW disposal. Where implemented, MSW user fees were credited with increasing recycling rates and reducing MSW disposal (State of New Hampshire, 2001).

The total quantity of municipal solid waste and total amount of recycled materials by weight in New Hampshire for the years 2000 – 2008 are illustrated in Figure 3. The mean level of MSW over this nine year period is 505,000 tons per year. The decline in total municipal solid waste from 2006 to 2008 is partially due to the 2007 economic recession. Fourteen additional towns adopted MSW user fees during the period 2000 - 2008. The state-wide recycling rate increased from 20% in 2000 to approximately 30% in 2008.

A survey of towns with MSW user fees identified the following factors influencing consideration of policy adoption (DSM Environmental Services 2008).

1. Generate another revenue stream to offset increasing refuse costs.
2. Creates more equity in who pays for refuse services.
3. Creates an incentive to reduce refuse generation and/or increase recycling.
4. Controls refuse coming in from neighboring communities.
5. Generates funds for landfill closure.

Data on town level municipal solid waste disposal is compiled by the New Hampshire Department of Environmental Services (DES). Data reporting by towns is voluntary, and is subject to variability due differences in recording keeping practices. For 2008, 27 towns did not submit MSW data, and MSW totals for 27 towns which transfer their MSW to another town are included in the receiving town's total. To transform total MSW to MSW per household, the housing stock of MSW receiving towns was adjusted to include housing units from MSW transferring towns. The data set for this study consists of 180 towns which account for 90% of the state population.

There are approximately 200 solid waste transfer and/or recycling centers in the state. Curbside collection of household solid waste is provided by 115 towns.

Figure 4 shows the comparison of mean annual town MSW per household by program (i.e. with and without MSW user fees) for the years 2000 – 2008. The bar graph shows annual town MSW per household for towns with MSW user fees is consistently less that towns without MSW user fees .However, results of a two-group mean comparison t-test indicate the difference in means is statistically significant at the 0.05 significance level for only 4 of the 9 years (2000, 2003, 2007, 2008).

Regarding recycling levels, Figure 5 shows the mean annual town recycled per household by program (i.e. with and without MSW user fees). The level of recycled material is higher for towns with MSW user fees; however, a comparison of mean differences between the two groups does not find the yearly differences to be statistically significant.

The propensity score matching (PSM) estimator relies on a set of explanatory variables to model the choice to adopt MSW user fees and the outcome variable, MSW disposal. The income and property tax rate are found to be statistically significant predictors of policy adoption in two studies (Callan and Thomas 1999, Huang, Halstead, and Saunders 2010). In both studies, the income coefficient is negative suggesting communities with relatively higher income are less likely to adopt MSW user fee. The coefficient on tax rate is positive for both studies, suggesting all else equal, communities with higher property tax rates are associated with using MSW user fees.

Callan and Thomas (1999) find housing values are positively associated with policy adoption. Estimates of median home value by town were obtained from the New Hampshire Financial Housing Authority for the year 2008.

Dijkgraaf (2004) reports empirical results from a panel data set of 538 towns in Netherlands for the period 1998 – 2000 which suggest that towns with relatively higher levels of environmental activism have 7% less MSW disposal prior to adopting MSW user fees compared to towns with low levels of environmental activism.

Unique to this study is a variable for membership in a private non-profit state-wide conservation organization and is used to partially control for household support for conservation programs. The mean value of members per 100 households is 1.7 for towns using MSW user fees and 1.3 for towns without MSW user fees and the difference is statistically significant at significance level of 0.05.

An indicator variable to control for regional fixed effects (Allers and Hoeben 2010) is used to control for the clustering of MSW user fee towns along the western state boundary as is observed in Figure 1.

Descriptive statistics of the variables used for the simple matching estimator and propensity score matching estimator are listed in Table 1 by treatment status. Towns enforcing MSW user fees have on average lower median household income, and relatively higher property tax rates, median housing values, membership in a statewide conservation organization, and higher concentration in counties located along the western state boarder.

To estimate treatment effects, regression and matching methods rely on a set of sufficiently rich explanatory to predict the outcome variable, such that after conditioning on the explanatory variables, the singular effect of treatment on the outcome variable can be estimated without any confoundedness. Regression models require the additional assumption of linear in parameters function form. If the differences in explanatory variable values are sufficiently large, local linear regression approximation of the average treatment effect may not be globally accurate (Imbens and Wooldridge 2009) suggest a normalized difference for each covariate be calculated.

One statistic used to estimate the relative difference in explanatory variables by treatment status is the normalized difference of a covariate. The normalized difference is the difference in sample means by treatment status weighted by the squared root of the sum of the sample variances. As a “rule-of-thumb” Imbens and Rubin (forthcoming) suggest linear regression methods tend to be sensitive to specification of functional form when the normalized difference exceeds 0.25.

