The Problem of Measurement Error in Self-Reported Receipt of Child-Care Subsidies: Evidence from Two States

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ABSTRACT Researchers frequently rely on survey responses to determine whether families receive government assistance and to study the effects of government programs, but these responses are often inaccurate. This study investigates misreporting in the child-care subsidy program by comparing survey responses on child-care subsidy receipt with program administrative data in two states. While we find a lower rate of misreporting than is typical for other government assistance programs, overreporting of benefit receipt is surprisingly common and generates overestimates of program participation. Analyses further suggest that the frequency and systematic nature of misreporting bias estimates of the predictors of program receipt and the effects of the program. These findings illustrate the necessity of assessing the frequency of response errors and understanding their implications in generating valid research results on the effects of government programs.

INTRODUCTION

Surveys are a vital source of data for studying government assistance programs and assessing programs' effects on participants' outcomes. The quality of survey data, especially the validity of responses about receipt of government assistance, therefore has important consequences for the quality and accuracy of research on such programs. Underreporting of benefit receipt is a common problem in surveys of participation in programs targeted to low-income families such as Temporary Assistance for Needy Families (TANF), the Supplemental Nutrition Assistance Program (SNAP, or food stamps), the

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Special Supplemental Nutrition Program for Women, Infants, and Children (WIC), and Medicaid (Bollinger and David 1997; Bound, Brown, and Mathiowetz 2001; Bitler, Currie, and Scholz 2003; Klerman, Ringel, and Roth 2005; Lynch et al. 2008; Meyer and Goerge 2011; Call et al. 2013). While underreporting of government assistance receives more attention in the literature, overreporting, even when relatively infrequent, can lead to overestimates of the program participation rate (Moore, Marquis, and Bogen 1996). In addition, systematic measurement problems (measurement error related to covariates) can substantially alter the estimates of program effectiveness and lead to erroneous policy conclusions. For example, a recent study of the SNAP program argues that the finding in previous studies that food stamps are associated with increased food insecurity is driven by misreporting (Gundersen and Kreider 2008).

Despite the increased importance of child-care subsidies as a work support since the 1996 welfare program changes, much less is known about misreporting of child-care subsidy receipt than about misreporting for other major government programs. Two previous studies examine the accuracy of reporting for the child-care subsidy program by comparing parent and child-care provider reports of subsidy receipt, and they find evidence that survey responses are generally consistent with each other (Bowman et al. 2009; Johnson and Herbst 2013). Although such comparisons are valuable, the studies recognize that both parents and child-care providers may report with error. To date, no study has linked and compared survey responses and administrative data on child-care subsidies. Our key contribution is to link survey respondents to administrative microdata in two states, Maryland and Minnesota, allowing for a comparison of survey responses to program benefit receipt data. We illustrate the extent to which systematic response errors may bias estimates of subsidy receipt and program effects by comparing models using administrative and survey data. We model both the predictors of subsidy receipt and the predictors of employment including subsidy receipt as a covariate.

BACKGROUND

CHILD-CARE SUBSIDIES

The current system of child-care subsidies for low-income families was instituted in 1996 with the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), when several child-care programs were consolidated into one block grant, the Child Care and Development Fund (CCDF). At the same time, funding for child-care subsidies, via CCDF, was greatly increased. In the 2012 fiscal year, federal and state CCDF spending on child-care subsidies for low-income families totaled $11.4 billion (Matthews and Schmit 2014). The CCDF allocation is comparable to TANF's $11.1 billion in state and federal expenditures for assistance (Administration for Children and Families, US Department of Health and Human Services 2011). Each month in federal fiscal year (FFY) 2012 there were 1.5 million children served by CCDF on average (Matthews and Schmit 2014). The program has dual objectives of supporting families' progress toward economic security and promoting child-care quality ("Child Care and Development Fund (CCDF) Program; Proposed Rule" 2013; US Senate Committee on Health Education Labor and Pensions 2015). As a major support program for low-income families, the CCDF program's effects on family and child outcomes are of great interest to researchers and policy makers.

Like most states, the two states in this study, Maryland and Minnesota, distribute child-care subsidies through certificates or vouchers that eligible families use to pay for child care. Obtaining a child-care subsidy begins with an application to the subsidy program, usually through a county social services office or, in some places, a child-care resource and referral agency.[1] Once eligibility is determined, the applicant chooses a child-care provider. The child-care provider bills the subsidy program for the authorized services. Once a subsidized arrangement has been selected, families may have no further contact with the subsidy program until something changes (e.g., the child-care arrangement or family income) or the applicant needs to recertify eligibility. The subsidy program's direct payment from the government to child-care providers may affect recipients' knowledge of their benefits and familiarity with the program name. Benefits such as SNAP or WIC, on the other hand, use a benefits card or coupon, which may remind recipients of the program's name and their receipt of benefits.

In Maryland, child-care subsidies are also called Purchase of Care (POC) vouchers, and families with income below 50 percent of the state median are eligible for the vouchers. In Minnesota, child-care subsidies are distrib-

1. While some states offer online applications (Adams and Matthews 2013), at the time of the study, Minnesota and Maryland did not; paper applications and documentation were required, which could be submitted in person or by mail.

uted through the Child Care Assistance Program (CCAP). Families may be eligible for the CCAP basic sliding fee program if the family's income is below 47 percent of the state median income or if they are part of the Minnesota Family Investment Program (MFIP, Minnesota's version of TANF) or the Diversionary Work Program.

Given its importance as a support for low-income families, it is not surprising that there is a substantial body of research on child-care subsidies. Studies examine the predictors of subsidy receipt (Meyers and Heintze 1999; Tekin 2005, 2007; Durfee and Meyers 2006; Blau and Tekin 2007; Herbst 2008; Johnson, Martin, and Brooks-Gunn 2011), as well as the effects of child-care subsidies on child and family outcomes. A particular focus of the literature is the effect of subsidy on parental employment (Meyers, Heintze, and Wolf 2002; Blau and Tekin 2007; Tekin 2007; Ahn 2012). There are also studies of how subsidy receipt is related to child-care choices (Tekin 2005; Weinraub et al. 2005; Ertas and Shields 2012), child-care quality (Rigby, Ryan, and Brooks-Gunn 2007; Ryan et al. 2011; Johnson, Ryan, and Brooks-Gunn 2012), child development and school readiness (Herbst and Tekin 2010; Forry, Davis, and Welti 2013; Johnson et al. 2013), and child and maternal well-being (Herbst and Tekin 2011*a*, 2012, 2014; Healy and Dunifon 2014). While these studies use a variety of methods and data sources, survey data are the most common data source for studies of the child-care subsidy program.[2] The validity of these data as an accurate measure of benefit receipt is therefore of importance to research and policy.

VALIDITY OF SURVEY REPORTS OF GOVERNMENT BENEFITS

There is a large body of literature demonstrating substantial response errors, usually underreporting, in surveys about receipt of government benefits, especially assistance targeted to low-income families. The estimated differences in monthly participation rates between administrative data and survey responses (often referred to as net underreporting) across 10 different

2. For instance, the Early Childhood Longitudinal Studies (ECLS-K and ECLS-B), the Survey of Income and Program Participation (SIPP), the National Survey of America's Families (NSAF), and the National Household Education Survey have all been used in studies of child-care subsidies. In addition, the National Survey of Early Care and Education (NSECE) was recently completed and included questions about child-care subsidy receipt.

government transfer programs range from about 10 percent for Social Security Old Age and Survivor's Insurance (OASI) to more than 50 percent for Worker's Compensation (Meyer, Mok, and Sullivan 2009).[3] Instead of measuring net misreporting, Kathleen Call and colleagues (2013) quantify misreporting by conditioning on receipt in administrative data, and they find that persons on Medicaid report benefit receipt between 57 percent and 89 percent of the time, depending on the study and the survey. Although underreporting is the predominant problem, overreporting (stating that one receives a benefit when one in fact does not) also occurs and may offset underreporting in net reporting rates. Jeffrey Moore and colleagues (1996) find that rates of overreporting for AFDC, Food Stamps, Unemployment Insurance, and Supplemental Security Income range from 1 percent to 4 percent using the Survey of Income and Program Participation (SIPP). Michael Davern and colleagues (2009) assess Medicaid reporting, and they find that overreporting partially offsets the rate of underreporting (41 percent) to generate a net underreporting rate around 31 percent. In sum, while underreporting is the more frequent problem, overreporting of benefits should also be considered as a threat to survey validity.

