**Title**

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# The Effectiveness of Ridesharing Incentives: Discrete-choice Models of Commuting in Southern California

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**Abstract**

This paper studies the effects of certain incentives designed to promote ridesharing on work trips to reduce congestion and air pollution. Ordered probit discrete choice models of commuters' mode choices (always rideshare, sometimes rideshare, and always drive alone) are estimated using a new study of full-time workers' commuting behavior in the greater Los Angeles area. We find that women and those who have larger households with multiple workers, longer commutes, and larger worksites are more likely to rideshare. Partial equilibrium policy simulations with our model indicate that providing all workers with reserved parking, ridesharing subsidies, guaranteed rides home, and high-occupancy vehicle lanes would reduce drive-alone commuting between 11 and 18 percent.

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## 1. Introduction

Transportation planners are relying upon transportation demand management (TDM) to reduce congestion and improve air quality without requiring major infrastructure investments and their attendant environmental impacts. TDM focuses almost entirely on the commute to work, utilizing incentives to encourage alternatives to solo driving, such as carpooling, vanpooling, and public transit. Also included in TDM are modifications in work patterns, such as staggered work hours, compressed work schedules, and telecommuting (formal off-site working arrangements), which would all help reduce peak congestion.

Large employers in the South Coast Air Basin of California are subject to mandatory regulations administered by the South Coast Air Quality Management District on the average vehicle ridership (the ratio of employees to vehicles arriving at a work site during the morning peak period) of their employees. The South Coast Air Basin encompasses the contiguous urbanized portions of the counties of Los Angeles, Orange, Riverside, and San Bernardino, also known as the Greater Los Angeles Area. Area employers with 100 or more employees are required to plan and implement TDM programs to attain a target average vehicle ridership (ranging from 1.3 to 1.75, depending on location), with mandatory yearly evaluations of their success. Employers not making acceptable progress in increasing their average vehicle ridership, as judged by the South Coast Air Quality Management District, are required to devote more resources to their TDM programs. Fines are levied on employers not in compliance with the requirements to submit and implement TDM programs.

The objective of the present research is to quantify the effectiveness of some of the most popular TDM incentives used to encourage ridesharing. There is intense interest in effectiveness measures among transportation planners, regulators such as the South Coast Air Quality Management District, and company officials faced with implementing TDM programs. However, very little recent quantitative information is available, as documented in Hwang and Giuliano (1990) and Wachs (1991); performance evaluations of ridesharing incentives are generally limited to qualitative assessments [e.g., Hwang and Giuliano (1990), Stevens (1990), and AQMD (1991)]. The present research addresses the need, articulated in Ferguson (1990), for quantitative assessments of the impacts of individual incentives on commuter mode choice.

The focus of this paper is on carpooling and vanpooling (hereafter called ridesharing), where carpooling is defined in the Southern California sense as two or more occupants per vehicle. Three TDM incentives are investigated: (1) reserved or other preferential parking for ridesharers, (2) direct carpooling and/or vanpooling cost subsidies by employers, and (3) guaranteed rides home for ridesharers. In addition, we investigate the effectiveness of high–occupancy vehicle (HOV) lanes, which have been added to many Southern California freeways to promote ridesharing [see Small (1983), Southworth and Westbrook (1986), Giuliano, Levine, and Teal (1990), and Golob, et al., (1991) for other studies of HOV lanes].

The evaluation of effectiveness is accomplished by estimating models of commuters' discrete choice between solo driving and ridesharing. The data, described in the next section, are extracted from the first wave of a panel survey of approximately 2,200 full–time workers in the South Coast Air Basin. The sample is split by employment location: half of the survey respondents are employed in the Irvine Business Complex (IBC), a large diversified employment center with approximately fifty five thousand employees near the Orange County Airport, and the remaining half are employed elsewhere throughout the South Coast Air Basin. Very few of these employment locations are well served by public transit, so our analysis excludes the 2 percent of the sample who use public transit. The survey supports choice models with three alternatives: exclusive solo driving over a two week period (about 78% of the sample), exclusive ridesharing over the same period (7% of the sample), or mixed use of both modes (15% of the sample).

Data restrictions prevent the study of additional TDM incentives; the incidence of incentives such as free introductory bus passes, employer reimbursement of bus passes, paid time off for ridesharers, and reduced parking fees for carpools and vanpools, is not sufficient to provide statistically reliable results. For example, less than two percent of employees reported that employers provided reduced parking rates, a consequence of the fact that there is very little paid parking in Southern California outside of the Los Angeles Central Business District. Differential parking rates have been shown to be effective in reducing solo driving [e.g., Feeney (1989) and Willson and Shoup (1990)], but, unfortunately, the present data do not permit assessment of the relative effectiveness of this incentive.

Sampling weights were estimated by statistically matching our sample to the 1987 March Current Population Survey (CPS). If these weights, and therefore the sample design, are correlated with ridesharing choice then standard unweighted estimators will yield inconsistent results. Tests of correlation between the sample design and ridesharing choice show that the hypothesis of exogenous weights (i.e. not correlated with ridesharing choice) cannot be rejected. Thus, unweighted estimates are consistent, but the weights are still needed for population predictions. Additional fixed weights were generated for the IBC half of the sample, which was stratified by employer type and size category. The entire weighting process is described in the third section, with details provided in Appendix A.