The normalized differences for the eight explanatory variables used in this study are listed in Table 2. Five values (-0.35, 0.38, 0.55, 0.51, 0.39) exceed 0.25 in absolute value, two are equal to 0.23, and one is 0.17. Unlike regression, matching methods do not require the assumption of a linear functional form in parameters. Matching methods do not require an assumption of functional form and may be a more appropriate estimation method compared to regression when the normalized difference of covariates is relatively large.

5. Propensity Score Matching Estimator and Results

A logit model is used to estimate propensity scores (i.e. balancing scores) conditioned on the set of explanatory variables listed in equation 16. The propensity score is the predicted probability of policy adoption calculated for each town using the estimated coefficients. The estimated coefficients are listed in Table 3. The effect of income and property tax rate are positive and has a diminishing effect, whereas housing value is negative and declines at an increasing rate. Environmental activism is positively associated with policy adoption as is the regional fix effect variable.

A key requirement of matching is covariate balancing. The term balance is used to imply two conditions. One is the average propensity score for participants and non-participant observations do not differ within blocks (Becker 2002), and the differences in covariate means for participants and non-participants are not statistically significant at conventional significance levels. Covariate balancing implies the values of the explanatory variables used to estimate the propensity score, also called a balancing score, are the same for matched pairs of towns.

Two methods to examine covariates balancing is to test for the statistical difference in group mean differences between towns with MSW user fees and towns without MSW user fees (Rosenbaum and Rubin 1983). The results of t-test to test for mean differences of explanatory variables by group are reported in Table 3. As observed, the t-statistics of group mean differences are statistically significant for each variable prior to matching and are not statistically significant after matching on propensity score.

This outcome emulates the outcome associated with random treatment assignment in which mean characteristics of participants and non-participants are similar (i.e. balanced) after random assignment to the treatment or control group. Assuming the conditional independence assumption (CIA) is satisfied based upon this set of variables, and the mean value of attributes for participants and non-participants which influence policy adoption and MSW generation are balanced, the mean group difference between the outcome variable, MSW, is attributed solely to the program effect of MSW user fees.

The variables also satisfy a second form of balancing criteria in which the values for propensity score are ranked from low to high and subdivide into quintiles which are referred to as blocks. Balancing of the propensity score and explanatory variables are evaluated for each block (Dehejei and Wahba 1999). The estimated propensity score and covariates satisfy the requirement that the mean difference of propensity scores for treated and non-treated groups within blocks and the mean group difference of the explanatory variables within blocks are not statistically significant.

Matching on propensity scores is restricted to a common support, and as such the estimates of average treatment effect on the treated is defined only for those participants with a propensity score within the common support. Figure 4 is a histogram showing the distribution of propensity score by treatment status. Observations with propensity scores between the values 0.05 – 0.65 are used to form matched pairs. Borlan (2005) defines common support as the requirement that for each program participant, there is some observation with the same (or sufficiently similar) characteristic that did not participate, and hence can be used as the matched comparison observation.

The number of observations dropped from analysis is 61 non-participant towns with propensity score less than 0.05 and 4 participant towns with propensity score above 0.65. The inference of treatment effect cannot be generalized to the population and limited to the subset of 30 towns with MSW user fees. Although matching on common support results in few observations, an advantage is the remaining set of towns are similar in covariates which influence the decision to adopt MSW user fees and in MSW waste generation.

Common support implies omitting all observations of participant towns' propensity scores that are above the maximum propensity score for the non-participant towns, and omitting all observations for non-participant towns' propensity scores that are below the minimum propensity score for the participant counties. Matching on a common support makes it evident whether or not comparable non-participant units are available for each participant unit. In the matching literature, the benefit of matching on common support is contrasted to regression analysis when observations of participant and non-participants are clustered into two distinct groups and effects are estimated "solely by projection into regions where there are no data points (Smith 2006)."

The average treatment effect on the treated (ATET) estimator is:

$$ATET = 1 / n_1 \sum_i \{ [Y_i \mid D_i = 1] - \sum_j w_{ij}(Y_j \mid D_j = 0]\} \qquad (17)$$

where n_1 is the number of treated cases, i is the index over treatment cases, j is the index over control cases, and w_{ij} is a set of scaled weights which depend of the distance between the propensity score for each non-participant unit paired to a participant unit. Different matching algorithms are used are used to construct the weights.

Matching methods can be categorized into two general approaches 1) one-to-one or one-to-n, where n is a fixed number of control units and 2) nonparametric regression matching referred to as either kernel-based matching or local linear regression (Guo and Fraser 2010, Heckman et. al. 1997). Although all matching methods are asymptotically equivalent, matching methods incur an inherent trade-off between efficiency and biasedness for finite sample size. Increasing the number of control units (i.e. no MSW user fees) as an estimate of the treated unit counterfactual increases estimator efficiency by increasing sample size and using more information, however, the increased number of control units comes at a price of decreased quality of matches.