It is important to consider why people misreport. Survey response errors could occur due to either intentional or unintentional misreporting by respondents. Memory or recall effects may lead to unintentional mistakes. The literature on the cognition of misreporting tends to show that the greater the length of the recall period, the greater the error in responses (Bound et al. 2001). Difficulties correctly identifying a program can contribute to misreporting, especially overreporting. For instance, individuals may confuse the benefit they are asked about with other programs or other types of assistance (Klerman et al. 2005; Davern et al. 2009; Call et al. 2013), a problem referred to as benefit confusion. Marietta Bowman, Rupa Datta, and Ting Yan's (2010) report on cognitive testing for a question about child-care subsidy receipt finds that respondents' understanding of the subsidy question was generally in line with the intended meaning. However, respondents' understanding of the word *subsidy* was somewhat variable, and

3. Five nationally representative surveys were used, including the Current Population Survey (CPS), SIPP, the Panel Study of Income Dynamics (PSID), the American Community Survey (ACS), and the Consumer Expenditure Interview Survey (CE Survey [Meyer et al. 2009]).

questions referencing help from a welfare office (a common way to ask about subsidy) confused some respondents. The report suggests that using the local program or office name could substantially improve the accuracy of reporting of child-care subsidy receipt. Respondents might also misreport intentionally. Social desirability bias leads individuals to underreport undesirable behaviors and overreport desirable behaviors (Bound et al. 2001). Benefit misreporting is often related to respondent characteristics (see, e.g., Bollinger and David [1997] or Meyer and Goerge [2011]).

All of these different sources of error could contribute to response biases in reports of child-care subsidy receipt, but no study to date has validated child-care subsidy reporting with administrative data. In describing their samples, some studies compare survey rates of subsidy take-up to population participation rates based on administrative data (Blau and Tekin 2007; Herbst and Tekin 2011*b*). Similar rates could, however, hide offsetting misreporting on the micro level.

Two studies compare child-care subsidy receipt using parent and child-care provider survey data. Marietta Bowman and colleagues (2009) compare subsidy receipt responses for 43 parents to that of their child-care providers. They find a 75 percent agreement rate and that underreporting by parents was the predominant form of disagreement. Anna Johnson and Chris Herbst (2013) find an overall agreement rate of 78 percent between parents and providers. They also identify a number of possible subsidy-specific measurement issues, including the fact that parents may not know exactly which funding stream is paying for care, whether a copayment is the full cost, or if the provider is paid directly by the subsidy program.

Johnson and Herbst (2013) also compare models predicting subsidy receipt using both parental and provider reports, and they find few systematic differences between the models. They model whether disagreement between parents and providers on subsidy receipt is related to respondent or child characteristics, and they find disagreement to be relatively random. However, Johnson and Herbst acknowledge that providers as well as parents may misreport due to recall error, uncertainty about which families or which children within a family receive subsidies, or confusion with regard to different funding streams. Providers may also be reluctant to share information about their finances, including subsidy income. Johnson and Herbst (2013) indicate that an important future direction for research is to compare parent survey data and administrative data, as well as to quantify how errors in subsidy reports from survey data generate erroneous estimates of the effect of subsidy on other outcomes.

MEASURING MISREPORTING

One approach to measuring misreporting is to estimate the proportion of cases for which two data sources disagree (e.g., Johnson and Herbst 2013). However, the probability of disagreement is affected by the population rate of participation as well as the rates of underreporting and overreporting (Bound et al. 2001). An alternative approach is to measure the net underreporting (or overreporting) rate as the difference between the proportion of the sample reporting benefit receipt in survey data and in the administrative data. Such net rates can disguise substantial offsetting misreporting on the individual level.

A third approach, and the one we employ, is to assess the conditional probabilities of accurate reporting assuming that administrative data are the authoritative source of information.[4] We denote the administrative value of subsidy as y^*, which takes on the values of zero (no subsidy) or one (subsidy receipt), and we denote the survey value of subsidy receipt as y. We examine whether someone receiving a subsidy in the administrative data reported not receiving a subsidy in the survey (underreporting, $\pi_{01} \equiv \Pr(y = 0 \mid y^* = 1)$), or alternatively whether someone not receiving a subsidy in the administrative data reported that they were receiving a subsidy in the survey (overreporting, $\pi_{10} \equiv \Pr(y = 1 \mid y^* = 0)$).[5] This approach allows us to distinguish between overreporting and underreporting, not just disagreement, on the micro level.

4. Treating the administrative data as true is common in research on misreporting (Klerman et al. 2005; Meyer et al. 2009; Meyer and Goerge 2011). As we discuss below, it is possible that there are some errors in the administrative data. But individuals have to provide identification to receive child-care subsidies, and the data are based on payments from states' subsidy programs, so they are relatively reliable. We discuss implications of administrative data errors in the limitations section.

5. The terminology of *overreporting* and *underreporting* is not standardized in the literature. *Errors of commission* and *omission* are also common terms equivalent to overreporting and underreporting. Additionally, the terms *false positive* and *false negative* are also often used for the same concepts we term overreporting and underreporting. However, below we use false positive and false negative in a different sense, conditioned on survey response, a use akin to their use in the medical literature.

IMPLICATIONS OF MISREPORTING FOR SURVEY ESTIMATES

The probabilities of accurate reporting in the survey depend on the probability of program receipt at the population level, denoted π, as well as the rates of overreporting and underreporting (Aigner 1973; Baker, Stabile, and Deri 2004). The conditional probability that the administrative data is yes given that the survey response is no (a false negative) is given by

$$\Pr(y^* = 1 \mid y = 0) = \frac{\pi_{01}\pi}{\pi_{01}\pi + \pi_{00}(1-\pi)}. \tag{1}$$

The conditional probability that the administrative data is no given that the survey response is yes (a false positive) is

$$\Pr(y^* = 0 \mid y = 1) = \frac{\pi_{10}(1-\pi)}{\pi_{10}(1-\pi) + \pi_{11}\pi}. \tag{2}$$

In cases where less than half the population receives a program benefit ($\pi < .5$), false negatives are less likely than false positives even if $\pi_{01} = \pi_{10}$ (equal underreporting and overreporting); that is, $\Pr(y^* = 1|y = 0) < \Pr(y^* = 0|y = 1)$.

THE PROBLEM OF SYSTEMATIC PATTERNS IN MISREPORTING

When using survey data on program receipt to estimate multivariate models, misreporting is particularly problematic if it is correlated with other variables in the model. Then all the estimated coefficients will be biased, with the size and direction of bias a function of the correlation between the measurement error and the other variables (Bound et al. 2001). We therefore test whether differences in reported subsidy receipt across the data sources are related to any respondent characteristics, modeling underreporting and overreporting separately, as relationships between different types of misreporting and covariates are likely to vary.

CONSEQUENCES WHEN THE DEPENDENT VARIABLE IS MISMEASURED

What are the consequences for empirical models in which mismeasured program receipt is the dependent variable, for instance, when the predictors of subsidy receipt are being estimated? In contrast to the case with continuous variable models, because program receipt is a binary variable, biased coefficient estimates will result even if the measurement error in subsidy receipt (the dependent variable) is independent of covariates (Hausman, Abrevaya, and Scott-Morton 1998; Bound et al. 2001). Because subsidy receipt is always zero or one, there is a negative correlation between the error and the true value (Bound et al. 2001).[6] This correlation means that measurement error in a binary covariate is not classical measurement error. If subsidy receipt is measured with random error, the coefficient estimates for predictors of subsidy receipt will be biased as a function of overreporting and underreporting (Bound et al. 2001):[7]

$$\frac{\partial \Pr(y=1 \mid x)}{\partial x} = [1-(\pi_{01}+\pi_{10})]\frac{\partial \Pr(y^{*}=1 \mid x^{*})}{\partial x^{*}}. \tag{3}$$

However, if misreporting is systematically related to the covariates, the estimated coefficients in a model with subsidy receipt as the dependent variable can be biased in either direction.

CONSEQUENCES WHEN SUBSIDY RECEIPT IS A COVARIATE

In numerous studies estimating the effects of child-care subsidies on child and family outcomes, subsidy receipt is used as an independent variable (a covariate).[8] In the case where the mismeasured subsidy variable, now denoted x since it is a covariate, is the only covariate, then the estimated coefficient, β, is a function of the true coefficient β^* and the rates of false positives and false negatives (Aigner 1973; Bound et al. 2001):

$$\beta = \beta^{*}[1-(\Pr(x^{*}=1 \mid x=0)+\Pr(x^{*}=0 \mid x=1))]. \tag{4}$$

If, for instance, both underreporting and overreporting are 25 percent (a plausible estimate from the literature), and if true subsidy receipt increases

6. If $y^* = 1$, then $y - y^* \leq 0$, and likewise if $y^* = 0$, then $y - y^* \geq 0$.

7. Although instrumental variable techniques can be used to correct for random measurement error when a continuous variable is mismeasured, instrumental variable techniques cannot be used in this case because measurement errors in binary variables are mean-reverting and correlated with the true value (Bound et al. 2001).

8. Studies examining the effect of subsidy receipt on various outcomes frequently use survey data (Meyers et al. 2002; Tekin 2005, 2007; Weinraub et al. 2005; Blau and Tekin 2007; Rigby et al. 2007; Herbst and Tekin 2010, 2011*a*, 2012, 2014; Ryan et al. 2011; Ahn 2012; Ertas and Shields 2012; Johnson et al. 2012; Forry et al. 2013; Johnson et al. 2013).

the probability of employment by 25 percentage points, the estimated effect would be only a 12.5 percentage point increase (using the numbers in this example). Biases become even more problematic and complex when misreporting is systematic and more covariates are included in the model.