We use an ordered probit model distinguishing three ridesharing alternatives: always rideshare, sometimes rideshare and sometimes drive alone, and always drive alone. One of the key exogenous variables, employer size, is missing for approximately one half of the sample, so multiple imputation techniques [see Rubin (1986) and (1987), and Brownstone (1990)] were used to permit consistent estimation using the entire sample. Comparisons to estimates from the IBC sample, where weighting is not required and employer size is observed, confirm the consistency of the full–sample estimates.

Two types of choice models are estimated and compared, based on different treatments of the incentive variables. In the first model, the dummy variables used to indicate the absence or presence of each of the three employer–provided incentives are assigned values based on whether or not an individual commuter indicated that the incentive was provided by his or her employer. It is possible that the perception of incentives is endogenous with respect to choice, as ridesharers are more likely to be aware of the presence of incentives than are solo drivers. In the second model, the incentive dummy variables are assigned values based on whether or not at least ten percent of the employees at a particular work site indicated that an incentive was provided. These two models provide alternative assessments of the effectiveness of the incentives as influences on mode choice, controlling for all other explanatory factors.

Policy simulations were then conducted for both models, as described in the fourth section. For each model, forecasts are made of the effects of providing various incentives, separately or in combination, to those persons not presently perceiving them as being available. Results indicate that if all of the incentives investigated were made available, exclusive solo driving could be reduced by up to 18 percent. Some of the specific results, such as the significant impact of a guaranteed ride home for ridesharers, contradict some earlier qualitative assessments, while other specific results confirm earlier qualitative assessments [Hwang and Giuliano (1990), Polena and Glazer (1991), Giuliano, et al. (1991)]. The final section presents conclusions.

## 2. Data Description

These data are from the first wave of a panel study of commuters in California's South Coast Air Basin. Approximately half of the survey respondents are employed in an area known as the Irvine Business Complex (IBC), a diversified employment center near Orange County Airport that had approximately 2400 employers and 55,000 employees in 1989. The remaining half of the sample is a sample of convenience comprised of commuters employed elsewhere throughout the Air Basin.

The panel, a mail survey, was instituted in 1990, and the first wave data used in this study are from the original sample and a refreshment sample introduced three months later. Prizes were offered as incentives, and the overall response rate for the first-wave mail survey was approximately fifty percent. The total sample size for the first wave is 2189 commuters, almost all of whom are employed full time. The IBC half of the sample (1123 commuters) was recruited from respondents to a cross-sectional survey conducted in 1989 for the purposes of establishing current levels of solo driving and ridesharing. The panel recruitment response rate was approximately thirty percent. The cross-sectional survey was a representative probability sample of employers, using a stratified cluster design based on a complete list of IBC business license holdings. The strata were six company types and eight company sizes. Fixed weights for the IBC subsample were generated using the stratified breakdowns by company type and size.

The first-wave panel questionnaire gathered detailed information about each respondent's most recent commuting trip to work, including mode, perceived distance, times of departure and arrival, and stops made. Respondents were also asked which other modes, if any, they used to commute to work over the previous two weeks. Thus, through retrospective questioning, the survey provides more information about mode choice than is available from the conventional single–day travel diary typically used in mode choice studies. An extended two–week travel diary would provide even more information (e.g. frequencies of mode use), but at too great a cost in terms of survey burden and resulting non–response bias.

The entire sample can be compared to the population of full–time workers in the South Coast Air Basin by using the March 1987 Current Population Survey of the U.S. Census Bureau. We statistically matched our sample to the CPS and derived estimated sampling weights. Details of the weight construction are provided in Appendix A. These weights reproduce the joint distribution of sex, race, age, number of children, and family income from the CPS sample. Therefore, the simplest way to compare our sample to the population distribution for these variables is to compare weighted and unweighted distributions. Table 1 compares the distributions for income and sex, respectively. We considerably oversample high income and female workers.

For other variables not used in weight construction, the comparison between weighted and unweighted distributions is also helpful. For these variables the weighted distributions will match the population distributions as long as our estimated weights accurately reflect the probability of finding observationally equivalent workers in the population. Table 1 also shows the weighted and unweighted distributions for the basic mode choice question (i.e. commute choice for the most recent working day). We appear to undersample bus riders, but otherwise the distributions are quite close. These results suggests that the weights, and therefore the sampling procedure, are not strongly correlated with the endogenous mode choice variable. If the weights are exogenous, then standard estimation procedures will yield consistent estimates. We employ a more sophisticated test of this hypothesis in the next section.

To provide information on perceived ridesharing incentives, respondents were

| CATEGORY | UNWEIGHTED FREQUENCY | UNWEIGHTED PERCENTAGE | WEIGHTED PERCENTAGE |
|---|---|---|---|
| INCOME | | | |
| less than $15,000 | 39 | 1.87 | 7.39 |
| $15,000 to $24,999 | 123 | 5.89 | 8.94 |
| $25,000 to $34,999 | 208 | 9.97 | 17.75 |
| $35,000 to $44,999 | 249 | 11.93 | 16.66 |
| $45,000 to $54,999 | 264 | 12.65 | 14.25 |
| $55,000 to $64,999 | 292 | 13.99 | 9.44 |
| $65,000 to $74,999 | 241 | 11.55 | 5.74 |
| $75,000 to $84,999 | 200 | 9.58 | 5.32 |
| $85,000 to $94,999 | 139 | 6.66 | 4.33 |
| $95,000 or more | 332 | 15.91 | 10.17 |
| GENDER | | | |
| Male | 1073 | 49.61 | 60.70 |
| Female | 1090 | 50.39 | 39.30 |
| MODE CHOICE ON SURVEY DAY | | | |
| Rideshare | 381 | 17.41 | 17.14 |
| Solo drive | 1748 | 79.85 | 78.30 |
| Other modes | 60 | 2.74 | 4.56 |