Nonparametric matching use a smoothing or weighting function, also called a kernel function, to fit an unknown density function to an observed distribution of the data (Hill, Carter and Lim 2011). A kernel function can be used to assign a weighted average to the value of each control units outcome variable (i.e. level of MSW) based on the control unit's distance from the treated unit where distance is measured as the difference in propensity score. The values of control variables, for which the propensity score are closer to the treatment propensity score, are weighted more heavily than outcomes for which propensity score are farther apart.

Bandwidth is the fraction of observations used to form a span or window centering on the selected control unit, which is also called the focal point. For example, a bandwidth of 0.05 will use 5% of the total control observations of which half have an outcome value above the focal point's outcome value, and half have an outcome value below the focal point's outcome value. As previously noted, different weights can be assigned to individual outcome values contained in the span. The method used to assign the weights for constructing a weighted average for the focal point is referred to as the kernel estimator. Matching estimators using kernel matching are sensitive to the choice of bandwidth.

The results for this study were estimated using the user-developed program psmatch2 in Stata Software (Leuevn and Sianesi 2003). Estimates are listed in Table 5. The two matching methods used are 1) nearest neighbor and 2) kernel matching. Nearest neighbor used the control observation with a propensity score closest to the treatment unit. Matching was with replacement which allows single control unit to be used for multiple matches with a treatment unit. The kernel estimator used the program default epanechnikov kernel for calculating weights. Estimates were derived using three bandwidths, the default of 0.06, and selection of 0.04 and 0.02.

The average treatment effect for the treated (ATET) estimates the impact of MSW user fees for those municipalities that adopted the program. The treatment impact ranges from a maximum reduction of 823 pounds of MSW per household using a kernel estimator with bandwidth (bw) equal to 0.06 to a minimum of 631 lbs per household using kernel estimator with bw = 0.02. The near neighbor estimator and kernel estimator with bw = 0.04 both estimate treatment impact as a reduction of 741 pound per household.

The estimate of a reduction of 741 pounds of MSW generation per household due to MSW user fees is calculated as the difference of an average generation rate of 1531 lbs. per household in towns without MSW user fees and an average generation rate of 790 pounds per household in towns using a form of MSW user fees. The impact of MSW user fees is a 48% reduction in MSW generation per household.

Standard errors and 95% confidence intervals were estimated using bootstrapping with 50 replicates and are listed in Table 5. The ATET estimates are statistically significant at the 0.05 significant level. The use of bootstrapping to calculate the variance of kernel matching estimators is subject to debate given there is no theoretical justification for bootstrapping to estimate the variance of matching estimators. Research suggests bootstrapping methods may not give correct results (Abadie et. al. 2004).

Estimates of average treatment effects (ATE) using ordinary least squares (OLS) with an indicator variable for policy adoption and the same set of explanatory variables used in the logit model and the estimated propensity score are listed in Table 5. Two estimates are listed. One is an ATE estimate of -801lbs. reduction per household using the full data set of 180 towns with 34 towns using MSW user fees. When OLS estimation is limited to the common support used for propensity score matching with 115 towns of which 30 enforce MSW user fees the estimate of program impact is – 748lbs. per household. Both estimates are statistically significant at a 0.01 significance level.

The property of unbiasedness of matching estimators is premised on the conditional independence assumption (CIA). When matching is conditioned on a set of variables that influence the decision to implement MSW user fees and MSW waste generation, then the potential outcome variable is independent of treatment assignment. This assumption also extends to variables which influence policy choice and waste generation and are not observed. For example, if households in communities with MSW user fees are also more motivated to reduce waste generation relative to households located in town without MSW user fees, then the above estimates of program effects may be partially attributed to the MSW user fee and the unobserved household motivation.

Sensitivity analysis is conducted to evaluate the robustness of empirical results to potential bias attributed to unobserved variables. By definition, because unobserved variables can not be directly modeled, the approach to sensitivity analysis is estimate the level of effect an unobserved variable would have to exert of the derived estimates such that the results are no longer statistically significant. If a relatively minor effect renders the results not statistically significant, then the estimates are not robust. If the level of effect from an unobserved variable must be relatively large to render the estimates not statistically significant, then the estimates are deemed to be robust.

The results of a Rosenbaum (2002, 2005) sensitivity analysis are listed in Table 6. The test statistic, Gamma, is calculated as a ratio of odds for two observations. Description and derivation of the Rosenbaum bounds are presented in Rubin (2006), Guo and Frasher (2010), and DiPrete and Gangl (2004). Following Guo and Frasher (2010), the odds ratio that two towns *i* and *j* adopt MSW user fees is:

$$[\pi_i / (1 - \pi_i)] / [\pi_j / (1 - \pi_j)] = \pi_i (1 - \pi_j) / \pi_j (1 - \pi_i) \quad (18)$$