DATA

SURVEY DATA

The survey data used in this study are from two similar surveys of families in Maryland and Minnesota, the Minnesota Child Care Choices Study (Tout et al. 2011) and the Maryland Child Care Choices Study (Goldhagen et al. 2013). The surveys sampled families with low incomes who had one or more children age 6 or younger. Potential survey respondents were identified when they applied to receive assistance (such as welfare or child-care subsidies) through their county's social services office and lived in one of the participating counties. The surveys were designed to target families who would be likely to be eligible for child-care subsidies.[9] In Minnesota, once potential survey respondents were identified at the county social services office, they were given packets of information on the study and asked if they wanted to participate. Initially, 437 families consented to participate in the survey.[10] Those who consented were subsequently contacted to complete the survey. Of the 437 families who consented, 323 (74 percent) completed the baseline interview.[11] In Maryland, there were 512 families who were initially recruited, and 289 (56 percent) ultimately completed the baseline interview.[12]

9. This sampling strategy generated a sample of subsidy recipients that receives welfare at somewhat higher rates than subsidy recipients overall. Comparisons between the reported characteristics of our sample (app. table A1) and those entering the subsidy system in the two states demonstrates that while slightly less than half of all subsidy entrants (47 percent in Minnesota, 43 percent in Maryland [Davis, Krafft, and Tout 2014; Davis et al. 2015]) received welfare benefits, around two-thirds (68 percent in Minnesota, 67 percent in Maryland) of our sample of subsidy recipients did so. These differences should be kept in mind when considering the generalizability of our findings.

10. Consent could occur either at the time in the office or by calling the study phone number later.

11. The reduction in sample from 437 to 323 in Minnesota is because 16 were not ultimately eligible, 24 refused to participate, and 74 could not be reached by telephone.

12. Among the 512 families recruited, ultimately 33 were not eligible, 10 refused, and 15 turned in consent forms too late (after the recruitment window closed).

The purpose of the surveys was to examine child-care decision making for low-income families, with a particular focus on the resources and supports that can help families access different care options. The same sampling strategies and survey firm were used in Minnesota and Maryland, and the questionnaires had many identical items. The surveys were conducted by telephone, and they collected data on families' characteristics, child-care use, and how families paid for child care—including whether they received assistance through subsidies. Questions were asked specifically in reference to the "focal child," a child age 6 or younger at the time of the survey. The survey respondent was the person with the most knowledge of the focal child's care arrangements, usually the mother.[13] The surveys were longitudinal, but we use only the baseline surveys for both Minnesota and Maryland to preclude any issues of nonrandom attrition. The Minnesota baseline survey was conducted between August 2009 and July 2010. The Maryland baseline survey was conducted from July 2011 to October 2012. Further details on the surveys can be found in Tout et al. (2011) and Goldhagen et al. (2013).

The key variable of interest in the surveys is respondents' report of child-care subsidy receipt for the focal child. The question about subsidy in both the Maryland and Minnesota survey was the same:[14] "I am going to read a list of sources that might help you pay for [FOCAL CHILD]'s child care. Please say yes or no to indicate whether you *currently* get help from this source." For Maryland, one of the response options was "Child-care subsidy program/Purchase of Care (POC) vouchers." In Minnesota, the equivalent response was "The County Child Care Assistance Program or CCAP." We consider a yes response to these questions to indicate child-care subsidy receipt. It is noteworthy that the survey questions explicitly used the name of the relevant program in the state in the response options, which is likely to improve the accuracy of responses (Call, Davern, and Blewett 2007; Bowman et al. 2010). Most other surveys, in contrast, ask about receiving help paying for child care from a government agency, welfare office, or social service agency (Johnson and Herbst 2013).[15] While our survey had an

13. Fewer than 10 percent of respondents are male.

14. Although the question was the same, the universe was slightly different: in Minnesota even parents whose current arrangement was parental care only were asked about subsidy receipt, while parents in Maryland were not (and are assumed to not receive subsidy).

15. Some national surveys asking about benefit receipt, such as the Current Population Survey, use state-specific names for programs such as Medicaid (Call et al. 2007). However, this practice is not common in the surveys that are used to assess child-care subsidy receipt and related outcomes (Johnson and Herbst 2013).

additional response option of “welfare agency or social services office” for assistance paying for child care, as we discuss later, including additional responses based on this question does not reduce measurement error.

ADMINISTRATIVE DATA

Each state provided data on child-care subsidy program participants from the program’s information management system through a data-sharing agreement. The Maryland State Department of Education provided administrative data on all children utilizing child-care subsidies between June 2007 and September 2012. The data indicate the week(s) in this time period during which the child received child care paid for (in part or full) by a subsidy. For the Minnesota sample, the Minnesota Department of Human Services provided monthly administrative data for children participating in the child-care subsidy program between January 2009 and June 2010.

An important issue that complicates quantifying misreporting is that different time frames, definitions, or universes may be applied across surveys and administrative data. Accurately matching responses across administrative and survey data is also critical to accurately quantifying misreporting. Jacob Klerman and colleagues (2005) describe the process of matching samples for validation and potential issues with the sampling frame that might create a false measure of misreporting. Interviewer and data entry error across both sources of data could also generate misreports, as could data mismatches.

In both states, information was provided to link records from the survey to the administrative data; however, the matching process was slightly different in the two states. In Minnesota, survey respondents’ households were matched to households in the administrative database based primarily on respondents’ name, gender, and date of birth. Additional variables were consulted if needed (including address and child’s date of birth and gender). Respondents were identified on a case-by-case basis and not through statistical matching. Because Minnesota survey respondents were recruited at the time of application for government assistance, nearly all of them were found in the administrative database. The survey respondents were in the state database because of their application and/or receipt of public benefits such as TANF, Medicaid, or SNAP, not necessarily for child-care assistance. Based on these look-ups, a matching household record in the administrative data was present for 98 percent of the survey respondents. After household matching, the focal child was identified among household members by matching on birth month and year. Because of the nature of the matching process, all matches of the survey data to the administrative data were unique.

In Maryland, the matching of families was based on the respondent's name and date of birth. The focal child was then identified based on the child's name and date of birth. As in Minnesota, respondents were identified on a case-by-case basis and not through statistical matching. Only complete matches for respondents were used, but partial matches of children's information were allowed. Of the survey respondents, 53 percent were located in the child-care subsidy administrative data, and 4 percent of the matches were partial matches for children. This rate is much lower than for Minnesota, where everyone in that sample received an identification number used across multiple programs. In Maryland, however, identification occurred only through the child-care subsidy program and therefore only among those who ever received a subsidy in Maryland. After locating individuals in Maryland's records, we identified a single best match in the linking data, making survey to administrative data mapping unique. Unlike in Minnesota, in Maryland we have no information from other state programs, so we cannot determine whether the lack of a match is due to lack of participation in the subsidy program or due to a failure of matching. Failure to match is particularly concerning when someone reports subsidy receipt in the survey but no record can be found in the administrative data.

Problems in matching and issues in the administrative data itself have different implications for the results, depending on the nature and severity of data problems. There could be inaccuracies in the administrative data. Although identifying documentation is required in order to obtain a subsidy, data entry errors may still occur, or different identifying information could be provided with the survey, such as an alternative name, than in the administrative data. Errors could also occur in billing that lead to an inaccurate measure of subsidy receipt when the correct individual is identified. Individuals with common names or common addresses may be more difficult to correctly identify, particularly if there are slight discrepancies in one of the data sources. Families who move more frequently or have greater variation in family composition may also be more difficult to identify. Although it is possible that we have some matching failures in Maryland, given the systematic patterns we find in misreporting and the nearly universal matching of Minnesota survey respondents, failures in record matching are unlikely to be driving our results. Likewise, inaccuracies in the administrative data that occur despite correct matching would have to occur at a very high rate in order to completely account for our results, which seems unlikely for data related to subsidy payments.

Another key issue to address in linking the survey responses to administrative records is matching the timing of the survey to the administrative data. The survey asks about current use of child-care subsidies. To allow for slight differences in timing, we use the calendar month of the survey interview date to match survey and administrative dates, and we undertake sensitivity analysis with alternative time windows. We identify use of subsidy in the administrative data based on whether the child received subsidized child care within the specified time period. The subsidy payment systems in each state provide information on when subsidized child-care services were received. Payment for these services may have occurred in subsequent months, but the data we use to identify subsidy receipt captures the month when the services took place. Thus, only individuals with a matching record in the administrative database and receiving subsidy in the calendar month of their survey interview are identified as receiving a subsidy (a binary variable) in the administrative data.