**Table 1: Sample Characteristics**

| INCENTIVE | PERCEIVED INCENTIVES UNWEIGHTED PROPORTION | PERCEIVED INCENTIVES WEIGHTED PROPORTION | EXOGENOUS INCENTIVES UNWEIGHTED PROPORTION | EXOGENOUS INCENTIVES WEIGHTED PROPORTION |
|---|---|---|---|---|
| Reserved parking | 0.361 | 0.350 | 0.503 | 0.498 |
| Cost subsidy | 0.143 | 0.129 | 0.324 | 0.338 |
| Guaranteed ride home | 0.093 | 0.085 | 0.264 | 0.258 |
| HOV lane available | 0.311 | 0.302 | 0.311 | 0.302 |

**Table 2: The Ridesharing Incentive Variables**

| MODE | FREQ. | MEAN | STD. DEV. | WEIGHTED MEAN | WEIGHTED STD. DEV. |
|---|---|---|---|---|---|
| Always Rideshare | 181 | 0.961 | 1.607 | 1.109 | 2.123 |
| Mixed Modes | 317 | 0.703 | 0.851 | 0.811 | 0.959 |
| Always Solo Drive | 1510 | 0.604 | 1.088 | 0.656 | 1.347 |
| Total Sample | 2008 | 0.652 | 1.116 | 0.717 | 1.383 |

**Table 3A: Travel Times (Hours)**

| MODE | FREQ. | MEAN | STD. DEV. | WEIGHTED MEAN | WEIGHTED STD. DEV. |
|---|---|---|---|---|---|
| Always Rideshare | 177 | 26.12 | 18.06 | 26.41 | 19.72 |
| Mixed Modes | 319 | 21.17 | 16.10 | 25.37 | 19.50 |
| Always Solo Drive | 1521 | 15.00 | 11.96 | 14.89 | 11.70 |
| Total Sample | 2017 | 16.95 | 13.79 | 17.40 | 14.65 |

**Table 3B: Travel Distances (Miles)**

asked to check which of a list of incentives were provided by their employer[1]. These variables represent commuters' perceptions of employer–provided ridesharing incentives, rather than employer TDM program planning statements. These perceptions are problematic because they might be endogenous with respect to choice; solo drivers uninterested in ridesharing might not notice if an incentive is provided by an employer. As exogenous estimates of the presence of ridesharing incentives, separate variables were computed where an incentive was assigned to all employees at a particular work site if at least ten percent of the employees at that work site perceived the incentive as being available. We chose the relatively low ten percent cutoff level in this construction to be sure that we captured all incentives offered by the employer. Increasing the cutoff level moves the exogenous incentive estimates closer to the perceived incentive measures. Both sets of incentive variables are listed in Table 2 for the unweighted and weighted samples.

Differences between the perceived and exogenously estimated incentive measures are greatest for the guaranteed ride home and the carpool and/or vanpool cost subsidies. Comparisons between the weighted and unweighted samples indicates a slight over–sampling of work sites which have perceived ridesharing incentives in place. With incentives defined exogenously, there is hardly any difference between the weighted and unweighted distributions.

Table 3 shows weighted and unweighted distributions of travel time and distance according to the mode choice breakdown used in the next section. They

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[1] In addition to the three ridesharing incentives studied here and an open–ended "other" category, we also collected information on reduced parking rates for ridesharers, free or on–site bus pass sales, and paid time off for ridesharers. The presence of these latter incentives is extremely rare, so they are excluded from the analysis in this paper.

show that those who rideshare face longer commute times and distances. The weights affect these distributions only slightly.

Since the sample used in this paper consists of initial and refreshment surveys carried out approximately three months apart, we check whether these data can be merged. We carried out tests for coefficient stability for a number of different models, and these tests never reject the hypothesis that the coefficients are the same for both subsamples.

## 3. Models

Previous studies have shown that a number of factors are helpful in explaining ridesharing behavior: commute time and distance, vehicle availability, household structure, income, schedule flexibility, and ridesharing incentives [Cervero and Griesenbeck (1988), Gensch (1981), Small (1983), Stevens (1990), and Teal (1987)]. People with longer commutes incur relatively less time penalty for rideshare participation, and they also gain more benefit from special HOV lane time savings and from sharing the costs and burdens of driving. We only include commute distance in our models since it is highly correlated with commute time, and commute time is endogenous since it depends on rideshare mode choice even when home and work location are fixed. The presence of additional workers in the household allows for the possibility of ridesharing with family members, which is perhaps more convenient and enjoyable than with co–workers. However, the presence of small children probably creates a need for the greater flexibility provided by solo commuting. Flexible work schedules make it more difficult to conveniently coordinate ridesharing [Cervero and Griesenbeck (1988), and Ferguson (1990)], and previous studies have found that females are more likely to rideshare

[Gensch (1981) and Teal (1987)].[2]

It has also been shown that size of the work site is important, because persons working at larger work sites have a greater opportunity to arrange ridesharing [Cervero and Griesenbeck (1988), and Ferguson (1990)]. Size of the worksite was available for only 55 percent of the sample, since we did not ask respondents for worksite size in the first wave of the panel survey. We were able to get worksite size from the sample frame for the IBC subsample. Additional information about worksite size was available from those respondents who returned the second wave survey. We used these observations where we observed worksite size to estimate a binomial probit model to impute values of the dummy variable for more than 200 employees at the worksite[3] for the remainder of the sample.