If two town have the same covariates $x_i = x_j$, then the probability of adopting MSW user fees is the same for each town $\pi_i = \pi_j$ and the above odds ratio will equal 1. However, if an unobserved variable affects the probability of policy adoption and MSW generation, the two towns with similar covariates may have different probabilities of policy adoption, $\pi_i \neq \pi_j$, and the above odds ratio will be different from 1. Rosenbaum derived a test statistics which can be used as a bound of the above odds ratio:

$$1/\Gamma \leq \pi_i (1 - \pi_j) / \pi_j (1 - \pi_i) \leq \Gamma \quad (19)$$

When $\Gamma = 1$, then $\pi_i = \pi_j$, then the odds ratio is 1 assuming $x_i = x_j$. When $\Gamma = 2$ and assuming similar town covariates, the two towns could differ in probability of policy adoption by as much as a factor of 2, and one town may be twice as likely to adopt MSW user fees due to an unobserved variable. A Wilcoxon test statistic is calculated based upon the statistical significance in the outcome variable which corresponds for each level of gamma Γ. Assuming the estimates are free of hidden bias, values of gamma close to 1 in which the corresponding p-value is at or above 0.05 indicate the results are sensitive to small change induced by a hidden bias. High values of gamma are associated with robust results in which the effect of hidden bias must be relatively large to render the estimates not significant.

Based upon the results listed in Table 6 a Gamma level of 3.75 has a p-value of 0.053. The interpretation is that the odds ratio would have to change by a factor of 3.75 to render the estimates statistically insignificant at a significance level of 0.05. Based upon the Rosenbaum bounds sensitivity analysis, the results listed in Table 5 from using the kernel estimator with bw = 0.06 are relatively robust.

Included in Table 6 are estimates of the equivalent hidden bias associated with different Gamma levels. To render the estimated treatment effect to be insignificant as a result of hidden bias is equivalent to increasing median housing values by 46,000 dollars above the current mean value of 224,000 (20% increase) or increasing membership in a conservation organization from the current mean level of 1.4 members per 100 households to 3.6 members per 100 households (257% increase).

6. Summary and Conclusions

The results of this study find MSW user fees reduce household waste disposal. Matching estimation is used as an alternative to the standard analytical approach of regression analysis.

The choice of evaluation method is motivated by 1) the type of policy question to be answered 2) whether program response is assumed to be heterogeneous or homogeneous across units. Matching methods are used to evaluate the program effect on subsets of the population when effects are expected to vary across units (Blundell and Dias 2002).

For this study, the average treatment effect on the treated (ATET) is used to estimate the effect of MSW user fees on communities which actually used some form of MSW user fees. Because communities self-select to implement MSW user fees, the program effects is assumed to vary across municipalities with those communities

This study used a cross-sectional data set of 180 towns located in New Hampshire with 34 towns using a form of MSW user fees in the year 2008. The average treatment effect on the treated (ATET) ranges from an average annual reduction of 823lbs. to 631 lbs. per household. This represents a reduction of 53% to 41% from an average MSW of 1530lbs per household for towns without MSW user fees. Based upon bootstrapping to estimate the estimator's standard error, the estimates are statistically significant at a significance level of 0.05.

An assumption of matching methods is the conditional independence assumption which assumes that assignment to treatment is independent of outcome conditioned on a set of variables which control for policy adoption and MSW generation. This study used a parsimonious logit model to estimate the probability of policy adoption, referred to as either a propensity score or balancing score. The selection of explanatory variables was premised on prior empirical studies. Unique to this study is the use of membership in a private non-profit conservation organization to partially control for environmental activism. A regional indicator variable is used to control for observed clustering of municipalities with MSW user fees in three counties.

Matching estimators are biased if unobserved variables effect the decision to adopt MSW user fees or MSW generation, and this effect varies across participants and non-participants. Rosenbaum and Rubin (1983) proposed a technique for evaluating the sensitivity of statistical significance of empirical results to the potential effect from an unobserved explanatory variable. Based upon the results of the Rosenbaum bounds for sensitivity analysis, the empirical results reported in this study are relatively robust to the potential effect of hidden bias. To render the estimates not statistically significant at a 0.05 significance level requires the odds ratio of treatment assignment to change by a magnitude of 3.75. This potential hidden bias effect is equivalent to an increase in the median home value by $45,000, or an increase in environmental membership by 3.6 members per 100 households.

Areas for further investigation include the effect of MSW user fees on recycling rates and the effect of MSW user fees overtime. Although there is an increase in the level of recycling associated with communities using MSW user fees, cursory examination of recycling rates for communities with and without MSW user fees does not find the difference to be statistically significant at conventional levels. This warrants further investigation.

This analysis was conducted using cross-sectional data for the year 2008. Results are sensitive to the selected time period. Analysis conducted for prior years finds smaller impact. The U.S. economy experienced a significant recession in 2008. Further consideration should be given to controlling for the potential effect of an economic downturn on household behavior of waste generation. MSW user fees may have a differential impact on household waste disposal behavior during economic recessions compared to economic expansions. Are households who pay a user fee for trash disposal relatively more responsive to MSW user fees during a recessionary period compared to periods of economic prosperity?

