Eligibility Recertification and Dynamic Opt-in Incentives in Income-tested Social Programs: Evidence from Medicaid/CHIP

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Working Paper Series

2013 | 58

Eligibility Recertification and Dynamic Opt-in Incentives in Income-tested Social Programs: Evidence from Medicaid/CHIP*

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First Draft: November 2011
This Draft: August, 2013

Abstract

Conventional labor supply studies assume the constant eligibility monitoring of income-tested program participants, but this is not true for most programs. For example, states can allow children to enroll in Medicaid/CHIP for 12 months regardless of family income changes. A long recertification period reduces monitoring costs but is predicted to induce program participation by temporary income adjustments. However, I find little evidence of strategic behavior from the Survey of Income and Program Participation. Given the lack of income responses, I propose a framework to compute the optimal recertification period and find 12 months to be its lower bound.

JEL codes: I38, J20
Keywords: Labor Supply, Medicaid, CHIP, Continuous Eligibility, Recertification

*I am indebted to David S. Lee for his guidance and support. I thank Orley Ashenfelter, Raj Chetty, Dong Beom Choi, Eleanor Choi, Hyun Soo Choi, Damon Clark, Janet Currie, Jo Duh, Hank Farber, Marjolaine Gauthier-Loiselle, Mike Geruso, Bo Honoré, Ilyana Kuziemko, Yan Lau, Nicholas Lawson, Lars Lefgren, Pauline Leung, Alex Mas, Jordan Matsudaira, Doug Miller, Tim Moore, Andrew Shephard, Lara Shore-Sheppard and seminar participants at APPAM, BLS, Brandeis, Brown, Mathematica, Princeton, Queen's, Swarthmore, UIC, UIUC and the Upjohn Institute for helpful suggestions. I also thank Rachel Licata at the Kaiser Family Foundation and Amy Steinweg and Becky Chenevert at the U.S. Census Bureau for answering my questions. All errors are my own.

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1 Introduction

An implicit assumption in labor supply studies of income-tested social programs is that program eligibility is being constantly monitored (e.g. Nichols and Zeckhauser (1982), Yelowitz (1995), Moffitt (2002)). However, this is not how many of these programs operate in practice today and the time between two consecutive eligibility certifications, or the "recertification period", can be as long as a year. Although this policy lever is recognized and its effect on program participation explored in studies of transfer programs,[1] a formal theoretical and empirical investigation has not been carried out to address how program participants may respond to the incentives created by the *dynamic* budget constraint resulting from the lack of constant income monitoring. In this paper, I attempt to fill this gap by examining families' behavioral responses to the continuous eligibility provision for children participating in Medicaid and the State Children's Health Insurance Program (SCHIP or simply CHIP). The positive analysis of income and labor supply responses is key in answering the important normative question of how often eligibility monitoring should be conducted, the policy motivation of this study.

Uninterrupted eligibility monitoring ensures that an income-tested program is effectively targeting the needy. However, if monitoring is costly and incomes of program participants change little over time, it may be sensible for the government to decrease the frequency of eligibility checks and offer a period of "continuous eligibility." Granting continuous eligibility increases the value of a transfer program to its participants through two channels. The first channel is the reduction in transaction costs as pointed out by Currie and Grogger (2001) and Kabbani and Wilde (2003). The second–and rarely considered–channel is the change of a participant's budget constraint over time. If the non-linearity in the budget constraint created by the eligibility requirements distorts a family's labor supply choices, the distortion is eliminated in any period in which eligibility is not checked, allowing the family to select a more optimal consumption bundle. Increasing the recertification period effectively decreases the number of periods in which a family faces the more stringent budget constraint, creating strong "dynamic opt-in incentives" for an otherwise ineligible family to participate in the program. That is, families may be induced to temporarily lower their income, gain program eligibility, and revert back to their "optimal" consumption bundle after having acquired the government benefit for the entire continuous eligibility period. As a result, the lengthening of the recertification

[1] Kornfeld (2002), Currie and Grogger (2001), Kabbani and Wilde (2003) and McKernan and Ratcliffe (2003) find that shortening the recertification period in Food Stamp reduces the participation rate. Staveley et al. (2002) and Ribar et al. (2008) find that exists from Food Stamp are much more likely to occur in months of eligibility recertification. Prell (2008) treats income volatility as exogenous and discusses the optimal recertification frequency in the Woman, Infants and Children (WIC) program.

period may *create* movements in the average income process around eligibility checks. These families that behave strategically–if they exist–are the "imposters" per Nichols and Zeckhauser (1982) in the dynamic context, and lowering the eligibility monitoring frequency decreases the cost of masquerading.

Setting the continuous eligibility period, therefore, involves the tradeoff between minimizing the number of imposters, who are not the intended beneficiaries of the program, and reducing the economic loss associated with monitoring. As mentioned above, the loss includes the administrative costs to the government, pecuniary and time costs of families participating in the program, and it also includes the deprivation of program benefits for some of the families most in need when the transaction costs of eligibility recertifications become insurmountable (Currie and Grogger (2001)). In the case of health insurance, studies (e.g., Olson et al. (2005)) have shown that children who experience interruptions in health insurance coverage are more likely to have unmet health care needs, and therefore imposing large transaction costs on otherwise eligible families may reduce targeting efficiency as well. Given these tradeoffs, understanding the behavioral response to the lack of eligibility monitoring has important policy implications. The recertification period may be too long if extensive dips and rebounds in income are found. If no strategic behavior is found, on the other hand, labor supply responses to the continuous eligibility provision can be ruled out, and the optimal eligibility recertification period can be computed based on the *mechanical* properties of income processes.

In this paper, I carry out an empirical investigation of the effect and optimality of the continuous eligibility provisions in the context of Medicaid/CHIP. Along with creating the SCHIP program, the Balanced Budget Act of 1997 gave states the option of continuously insuring children for up to 12 months in their public insurance programs regardless of changes in family income during that period. A third of the states implemented the continuous eligibility option in their public insurance program for children. These states present an opportunity to gauge the significance of the aforementioned strategic behavior, which then sheds light on the choice of the optimal continuous eligibility period. It should be pointed out that the incentives explored and the frameworks proposed in this study are not limited to public insurance for children: they also apply to other policy contexts such as cash welfare (TANF), food assistance (SNAP and WIC) and adult Medicaid. With many states opting into the Medicaid expansion under the Affordable Care Act of 2010 that grants eligibility to families between 100% and 133% of the Federal Poverty Line (FPL), for example, policy makers may need to redetermine the frequency of recertifying program eligibility given the influx of families from a different income stratum.

The contributions of this paper are three-fold. First, I recognize the potential dynamic impact of a long continuous eligibility period on the labor supply decisions of program participants. I derive qualitative and quantitative predictions of the family income process using neo-classical labor supply models that incorporate the dynamic budget constraint. Second, I empirically examine the model predictions using data from the Survey of Income and Program Participation (SIPP). Third, I propose a simple framework to compute the optimal length of the continuous eligibility period for children on Medicaid/CHIP; the framework extends the work in Prell (2008), which studies the optimal recertification frequency in WIC, by removing the parametric restrictions on the income process, incorporating partial benefit take-up and allowing alternative social welfare formulations.

Empirically, I follow the event-study specification from Jacobson et al. (1993) and trace out the family income process over time as children enrolled in Medicaid/CHIP. The graphical analysis provides no strong evidence of the dip-and-rebound strategic behavior in average income for families residing in states that provided 12 months of continuous eligibility even in subsamples where it was most likely to occur. Formal statistical tests are conducted to compare the empirical rebound magnitudes to those calibrated using dynamic variants of the Saez (2010) model. The tests account for the SIPP seam bias, which blurs the timing of Medicaid/CHIP transition and dilutes the observed income responses, by adopting behavioral assumptions from Ham et al. (2009). The results of the tests largely reject the calibrated rebound magnitudes, and the lack of labor supply response is consistent with Meyer and Rosenbaum (2001) and Ham and Shore-Sheppard (2005).

Comparisons of income processes between counterfactual groups are also carried out to address the issues of unaccounted income trends over a Medicaid/CHIP spell, concentration of strategic behavior in only a subset of the families as well as possible model misspecification in the calibration exercise. I compare the income processes between (1) high and low income families and (2) families in states that did and did not provide 12 months of continuous of eligibility to simultaneously address all three issues above. Both comparisons follow a difference-in-difference type approach; in particular, a symmetric difference-in-difference strategy from Heckman and Robb (1985) and Ashenfelter and Card (1985) is adopted for (1) to eliminate the biases created by the selection on and serial correlation in income. Statistical tests do not provide strong evidence that the groups in (1) and (2) have different income trends, underscoring the lack of strategic imposter behavior.