Maximum likelihood estimates of this imputation model are given in Table 4. The model fits quite well, with 78% of the cases correctly predicted in the estimation sample. The parameter estimates show that working at a large worksite is positively correlated with large households, older workers, lower incomes, longer commute distances, and the presence of employer–provided ridesharing incentives. HOV lane availability is negatively correlated with large worksite size because the majority of small worksites are located near one of the few freeways with HOV lanes. Working at a large worksite is also correlated with the endogenous dummy

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[2] While commenting on the preliminary results of this study, Sally Ann Sheridan, the Mayor of the City of Irvine, remarked that women are more likely to rideshare because they like to talk more than men.

[3] We use a dummy variable for worksite size instead of a continuous measure because respondents have trouble accurately reporting worksite (as opposed to employer) size. Experiments with the continuous measure showed no significant differences from the results using the dummy variable.

| INDEPENDENT VAR. | COEFF. | t-STAT |
|---|---|---|
| Household size | 0.0555 | 2.11 |
| Age < 26 (dummy) | -.373 | -4.55 |
| Household income | -.00249 | -1.85 |
| Managerial occupation (dummy) | 0.127 | 1.50 |
| Logarithm of commute distance | 0.00881 | 2.65 |
| If rideshare in 2 weeks (dummy) | 0.225 | 1.99 |
| Female gender (dummy) | 0.0513 | 0.61 |
| Fixed schedule (dummy) | 0.00891 | 0.10 |
| Constant | -.779 | -3.38 |
| Reserved parking (dummy) | 1.41 | 14.4 |
| Cost subsidy (dummy) | 0.686 | 3.98 |
| Guaranteed ride home (dummy) | 0.186 | 1.00 |
| Other incentive (dummy) | 0.493 | 4.47 |
| HOV lane available (dummy) | -.485 | -5.37 |

| VALUE | FREQ. | PERCENT |
|---|---|---|
| Employer size ≤ 200 | 750 | 56.31 |
| Employer size > 200 | 582 | 43.69 |

LOG LIKELIHOOD

| INITIAL | AT CONVERGENCE |
|---|---|
| -923.27 | -649.43 |

TABLE 4: Probit Model for Imputing Employer Size > 200

variable for any ridesharing in the last two weeks. Note that it is legitimate to include endogenous variables in the imputation model since we are only trying to estimate the expected value of the worksite size dummy variable conditional on the variables included in the model. The use of this model for imputation is described later in this section.

The survey allows measurement of mode choice as a 3–alternative split: always rideshare, sometimes rideshare, and always solo drive. We model this choice by an ordered probit model based on the index function,

$$Y_i^* = \beta' x_i + \varepsilon_{i}, \qquad (1)$$

where $\beta$ is a vector of unknown parameters, $x_i$ is a vector of known exogenous attributes (including a constant term) for the $i^{th}$ respondent, and $\varepsilon_i$ are independent Normal disturbances with mean zero and unit variance. The observed discrete dependent variable, $Y_i$, takes on its lowest value ("always rideshare") if $Y_i^* < 0$, its medium value ("sometimes rideshare") if $0 \leq Y_i^* < \tau$, and its highest value ("always solo drive") if $Y_i^* \geq \tau$. Under the restriction that $\tau>0$, this specification implies that:

$$\begin{aligned} &\text{Prob } (Y_i = \text{always rideshare}) = \phi\,(-\beta' x_i) \\ &\text{Prob } (Y_i = \text{sometimes rideshare}) = \phi\,(\tau-\beta' x_i) - \phi\,(-\beta' x_i) \qquad (2) \\ &\text{Prob } (Y_i = \text{always solo drive}) = 1 - \phi\,(\tau-\beta' x_i), \end{aligned}$$

where $\phi$ is the standard normal cumulative distribution function. For more general ordered probit models, see McKelvey and Zavoina (1975). The ordered probit model is more parsimonious than the unordered logit model, but its application requires some natural order in the discrete alternatives. All models are parameterized so that positive coefficients favor driving alone.

The ordered probit model given in equation (2) is typically estimated by maximum likelihood techniques[4], but we cannot use these methods because the dummy variable for employer size >200 is missing for approximately half of the sample. Since the missing observations are due to the sample design and are therefore uncorrelated with commute mode choice or employer size, we can use the observations where employer size is observed to calibrate an imputation model to predict missing values of the employer size dummy variable. We use the binomial probit model estimates given in Table 4 to predict the probability that the employer size dummy will equal 1 for each missing observation, and then impute a value of 1 if this probability is greater than .5 (and impute 0 otherwise).