Some studies have suggested the observed decrease in MSW disposal but the absence of a corresponding increase in recycling may be attributed to illegal waste disposal. When a community adopts MSW user fees, there is concern that households choosing to avoid the additional cost will resort to “illegal dumping”, such as disposing garbage at trash collections bins used by locations serviced by private waste haulers. A limited number of empirical results suggest the practice of illegal dumping may partially account for the observed decrease in household solid waste associated with unit based pricing (Fullerton and Kinnaman, 1993). To date there is no empirical results indicating illegal dumping is a significant ongoing practice associated with communities using MSW user fees. The results for this study are premised on the assumption that the estimated reduction in household waste disposal is not off-set by an increase in illegal solid waste disposal.

Adoption of MSW user fees increase costs and revenues from delivery of MSW services. The results of this study indicate MSW user fees are effective in reducing MSW generation. The next stage in the MSW policy evaluation process is to evaluate the net economic benefits associated with MSW user fees.

Figure 1 Study Area

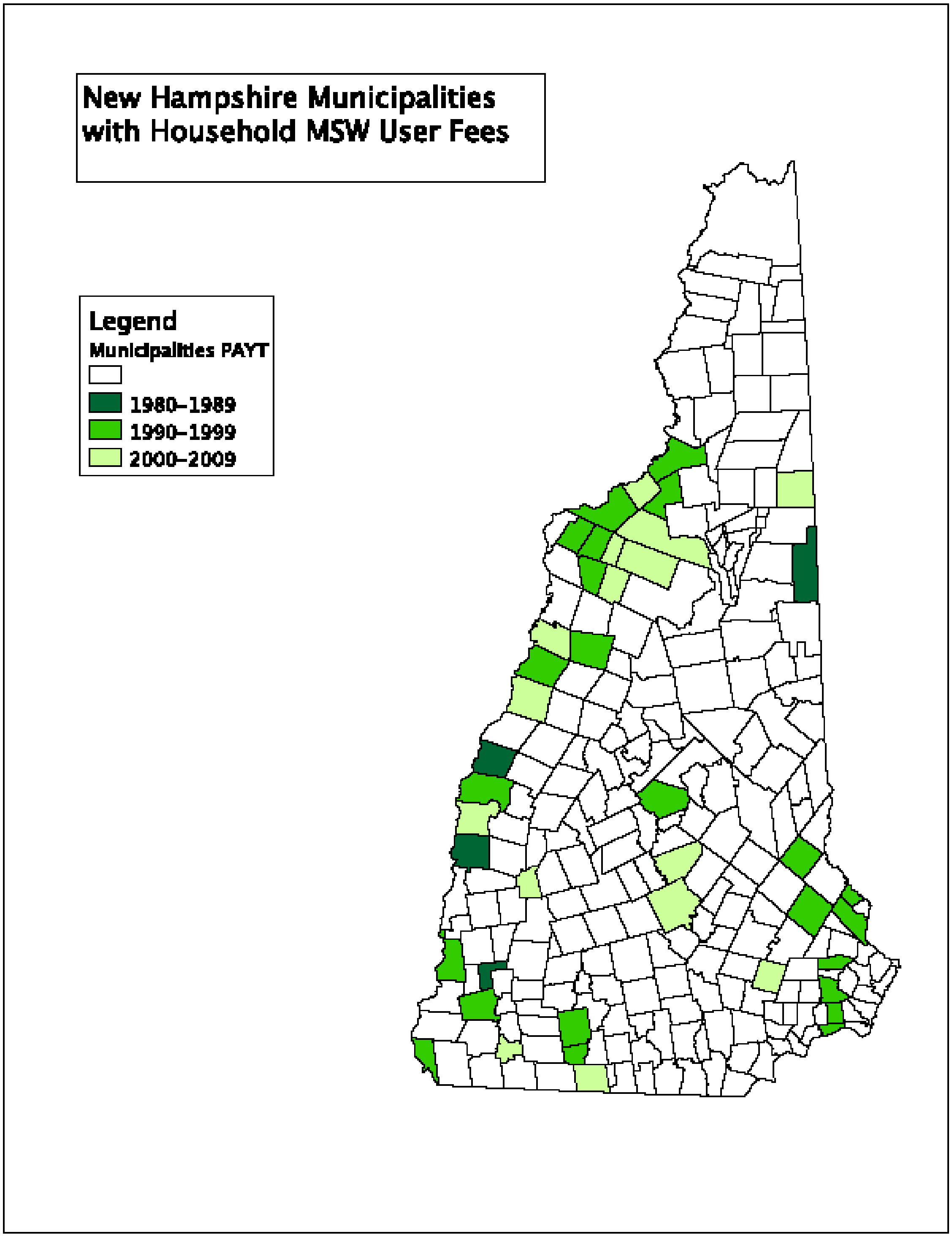

Figure 2.