The original survey samples were 289 individuals in Maryland and 323 in Minnesota. Observations are excluded if the respondent did not consent to administrative data access or if there is not adequate overlap between the administrative data and the survey completion date, as some surveys were completed outside the time frame covered by the administrative data. For instance, individuals surveyed in October 2012 in Maryland are excluded from the sample since the administrative data only covered through September 2012. The resulting sample includes 267 individuals in Maryland and 319 in Minnesota.

THE EXTENT OF MISREPORTING OF CHILD-CARE SUBSIDY RECEIPT

RATES OF OVERREPORTING AND UNDERREPORTING

Table 1 presents the key results on the frequency of misreporting by showing the survey responses about subsidy receipt, conditioned on subsidy status from the administrative data. In Maryland, 17.9 percent of those not receiving a subsidy in the administrative data reported that they were receiving a subsidy in the survey (i.e., a 17.9 percent rate of overreporting). Additionally, 14.5 percent of those receiving a subsidy in the administra-

TABLE 1. Overreporting and Underreporting of Subsidy Receipt, Percentage by State

	Administrative Data: Subsidy Receipt					
	Maryland			Minnesota		
Survey: Subsidy Receipt	No	Yes	Total	No	Yes	Total
No	82.1	14.5	61.0	78.4	19.2	56.1
Yes	17.9	85.5	39.0	21.6	80.8	43.9
Total	100.0	100.0	100.0	100.0	100.0	100.0

Sources.—Authors' calculations based on Maryland and Minnesota parent surveys and Maryland and Minnesota administrative data.

Note.—Overreporting occurs when a respondent who is not receiving subsidy based on the administrative data reports receiving subsidy in the survey. Underreporting occurs when a respondent who does receive subsidy based on the administrative data reports not receiving subsidy in the survey. The sample size was 267 in Maryland and 319 in Minnesota.

tive data reported that they were not receiving a subsidy in the survey (14.5 percent underreporting). Looking at Minnesota, there are near equal probabilities of overreporting (21.6 percent) and underreporting (19.2 percent), rates that are slightly higher than Maryland's. While the data indicate that misreporting occurred, a large majority of survey respondents reported subsidy receipt accurately. Notably, however, both underreporting and overreporting occurred. This pattern suggests that people are not systematically unwilling to report the support they receive but instead did not know what this type of support is called (benefit confusion) or misunderstood the survey question.

Because the survey asked about current use of child-care subsidies, but the administrative data was on a weekly or monthly basis, one issue that may affect the calculated rates of misreporting is the time window used to match the two data sources. We checked a variety of different administrative time windows for comparison with the 1-month definition (see table A2 in the appendix). The availability of weekly administrative data in Maryland allowed us to test a definition based on the week of the interview plus or minus 1 week ("3 weeks" definition). For both states, we lengthened the time window to examine the administrative data on subsidy receipt based on a 3-month window around the interview date, and we also checked whether the child was receiving a subsidy at any time covered by the administrative data.[16] Table A2 compares the percentages of respondents who overreported and underreported using these alternative definitions of the time window. The results indicate that alternative time windows do not lead to substantial improvements in response alignment. It is also notable that extending the

16. The administrative data covers 18 months in Minnesota and 63 months in Maryland.

definition of subsidy to include individuals who were observed as receiving a subsidy at any time does not decrease overreporting substantially.

The survey questions also allow us to test an alternative, broader definition of child-care subsidy receipt. We consider broad subsidy receipt to include anyone who reported they received help paying for child care through a welfare agency or social services office (in addition to those who said yes to POC voucher or CCAP) or gave a response such as "through the county" in the open-ended "other" responses. Table A3, in the appendix, presents the percentage of individuals receiving a subsidy in the administrative data using the broader definition of subsidy receipt. The broader definition leads to some decreases in underreporting, but these are accompanied by larger increases in overreporting. Using a broader definition of subsidy seems to generate more measurement error in survey responses of subsidy receipt in this sample.

RATES OF FALSE POSITIVES AND FALSE NEGATIVES

The misreporting rates observed in both Maryland and Minnesota have implications for studies of the child-care subsidy program using survey data because of the low program participation rate. Table 2 uses the same data as table 1, but instead of column percentages (that measure overreporting and underreporting), row percentages are used to calculate the rates of false positives and false negatives. In other words, while table 1 conditions on status in the administrative data, table 2 conditions on survey status. Overall, around a third (31.1 percent in Maryland and 37.6 percent in Minnesota) of individuals were receiving a subsidy at the time of the survey, based on the

TABLE 2. False Positive and False Negative Reports of Subsidy Receipt, Percentage by State

	Administrative Data: Subsidy Receipt					
	Maryland			Minnesota		
Survey: Subsidy Receipt	No	Yes	Total	No	Yes	Total
No	92.6	7.4	100.0	87.2	12.9	100.0
Yes	31.7	68.3	100.0	30.7	69.3	100.0
Total	68.9	31.1	100.0	62.4	37.6	100.0

Sources.—Authors' calculations based on Maryland and Minnesota parent surveys and Maryland and Minnesota administrative data.

Note.—The percentage of the sample that is false positives is defined as the percentage of those who say they received subsidy in the survey who were not found to be receiving subsidy based on the administrative data. The percentage of false negatives is the percentage of those who report not receiving subsidy in the survey who were found to be on subsidy in the administrative data. The sample size was 267 for Maryland and 319 for Minnesota.

administrative data. When conditioned on their status in the administrative data, as table 1 demonstrated, individuals are approximately equally likely to overreport or underreport, but the results are quite different in terms of the false positives and false negatives in table 2. Among those responding that they were not receiving a subsidy in the survey in Maryland, only 7.4 percent had received a subsidy according to the administrative data, a low rate of false negatives. Similarly in Minnesota, the rate of false negatives is 12.9 percent. The rate of false positives is much higher; 31.7 percent of those in Maryland who stated in the survey that they received a subsidy were not recorded in the administrative data as having actually received a subsidy. Similarly, 30.7 percent of those who stated that they received a subsidy in the Minnesota survey data are false positives. Conditioning on survey reports, false positives are a far more common problem than false negatives, and this will attenuate estimates of program effectiveness if survey data are used to estimate program effects.

As a result of misreporting, estimates of subsidy receipt based on the survey responses overestimate the rate of subsidy participation compared to the administrative data. The survey estimate of participation is $\pi_{10}(1 - \pi) + \pi_{11}\pi$, and therefore in cases in which there is less than 50 percent participation in the program, overreporting will influence the survey estimate of participation more than underreporting. In Maryland, the survey estimate of subsidy receipt is 39.0 percent, a rate about 8 percentage points higher than the administrative data. Likewise in Minnesota, while 37.6 percent of respondents received subsidy in the administrative data, 43.9 percent of respondents reported subsidy receipt in the survey.

SYSTEMATIC MISREPORTING

This section presents models to identify possible systematic patterns in overreporting and underreporting of child-care subsidy program participation in the two states. We hypothesize that different factors may contribute to overreporting versus underreporting, and so we estimate separate models rather than a model of disagreement that combines overreporting and underreporting. For instance, having several children, only one of whom is receiving a subsidy, likely increases overreporting (i.e., reporting subsidy because the family receives it or because of uncertainty as to which child receives it), but it decreases underreporting because a family with several children might be more likely to know the name of the program. Therefore, we estimate separate probit models for the probability of underreporting or overreporting, including a number of covariates to test which, if any, are related to misreporting. The dependent variables are the conditional probabilities, $\pi_{01}(n) = \Pr(y = 0|y^* = 1)$ and $\pi_{10}(n) = \Pr(y = 1|y^* = 0)$ of underreporting and overreporting, respectively. These probabilities are conditioned on the administrative value of subsidy receipt. These are the probabilities that determine the degree of bias when benefit receipt is used in a regression (Bound et al. 2001), and they are the dependent variables commonly used in other studies that test for systematic benefit misreporting (e.g., Bollinger and David [1997] and Meyer and Goerge [2011]).

We use a number of family and child characteristics from the surveys to examine whether there are systematic patterns in misreporting.[17] The covariates in the binary models for overreporting and underreporting are the same, selected based on possible connections with drivers of misreporting generally as well as those which past studies examine (Johnson and Herbst 2013); factors that affect one type of misreporting but not the other will be insignificant when they do not matter. The covariates include the respondent's education level, which may be related to the respondent's knowledge about the program. Individuals are categorized as having less than a high school education, (exactly) a high school education, some college (but less than a BA), or a BA or higher. We also include the respondent's employment status, comparing those with no job to those with a part-time (less than 30 hours) or full-time (30 hours or more) job. Individuals' employment status is a precondition for certain types of subsidy eligibility, and individuals may misreport subsidy receipt in relation to their employment. Household structure may also affect reporting. Single parents are more likely to be informed about subsidy than respondents from two-parent households, where the other parent may be responsible for child-care payment, subsidy eligibility, or enrollment. We therefore include a dummy variable for being in a single-parent, as compared to two-parent, household. Receipt of TANF might affect respondents' use of subsidy and familiarity with the program, so we include a dummy variable for household receipt of welfare. A child's age might influence respondents' familiarity with the program and its name, so we include a categorical variable for child age, comparing infants (less than 16 months) to toddlers (16–32 months) and preschool/school-aged children (33+ months). Cross-cultural communication may be also be a factor in reporting, so we include respondent's race as a covariate, categorized as

17. Characteristics from the administrative data are not used, since these are only available for those receiving a subsidy in the administrative data.