Estimating the ordered probit model treating the imputed values as known leads to consistent parameter estimates but inconsistent standard errors and test statistics. We use Multiple Imputations [see Brownstone (1990) and Rubin (1987)] to consistently estimate standard errors. This methodology requires drawing multiple imputed values of the employer size dummy variable for each case where it is missing in a way that reflects the total variability of the imputation process. There are two independent sources of uncertainty in our binomial probit imputation model: the error in the parameter estimates and the error in predicting the actual value of the dummy variable from the probit probability. The estimation error is simulated by drawing from the sampling distribution of the binomial probit parameter estimates. The prediction error is simulated by imputing values of the dummy variable as draws from a binomial distribution with probability of success

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[4] All of the computations for this paper, including maximum likelihood estimation of the ordered probit model in equation 2, were carried out using the SST statistics package from Dubin/Rivers Co.

equal to the predicted probability from the probit model. For each such imputation of the missing values of the employer size dummy we compute the maximum likelihood estimate of the ordered probit model, $\hat{\beta}$, and its asymptotic covariance estimator $\hat{U}$. The final estimate of $\beta$ is given by:

$$\bar{\beta} = \sum_{i=1}^{m} \hat{\beta}_i / m \, , \tag{3}$$

where m is the number of independent imputations and $\hat{\beta}_i$ is the estimator from the $i^{th}$ imputation set. If $\bar{U}$ is the corresponding average of the covariance estimates $\hat{U}_i$ and

$$S = \sum_{i=1}^{m} (\hat{\beta}_i - \bar{\beta})(\hat{\beta}_i - \bar{\beta})' \,/\, (m-1), \tag{4}$$

is an estimate of the covariance among the m estimates for each imputation, then

$$\Omega = \bar{U} + (1 + m^{-1})S \tag{5}$$

is the estimate of the total covariance of $\bar{\beta}$. The $(1 + m^{-1})$ factor in equation 5 is to correct for small sample effects when the number of imputations is very small. Note that $\Omega$ can be interpreted as the sum of the average covariance conditional on fixed values for the missing dummy variable and the covariance across the different imputations.

The validity of the above multiple imputation procedure was checked by randomly removing 30% of the employer size observations from the IBC subsample (where employer size was observed for the entire subsample). The imputation and estimation procedures described in the previous paragraph were then used to estimate the ordered probit model described in Table 5 for the entire IBC subsample. These estimates were then compared to maximum likelihood estimates using the observed employer sizes for the entire IBC subsample. There was no more than 5% difference between any of the coefficient estimates, and the standard errors of the multiple imputation estimates were only 5% higher than the maximum likelihood results.

The first two columns of Table 5 show estimates of the ordered probit model in equation 2 with the perceived incentive variables. These estimates are computed using the multiple imputation formulas in equations 3 – 5. Except for income and fixed schedules, all non–incentive variables had the expected signs and were statistically significant at the 5 per cent level. The incentive variables all have the expected negative sign, but the coefficient on cost subsidies for ridesharers is small and not significantly different from zero. These estimates imply that the most effective incentives are the "other" category, followed closely by guaranteed rides home for ridesharers. The "other" incentives category is difficult to interpret since it includes a large number of rarely offered incentives such as free coffee and doughnuts for ridesharers.

The ordered probit model estimates for mode choice using exogenous incentive variables are listed in the last two columns of Table 5. A comparison with the ordered probit model using perceived incentives shows that there are no substantial differences in the coefficients and standard errors of the non–incentive explanatory variables between the two versions. The coefficient and standard error of HOV Lanes is also the same. Howrver, relative to the perceptual measure estimates, the coefficients of "other" incentives, cost subsidy and guaranteed ride home are substantially lower and not significantly different from zero. Conversely, the coefficient of the reserved parking incentive increased by 75%, although this change is not statistically significant. These comparisons suggest that perceptions of cost subsidies, guaranteed ride home, and "other" incentives are indeed endogenous to ridesharing choices and thus under–reported by solo drivers. However, even diehard solo drivers are forced to notice HOV lanes and reserved parking spaces for ridesharers, so they accurately report the presence of these incentives. The increase in the coefficient for reserved parking is due to the high correlations between all of the employer–provided incentive variables. Note that it is possible that the exogenous incentive measures overstate the presence of some incentives. Firms may formally offer incentives but not publicize them widely, or they may only offer them to some employees. Therefore the "true" results probably fall between the two sets of estimates given in Table 5.

To show that these estimates are unaffected by the unusual sampling scheme we use a Hausman test [see Hausman (1978)] to compare the full sample estimates in Table 5 with maximum likelihood estimates computed from the IBC subsample. Since the IBC sample is a traditional stratified random sample, the estimates from this subsample will be consistent regardless of the sampling scheme used in the remaining sample. Hausman's test statistic is given by:

$$T = (\hat{\beta} - \tilde{\beta})' \ (\tilde{\Omega} - \Omega) \ (\hat{\beta} - \tilde{\beta}) \ , \qquad (6)$$

where $\tilde{\beta}$ and $\tilde{\Omega}$ are the maximum likelihood coefficient vector and covariance matrix estimates from the IBC subsample. Under the null hypothesis that the weights (and therefore the sampling scheme) are exogenous to the mode choice, T is distributed as a Chi–squared with degrees of freedom equal to the rank of $(\tilde{\Omega} - \Omega)$, which equals