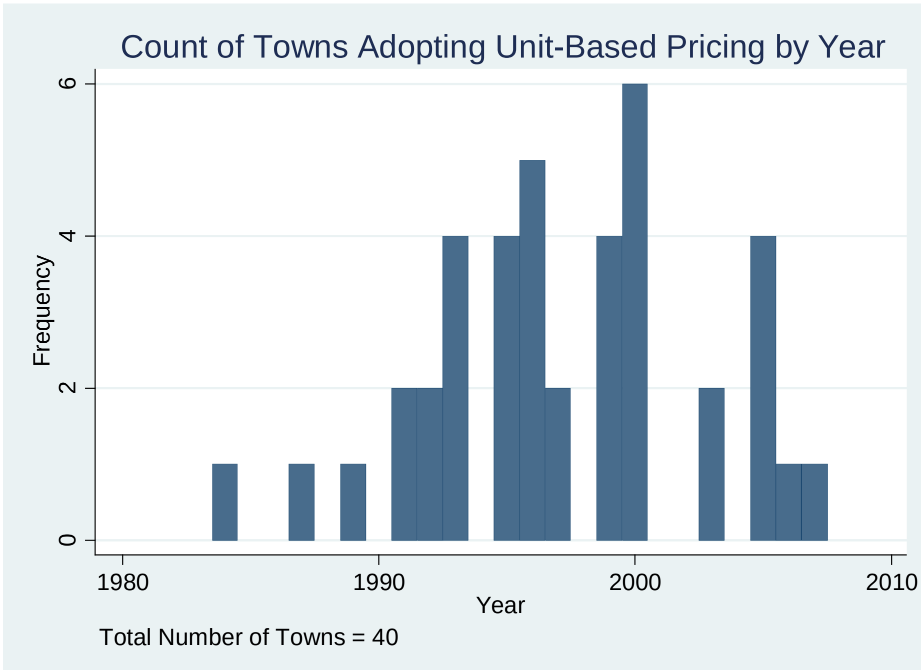

Figure 3

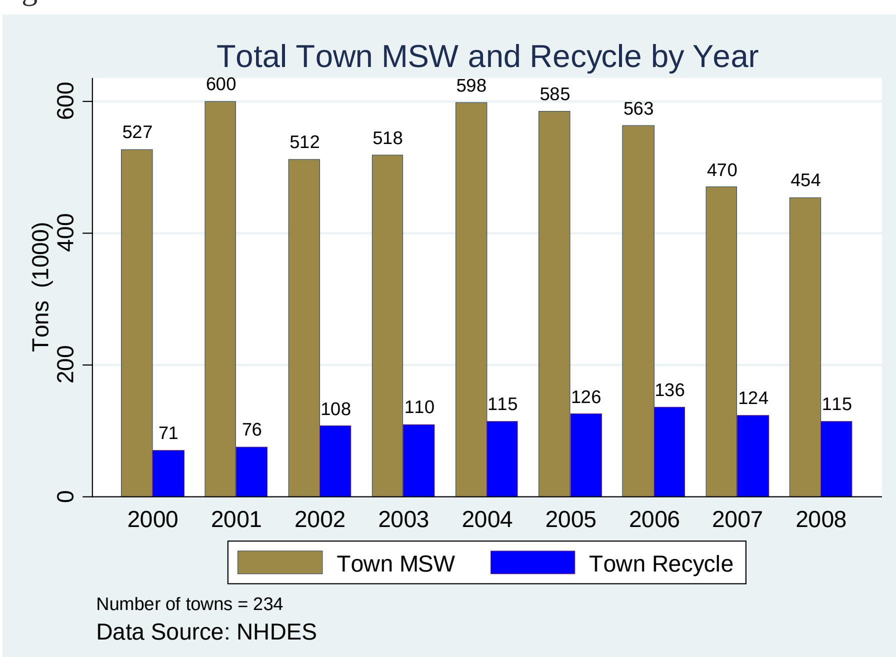

Figure 4.