(1) white, non-Hispanic, (2) Hispanic, or (3) nonwhite, non-Hispanic. Additionally, the number of children for whom the respondent is the primary caregiver is included in the model. Parents with more children may be less well informed about how they are paying for different children's care than parents with fewer children, or they may report subsidy based on receipt of subsidy for other children. The descriptive statistics for these characteristics are presented in the appendix in table A1.

TESTING FOR SYSTEMATIC PATTERNS IN UNDERREPORTING AND OVERREPORTING

The marginal effects for probit models of underreporting and overreporting, shown in table 3 and discussed below, demonstrate that a number of respondent characteristics are significantly related to subsidy misreporting. We estimated separate models for Minnesota and Maryland, but finding that the models are similar, we present a pooled model for both states in the text (separate models for the two states are provided in the appendix in table A4). The pooled model benefits from a larger sample size and suggests the types of misreporting that might be found in survey data that include multiple states.

Systematic Patterns in Underreporting

The pooled model shows a number of statistically significant predictors of underreporting. Those with full-time jobs are less likely to underreport, compared to those without jobs. Hispanic respondents and non-Hispanic nonwhites are significantly more likely to underreport than white, non-Hispanic respondents. Being a single parent or on welfare is negatively associated with underreporting. Parents are less likely to underreport if they had a preschool or school age child compared to those with infants. Notably, only two variables are not statistically significantly associated with underreporting: respondent education and the number of children in the household. Additionally, the relationships between covariates and underreporting are sizable. For instance, the marginal effect of TANF receipt in the pooled model is a decrease of 15.1 percentage points in the probability of underreporting.

Systematic Patterns in Overreporting

When it comes to overreporting, part-time and full-time workers are significantly less likely to overreport than respondents who were not working.

TABLE 3. Marginal Effects for Probit Models of Underreporting and Overreporting

	Dependent Variable	
	Subsidy Received: Pr(Underreporting)	No Subsidy: Pr(Overreporting)
Respondent's education (less than high school omitted):		
High school	−.018	.005
	(.073)	(.051)
Some college	−.064	−.059
	(.070)	(.049)
BA+	.041	.135
	(.136)	(.116)
Respondent's employment (no job omitted):		
Part-time (< 30 hours/week)	−.008	−.093+
	(.074)	(.055)
Full-time (30+ hours/week)	−.221***	−.114*
	(.046)	(.050)
Respondent's race (white, non-Hispanic omitted):		
Hispanic	.309*	.189*
	(.149)	(.089)
Nonwhite, non-Hispanic	.113*	.142***
	(.047)	(.039)
Single parent	−.203**	.047
	(.075)	(.044)
Welfare	−.151*	.062
	(.065)	(.043)
Child's age (infant omitted):		
Toddler	−.116	.011
	(.071)	(.052)
Preschool or school age	−.122+	.041
	(.069)	(.048)
Number of children	.004	.030+
	(.026)	(.017)
N (observations)	202	382
Probability of model	.000	.000
Pseudo R^2	.225	.097

Sources.—Authors' calculations based on Maryland and Minnesota parent surveys and Maryland and Minnesota administrative data.

Note.—Marginal effects are calculated at observed values for all characteristics. Standard errors are in parentheses.

+ $p < .10$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

Overreporting is also more likely among Hispanic respondents and nonwhite, non-Hispanic respondents than non-Hispanic whites. Each additional child increases the probability of overreporting. We suspect this is because parents may be uncertain or misreport if they receive a subsidy for another child but not the focal child. As is the case for underreporting, the marginal effects are sizable. For instance, the marginal effect for being Hispanic as compared to white, non-Hispanic is an increase of 18.9 percentage points in the probability of overreporting. Clearly, there are systematic patterns of overreporting benefit receipt in the survey data.[18]

THE CONSEQUENCES OF MISREPORTING

Having established that misreporting in survey reports of subsidy receipt is related to family and child characteristics, this section demonstrates the potential consequences of misreporting for drawing valid conclusions about the predictors and consequences of subsidy receipt. Misreporting can potentially generate erroneous estimates of program participation and program effects. Below, the case of benefit receipt as a dependent variable is examined first, followed by an example in which benefit receipt is a covariate.

COMPARISON OF RESULTS WHEN SUBSIDY RECEIPT IS THE DEPENDENT VARIABLE

While systematic relationships between covariates and misreporting theoretically result in biased estimates of the relationships between covariates and subsidy receipt measured using survey data, an important question is the degree of bias. If both administrative data and survey data consistently lead to similar substantive results, then research findings with survey data may be considered credible despite measurement error. We investigate

18. We examined a number of additional variables that might be systematically related to misreporting and could shed light on the mechanisms driving underreporting and overreporting. Given the findings of significant relationships between misreporting and race, we considered whether language barriers might be partially responsible for misreporting. We tested whether a dummy for speaking a language other than English at home predicted misreporting. It was not significant, so we omitted this variable from our final models. We also considered including respondent gender, since interactions between gender roles and perceptions of government assistance might affect reporting. There were too few male respondents in Maryland to successfully model, but in Minnesota and the pooled models there were not patterns of significant or substantive effects, so we did not include respondent gender in our final models. We also tested whether there was a relationship between participation in a number of other public assistance programs (such as Medicaid and SNAP/food stamps) and misreporting, since familiarity with other benefits or savvy in navigating benefit receipt systems might relate to reporting; we found no significant relationships with common public programs aside from welfare (TANF).

whether conclusions drawn from analyses are likely to be similar by estimating models of subsidy receipt, comparing the results when the dependent variable is based on survey responses to the model in which subsidy receipt is based on the administrative data.[19] We make two types of comparisons. First, we compare the survey and administrative data models to examine whether researchers would reach different conclusions about which variables are statistically significant and the strength of relationships with subsidy receipt. Second, we assess whether the estimated coefficients are different to a statistically significant degree across models. We present the estimated marginal effects for the pooled probit models of subsidy receipt in table 4. Models by state are presented in the appendix in table A5.[20] In the models, the covariates are based on the survey responses, and the only difference between the two models is the use of administrative data on subsidy receipt as the dependent variable in one, while the other uses survey responses on subsidy receipt.

In the pooled model, the relationships between covariates and subsidy receipt are substantively different depending on the data source used for the dependent variable. The estimated marginal effect for high school is statistically significant using the administrative data on subsidy receipt but not when using the survey data (and the estimated marginal effect is much reduced). Additionally, the coefficients for some college and BA or higher have smaller marginal effects in the model using survey data on subsidy receipt than in the administrative data model. Using the administrative data, there is a large and significant estimated marginal effect for part-time and full-time work, but the estimated effect of part-time work becomes smaller and insignificant when using the survey data for subsidy receipt, and the marginal effect for full-time work also decreases. While many marginal effects are smaller in the model using survey data on subsidy receipt, some marginal effects are larger than in the model using administrative data for subsidy receipt. Thus, the direction of changes is not consistent. The different results are related to the systematic patterns of misreporting; for instance, single parents and those receiving welfare are much less likely to underreport—

19. These empirical models of subsidy receipt are intended only to illustrate the problem of biased estimates due to measurement error. As such we included a standard set of child and family demographic characteristics and did not address the issues of endogeneity of subsidy receipt with employment or other variables.

20. We have also provided models of subsidy participation without employment as a covariate in the appendix, in table A7, since employment may be endogenous to subsidy participation. Results are similar to the models with employment included.

TABLE 4. Comparison of Models of Subsidy Receipt Using Administrative and Survey Data (Marginal Effects for Probit Models)

	Dependent Variable	
	Administrative Subsidy Receipt	Survey Subsidy Receipt
Respondent's education (less than high school omitted):		
High school	.103*	.058
	(.048)	(.052)
Some college	.176***	.089+
	(.049)	(.052)
BA+	.201*	.180+
	(.092)	(.093)
Respondent's employment (no job omitted):		
Part-time (< 30 hours/week)	.184**	.033
	(.057)	(.058)
Full-time (30+ hours/week)	.173**	.138*
	(.054)	(.055)
Respondent race (white, non-Hispanic omitted):		
Hispanic	−.063	.028
	(.081)	(.085)
Nonwhite, non-Hispanic	−.043	.030
	(.043)	(.044)
Single parent	.136**	.169***
	(.043)	(.045)
Welfare	.119**	.156***
	(.041)	(.043)
Child's age (infant omitted):		
Toddler	.099+	.103+
	(.051)	(.053)
Preschool or school age	.043	.087+
	(.047)	(.050)
Number of children	−.004	.025
	(.018)	(.018)
N (observations)	584	584
Probability of model	.000	.000
Pseudo R^2	.070	.059

Sources.—Authors' calculations based on Maryland and Minnesota parent surveys and Maryland and Minnesota administrative data.