With labor supply responses practically ruled out, I rely on the mechanical properties of the income processes observed in SIPP to compute the optimal monitoring frequency in a simple framework extending the textbook model of Salanie (2003). Under various functional forms of social welfare and assumptions regarding take-up rate, I derive mappings from the recertification cost parameters to the optimal monitoring frequency. For moderate costs, the calculation suggests that 12 months may serve as a lower bound on the length of the optimal continuous eligibility period.

The remainder of the paper is organized as follows. Section 2 provides an overview of the Medicaid/CHIP institutions. Section 3 presents a series of models to illustrate the tradeoff in a continuous eligibility provision and to theoretically analyze families' responses to such a provision. Section 4 describes the data used, and empirical results are presented in Section 5. Section 6 tests the labor supply theory by calibrating the dynamic variants of the Saez (2010) model and by carrying out counterfactual analyses. Section 7 investigates the optimal length of the continuous eligibility period. Section 8 concludes.

2 Institutional Background of Medicaid and CHIP

The Medicaid program was created by the Social Security Amendments of 1965 and provides health insurance to low-income populations. The program originally targeted those traditionally eligible for welfare–single-parent families, the aged, blind and disabled. However, eligibility for public insurance through Medicaid has expanded substantially over time particularly for dependent children.

Over the 1980s and 1990s, the link between Medicaid and welfare for children was gradually severed through a series of legislative acts. Two of the largest federal expansions were included in OBRA 1989 and OBRA 1990, which became effective in April 1990 and July 1991 respectively. OBRA 1989 required states to offer Medicaid coverage to pregnant women and children up to age six with family incomes below 133% of the FPL. OBRA 1990 required states to cover children born after September 30, 1983 with family incomes below 100% of the FPL. The mandated minimum federal standards from the two expansions have remained until this day; since all children today were born after 1983, a child is eligible for Medicaid when her family income is below 100% of the FPL or 133% if she is under six (for a detailed account of the major Medicaid legislations by 1997, see Gruber (2003)).

The creation of the State Children's Health Insurance Program (SCHIP or CHIP) in 1997 has allowed many states to further expand their public insurance programs above these standards. Unlike Medicaid, SCHIP provides states with block grants to fund coverage for children and has by and large left the implementation of the program to the individual states. Specifically, states can use the funds to expand their existing Medicaid program, create a separate program for children who do not qualify for the existing Medicaid program, or a combination of both. Because of the freedom in implementation, state public insurance programs for children vary widely in their eligibility requirements. One particular feature of some state programs is a continuous eligibility period as permitted by the Balanced Budget Act of 1997, which provides eligible children with uninterrupted coverage for up to 12 months, regardless of whether their families' incomes rise above the eligibility threshold during this period. Table 1 lists the states by whether they provided 12 months of statutory continuous eligibility during 2000 and 2007, the time period underlying the empirical analysis of this study. For many of the states that did not provide 12 months of continuous eligibility, the recertification period is still 12 months but participating families are required to report changes in their circumstances soon after they occur.

In most states, Medicaid and CHIP eligibility is established based on the most recent monthly income for wage earning families, giving rise to the dynamic opt-in incentives. Table 2 summarizes the income proof requirements in states providing 12-month continuous eligibility, and the vast majority require one or multiple pay stubs from each job within a month of application. California requires one pay stub within 45 days of application, and a pay stub within the last month is admissible. Only Kansas requires all pay stubs within two months of application. Towards the end of the benefit year, families need to have their incomes verified again in order to continue their children's coverage by public insurance. A package containing renewal materials is typically sent to the current beneficiaries some time before their coverage is scheduled to end. As seen from Table 2, however, the timing of this renewal process varies across states, which can start as long as three months or as short as two weeks before the benefit year is over; in a few of the states, it is the local offices that determine when to initiate the recertification process. Because of differences in the timing of renewal, the month in which a strategically behaving family dips its income in order to pass eligibility renewal may vary. Given this ambiguity, I will focus on the behavioral response at the time of the initial program enrollment.

Along with the continuous eligibility provision, the Balanced Budget Act of 1997 also gave states the option of allowing presumptive eligibility for children. That is, states may allow children who appear eligible to obtain temporary Medicaid/CHIP eligibility (so that they may immediately access health care services) while their eligibility based on income is being confirmed.[2] Presumptive eligibility is relevant to the study at hand because children do not always need to meet the usual income requirements to receive

[2] Presumptive eligibility to infants and pregnant women was granted a decade earlier by OBRA '86.

public insurance coverage when it is granted. The states that provided presumptive eligibility to children in addition to allowing 12-month continuous eligibility between 2000 and 2007 are indicated in Table 2.

The last columns of Table 2 indicate whether the states have mandated a waiting period (three or six months) for children whose private insurance was voluntarily dropped before enrolling in CHIP. The intent of the waiting period is to mitigate the potential crowding-out of private insurance. Because the measure does not apply to Medicaid children nor to those who had not been enrolled in private insurance, however, the population it affects is likely to be small. Nevertheless, the existence of the waiting period still complicates the timing of the potential strategic behavior, and will be addressed in the empirical analyses.

3 Theoretical Framework of Eligibility Recertification in Transfer Programs

Means testing in transfer programs reflects the government's redistributive taste and its intention to target the needy. If income does not change over time and those in need remain in need, there is no point in monitoring program eligibility once families are allowed in the program. Therefore, the necessity of eligibility recertification stems from the possibility that a family having entered the program with a low income is no longer in need following a large positive income change. From the perspective of the government, this family should be taken off the program roster based on an income eligibility recertification. If monitoring eligibility is costless for both the government and family, then the government should perform an eligibility check every period to ensure targeting efficiency, as shown formally below. When there is a cost to eligibility monitoring, the choice of the length of the recertification period in part reflects the compromise between incurring this cost and transferring benefits to the non-needy.

It may be tempting to propose a long continuous eligibility period in the case of high administrative cost or low income volatility, but policy makers should also be wary of the potentially large labor supply distortions it may induce. In the extreme scenario mentioned above where the continuous eligibility period is infinite–once eligible due to a low monthly income, families can claim program benefits for a life-time–families of all income levels face strong incentives to temporarily lower their income and participate in the program, which will render the system unsustainable. Therefore, understanding families' income and labor supply response to the dynamic opt-in incentive is key in the consideration of the optimal monitoring frequency. In subsection 3.1, I first review the prediction of a class of standard static (i.e., assuming constant eligibility monitoring) neo-classical labor supply models in the presence of an in-kind transfer, and I show in subsection 3.2 that the dynamic problem with continuous eligibility provisions can be reduced to two static problems with different budget constraints. The solution to these problems predicts a dip and rebound in average income at each eligibility check.

3.1 Transfer Program and Labor Supply: Static Models

In this subsection, I analyze the family labor supply decisions when eligibility for Medicaid/CHIP is re-certified every period. This is the standard assumption in the conventional labor supply literature, and it stipulates that families are eligible for benefits only if their income is below a cutoff. The implications of this assumption have been explored in other studies (e.g., Blank (1989) and Yelowitz (1995)). I review it here using the utility functional forms based on Saez (2010) because results derived below will be relevant for the dynamic problem with continuous eligibility provision in section 3.2.

The Saez model is used to study the bunching behavior of economic agents in response to non-linearities in the tax schedule. The particular utility functional form has also been adopted in other recent papers, e.g., Chetty et al. (2011), that study the response to non-linearities in the budget constraint. In the model, agents maximize utility by choosing pre-tax income Z and post-tax income (consumption) C, and utility is assumed to increase with C and decrease with Z. The implicit assumption is that Z is an increasing function of labor supply, and it is equivalent to formulating the utility function in terms of C and hours worked H as noted below. Specifically, the utility function is of quasi-linear form

$$u(C,Z) = C - \frac{n}{1+1/e}\left(\frac{Z}{n}\right)^{1+1/e} \tag{1}$$

where n and e are parameters indicating the taste for work and the responsiveness of pre-tax income to a change in the tax rate. Solving the optimization problem implies that an agent chooses pre-tax income $Z^* = n(1-t)^e$ when facing the budget constraint $C = (1-t)Z$. As pointed out by Saez (2010), $Z^* = n$ when $t = 0$, and n can be interpreted as the income choice in the absence of tax and transfer programs. An agent with a larger n both works and consumes more, and n is assumed to be smoothly distributed according to density f_n across the population.