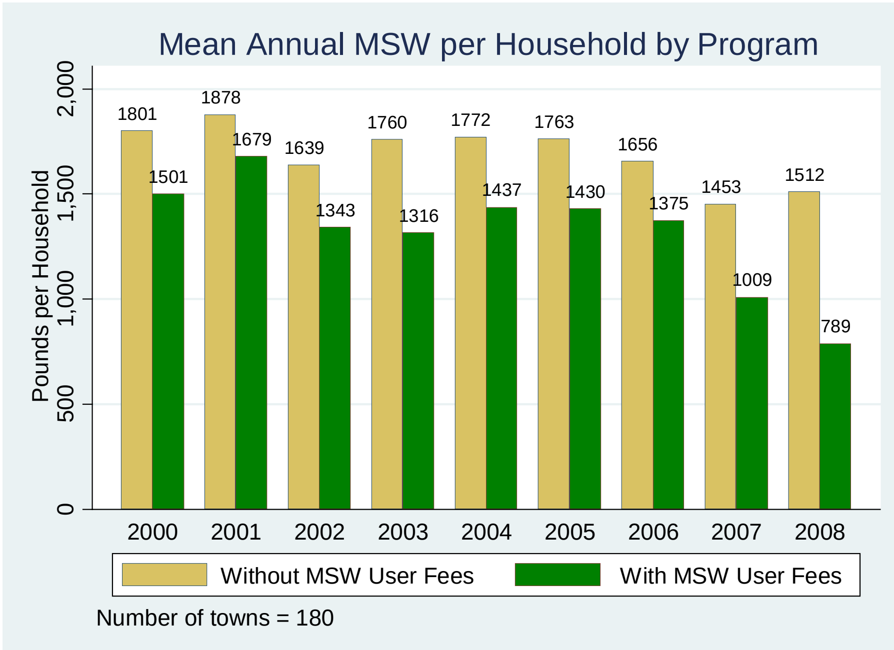

Figure 5

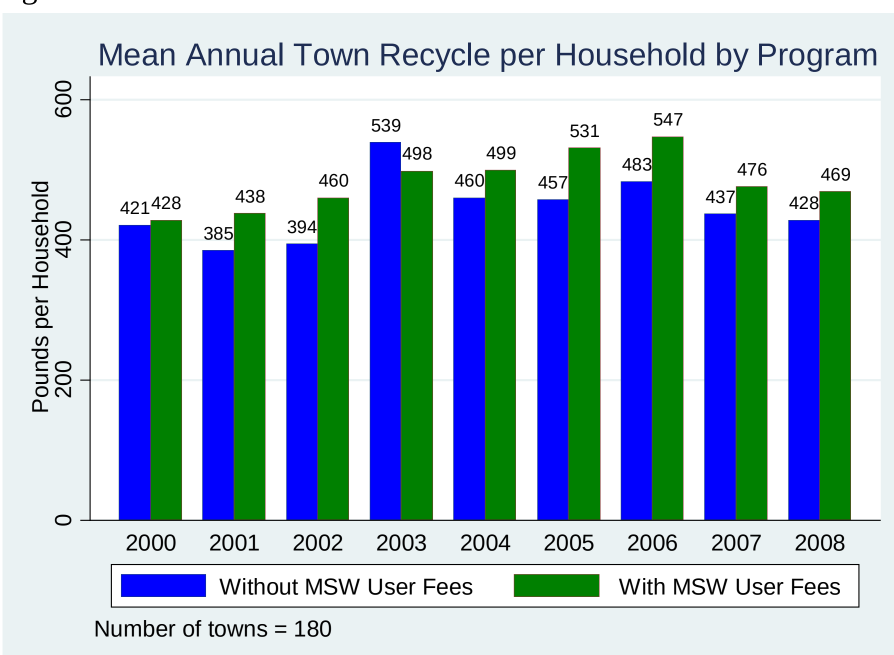

Figure 4. Histogram of Propensity Score by Treatment Status

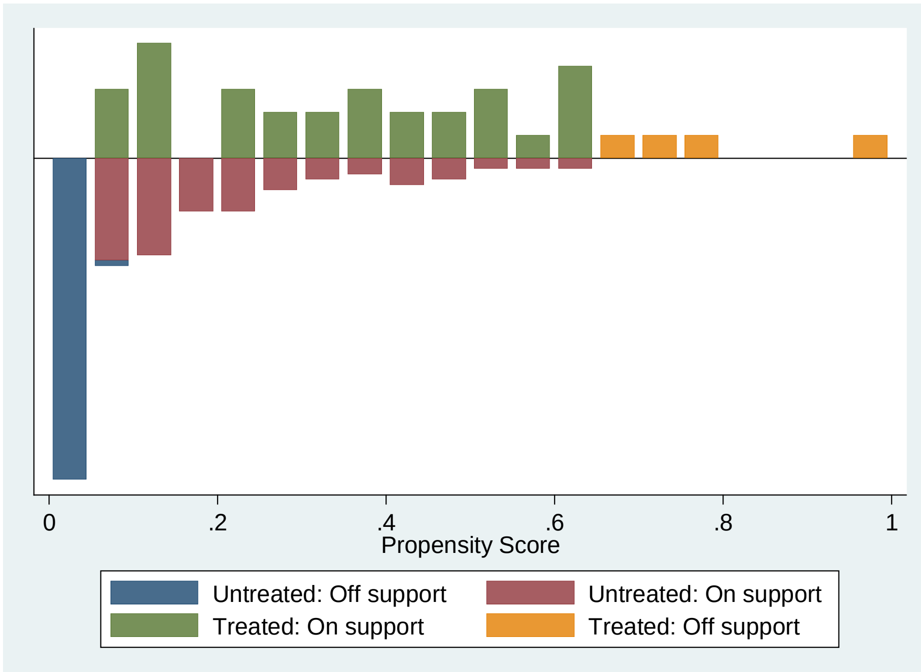

Table 1. Descriptive Statistics for Explanatory Variables

Variable	Full Sample		Towns with PAYT		Towns without PAYT		Variable Description
	Mean	s.d.	Mean	s.d.	Mean	s.d.	
Income	49.0	12.9	44.5	8.0	50.0	13.6	Median household income (1000s)
Tax Rate	18.8	4.8	21.5	4.0	18.1	4.8	Residential property tax rate
House Value	224.7	96.1	259.7	167	216.8	68.7	Median price of homes sales (1000s)
Membership	1.4	1.5	1.7	1.8	1.3	1.4	Conservation membership per 100 houses
Region Fixed Effects	0.31	0.46	0.53	0.50	0.27	0.44	Indicator variable =1 if town located in Sullivan, Grafton, or Coos county
Sample size	180		34		146		