| INDEPENDENT VAR. | PERCEIVED INCENTIVES | | EXOGENOUS INCENTIVES | |
|---|---|---|---|---|
| | COEFF. | t-STAT | COEFF. | t-STAT |
| Household size | -.0570 | -2.53 | -.0580 | -2.58 |
| Household workers ≥ 2 (dummy) | -.736 | -7.27 | -.758 | -7.49 |
| Household income | .00133 | 1.13 | .00143 | 1.20 |
| Logarithm of commute distance | -.376 | -8.57 | -.382 | -8.76 |
| Household cars ≥ 2 (dummy) | .512 | 4.62 | .501 | 4.53 |
| Female gender (dummy) | -.224 | -3.36 | -.218 | -3.29 |
| Fixed schedule (dummy) | .0120 | 0.11 | .0842 | 0.77 |
| Employment site > 200 (dummy) | -.379 | -2.86 | -.306 | -2.25 |
| Site > 200 *and* fixed schedule (interaction dummy) | 0.199 | 1.37 | .0978 | 0.66 |
| Constant | 3.41 | 15.0 | 3.42 | 14.8 |
| Reserved parking (dummy) | -.161 | -1.92 | -.289 | -3.12 |
| Cost subsidy (dummy) | -.0838 | -0.83 | .0490 | 0.52 |
| Guaranteed ride home (dummy) | -.236 | -2.10 | -.122 | -1.28 |
| Other incentive (dummy) | -.295 | -3.50 | -.0142 | -0.16 |
| HOV lane available (dummy) | -.183 | -2.64 | -.170 | -2.43 |
| Threshold ($\tau$ in eqn. 2) | .771 | 19.0 | .766 | 19.0 |

| MODE | FREQ. | PERCENT |
|---|---|---|
| Rideshare | 165 | 8.67 |
| Mixed Mode | 301 | 15.81 |
| Solo Drive | 1438 | 75.53 |

**TABLE 5: Ordered-Response Probit Choice Models (Positive coefficients favor solo driving)**

16 in our application.[5] The significance value of T computed using the perceived incentives specification in Table 5 is .83, which shows that there is no significant evidence against our use of unweighted estimators. Similar results are obtained using the exogenous incentives.

Although we do not need to weight the sample for estimation purposes, this does not imply that the sample is representative. It just means that our models include enough variables, such as income, household size, and sex, to control for its unrepresentativeness. The sampling weights are still needed to generate consistent population predictions from these models, which is done in the next section.

Another specification issue is whether the models can be further simplified into a binary choice between always driving alone and ridesharing at least sometimes. In the context of the ordered probit model, this is equivalent to the hypothesis that the Threshold ($\tau$ in equation 2) is equal to zero. Since this hypothesis is rejected at any reasonable level, we conclude that the three–alternative model is necessary to represent these data.

## 4. Policy Predictions

The parameterization of disaggregate discrete choice models makes it difficult to interpret coefficient estimates for policy purposes. This section provides policy

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[5] Note that this distribution for the Hausman test is only valid if $\bar{\beta}$ is asymptotically efficient. Although $\bar{\beta}$ is not fully efficient here, the results from the experiment described in the text using the IBC subsample suggests that the inefficiency is quantitatively small, so the limiting Chi–squared distribution of T should be a close approximation in our application.

simulations from the ordered probit models in Table 5. These simulations are useful both as an aid to understanding the model estimates and for their policy implications. We consider the effects of giving all respondents one or all of the following ridesharing incentives: reserved parking, subsidized rideshare costs, guaranteed ride home, and HOV lanes. In our HOV lane simulations we only include those commuters who currently use some freeway during their commute.

Population predictions are derived from our model by assuming that each respondent represents $\omega_i$ observationally equivalent people in the population, where $\omega_i$ is the inverse sampling probability (weight) for the $i^{th}$ respondent. Therefore the choice probability $P_{ij}$ is the proportion of these people who choose discrete alternative j. The population prediction for the number of people choosing alternative j is then given by:

$$D_j = \sum_i \omega_i P_{ij}(\beta, x_i) \tag{7}$$

Our estimates are derived by replacing the unknown parameters $\beta$ by their estimates from Table 5. Policy simulations are carried out by comparing the $D_j$ for different values of the policy variables in $x_i$.

There are two sources of error in our estimates of $D_j$: the estimation errors in the parameters $\beta$ and the sampling weights $\omega_i$. Conditional on the sampling weights, the variance in $\hat{D} = (D_1(\hat{\beta}), D_2(\hat{\beta}), D_3(\hat{\beta}))$ can be estimated by:

$$\hat{U} = \hat{D}_\beta' \hat{V} \hat{D}_\beta , \tag{8}$$

where $\hat{D}_\beta$ is the matrix of first derivatives of D with respect to $\beta$ evaluated at $\hat{\beta}$ and $\hat{V}$ is a consistent estimator of the covariance of $\hat{\beta}$ [see Chow (1983) Pp. 182–183 for more details].

We do not have an explicit model of the variability of the sampling weights $\omega_i$, so we use Multiple Imputations [see Section 3 of this paper, Brownstone (1990), and Rubin (1987)] to estimate their contribution to the covariance of $\hat{D}$. The application of Multiple Imputations to this problem requires that we be able to draw multiple sampling weights for each respondent in a way that reflects the variability of the weight estimation process. Appendix A describes the procedures we use to draw 9 such weight vectors. For each weight vector we compute $\hat{D}$ and its covariance estimator $\hat{U}$. The final estimate of D is given by:

$$\bar{D} = \sum_{i=1}^{m} \hat{D}_i/m \, , \tag{9}$$

where m (=9) is the number of simulated weight vectors and $\hat{D}_i$ is the estimator for the $i^{th}$ weight vector. If $\bar{U}$ is the corresponding average of the covariance estimates $\hat{U}_i$ and

$$B = \sum_{i=1}^{m} (\hat{D}_i - \bar{D})\,(\hat{D}_i - \bar{D})' \,/\, (m-1), \tag{10}$$

is an estimate of the covariance among the m estimates for each weight vector, then

$$\Psi = \bar{U} + (1 + m^{-1})B \tag{11}$$

is the estimate of the total covariance of $\bar{D}$. Note that $\Psi$ can be interpreted as the sum of the average covariance for a fixed weight vector and the covariance across the weight vectors.