The parameter e is the elasticity of pre-tax income with respect to (one minus) the marginal tax rate because of the following identity $\frac{(1-t)}{(Z^*)}\frac{d(Z^*)}{d(1-t)} = e$. Note that if the utility function is written in terms of consumption and hours worked $u(C,H) = C - \frac{n}{1+1/e}(\frac{wH}{n})^{1+1/e}$ for an agent with taste n and wage rate w, it is also true that $\frac{(1-t)}{(H^*)}\frac{d(H^*)}{d(1-t)} = e$. Therefore I will refer to e interchangeably as the income or labor supply elasticity in the subsequent sections of this paper. As is well known, the quasi-linear utility functional form implies no income effects, so e is both the compensated and uncompensated elasticity (in fact, e is also the Frisch elasticity of income/labor supply).

The existence of Medicaid/CHIP induces at least one discontinuity in the relationship between consumption and income. For simplicity of exposition, however, I include only one such notch and a single marginal tax rate in the presentation of this section.[3] When eligibility is checked every month, this budget constraint can be thought of as being static. Following the "notch" specification adopted by Blank (1989) and Yelowitz (1995) with no saving or borrowing, the budget constraint is

$$C = [Z(1-t)+g]1_{[Z\leqslant\gamma]} + Z(1-t)1_{\{Z>\gamma\}} \tag{2}$$

where γ is the Medicaid/CHIP eligibility cutoff, g the monthly value of public insurance and t the marginal tax rate. As pointed out by studies of Blinder and Rosen (1985), Blank (1989) and Kleven and Waseem (2011), no family will choose income to be just above the threshold. This is intuitive because a family consumes more and works less by choosing its income to be at the eligibility cutoff rather than just above it. Certain families who would have chosen $Z > \gamma$ in the absence of Medicaid/CHIP would now switch to γ. Solving the optimization problem predicts the choice of Z^* for a family of type n:

$$Z^* = \begin{cases} n(1-t)^e & \text{if } n \leqslant n_\gamma \text{ or } n > \bar{n} \\ \gamma & \text{if } n \in (n_\gamma, \bar{n}) \end{cases}$$

where $n_\gamma = \frac{\gamma}{(1-t)^e}$ is the type of agent who would choose income at γ in the absence of the notch and $\bar{n}$ is the highest type of agent who would choose γ in the presence of the notch. Figure 1 provides a graphical illustration: an agent with $\bar{n}$ is indifferent between the consumption-income bundle at the notch $(\gamma(1-t)+g, \gamma)$ and her optimal choice in the absence of the notch $(\bar{n}(1-t)^{1+e}, \bar{n}(1-t)^e)$. Therefore, $\bar{n}$ is the solution to the equation

$$\gamma(1-t)+g-\frac{\bar{n}}{1+1/e}(\frac{\gamma}{\bar{n}})^{1+1/e} = \bar{n}(1-t)^{1+e} - \frac{\bar{n}}{1+1/e}(1-t)^{1+e} \tag{3}$$

[3]Families enrolled in CHIP with income above the 150% FPL may be subject to moderate premiums and co-payments, which implies a lower CHIP notch than that of Medicaid. The empirical impacts of presence of other notches (induced by Medicaid or other transfer programs) are discussed in section 6.1.

In summary, the standard static model makes the prediction that, when a benefit notch is introduced, agents originally choosing income just above the eligibility cutoff may lower their labor supply and become eligible for program benefits. This distortion is well established in the literature, and it is robust to various extensions of the baseline model. I discuss one important extension that adds income effects in the remainder of this subsection, which is examined in the empirical analyses along with the baseline model. Other extensions of the baseline model–allowing for discrete labor supply choices, heterogeneous elasticity and program participation cost–are investigated in the Supplemental Appendix.[4]

As mentioned before, the quasi-linear functional form of (1) does not allow income effects. This may be reasonable in the context of a tax rate change (i.e. a kink in the budget constraint), since tax rate changes have little effect on average tax rates as noted by Chetty et al. (2011). In the case of a notch, however, the absence of income effects in modeling may no longer be appropriate. Here I explore the implication of using a functional form that allows non-zero income effects.

Consider the utility function

$$u(C,Z) = \frac{C^{1-\rho}}{1-\rho} - \frac{n}{1+1/e}(\frac{Z}{n})^{1+1/e} \tag{4}$$

which displays constant relative risk aversion in consumption, and which encapsulates the quasi-linear utility (1) as a special case when $\rho = 0$. When facing a budget constraint $C = (1-t)Z$, the optimal interior pre-tax income choice is $Z^* = (1-t)^{\frac{e-\rho e}{\rho e+1}} n^{\frac{1}{1+\rho e}}$. Therefore, $n^{\frac{1}{1+\rho e}}$ is the agent's desired income choice when $t = 0$. Instead of the Marshallian, Hicksian and the Frisch elasticity all being e as in the quasi-linear case, e for the utility functional form (4) is only interpreted as the Frisch elasticity of income/labor supply. This interpretation of e will be convenient for the dynamic problem I consider below. Note that the Marshallian elasticity of labor supply with respect to the marginal tax rate reduces to $\frac{\partial Z^*}{\partial(1-t)} \frac{1-t}{Z^*} = \frac{e-e\rho}{\rho e+1} < e$ when $\rho, e > 0$, whereas the Hicksian elasticity varies across agents.[5]

The analyses undertaken in section 3.1 carry through with the more general utility function (4) although the expressions for the various $\bar{n}$'s will change. Therefore, the introduction of non-zero income effects does not change the qualitative prediction that certain agents will lower their labor/income supply when a benefit notch is introduced. Intuitively, the labor supply distortion hinges on the convexity of the indifference curves, which is not altered when curvature in consumption utility is introduced.

[4]The Supplemental Appendix is online at https://sites.google.com/site/peizhuan/files/Supplemental Appendix.pdf.

[5]See MaCurdy (1981) and Browning et al. (1985) for discussions on the magnitudes of the three elasticities.

3.2 Continuous Eligibility and Labor Supply–Dynamic Models

This section extends the static framework in the previous section to incorporate continuous eligibility provisions. In essence, the provisions allow a set of *dynamic* budget constraints as opposed to the static expression of (2). More specifically, families that are just approved for public insurance can have income above γ and remain covered until the eligibility recertification a year later. To characterize a family's consumption and labor supply decisions in the presence of continuous eligibility provisions, I cast the family's utility maximization problem in a dynamic programming framework.

Formally, the state variable s is the number of months until recertification (s is defined to be 0 for those not claiming benefits since they will face the eligibility check when they apply), and let τ be the number of months of provided continuous eligibility. In each period, an agent chooses whether or not to participate in the program:

$$V_s = \max_{P_s} P_s V_s^1 + (1-P_s)V_s^0$$

where $P_s = 0,1$ denotes participation choice, and V_s^1 and V_s^0 are utilities associated with participating and not participating in the program when agents are s months away from an eligibility check. Formally, the expressions for V_s^1 and V_s^0 are

$$V_s^1 = \max_{C,Z}\{u(C,Z)+\beta V_{s'}\}$$
$$\text{s.t. } Z<\gamma \text{ if } s=0;\ C=(1-t)Z+g$$
$$s' = \begin{cases} s-1 & \text{if } s>0 \\ \tau-1 & \text{if } s=0 \end{cases}$$

$$V_s^0 = \max_{c,z}\{u(C,Z)+\beta V_{s'}\}$$
$$\text{s.t. } C=(1-t)Z$$
$$s' = \begin{cases} s-1 & \text{if } s>0 \\ 0 & \text{if } s=0 \end{cases}$$

where β is the monthly discount rate. For illustration purposes, first consider the simple case when $\tau = 2$, in which case s takes on the value 0 or 1. Let $\{C_s^p, Z_s^p\} = \text{argmax} V_s^p$ for $p = 0,1$. The dynamic problem simplifies to

$$\begin{aligned} V_0 &= \max_{P_0} P_0\{u(C_0^1,Z_0^1)+\beta V_1\}+(1-P_0)\{u(C_0^0,Z_0^0)+\beta V_0\} \\ V_1 &= \max_{P_1} P_1\{u(C_1^1,Z_1^1)+\beta V_0\}+(1-P_1)\{u(C_1^0,Z_1^0)+\beta V_0\} \end{aligned} \tag{5}$$

and I will characterize the optimal P_s, C_s^p and Z_s^p's below.