Note.—Marginal effects are calculated at observed values for all characteristics. Standard errors are in parentheses.

+ $p < .10$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

and therefore they have larger estimated coefficients in the model using survey data. While there is not a consistent set of covariates or clear consensus in the literature as to the factors associated with subsidy receipt (Blau and Tekin 2007; Meyer et al. 2009; Herbst and Tekin 2011*b*; Johnson et al. 2011; Johnson et al. 2012), our findings, particularly in the administrative data model, that child age and parent education are associated with child-care subsidy receipt are consistent with other studies.

Overall, using the administrative data pooled model, 7 of 12 estimated marginal effects were statistically significant, and in the survey 7 of 12 are significant, but only 6 overlap, and a number of the significant effects change size substantially. To determine whether the differences in estimates between the models are statistically significant, we conducted Wald tests for the equality of coefficients across the administrative and subsidy models using the same set of covariates. In the pooled model, 4 coefficients are significantly different across the models, specifically some college, part-time work, nonwhite non-Hispanic, and number of children. The key point is that researchers would draw different conclusions as to the factors associated with subsidy use depending on whether their measure of subsidy receipt came from survey or administrative data. This result illustrates how estimating models of participation in government programs using survey responses may be misleading.

COMPARISON OF RESULTS FOR MODELS USING SUBSIDY RECEIPT AS A COVARIATE

In order to demonstrate the extent of potential bias in a regression context using mismeasured subsidy receipt as a covariate, we estimated binary probit models for employment using the same set of covariates as in the misreporting models (with the exception of employment and with the addition of subsidy receipt as a covariate). The estimated marginal effects for the pooled models using administrative data and survey reports on subsidy receipt are compared in table 5. The models by state are presented in the appendix, in table A6.

In the pooled administrative data model, the estimated marginal effect for subsidy on the probability of employment is 16.5 percentage points ($p < .01$). Using the survey reports of subsidy receipt, the estimated marginal effect is less than half that size, 7.6 percentage points, and it is only marginally significant ($p < .10$). The estimated marginal effect sizes for education and their significance levels also vary across the models. A researcher who has administrative data on subsidy receipt would reach a different conclusion about the relationship between subsidy receipt and employment than one who has only survey data. Estimates of the effect of subsidy on employment in the literature correcting for endogeneity range from none

TABLE 5. Comparison of Models of Employment Using Administrative and Survey Data on Subsidy Receipt (Marginal Effects for Probit Models)

	Dependent Variable: Employed	
	Administrative	Survey
Subsidy receipt	.165***	.076+
	(.041)	(.039)
Respondent's education (less than high school omitted):		
High school	.100*	.116*
	(.048)	(.048)
Some college	.112*	.139**
	(.050)	(.049)
BA+	.214*	.239**
	(.092)	(.092)
Respondent's race (white, non-Hispanic omitted):		
Hispanic	.106	.101
	(.082)	(.083)
Nonwhite, non-Hispanic	−.027	−.035
	(.042)	(.042)
Single parent	.004	.013
	(.044)	(.044)
Welfare	−.265***	−.262***
	(.042)	(.043)
Child's age (infant omitted):		
Toddler	−.012	−.005
	(.050)	(.050)
Preschool or school age	.035	.035
	(.047)	(.048)
Number of children	−.015	−.017
	(.018)	(.018)
N (observations)	584	584
Probability of model	.000	.000
Pseudo R^2	.108	.091

Sources.—Authors' calculations based on Maryland and Minnesota parent surveys and Maryland and Minnesota administrative data.

Note.—Marginal effects are calculated at observed values for all characteristics. Standard errors are in parentheses.

+ $p < .10$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

(Michalopoulos, Lundquist, and Castells 2010) to a 33 percentage point increase in employment (Blau and Tekin 2007). David Blau and Erdal Tekin's (2007) estimate of a 13 percentage point effect in models not correcting for endogeneity falls between our survey and administrative data results.

Based on statistical tests for differences in the estimated coefficients across the two models, in the pooled model, there was a significant difference in the estimated coefficient for subsidy receipt between the administrative and survey data models. In addition, there are significant differences in the estimated coefficients for high school education and for some college in the pooled model. Overall, including mismeasured subsidy receipt as a covariate can generate different estimated coefficients for the subsidy receipt variable and potentially for other covariates as well.

DISCUSSION AND CONCLUSIONS

RESEARCH AND POLICY IMPLICATIONS

Surveys are frequently used to estimate the predictors of receipt of government benefits as well as program effects on participants. This research illustrates that, for child-care subsidies in two states, both underreporting and overreporting occur and are systematically related to survey respondents' characteristics. Notably, respondent education, employment, and race, number of parents in the household, TANF receipt, child age, and number of children are all systematically related to misreporting in at least one of the models estimated in this study. The pattern of the estimated covariates suggests that information problems and benefit confusion are major contributors to misreporting. To illustrate the potential effects of these systematic measurement errors on conclusions drawn by researchers, we report how findings for models of subsidy receipt as a dependent variable, and as a covariate, differ using measures of subsidy receipt from survey versus administrative data.

The findings here have implications for the accuracy of estimates in studies relying on survey data to examine the predictors of child-care subsidy receipt and the effects of child-care subsidies on various outcomes. Our findings suggest that systematic mismeasurement in subsidy receipt from surveys can bias the estimated predictors of participation as well as the estimated effects of subsidy on family and child outcomes. These findings have implications not just for child-care subsidy research but for any research using survey data to study government benefits.

Researchers often validate survey responses or check for measurement error by comparing program receipt in a survey sample to the overall population rate from administrative data (Blau and Tekin 2007; Meyer et al. 2009; Herbst and Tekin 2011*b*; Johnson et al. 2011; Johnson et al. 2012). Given our findings that both underreporting and overreporting are occurring, we caution that it is possible to obtain similar sample and population level estimates of participation and still have problematic measurement error in the sample due to an offsetting combination of underreporting and overreporting. It is also important to model the predictors of underreporting and overreporting separately, as different processes may contribute to these errors. The rates of underreporting and overreporting and the population rate of benefit receipt all interact in shaping survey estimates. For instance, in the two states examined, we find that estimates of subsidy program participation based on the survey data are inflated by 6–8 percentage points due to overreporting.

LIMITATIONS

While our findings are an illustration of the problems misreporting can generate when using survey data, one should be cautious in generalizing these findings to other surveys and other samples. Our sample provides a case study of response problems in only two states, focusing on low-income families. That similar misreporting problems occur in both Maryland and Minnesota suggests that misreporting is likely to be an issue in other states as well. Since our study targets families likely to be eligible for child-care subsidies, our misreporting rates are indicative of measurement error in research using surveys of eligible families. Similar problems are likely to occur in research that extracts a sample of eligible families from a national survey.[21] If eligible families in national surveys misreport in a similar fashion to our sample, estimated rates of benefit receipt would be similarly distorted. Using all respondents in a nationally representative survey, rather than just an eligible subsample, is likely to yield different rates of misreporting than found here. Underreporting, which conditions on subsidy receipt, should occur at a similar rate. Because a smaller share of individuals is eligible, the overreporting rate is likely to be lower. However, because participation rates are much lower in a national population than an eligible one, estimates of subsidy receipt are likely to be overestimated to even a greater extent than in our surveys.

Our sampling strategy also specifically targeted individuals applying for assistance at their county social services office. Thus, our respondents were disproportionately receiving TANF benefits relative to the typical subsidy

21. An additional issue in studying government benefits is correctly identifying the eligible population, as eligibility markers may be misreported. Mismeasured eligibility can substantially bias research on government assistance (Duclos 1995; Hernandez and Pudney 2007).

entrant. Although we cannot be certain how this nonrepresentative sample influenced our results, the fact that we find that those receiving welfare are significantly less likely to underreport suggests that, if anything, our estimates of misreporting could be lower than we would have found in a random sample. Likewise, we would expect that having recently been at the social services office would raise survey respondents' awareness of programs, and this would further lower misreporting in our sample as compared to a random sample. The effect of the moderate response rates to the survey on measures of misreporting is uncertain, and it depends on whether those who responded to our survey would be more or less likely to report accurately than a random sample of low-income families.

Although match quality is an important concern for validation studies of this kind, our near-universal match rate in the Minnesota administrative data system means that we can be confident in the linking of those individuals in the subsidy administrative data to their survey responses. In Maryland, because we could only identify individuals who appear in the subsidy administrative system, we only located records for 53 percent of survey respondents. These individuals had received a child-care subsidy at some point during the administrative data window, but they may or may not have been receiving a subsidy at the time of the survey. Overall, however, it is unlikely that match problems could fully account for our results. We use similar procedures for matching in both states and find similar rates of misreporting.