The results of the policy simulations and standard errors computed from the above formulas are given in Table 6 for the models using both perceived and exogenously estimated incentives. The results in both tables are presented in terms of percentage changes from the baseline predictions ($\bar{D}$) computed at initial $x_i$ values with the perceived incentive measures (see Table 2 for the proportions of commuters who perceive exposure to each incentive). Each row gives the predicted change from offering the particular incentive to all persons in the sample who do not already perceive having it. Table 6 shows results from identical policy simulations; the only difference between them is the different parameter estimates used in calculating the predictions from equation 7. For HOV lanes, only commuters using at least one freeway are considered to be potential recipients. Thus our simulations assume that there is no rerouting of commute travel as a result of changing modes. The "All Incentive" row corresponds to giving all persons all four incentives simultaneously.

Reserved parking is the only single employer-provided incentive which, taken alone, yields a significant reduction in solo driving whether measured either perceptually or exogenously. Guaranteed ride home is significant for the perceived measure, but this is contradicted by the results for the exogenous measures and those in Hwang and Giuliano (1990) and Polena and Glazer (1991). The effectiveness of HOV lanes is limited because they are only available to those who use a freeway during their commute. Nevertheless, the simulations forecast that additional HOV lanes would produce a significant decrease (either 2.6 or 2.3 percent) in the proportion of commuters always solo driving. This result is consistent with the results in Giuliano, Levine, and Teal (1990) and Golob, et al. (1991), where 3 to 4 percent shifts from solo driving to ridesharing were observed subsequent to the opening of HOV facilities in Southern California.

It is clear that the perceived measures understate the presence of some incentives, including "other," cost subsidy, and guaranteed ride home. Conversely, it is possible that the exogenous incentive measures overstate the presence of some

| VARIABLE | ALWAYS RIDESHARE | | MIXED MODES | | ALWAYS SOLO DRIVE | |
|---|---|---|---|---|---|---|
| | CHANGE | STD.ERR | CHANGE | STD.ERR | CHANGE | STD.ERR |
| Reserved parking | 14.8% | 8.6% | 9.2% | 10.9% | -3.5% | 1.8% |
| Cost subsidy | 11.3% | 8.3% | 6.4% | 10.7% | -2.5% | 3.0% |
| Guaranteed ride | 37.4% | 10.3% | 19.2% | 12.0% | -8.0% | 3.8% |
| HOV lanes | 13.0% | 8.5% | 5.8% | 10.6% | -2.6% | 0.9% |
| All incentives | 91.7% | 14.4% | 39.6% | 14.1% | -17.9% | 4.3% |
| BASELINE PREDICTIONS | $3.09 \times 10^7$ | | $5.75 \times 10^7$ | | $2.84 \times 10^8$ | |

**TABLE 6A: Policy Predictions Based on Perceived Incentives**

| VARIABLE | ALWAYS RIDESHARE | | MIXED MODES | | ALWAYS SOLO DRIVE | |
|---|---|---|---|---|---|---|
| | CHANGE | STD.ERR | CHANGE | STD.ERR | CHANGE | STD.ERR |
| Reserved parking | 29.2% | 14.3% | 17.6% | 13.1% | -6.2% | 1.9% |
| Cost subsidy | -6.4% | 10.3% | -3.9% | 10.7% | 1.3% | 2.5% |
| Guaranteed ride | 19.0% | 13.2% | 10.4% | 12.2% | -3.8% | 3.0% |
| HOV lanes | 12.5% | 12.4% | 5.8% | 11.7% | -2.3% | 0.9% |
| All incentives | 59.4% | 17.7% | 29.9% | 14.5% | -11.4% | 3.1% |
| BASELINE PREDICTIONS | $2.81 \times 10^7$ | | $5.48 \times 10^7$ | | $2.90 \times 10^8$ | |

**TABLE 6B: Policy Predictions Based on Exogenously Estimated Incentives**

incentives. Firms may formally offer incentives but not publicize them widely, or they may only offer them to some employees. Therefore the perceived measure results probably overstate the effectiveness of ridesharing incentives, while the exogenous measure results may understate their effectiveness. The simulation results in Table 6 should therefore be interpreted as upper and lower bounds on the actual effects.

These policy simulations are partial equilibrium calculations conducted holding all of the non–incentive variables in the choice models fixed. Since in reality a decrease in drive alone commuting will decrease congestion and therefore decrease travel time and thus the time benefits of ridesharing, the effects in Table 6 are probably overestimates of the final equilibrium results. This caveat applies particularly to the "All Incentive" predictions.

The results in Table 6 were also computed using weights fixed at the average of all the weight vectors. The standard errors only changed in the third decimal place, which shows that the effects of uncertain sampling weights are not very important for this application.

The same simulations were also computed for the IBC subsample. The predictions are very similar to the ones for the full sample, except that the magnitude of the coefficients for the rideshare cost subsidy and HOV lane incentives are slightly higher. Relative to the full sample, employees in the IBC seem to respond slightly more to ridesharing incentives, but the differences are not statistically significant.