First note that choosing $P_1 = 1$ strictly dominates $P_1 = 0$ because (C_1^0, Z_1^0) lies in the interior of the budget set for an agent with $s = 1$. In other words, when benefits can be claimed without having to lower income, a rational family will do so. This reasoning simplifies the expression for V_1 to $V_1 = u(C_1^1, Z_1^1) + \beta V_0$. Plugging in this expression of V_1 into that of V_0 leads to

$$V_0 = \max_{P_0} P_0\{u(C_0^1, Z_0^1) + \beta u(C_1^1, Z_1^1) + \beta^2 V_0\} + (1 - P_0)\{u(C_0^0, Z_0^0) + \beta V_0\}$$

For the agents indifferent between choosing $P_0 = 0$ and $P_0 = 1$,

$$V_0 = u(C_0^1, Z_0^1) + \beta u(C_1^1, Z_1^1) + \beta^2 V_0 = u(C_0^0, Z_0^0) + \beta V_0$$

and therefore $V_0 = \frac{u(C_0^0, Z_0^0)}{1-\beta}$. It follows that

$$u(C_0^1, Z_0^1) + \beta u(C_1^1, Z_1^1) = u(C_0^0, Z_0^0) + \beta u(C_0^0, Z_0^0) \tag{6}$$

If u has the functional form in (1), then $C_1^1 = C_0^0 + g$ and $Z_1^1 = Z_0^0$ because of quasi-linearity. Consequently, $u(C_1^1, Z_1^1) = u(C_0^0, Z_0^0) + g$, and (6) leads to

$$u(C_0^1, Z_0^1) + \beta g = u(C_0^0, Z_0^0) \tag{7}$$

Suppose (C_0^1, Z_0^1) satisfying (7) is an interior solution. Then the convex indifference curve passing through the bundle (C_0^1, Z_0^1) is tangent to the program segment of the budget constraint and therefore lies above the non-program budget constraint $C = (1-t)Z$. Consequently, $u(C_0^1, Z_0^1) > u(C_0^0, Z_0^0)$ implying that $u(C_0^1, Z_0^1) + \beta g > u(C_0^0, Z_0^0)$, contradicting (7). Therefore, the (C_0^1, Z_0^1) that satisfies (7) has to be a corner solution with $Z_0^1 = \gamma$. Denote the indifferent agent's type by $\bar{n}^{dynamic}$ and expanding (7) using the quasi-linear functional form leads to

$$\gamma(1-t) + (1+\beta)g - \frac{\bar{n}^{dynamic}}{1+1/e}\left(\frac{\gamma}{\bar{n}^{dynamic}}\right)^{1+1/e} = \bar{n}^{dynamic}(1-t)^{1+e} - \frac{\bar{n}^{dynamic}}{1+1/e}(1-t)^{1+e} \tag{8}$$

Equation (8) states that an agent of type $\bar{n}^{dynamic}$ is indifferent between choosing her interior solution on the budget constraint segment $C = (1-t)Z1_{\{Z>\gamma\}}$ and the post-tax/pre-tax income bundle $(\gamma(1-t) + (1+\beta)g,\ \gamma)$. Analogous to the analyses in section 3.1 and illustrated in the right panel of Figure 1, agents with $n \leqslant \bar{n}^{dynamic}$ choose to participate in the program and those with $n > \bar{n}^{dynamic}$ do not. While those in the low potential income range with $n \in (0, n_\gamma]$ participate without altering their labor supply, those with higher potential income $n \in (n_\gamma, \bar{n}^{dynamic}]$ will lower their income when $s = 0$ to gain eligibility but revert back to their desired interior solution with an income above γ when their eligibility is not checked (i.e. $s > 0$). Comparing (8) to (3) reveals that doubling the length of the recertification period in effect doubles the benefit notch if $\beta \approx 1$. It is easy to show that for a general recertification period τ, the size of the benefit notch an agent faces is effectively $\sum_{i=0}^{\tau-1} \beta^\tau g \approx \tau g$ when making her participation decision in month $s = 0$. Because of the quasi-linearity of the flow utility function, adding assets and allowing agents to borrow inter-temporally have no effect on the optimal income path.

When income effects are take into account using flow utility (4), the solution may no longer be obtained analytically. But qualitatively speaking, once a new applicant family is approved for benefit, the transfer may reduce labor supply through the income effect channel. This implies that the rebound in income after starting a public insurance spell will not be as large as when income effects are absent. In addition, the curvature puts pressure on agents to smooth consumption, and fewer agents choose to participate in the program as a result. Different from the quasi-linear case, the rebound can be shown to be stronger when assets and inter-temporal borrowing are introduced to the model with income effects. In a nutshell, agents applying for benefits will borrow from savings or against the future to smooth consumption and consequently want to work more when their income choices are not constrained.[6] Assets are arguably not very relevant to the population interested in participating in public insurance, and even if they are, omitting assets from the model will lead to *conservative* predictions on the magnitudes of the dip and rebound.

To summarize, the dynamic labor supply models make the prediction that average income drops at the income eligibility check and rebounds afterward. The magnitudes of the dip and rebound depend on the model parameters ρ and e. In the following sections, I will investigate whether families' empirical behaviors conform to the qualitative and quantitative predictions of the neoclassical models.

4 Data and the Construction of the Analysis Sample

To examine the income and labor supply responses to the continuous eligibility provisions in Medicaid/CHIP, I use data from the 2001 and 2004 panels of the Survey of Income and Program Participation. SIPP is a representative household survey designed to provide detailed information on income dynamics and gov-

[6]Few states mandated asset tests in determining children's eligibility for Medicaid/CHIP during the sample period. In 2007, for example, only South Carolina required the asset test among the states that provided 12-month continuous eligibility.

ernment program participation. Each of the panel files contains four rotation groups, only one of which is interviewed in a given calendar month. This means that each adult member of the participating household was interviewed every four months about his or her experiences since the last interview (the four-month interval is called the reference period, and the four months are referred to as reference month 1 to reference month 4). The two SIPP panels span the period from October 2000 to December 2003 and from October 2003 to December 2007 respectively. All four rotation groups in the 2001 panel provide information for 36 consecutive months, and those in the 2004 panel for 48 months.

The chief advantage of SIPP over other candidate data sets (CPS, PSID, HIS, etc.) is its panel structure at the monthly frequency and the rich array of variables on income, program participation and family structures. Since the focus of the study is to examine family income responses immediately before and during a child's Medicaid/CHIP spell, SIPP is the best choice among public use survey data sets. There are, however, several important limitations of the SIPP data.

The first limitation is the existence of the well-known seam bias, which refers to the fact that changes in income and program participation are *under*-reported *within* a single four-month reference period and *over*-reported *between* two reference periods (see Pischke (1995) and Ham et al. (2009)). For example, children's reported public insurance coverage spells are much more likely to start on reference month 1 than reference month 2, 3 or 4. In fact, about 83% of the fresh spells in the 2001 panel analysis sample and 91% of the fresh spells in the 2004 panel analysis sample start on the first month of the reference period. The seam bias dilutes the income responses, and I address it in section 6 by adopting the behavioral assumptions of Ham et al. (2009). Second, as is true with all survey data, Medicaid and CHIP coverages cannot be reliably distinguished.[7] Therefore, I will use public insurance coverage which encapsulates both Medicaid and CHIP, and the phrase "public insurance" will be used interchangeably with Medicaid/CHIP. Third, identifiers of families in less populated states are missing from the 2001 panel. Families in Maine and Vermont share the same geographical identifier, and the state of residence for North Dakota, South Dakota and Wyoming families cannot be identified either. Because these states have different Medicaid/CHIP policy parameters, they are excluded from the analysis sample. Due to the larger sample size of the 2004 panel, all fifty states plus the District of Columbia have their own identifier, and therefore every state can be included.