A final concern in identifying misreporting is whether, having matched individuals, we accurately match the measures of subsidy receipt across the administrative and survey data. The survey data ask about current subsidy receipt, while our measure from the administrative data is based on services received in the calendar month of the interview. These data are drawn from the payment system, which should provide accurate information about services for which the subsidy program paid. We do not use the date of payment but rather the date of service, which should align with respondents' reports of current subsidy receipt. However, data entry errors, payment system errors, and other similar issues could still affect our estimates.

HOW CAN THE RELIABILITY OF RESEARCH BE IMPROVED?

Since our findings are consistent with a substantial body of literature identifying measurement error in surveys, an important issue in future research is improving the accuracy of survey responses. Several approaches can be taken to improve the validity of measures of receipt of government benefits. The most obvious way is to use alternative data sources, especially administrative data, for information on program receipt. Studies of the effects of subsidies using administrative data, including waitlists and subsidy leavers, are less common than survey-based research, but these should be encouraged (e.g., Berger and Black 1992; Lee et al. 2004; Grobe, Weber, and Davis 2008; Goerge 2009; Forry et al. 2013). Obtaining administrative data typically requires data-sharing agreements between program administrators and researchers as well as informed consent from the respondents. Ensuring the privacy of data is an important element of obtaining administrative data. Government support for studies that incorporate administrative data could also improve the validity of results. Additional approaches, such as experiments (Michalopoulos et al. 2010) or the use of state-level policy variables combined with surveys (Rigby et al. 2007) should also be encouraged.

Advanced econometric methods to correct for response errors can also be implemented in conjunction with encouraging validation studies for different government programs. For instance, Christopher Bollinger and Martin David (1997) model underreporting and overreporting for food stamps and then use the models of response error to adjust the determinants of participation in a wider sample. Although popular as a correction for measurement error, instrumental variable approaches generally fail in cases where measurement error is not classical (Bound et al. 2001). Using instrumental variable techniques with a mismeasured binary variable will generate coefficient estimates that are inflated relative to both the estimate with mismeasured survey data and the true value (Bound et al. 2001). Measurement error in survey reports of subsidy receipt may be why some studies (e.g., Herbst and Tekin 2010, 2012, 2011*b*) generate much larger instrumental variable estimates of the effect of subsidy receipt than their ordinary least squares estimates; true values are likely to be in between the two estimates.

Another approach is to study and improve how survey questions are worded and asked. The surveys we used asked about current subsidy receipt. The relatively low rates of misreporting may be in part due to asking for contemporaneous rather than recalled information. Having to recall benefit receipt, especially for longer recall periods, increases error and bias (Bound et al. 2001; Klerman et al. 2009; Michalopoulos et al. 2010; Call et al. 2013). For instance, the Early Childhood Longitudinal Survey-Kindergarten cohort (ECLS-K), which is used to study the effect of child-care subsidies on a variety of outcomes (e.g., Herbst and Tekin 2011*a*, 2011*b*, 2014), includes a question about receipt of child-care assistance in the previous year. This question is likely to suffer from more misreporting than a question asking about contemporaneous receipt.

Additional research is needed to understand why misreporting is occurring, particularly overreporting, which is often neglected in the literature. Our investigation of the characteristics of overreporters in the two states finds that they were often paying for child care in amounts comparable to those receiving a subsidy. Overreporters were not particularly likely to be attending Head Start or public prekindergarten programs, which, given that they are free for parents, might be considered a subsidy by some. Overreporters also frequently reported TANF receipt, suggesting that benefit confusion may be the cause of overreporting in some cases.

Research to develop survey questions that will more accurately identify benefit receipt is merited; using the state-specific name of the program may factor into relatively low underreporting rates in our study. However, adding in questions about help paying for child care from a welfare office did not improve estimates. Studies that compare the accuracy of responses about benefit receipt under different question phrasings will be valuable, along with additional qualitative work investigating respondents' understanding of different ways to ask about benefit receipt.

It is critical to have accurate estimates of the effects of government programs in order to assess their value to individuals and society. Surveys are important research tools in assessing government programs, and sometimes alternative data sources are limited, problematic, or simply unavailable. However, as this work demonstrates, researchers and policy makers need to understand how analyses based on survey data may misrepresent the relationships between government programs, participants' characteristics, and other outcomes. Additionally, researchers may need to reassess the conclusions drawn from studies based on survey data, which have served as the empirical foundation for subsidy policy and many other government programs. Moving forward, it will be important to apply existing methods for addressing measurement error, as well as to develop new methods for correcting or bounding estimates for bias induced by measurement error.

APPENDIX

Supplementary Tables

TABLE A1. Sample Descriptives

	Subsidy Received (Sample for Underreporting)			No Subsidy (Sample for Overreporting)			All		
	MD	MN	Pooled	MD	MN	Pooled	MD	MN	Pooled
Report of child-care subsidy:									
Report accurately	85.5	80.8	82.8	82.1	78.4	80.2	83.1	79.3	81.1
Misreport	14.5	19.2	17.2	17.9	21.6	19.8	16.9	20.7	18.9
Respondent's education:									
Less than high school	12.0	17.5	15.3	26.1	30.7	28.5	21.7	25.7	23.9
High school	34.9	35.8	35.5	38.6	32.2	35.2	37.5	33.5	35.3
Some college	44.6	40.0	41.9	27.7	34.2	31.1	33.0	36.4	34.8
BA+	8.4	6.7	7.4	7.6	3.0	5.2	7.9	4.4	6.0
Respondent's employment:									
No employment	72.3	42.5	54.7	76.1	66.8	71.3	74.9	57.7	65.5
Part-time employment (< 30 hours)	14.5	23.3	19.7	10.3	14.6	12.5	11.6	17.9	15.0
Full-time employment (30+ hours)	13.3	34.2	25.6	13.6	18.6	16.2	13.5	24.5	19.5
Number of parents:									
Two parents	9.6	26.7	19.7	21.9	42.7	32.7	18.0	36.7	28.2
Single parent	90.4	73.3	80.3	78.1	57.3	67.3	82.0	63.3	71.8
Welfare status:									
Not on welfare	32.9	32.5	32.7	51.6	26.6	38.6	45.9	28.8	36.6
On welfare	67.1	67.5	67.3	48.4	73.4	61.4	54.1	71.2	63.4
Child's age:									
Infant	21.7	21.7	21.7	22.3	36.2	29.5	22.1	30.7	26.8
Toddler	30.1	35.0	33.0	29.3	23.1	26.1	29.6	27.6	28.5
Preschool or school age	48.2	43.3	45.3	48.4	40.7	44.4	48.3	41.7	44.7
Respondent's race:									
White, non-Hispanic	19.3	44.2	34.0	32.6	34.7	33.7	28.5	38.2	33.8
Hispanic	4.8	6.7	5.9	4.3	9.5	7.0	4.5	8.5	6.7
Nonwhite, non-Hispanic	75.9	49.2	60.1	63.0	55.8	59.3	67.0	53.3	59.6
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Mean number of children	1.6	2.0	1.8	2.0	1.8	1.9	1.9	1.9	1.9
SD number of children	.9	1.0	1.0	1.2	1.2	1.2	1.1	1.1	1.1
N (observations)	83	120	203	184	199	383	267	319	586

Source.—Authors' calculations based on Maryland and Minnesota parent surveys and Maryland and Minnesota administrative data.

TABLE A2. Overreporting and Underreporting of Subsidy Receipt Using Alternative Time Windows, Percentage by State

	Administrative Data: Subsidy Receipt					
	Maryland			Minnesota		
Survey Subsidy Receipt	**No**	**Yes**	**Total**	**No**	**Yes**	**Total**
3-week definition:						
No	83.7	12.9	60.3			
Yes	16.3	87.1	39.7			
Total	100.0	100.0	100.0			
1-month definition:						
No	82.1	14.5	61.0	78.4	19.2	56.1
Yes	17.9	85.5	39.0	21.6	80.8	43.9
Total	100.0	100.0	100.0	100.0	100.0	100.0
3-month definition:						
No	83.8	23.5	61.9	78.8	21.4	56.1
Yes	16.2	76.5	38.1	21.2	78.6	43.9
Total	100.0	100.0	100.0	100.0	100.0	100.0
Ever received a subsidy:						
No	84.6	42.0	61.9	85.4	36.0	56.1
Yes	15.4	58.0	38.1	14.6	64.0	43.9
Total	100.0	100.0	100.0	100.0	100.0	100.0

Sources.—Authors' calculations based on Maryland and Minnesota parent surveys and Maryland and Minnesota administrative data.

Note.—For Maryland, N (observations) = 257 for 3-week definition, N = 267 for 1-month definition, N = 223 for 3-month and ever definitions. N = 319 for all definitions in Minnesota.

TABLE A3. Overreporting and Underreporting of Subsidy Receipt Using Broader Definition of Subsidy, Percentage by State

	Administrative Data: Subsidy Receipt					
	Maryland			Minnesota		
Survey Subsidy Receipt	**No**	**Yes**	**Total**	**No**	**Yes**	**Total**
No	76.1	13.3	56.6	67.8	11.7	46.7
Yes	23.9	86.7	43.4	32.2	88.3	53.3
Total	100.0	100.0	100.0	100.0	100.0	100.0

Sources.—Authors' calculations based on Maryland and Minnesota parent surveys and Maryland and Minnesota administrative data.