## 5. Conclusions

The principal results of this analysis are that employer–provided preferential parking and the presence of freeway HOV lanes are significant explanators of the choice between solo driving and ridesharing. These results are consistent across two subsamples based on employment location and for two different measures of incentive availability. Simulation results for the South Coast Air Basin show that providing these incentives to all commuters, together with rideshare cost subsidies and guaranteed rides home for ridesharers, can significantly reduce solo commuting. If all employees who do not report the presence of all incentives were to receive them, our simulations predict that solo driving would be reduced by between 11 and 18 percent.

These results are based on an ordered–response probit model with choice categories "always rideshare," "mixed mode," (rideshare and solo drive) and "always solo drive." Significant non–incentive explanatory variables in this model are: household size, logarithm of commuting distance, and dummy variables for more than one worker households, two or more car households, gender, and employer size greater than 200. Income and having a fixed work schedule were not significant in any of our models. The ridesharing incentives are measured in two ways to account for the possibility that employees may under or over report the presence of employer–provided incentives.

The forecasts assume that there are no constraints to prevent choice of ridesharing by solo drivers, beyond the predictive effects of the included explanatory variables. They also do not account for general equilibrium effects such as an overall reduction in driving time caused by a substantial shift from solo driving. Therefore these policy simulation results should be viewed as upper bounds on the effectiveness of ridesharing incentives. Further research on specific definitions of employer–offered incentives is needed to refine these forecasts.

## Appendix A: Census Weight Construction

The idea behind our sampling weight construction is to estimate the probability that a person from our survey would be selected in a simple random sample of the greater Los Angeles area. We use all the full time workers (2917 observations) in three Metropolitan Statistical Areas: Los Angeles–Long Beach, Anaheim–Santa Ana, and Riverside–San Bernardino, from the March, 1987 Current Population Survey (CPS). Since the sampling weights are given for each observation in the CPS, we need to match each respondent in our survey to a group of respondents in the CPS. We do this by creating "bins" in both the CPS and our survey sample which have roughly the same sex, race, family structure, age, and income. We then assume that the respondents in our survey have the same sampling weights as those in the corresponding CPS bin. The weights which result from this process have the property that the weighted distribution of the variables used in the matching process should be similar in both the CPS and our survey.

The bin numbers are generated as follows. First, numerical values are assigned to each category of sex, race, with or without children under 6 years old, with or without children from 7 to 15 years old, age, and family income. The categories are as follows: Sex: male–1 female–2, Race: white–1 black–2 others–3, Children under 6 years old: without–1 with–2, Children from 7 to 15 years old: without–1 with–2, Age: 24 years old and younger–1, 25 to 34–2, 35 to 44–3, 45 to 54–4, 55 to 64–5, 65 and older–6. Family income is coded according to Table A1. Bin Numbers are generated using: Bin Number = Sex*100,000 + Race*10,000 + Child6*1,000 + Child15*100 + Age*10 + Family income.

The bin numbers for the survey sample (2189 observations) are made following the same procedure as above, except for income where the categories do not exactly match. Survey income category 8 is matched to census category 7, and survey income category 7 is randomly allocated to census categories 6 or 7 by drawing a random number proportional to the overlap between the intervals. Missing responses (50 in sex, 81 in race, 49 in age, 125 in family income) are assigned random numbers within the category, for example, by picking uniform random numbers from 1 to 8 for income.

Survey bin numbers are matched to census bin numbers. If there are survey bin numbers which do not appear in the census bins, the survey income category numbers are moved one up or one down. If the bin numbers still do not match, the age category numbers are moved one up or one down. If there are bin numbers that still do not match, the same category shifting process is done for the child category numbers, until all the observations are matched to census bins. Once all survey respondents have been matched to a census bin, the survey respondents are given a weight which is a random draw from the set of weights in the census bin. These weights are then rescaled so that the sum of the weights in each survey bin matches the sum of the weights in the corresponding census bin. This ensures that the total weights are the same for the same census and survey bins.

Eight more sets of census weights are produced by using the same procedure except the last bin matching step. Instead, if there are survey bin numbers which do not appear in census bin numbers, the category numbers of income and age of survey bins are replaced by the numbers drawn randomly from the category numbers of income and age of census bins. If there are survey bin numbers that still do not match, the same replacement procedure of the category numbers is done for child15 and child6.

| CENSUS CODE | CENSUS INCOME IN 1987 DOLLARS | CENSUS INCOME IN 1989 DOLLARS | SURVEY INCOME IN 1989 DOLLARS | SURVEY CODE |
|---|---|---|---|---|
| 1 | less than $12,499 | less than $14,511 | less than $15,000 | 1 |
| 2 | $12,500 to $19,999 | $14,512 to $23,219 | $15,000 to $24,999 | 2 |
| 3 | $20,000 to $29,999 | $23,220 to $34,829 | $25,000 to $34,999 | 3 |
| 4 | $30,000 to $39,999 | $34,830 to $46,439 | $35,000 to $44,999 | 4 |
| 5 | $40,000 to $49,999 | $46,440 to $58,049 | $45,000 to $54,999 | 5 |
| 6 | $50,000 to $59,999 | $58,050 to $69,659 | $55,000 to $64,999 | 6 |
| 7 | $62,000 or more | $69,660 or more | $65,000 to $74,999 | 7 |
| | | | $75,000 or more | 8 |

TABLE A.1: Household Income Codes

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