[7] An expert at the U.S. Census Bureau noted in a correspondence that "respondents rarely know with certainty whether their child is in Medicaid or CHIP... We found this out with the 2004 SIPP instrument, where question order happened to be revised so that CHIP was asked about before Medicaid. Here we observed that respondents were most likely to answer the question asked first, resulting in higher reported levels of CHIP than of Medicaid for Panel 2004)."

The main analysis sample consists of children living in states providing 12-month continuous eligibility, who had started a public insurance spell during the SIPP panel. The reason for choosing children instead of families as observation units is that individuals are be tracked through the panel without ambiguity but the definition of a family may change. The restriction to "fresh" spells comes from the necessity of identifying the timing of benefit application, which is not possible with the left-truncated spells, which start with the child's first appearance in the panel. In addition, spells started by infants, by children who moved to another state[8] or by children on the Supplemental Security Income (SSI) program are excluded from the analysis sample. Infants are excluded because most states have been extending presumptive eligibility to infants since the 1990's; children whose families moved across states pose a challenge in the assignment of Medicaid/CHIP parameters; and children on the SSI program were conferred automatic eligibility for public insurance. As shown in Table 3, the analysis sample consists of 2582 and 2821 fresh public insurance spells for the 2001 and 2004 panels respectively.

Nuclear families for each child are constructed using information on the relationship to the household and family reference person (head). In the cases where a child and his or her parent(s) live with other adults, however, families include only the children and parent(s) of the appropriate subfamily. This definition corresponds to the family assistance unit typically used in the determination of eligibility for means-tested programs. Family level variables are calculated by aggregating over individual family members, and family income includes earned and unearned incomes excluding welfare receipts and children's incomes.[9]

Finally, the state level Medicaid/CHIP data are extracted from reports issued and databases maintained by various organizations. The policy parameters (e.g., continuous eligibility, presumptive eligibility, income eligibility cutoffs, etc.) come from NGA (2000-2008), Kaiser (2000-2011) and CMS (Various Years). Medicaid/CHIP spending and enrollment data are extracted from the Kaiser Foundation State Health Facts database and the CMS Medicaid Statistical Information Statistics System.

Table 4 presents variable averages for both the 2001 and 2004 SIPP panels immediately before and during the first month of public insurance spells (spell month 0 and spell month 1 respectively). The 2001 and 2004 summary statistics are reasonably similar. On average, a child switched onto public insurance

[8]I do include children who moved across states during the panel as long as the move did not occur during a public insurance spell. As a result, the fourth row of Panel (a) of Table 3 is the sum of the last three rows for columns 1 and 2, but not for columns 3-6.

[9]The exclusion of children's income is due to the fact that student income is disregarded for the purpose of Medicaid/CHIP eligibility determination. Whether or not other adult family members' incomes are included in the computation of family income in addition to those of the parents makes little difference empirically.

during a SIPP panel when she was between 8 and 9 years old. About half of the children were female, and around 20% were black. The average child lived in a four-person nuclear family, and 50% to 60% came from two-parent families. The vast majority of the families were working families–only around 10% did not have non-welfare income around the time their child started a public insurance spell and less than 5% of the parents claimed unemployment benefits. Around 20% of the families were on Food Stamp, and less than 5% of the families received welfare cash transfers from TANF programs. The median family income in spell month 0 was just below \$1900 in 2010 dollars for the 2001 panel and just below \$2300 for the 2004 panel. In comparison, the spell-month-1 median income was slightly lower (1.5-2%) in both panels. In the next section, I will provide a description of the income processes over a much wider time window, while controlling for individual and time fixed effects and economic conditions.

5 Descriptive Analysis of Income and Labor Supply Responses

5.1 Empirical Specification and Full-sample Results

In this section, I present descriptive evidence on families' income responses over their childrens' Medicaid/CHIP spell. First, I follow a flexible event-study specification adopted by Jacobson et al. (1993), and estimate the fixed effect regression

$$Y_{it} = \omega_i + \lambda_t + \sum_{|k| \leqslant m} D_{it}^k \delta_k + X_{it}\beta + \varepsilon_{it} \tag{9}$$

where ω_i and λ_t are individual and calendar month fixed effects. k is the "spell month": $k = 1$ indicates the first month of public insurance coverage, and $k = 0$ the month immediately before coverage begins. D_{it}^k is a dummy variable, which takes the value of 1 if child i in calendar month t started her public insurance $(k-1)$ months earlier (or, if k is negative, child i began her coverage k months later). X_{it} is the unemployment rate of individual i's state in calendar month t. Spell month 0 is the omitted category in the regression. As a result, $\delta_0 = 0$ by construction, and the δ_k's measure the difference in the average outcome in spell month k relative to spell month 0.

Since families were interviewed for 36 months in the 2001 panel and 48 months in the 2004 panel, it is possible to allow $m = 35$ for the 2001 panel and $m = 47$ for the 2004 panel. However, there are few families who start a spell at the second or the last month of the panel which render the estimation of δ_m and

δ_{-m} imprecise for large m. Therefore, I set $m = 24$ and examine the income and labor supply responses 24 months before and after the beginning of a spell. Even though the sample period for $m = 24$ covers two recertifications, I will only focus on the initial entry of program participants as discussed in section 2, due to the variability in the renewal process and the resulting ambiguity in the timing of potential strategic behaviors.

As is evident from Table 3, some of the children in the analysis sample experience multiple public insurance spells. Since an event-study framework usually calls for single status transitions, each fresh spell is constructed such that it only contain a single transition from non-coverage to coverage. Specifically, let k^- denote the last month before spell month 0 ($k^- < 0$) the child was covered by public insurance, and let k^+ denote the first month after spell month 0 ($k^+ > 0$) the child switches off public insurance. For each fresh spell, I discard all observations before k^- and after k^+, which eliminates status transitions from coverage to non-coverage. All fresh spells constructed in this manner are included in the estimation of (9), and the clustering of standard errors at the individual level or higher accounts for the correlation in the outcome variables across multiple spells for the same child.

Figure 2 plots the movement of average family incomes, i.e. the estimates of the $\delta_k's$ from (9), over the 48 months around the beginning of a public insurance spell.[10] Both the point estimates and the point-wise 95% confidence intervals are shown, for which the standard errors are clustered at the SIPP variance stratum level.[11] Neither of the figures show a pronounced dip-and-rebound in the six months before and after the spell start. For the 2001 Panel, the income trend leading up to the beginning of spell is practically flat; the average income increases gradually during the public insurance spell especially after 12 months, but the period immediately following the spell start shows no rebound. In the 2004 panel, the income process shows a persistent downward trend throughout the 4-year window without a visible rebound.

Unfortunately, SIPP does not collect information on hours worked at the monthly level but usual hours worked are reported for the entire wave (four months). Using this wave-frequency variable, however, clouds the model-predicted dip-and-rebound movements in the *immediate* months surrounding the spell start. Therefore, I describe the labor supply movement using a less precise variable than hours worked but at

[10]The regression is weighted using the person weight at the beginning of a spell. Weighting has little impact on the empirical estimates.

[11]The primary sampling unit (PSU) and strata codes are not included in the SIPP public use data files due to confidentiality concerns. Instead, SIPP constructs variance units and strata, which are meant to be treated as PSUs and strata in variance estimations. There are 105 and 114 variance strata in the 2001 panel and 2004 panel full analysis sample, respectively. See Chapter 7 of Westat (2008) for details.

the monthly frequency. In particular, I construct a dummy variable indicating whether or not the head of the family–defined to be father in a two-parent family and all single-parent family heads–worked more than 35 hours for all weeks during a month, and plot its movement in Figure A.1. There is a downward trend for the two years before the spell start in both panels. The downward trend continues in the 2001 panel for about 6 months, whereas it flattens in the 2004 panel. As with the income processes in Figure 2, no salient rebound is present in the labor supply processes.

Even though the strategic behavior predicted by the labor supply model is not salient in the full sample, certain subgroups may be expected to exhibit stronger responses than others. Examining these subgroups separately may help to isolate the effects that are otherwise masked in the full sample. I carry out subsample analyses in the following subsection.

5.2 Subsample Analyses

In this subsection, I estimate (9) in numerous subsamples. A particular subsample is chosen because: (a) it allows a lower degree of error in identifying a fresh public insurance spell, (b) it removes some of the ambiguities from other institutional provisions that complicate the incentives represented in the simple labor supply model, (c) it is easier for the families in the subsample to adjust their labor supply, (d) it consists of families more likely to understand program rules, or (e) the families in the subsample have a stronger incentive to behave strategically. As a result, I expect the strategic behavior to be more easily detected in the chosen subsamples.