Note.—The broader definition includes additional response categories as positive indicators of subsidy receipt. Maryland N (observations) = 267; Minnesota N (observations) = 319.

TABLE A4. Marginal Effects for Probit Models of Underreporting and Overreporting: Minnesota and Maryland

	Dependent Variable			
	Subsidy Received: Pr(Underreporting)		No Subsidy: Pr(Overreporting)	
	Maryland	Minnesota	Maryland	Minnesota
Respondent's education (less than high school omitted):				
High school	−.057	.011	−.082	.082
	(.148)	(.079)	(.077)	(.071)
Some college	−.138	.038	$-.136^{+}$	−.042
	(.139)	(.091)	(.077)	(.062)
BA+	.208	.080	−.065	.458*
	(.313)	(.189)	(.123)	(.217)
Respondent's employment (no job omitted):				
Part-time (< 30 hours/week)	.156	−.178*	−.058	$-.127^{+}$
	(.142)	(.091)	(.083)	(.072)
Full-time (30+ hours/week)	A	−.321***	.002	−.201***
		(.075)	(.089)	(.058)
Respondent's race (white, non-Hispanic omitted):				
Hispanic	B	.442**	$.315^{+}$	.156
		(.156)	(.171)	(.106)
Nonwhite, non-Hispanic	.075	.141*	.177***	.128*
	(.111)	(.067)	(.050)	(.058)
Single parent	$-.312^{+}$	$-.151^{+}$	−.013	.117*
	(.178)	(.087)	(.075)	(.057)
Welfare	−.230*	−.129	.068	−.003
	(.115)	(.093)	(.060)	(.078)
Child's age (infant omitted):				
Toddler	−.137	−.132	.033	−.000
	(.126)	(.094)	(.076)	(.072)
Preschool or school age	−.098	−.185*	.053	.030
	(.113)	(.092)	(.068)	(.065)
Number of children	.008	−.013	−.019	.079***
	(.052)	(.033)	(.024)	(.024)
N (observations)	69	120	183	199
Probability of model	.078	.001	.066	.000
Pseudo R^2	.264	.288	.116	.172

Sources.—Authors' calculations based on Maryland and Minnesota parent surveys and Maryland and Minnesota administrative data.

Note.—Marginal effects are calculated at observed values for all characteristics. A = predicts failure perfectly ($N = 11$), B = predicts failure perfectly ($N = 2$). Standard errors are in parentheses.

$^{+}$ $p < .10$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

TABLE A5. Comparison of Models of Subsidy Receipt Using Administrative and Survey Data (Marginal Effects for Probit Models): Minnesota and Maryland

	Dependent Variable: Administrative or Survey Subsidy Receipt			
	Maryland		Minnesota	
	Administrative	Survey	Administrative	Survey
Respondent's education (less than high school omitted):				
High school	.140*	.037	.087	.078
	(.065)	(.076)	(.065)	(.068)
Some college	.243***	.071	.099	.074
	(.069)	(.078)	(.066)	(.068)
BA+	.179	.053	.327*	.357**
	(.119)	(.124)	(.134)	(.127)
Respondent's employment (no job omitted):				
Part-time (< 30 hours/week)	.086	−.087	.207**	.078
	(.090)	(.088)	(.072)	(.073)
Full-time (30+ hours/week)	.026	.058	.192**	.115
	(.087)	(.092)	(.070)	(.070)
Respondent's race (white, non-Hispanic omitted):				
Hispanic	.073	.320*	−.108	−.086
	(.142)	(.146)	(.098)	(.100)
Nonwhite, non-Hispanic	.097	.189**	−.108+	−.039
	(.061)	(.062)	(.059)	(.061)
Single parent	.126+	.114	.219***	.265***
	(.069)	(.077)	(.053)	(.055)
Welfare	.172**	.219***	.031	.056
	(.057)	(.060)	(.064)	(.067)
Child's age (infant omitted):				
Toddler	−.028	.045	.200**	.155*
	(.076)	(.080)	(.068)	(.070)
Preschool or school age	−.006	.054	.061	.096
	(.071)	(.073)	(.062)	(.065)
Number of children	−.062*	−.042	.047*	.086***
	(.027)	(.027)	(.024)	(.024)
N (observations)	265	265	319	319
Probability of model	.000	.000	.000	.000
Pseudo R^2	.110	.103	.116	.106

Sources.—Authors' calculations based on Maryland and Minnesota parent surveys and Maryland and Minnesota administrative data.

Note.—Marginal effects are calculated at observed values for all characteristics. Standard errors are in parentheses.

+ $p < .10$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

TABLE A6. Comparison of Models of Employment Using Administrative and Survey Data on Subsidy Receipt (Marginal Effects for Probit Models): Minnesota and Maryland

	Dependent Variable: Employed			
	Maryland		Minnesota	
	Administrative	Survey	Administrative	Survey
Subsidy receipt	.029	−.023	.196***	.093+
	(.056)	(.052)	(.056)	(.055)
Respondent's education (less than high school omitted):				
High school	.134*	.137*	.102	.118+
	(.061)	(.060)	(.066)	(.067)
Some college	.122+	.130*	.099	.120+
	(.063)	(.061)	(.066)	(.067)
BA+	.357**	.363**	.075	.111
	(.111)	(.110)	(.144)	(.147)
Respondent's race (white, non-Hispanic omitted):				
Hispanic	.152	.163	.060	.054
	(.126)	(.126)	(.099)	(.101)
Nonwhite, non-Hispanic	.147**	.154**	−.091	−.110+
	(.052)	(.051)	(.060)	(.061)
Single parent	.085	.090	.042	.061
	(.058)	(.058)	(.059)	(.059)
Welfare	−.275***	−.265***	−.319***	−.327***
	(.051)	(.052)	(.065)	(.065)
Child's age (infant omitted):				
Toddler	−.076	−.077	.041	.065
	(.068)	(.068)	(.068)	(.068)
Preschool or school age	.005	.007	.075	.076
	(.065)	(.065)	(.063)	(.063)
Number of children	−.016	−.019	−.022	−.020
	(.024)	(.024)	(.024)	(.025)
N (observations)	265	265	319	319
Probability of model	.000	.000	.000	.000
Pseudo R^2	.181	.180	.144	.122

Sources.—Authors' calculations based on Maryland and Minnesota parent surveys and Maryland and Minnesota administrative data.

Note.—Marginal effects are calculated at observed values for all characteristics. Standard errors are in parentheses.

+ $p < .10$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

TABLE A7. Comparison of Models of Subsidy Receipt Using Administrative and Survey Data (Marginal Effects for Probit Models): Without Employment

	Dependent Variable: Administrative or Survey Subsidy Receipt					
	Maryland		Minnesota		Pooled	
	Administrative	Survey	Administrative	Survey	Administrative	Survey
Respondent's education (less than high school omitted):						
High school	.145*	.037	.116+	.089	.124**	.065
	(.064)	(.076)	(.066)	(.068)	(.047)	(.052)
Some college	.249***	.072	.127+	.089	.200***	.104*
	(.068)	(.077)	(.066)	(.068)	(.049)	(.052)
BA+	.185	.081	.352**	.374**	.244**	.221*
	(.113)	(.120)	(.136)	(.126)	(.091)	(.091)
Respondent's race (white, non-Hispanic omitted):						
Hispanic	.079	.330*	−.094	−.086	−.043	.032
	(.140)	(.145)	(.101)	(.100)	(.083)	(.085)
Nonwhite, non-Hispanic	.106+	.186**	−.128*	−.053	−.048	.020
	(.060)	(.062)	(.059)	(.060)	(.043)	(.044)
Single parent	.127+	.119	.231***	.273***	.139**	.174***
	(.068)	(.076)	(.053)	(.055)	(.043)	(.045)
Welfare	.158**	.218***	−.030	.020	.075+	.128**
	(.054)	(.057)	(.064)	(.065)	(.041)	(.042)
Child's age (infant omitted):						
Toddler	−.032	.044	.213**	.164*	.098+	.106*
	(.076)	(.080)	(.069)	(.070)	(.052)	(.054)
Preschool or school age	−.005	.051	.076	.108+	.050	.093+
	(.071)	(.073)	(.062)	(.065)	(.048)	(.050)
Number of children	−.062*	−.040	.044+	.086***	−.006	.025
	(.027)	(.027)	(.024)	(.024)	(.018)	(.018)
N (observations)	265	265	319	319	584	584
Probability of model	.000	.000	.000	.000	.000	.000
Pseudo R^2	.107	.099	.087	.099	.047	.051

Sources.—Authors' calculations based on Maryland and Minnesota parent surveys and Maryland and Minnesota administrative data.
Note.—Marginal effects are calculated at observed values for all characteristics. Standard errors are in parentheses.
+ $p < .10$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

NOTE

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