Table 2. Normalized Difference for Explanatory Variables

Variable	Sample	Mean	Variance	Normalized Difference
Income	Control	50	186.3	-0.35
	Treatment	44.5	63.2	
$Income^2$	Control	2687.6	2298369	-0.38
	Treatment	2042	585881	
TaxRate	Control	18.1	22.8	0.55
	Treatment	21.5	15.8	
$TaxRate^2$	Control	351	29595	0.51
	Treatment	479	33607	
HouseValue	Control	216	4715	0.23
	Treatment	259	27865	
$HouseValue^2$	Control	51666	$1.08*10^9$	0.23
	Treatment	93973	$3.4*10^{10}$	
Members	Control	1.3	2	0.17
	Treatment	1.7	3	
Region	Control	0.27	0.20	0.39
	Treatment	0.53	0.26	

Table 3. Estimated Coefficients from Logit Model Used to Compute Propensity Score

Dependent Variable MSWH(lbs)	Estimated Coefficient	Standard Error	P-value
Income	0.475	0.31	0.12
Income^2	-0.01	0.003	0.09
Tax	0.856	0.44	0.05
Tax^2	-0.016	0.01	0.10
HouseValue	-0.020	0.01	0.12
HouseValue^2	0.0001	0.0001	0.08
Members	0.363	0.14	0.01
Region	0.867	0.48	0.07
Constant	-20.026	8.37	0.02
Number of Observations	180		

Table 4. Balancing of Sample Means Before and After Matching

Variable	Sample	Mean		% Bias	% |Bias| Reduction	t-test	
		Treated	Control			t	p>| t |
Income	Unmatched	44.5	50	-49		-2.27	0.03
	Matched	43.3	44.7	-12	75	-0.78	0.44
Income^2	Unmatched	2042	2687	-54		-2.41	0.02
	Matched	1923	2040	-10	82	-0.76	0.45
TaxRate	Unmatched	21.5	18.1	77.8		3.87	0.001
	Matched	21.5	21.1	8.4	89	0.37	0.71
TaxRate^2	Unmatched	479	351	72		3.87	0.001
	Matched	478	461	9.8	86	0.39	0.70
HouseValue	Unmatched	258	216	33		2.32	0.02
	Matched	228	228.2	-0.1	99	-0.01	0.99
HouseValue^2	Unmatched	93973	51666	32		2.62	0.01
	Matched	58918	59441	-0.4	98	-0.05	0.96
Members	Unmatched	1.7	1.3	24		1.38	0.16
	Matched	1.5	1.7	-16	32	-0.54	0.58
Region	Unmatched	0.52	0.26	55.1		3.02	0.003
	Matched	0.53	0.46	15	72	0.54	0.58

Table 5. Estimates of Average Treatment Effects for the Treated (ATT)

Method	Treatment Effect			
	ATE	ATET	s.e.	95% Conf. Interval
Nearest Neighbor		-741	172	(-1302 , -129)
Kernel				
Bandwidth 0.06		-823	184	(-1178 , -436)
Bandwidth 0.04		-741	237	(-1206 , -254)
Bandwidth 0.02		-631	170	(-967 , -283)
OLS				
Model 1 Full sample	-801***		188	(-1172, -431)
Model 2 On support	-748***		216	(-1177 ,-319)

Standard errors and confidence intervals for nearest neighbor and kernel matching methods estimated using bootstrapping with 50 replicates.

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Table 6. Rosenbaum bounds

	p-Value for Gamma		Hidden bias equivalent	
Gamma	Upper bounds	Lower bound	Housing Value	Membership
1	0.000022	0.000022	0	0
1.25	1.70E-06	0.000176	8.65	0.6
1.5	1.40E-07	0.000709	15.31	1.1
1.75	1.10E-08	0.001938	20.71	1.5
2	8.70E-10	0.004149	25.27	1.9
2.25	7.00E-11	0.007539	29.39	2.2
2.5	5.70E-12	0.012209	32.50	2.5
2.75	4.70E-13	0.018173	35.50	2.8
3	3.90E-14	0.025383	38.20	3.0
3.25	3.20E-15	0.03375	40.62	3.2
3.5	2.20E-16	0.043163	44.63	3.5
3.75	0	0.053502	44.91	3.6
4	0	0.064644	46.80	3.8

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