Subsample 1 consists of children who report no public insurance coverage for 12 consecutive months before the start of a spell. The selection of this "long gap" sample follows the observation that many fresh spells are preceded by a short gap in public insurance coverage. Short coverage gaps are not inconsistent with what is found using administrative data; for example, Fairbrother et al. (2011) show that 40% of the children whose coverage was not renewed at month 12 re-enroll within a short period. However, the presence of short gaps before spell start does cause concern regarding the reliability of the identification of fresh spells. Although it is rare for families to report coverage while not covered by public insurance (Card et al. (2004)), the converse is more common, and the under-reporting of coverage at a particular month during a long spell will lead to the false identification of starting a fresh spell. This measurement error problem is mitigated by restricting observations to the long gap sample. By virtue of its construction, subsample 1 consists mostly of single public insurance spells.

The next two subsamples are chosen to minimize interactions with other aspects of the Medicaid program and isolate the effect of the 12-month continuous eligibility provision. As mentioned in section 2, children in a presumptive eligibility state do not always need to meet the usual income requirements when receiving public insurance coverage, and the existence of a CHIP waiting period for children whose private insurance voluntarily dropped immediately before enrolling in CHIP complicates the timing of the strategic behavior. Therefore, subsample 2 is restricted to children residing in states that neither provided presumptive eligibility nor mandated a CHIP waiting period in the sample period. The other subsample intends to resolve the complications created by parental Medicaid. Since the renewal period for adult Medicaid tends to be shorter and that income change reporting is expected, family income for children who transitioned into public insurance with their parents may not rebound much. Therefore, I exclude children whose parents took up Medicaid when they began their public insurance spell in subsample 3.

Subsamples 4 and 5 consist of children living in families that have more flexibility in adjusting labor supply. Children in two-parent families form subsample 4 and those whose parents had worked in the construction or retail industries during the sample period constitute subsample 5. Two-parent families may be less credit constrained and can shift their labor supplies more easily. Many jobs in the construction and retail industries are of seasonal nature and parents may time their government benefit application when they do not work.

Children who has at least one college-educated parent are grouped in subsample 6. The expectation is that highly educated parent(s) may be more likely to understand program rules and the incentives therein. Finally, subsample 7 consists of children who have more than one child sibling. Public insurance is more valuable for families with more children resulting in a stronger dynamic-opt incentive.

The sample sizes of the various subsamples are enumerated in Panel (b) of Table 3, and the income responses from the estimation of (9) are plotted in Figures A.2-A.8. A downward trend before the starting of a public insurance spell is visible in many of the figures, especially in the 2004 panel. The rebound after the spell start, however, is absent in all of the subsample figures as is the case with the full sample. The income processes during the year after spell start are either flat or declining.

In summary, no strong descriptive evidence from SIPP 2001 and 2004 supports the dip-and-rebound in the income processes as predicted by the labor supply theory. This is true even in the various subsamples where strategic behavior–if it exists–should be more easily detected. However, strategic behavior cannot yet be strictly ruled out since it cannot be rejected that the δ_k's are positive for $k > 0$. In the next section, I investigate whether the empirical estimates are consistent with the *quantitative* theoretical predictions–formal tests are conducted by comparing the empirical rebound magnitudes to the calibrated model predictions.

6 Testing Model Predictions

6.1 Calibration of the Labor Supply Model

In this subsection, I calibrate the simple dynamic model from section 3 to benchmark the observed empirical income processes. The calibration exercise requires the assignment of sensible values or distributions to the set of parameters in the model: income supply elasticity e, consumption curvature ρ, discount factor β, taste/potential income n, public insurance eligibility threshold γ, value of public insurance g, and marginal tax rate t. I present the calibration results under combinations of e, ρ and β that are commonly found in the empirical literature whereas the choices for the distributions of n, γ, g and t are data-driven.

Specifically, I sample with replacement 100,000 children without health insurance living in states providing 12-month continuous eligibility from both the 2001 and 2004 SIPP panel. Based on the state of residence, calendar year and family structure and family income of the observation, each child's family is assigned a marginal tax rate t[12]. Recall that the optimal pretax income choice for a family with utility function (4) facing the budget constraint $C = (1-t)Z$ is $Z^* = (1-t)^{\frac{e-\rho e}{\rho e+1}} n^{\frac{1}{1+\rho e}}$. For each choice of ρ and e, therefore, I solve for the taste parameter of each child's family by $n = \frac{Z^*}{(1-t)^e}[(1-t)Z^*]^{\rho e}$ in the spirit of Brewer et al. (2010) where Z^* is the observed family income. Calculating n as such ignores the fact that some of the families in the calibration sample may be behaving strategically and that their Z^* is smaller than $(1-t)^{\frac{e-\rho e}{\rho e+1}} n^{\frac{1}{1+\rho e}}$. It follows that the n's for these families are underestimated, and the calibration exercise becomes conservative with a downward bias in the predicted dip-and-rebound magnitude.

For the program parameters, each child in the calibration sample is assigned a CHIP eligibility threshold γ (the highest public insurance eligibility cutoff) given the state and year of the observation. g is taken to be the benefit notch associated with the CHIP program and is calibrated from the annual spending data. The spending data exclude beneficiary and third-party payments and reflect expected government subsidy, and using the considerably smaller CHIP notch as compared to its Medicaid counterpart[13] again renders

[12] Parents in a dual-headed family are assumed to file jointly and claim the deduction accordingly and that all families are assumed to claim standard deductions.

[13] According to the Kaiser Family Foundation, the annual per child government spending in Medicaid and CHIP are \$2171 and \$1363, respectively, for the 2008 fiscal year.

the calibration exercise conservative. State-by-state spending per-enrollee figures are not available for years earlier than 2004 from the Kaiser Foundation or the Center for Medicare and Medicaid Services. Therefore, I use the average per-enrollee spending for the entire U.S. as a measure of the notch, which grew 5% annually from \$835 in 2001 to \$1217 in 2007 in nominal terms. The monthly benefit amount per child g_0 in a given year is $\frac{1}{12}$ of the annual per-enrollee spending that year, and the benefit notch g for that child's family is calculated as g_0 times the number of children therein.

In order to generate the dip-and-rebound magnitudes implied by the model, I calculate the pre-tax income path for each child's family and the resulting utilities of enrolling and not enrolling her in public insurance, i.e. the 12-month analog of Z_s^p, V_1 and V_0 in (5). It follows that the predicted strategic income responses are the average changes in pre-tax income around month 0 among those choosing to participate in public insurance ($V_1 > V_0$): the dip is the average of $Z_0^0 - Z_0^1$ and the rebound $Z_s^1 - Z_0^1$ for $0 < s < 12$. The participant group, which the income responses are averaged over in the calibration exercise, is consistent with the sample choice in section 5 where I focus on the children who transition into public insurance. The SIPP sampling weights are accounted for in calibrating the income responses so that they are comparable to the empirical estimates.

For the remaining parameters of the model, the standard annual discount factor of 0.95 is used, which implies a monthly discount factor of $\beta = (0.95)^{1/12} = 0.996$. Three values for ρ and two for e are chosen to calibrate the model : $\rho = 0$, $\rho = 0.57$, $\rho = 1.47$, $e = 0$ and $e = 0.15$. As ρ increases, the predicted dip-and-rebound magnitude decreases largely because the desire to smooth consumption mitigates the dynamic opt-in incentives. The predicted income response goes up with e because larger labor supply elasticity makes families more responsive to non-smoothness in their budget constraint. Therefore, setting $\rho = 0$ gives the maximum income responses for any given e whereas letting e go to 0 gives the minimum income responses for any given ρ.[14] For the other two choices of ρ, 0.57 is the average curvature from empirical micro studies using an additive utility function according to Chetty (2006)[15] and 1.47 is the largest estimate reported within the group of studies. As surveyed by Chetty (2012), 0.15 is the average Hicksian elasticity in the empirical micro literature for non-top income population.[16] Setting $e = 0.15$ is meant to present the range income responses predicted by the model, but the focus will be on the case of $e = 0$, which deliver the

[14]Different from a kinked budget constraint as in the case of Saez (2010), strategic behavior is expected in the presence of a notch even for the limiting case of $e = 0$.

[15]It is the average of the estimates in Column (6) across Panel A, B and C in Table 1 of Chetty (2006).

[16]As mentioned in section 3, e is the Hicksian elasticity only in the quasi-linear model but is generally larger than the Hicksian elasticity with consumption curvature.

minimum dip-and-rebound magnitudes.

Table 5 summarizes the calibration results, and the predicted income responses range from \$183 to \$387 in 2010 dollars. The models calibrated using 2001 and 2004 SIPP data lead to similar magnitudes with the latter being slightly larger. As expected, the degree of income responses increases with e and decreases with ρ, and the rebound is smaller in magnitude than the dip for $\rho, e > 0$, reflecting the demand for more leisure as children in families acquire public insurance. It is an interesting feature of the model (4) that the income effect vanishes when e goes to 0, and the dip and rebound are again symmetric even when $\rho > 0$.[17]

With the quantitative theoretical predictions established, formal statistical tests are carried out in the reminder of this subsection to examine whether the empirical rebound magnitudes are consistent with the model. A challenge in comparing observed and model predicted income processes is the seam bias in the SIPP data mentioned in section 4. As explained in Pischke (1995) and Ham et al. (2009), seam bias is largely the result of the "telescoping behavior", meaning that survey respondents answer retrospective questions using their most recent income and program participation status. The seam bias leads to complications because it attenuates income responses even if every family behaves according to the model. To illustrate this point, let T^* and T denote the true and reported reference month of the beginning of a public insurance spell; since there are four reference months within a wave, T^* and T both take on values of 1 through 4. Let Y_m^* and Y_m denote the true and reported family income for month m of the public insurance spell.[18] As an example, consider a child who started her spell in October and her family is interviewed by SIPP in December. Since September, October, November and December will be reference 1 through 4 for her family, her true transition reference month of October implies that $T^* = 2$. If her family telescopes, public insurance coverage will be reported for the entire wave based on the fact that she is covered in December, and the reported reference month for starting public insurance is September, implying $T = 1$. Telescoping in income suggests that her family's reported September income is the same as their December income. Because September is month 1 in the reported public insurance spell and December is month 3 in the true public insurance spell, it follows that $Y_1 = Y_3^*$. By the same logic, $Y_0 = Y_{-1}^*$, and the observed rebound magnitude reported for spell month 1 is $\Delta Y_1 \equiv Y_1 - Y_0 = Y_3^* - Y_{-1}^*$, which is zero if the family behaves

[17] Since $\lim_{e\to 0} u(C,Z) = \begin{cases} \frac{C^{1-\rho}}{1-\rho} & \text{if } Z \leqslant n \\ -\infty & \text{if} Z > n \end{cases}$, families with taste parameter n will choose $Z = n$ when the budget constraint is of the form $C = (1-t)Z + g$. The optimal Z does not decrease with g implying 0 income effect.

[18] For exposition, I only focus on the measurement error induced by the seam bias here and assume that families report their true income in the absence of telescoping behavior. Adding a standard classical measurement error component will not affect the analysis since it will be averaged out when examining the mean income responses.

according to a model with no income effect. Applying the same line of reasoning reveals that ΔY_1 is a valid measure of the model-predicted rebound magnitude only for those with $T^* = 1$ among the telescoping families.

To address the complications resulting from the seam bias, I adopt some of the assumptions proposed in Ham et al. (2009):[19]

Assumption 1) a respondent either telescopes or reports truthfully: $T = 1$ or $T = T^*$,

Assumption 2) the true reference month for starting a public insurance spell has a uniform distribution, implying $\Pr(T^* = t^*) = \frac{1}{4}$ for $t^* = 1, 2, 3, 4$.

It follows from Assumption 1) that if a transition into public insurance truly happened in reference month 1, then the respondent reports so: $T^* = 1 \Rightarrow T = 1$. It also implies that if a child's public insurance spell was reported to start in the second, third or fourth reference month, then it truly started in that month: $T = s \Rightarrow T^* = s$. Assumption 2) is justified by the SIPP survey design as noted in Ham et al. (2009). The entire sample is randomly split into four rotation groups, and one rotation group is interviewed each calendar month. Therefore, each calendar month is reference month 4 for rotation group 1, reference month 3 for rotation group 2, reference month 2 for rotation group 3 and reference month 1 for rotation group 4.

Under these assumptions and the null hypothesis that the families behave according to the labor supply model, the following relationship is established linking the observed average rebound in spell month k, $E[\Delta Y_k]$, to the predicted magnitude $y^{\rho,e}$ from a model with curvature parameter ρ and elasticity e:[20]

$$\frac{E[\Delta Y_k]}{\frac{1}{4} + \sum_{s=2}^{4} \Pr(T = s)} = y^{\rho,e} \tag{10}$$

Equation (10) holds for $k = 1, ..., 8$; it does not necessarily hold for $k = 9, 10, 11$ because the timing of the strategic behavior for families facing eligibility recertification is ambiguous as discussed in section 2 and that telescoping affects these three spell months differently even if the recertification process requires income proof from month 12[21]. The formal derivation of (10) is shown in the Supplemental Appendix, but on an intuitive level, it is easy to see that $\sum_{s=2}^{4} \Pr(T = s) = \frac{3}{4}$ and consequently $E[\Delta Y_k] = y^{\rho,e}$ if no one telescopes.

[19] Assumptions 1) combines A1), A2) and A4) , and Assumption 2) is in the spirit of A5) of Ham et al. (2009). Unlike Ham et al. (2009), however, I do not attempt to impose parametric restrictions and estimate the probabilities of $\Pr(T = 1|T^* = t^*)$ for $t^* = 2, 3, 4$.

[20] To be concise, I only test the model using the rebound magnitude in this section since a lack of rebound rules out the strategic behavior predicted by the model.

[21] In this case, (10) changes to $\frac{E[\Delta Y_k]}{\frac{1}{4}\Pr(T^*=1|T=1) + \sum_{s=2}^{4} \Pr(T=s)} = y^{\rho,e}$, which depends on the unknown fraction of the non-telescopers among those with $T = 1$.

If everyone telescopes, on the other hand, seam bias is at its worst as all spells are reported to begin at the beginning of a wave and consequently $E[\Delta Y_k] = \frac{1}{4} y^{\rho,e}$, confirming the aforementioned attenuation bias.

Denoting the left hand side of (10) by $E[\Delta Y_k^{adj}]$, the equation says that the observed rebound adjusted for seam bias $E[\Delta Y_k^{adj}]$ should be equal to the calibrated magnitudes if families behave according to the labor supply model. Therefore, (10) forms the basis of the statistical tests of the model. Since (10) holds for all $k = 1,...,8$, it also holds for the average of $E[\Delta Y_k^{adj}]$ across k (denoted by $E[\Delta \bar{Y}^{adj}]$), and the subsequent statistical analysis involves testing the null hypothesis of H_0: $E[\Delta \bar{Y}^{adj}] = y^{\rho,e}$ against the one-sided alternative H_1: $E[\Delta \bar{Y}^{adj}] < y^{\rho,e}$.

To implement the test, I use $\frac{1}{N}\sum_i 1_{[T_i=s]}$ (N is the number of public insurance spells) as the sample analog for $\Pr(T = s)$ and $\hat{\delta}_k$ from the regression of (9) as the estimator for $E[\Delta Y_k]$. The implied estimator for $E[\Delta \bar{Y}^{adj}]$ is $(\frac{1}{8}\sum_{k=1}^{8}\hat{\delta}_k)/(\frac{1}{4} + \frac{1}{N}\sum_i \sum_{s=1}^{3} 1_{[T_i=s]})$, and the point estimates as well as the p-values from the tests are summarized in Table 6. The tests are conducted via bootstrap with 500 repetitions, and only the p-values from tests of the zero-elasticity models, which are the hardest to reject, are displayed for brevity. The average adjusted rebound magnitudes are negative in both the 2001 and 2004 SIPP panels and across all subsamples examined in section 5.2. For the 2001 panel, the models with $\rho = 0$ and $\rho = 0.57$ are rejected at the 0.05 level in all samples and that with $\rho = 1.47$ is rejected at the 0.05 level in most samples with the exception of subsamples 3 and 6; it is nevertheless rejected at the 0.1 level in these two subsamples. The adjusted average empirical rebound magnitudes are more negative in the 2004 panel, and all three models with $\rho = 0, 0.57, 1.47$ are rejected at the 0.05 level in every subsample.

According to GAO (2012), the average application processing time is around 25 days, and it is possible that strategically behaving families may choose to maintain a low income throughout the processing period in addition to the month before program application. Additional calibrations and tests are carried out to account for this possibility. The model incorporating processing time predicted magnitudes are lower but the seam bias also causes less attenuation. The net impact of processing time on the hypothesis test results is limited: for the 2001 panel, the 24 tests corresponding to those in Panel (a) of Table 6 all reject the null hypothesis at the 10% level, 13 reject it at the 5% level and 6 at the 1% level; for the 2004 panel, all reject the null at the 5% level and 18 at the 1% level.

As mentioned previously, the calibrated rebound magnitudes may be underestimated, which makes the labor supply model even less consistent with the empirical income responses. First, because of the high income cutoff of CHIP, CHIP eligible families may qualify for other programs if they reduce their income.

For example, even though a five-year-old child in a family with income at 140% of the FPL is eligible for CHIP in practically every state, the family will face a more generous transfer by reducing their income to 133% of the FPL. At that level, the child will qualify for Medicaid in every state and enjoy more generous health care benefits. If the family is willing to reduce their income further to 130% of the FPL, they will also gain eligibility for the Supplemental Nutrition Assistance Program (formerly Food Stamps). Second, an extension of the model predicts that families will work more in the months not facing eligibility certification if they are allowed to save and borrow inter-temporally as noted in section 3.2, leading to larger predicted rebound magnitudes. Third, as mentioned earlier in this subsection, the calculation of n ignores the fact that some families in the calibration sample may be behaving strategically and hence underestimates their rebound magnitudes. Given the statistical rejection of the conservatively predicted magnitudes, the evidence presented in this subsection provides no support for the neoclassical model.

6.2 Tests Using Counterfactual Groups

There are three issues with the calibration approach undertaken in the previous subsection. First, even though calendar month effects are controlled for in (9), there may also be spell month effects ξ_k that are common across individuals. It is impossible to distinguish ξ_k from the strategic income responses y_k in the estimation of (9) as δ_k is the sum of ξ_k and y_k, and a downward spell-time trend may mask a moderate y_k. Second, the calibration exercise would not reliably detect model-predicted rebounds if only a fraction of families had behaved strategically. Third, an intensive-margin labor supply model is used to predict the rebound magnitudes, but the labor supply adjustments may be made along the extensive margin as discussed in the Supplemental Appendix and imply different effect sizes than those in Table 5.[22]

In order to address these issues, I construct counterfactual groups which may share the same spell-time trend but are predicted to show different income responses by both the intensive- and extensive-margin labor supply models even when only a fraction of families behaves strategically. The comparison between the treatment group and the counterfactual group, therefore, accounts for the spell-time trend, the presence of "never-takers" who would never behave strategically, and is robust to the choice of labor supply model. The lack of differences between the treatment and counterfactual groups should strengthen the case against strategic behavior.

[22]The calibration of the extensive-margin labor supply model requires identifying the joint distribution of the wage rates and tastes from the observed income distribution, which is beyond the scope of this paper.

I conduct two counterfactual analyses in this subsection. The first comparison is between children in high and low income families in states providing 12-month continuous eligibility. Both the intensive- and extensive-margin labor supply models predict that the strategic behavior comes from families with an income above a certain threshold $\tilde{y}$ in the months before and after they apply for benefits. This implies that the income process can be re-written as

$$Y_{ik} = \omega_i + \sum_t D_{it}^k \lambda_t + (\xi_k + y_k \cdot H_i \cdot O_i) + \varepsilon_{ik}$$

where H_i indicates whether a child lives in a high-income family, and O_i indicates whether a family behaves strategically and opts into public insurance conditional on being high-income.[23] A simple approach is to divide the children into two groups based on whether their family income in spell month r ($r \neq 0$) is below or above $\tilde{y}$, and compare their income processes through the public insurance spell. The problem, however, is that families with a high (or low) month-r income tend to have a high (or low) transitory shock in month r, and the comparison of income processes between the two groups will be a biased estimate of the "intent-to-treat" income rebound magnitude due to serial correlation in the transitory shocks. This argument is well known in the program evaluation literature, and Heckman and Robb (1985) and Ashenfelter and Card (1985) have proposed and implemented the symmetric difference-in-difference estimator to eliminate the bias.

I adapt their strategy for the problem at hand. Specifically, if the shocks in the income process are joint normal, then the conditional expectation of income in spell month k is a linear function of that in month r:

$$E[Y_{ik}|Y_{ir} < \tilde{y}] = \omega_i + \sum_t D_{it}^k \lambda_t + \xi_k + \frac{Cov(Y_{ik}, Y_{ir})}{Var(Y_r)} \{E[Y_{ir}|Y_{ir} < \tilde{y}] - E[Y_{ir}]\} \quad (11)$$

$$E[Y_{jk}|Y_{jr} \geqslant \tilde{y}] = \omega_i + \sum_t D_{jt}^k \lambda_t + \xi_k + y_k E[O_i] + \frac{Cov(Y_{jk}, Y_{jr})}{Var(Y_r)} \{E[Y_{jr}|Y_{jr} \geqslant \tilde{y}] - E[Y_{jr}]\} \quad (12)$$

Because the existence of the last term in (11) and (12), which reflects serial correlation in transitory shocks, running fixed-effect regressions separately for the high and low income groups and taking the differences in the estimated $\hat{\delta}_k$'s will not in general consistently estimate the "intent-to-treat" effect $y_k E[O_i]$. Assuming covariance stationarity in the income process, however, the symmetric difference-in-difference centered around month r for children who started their public insurance spell in the same calendar month, $SDD_r \equiv (E[Y_{jk}|Y_{jr} \geqslant \tilde{y}] - E[Y_{j0}|Y_{jr} \geqslant \tilde{y}]) - (E[Y_{ik}|Y_{ir} < \tilde{y}] - E[Y_{i0}|Y_{ir} < \tilde{y}])$ where $0 < 2r = k < 12$, identifies the

[23] This representation is equivalent to (9) with $\delta_k = \xi_k + y_k \cdot H_i \cdot O_i$. For simplicity, state unemployment rates are not an explanatory variable in this subsection since it is both statistically and economically insignificant from the estimation of (9) in section 5.

“intent-to-treat” income rebound $(y_k - y_0)E[O_i]$.

In practice, I carry out the estimation with $r = 3$ and $r = 4$. As mentioned in section 6.1, the maximum k for gauging income rebound needs to be capped at $k = 8$, implying a maximum r of 4. The values of $r = 1$ and $r = 2$ are not selected because the presence of seam bias threatens covariance stationarity.[24] To implement the test, I estimate each of the differences $E[Y_{jk}|Y_{jr} \geqslant \tilde{y}] - E[Y_{j0}|Y_{jr} \geqslant \tilde{y}]$ and $E[Y_{ik}|Y_{ir} < \tilde{y}] - E[Y_{i0}|Y_{ir} < \tilde{y}]$ by fixed effect regression accounting for the seam bias as described in section 6.1 using spells that began in the same calendar month t, and the estimator for SDD_r is averaged across t. Testing the labor supply model amounts to testing H_0: $SDD_r = 0$ versus $H_1 : SDD_r > 0$ for $r = 3, 4$, and bootstrap with 500 replications is used.

The point estimates and p-values from estimating SDD_r are presented in the first two rows of Table 7. Even though the point estimates of both SDD_3 and SDD_4 for the 2001 panel are around \$700, a large magnitude in light of the calibration exercise, the failures to reject the null hypothesis of $SDD_r = 0$ due to a p-value of around 0.2 are not strong evidence in favor of strategic behavior. In the 2004 panel, the point estimates of SDD_3 and SDD_4 are both negative, and the statistical tests provide even less support of the labor supply models. Therefore, the evidence from comparisons of income groups does not indicate strategic behavior.

The second counterfactual group consists of children living in states that did not provide 12-month continuous eligibility, i.e. states in the second row of Table 1. As mentioned in section 2, many of the states on this list allow a 12-month renewal interval, but families are required to report income changes when their children are covered by public insurance. To the extent that income change reporting is enforced in these states, the difference between the average rebound magnitudes from states with and without the continuous eligibility provision, denoted by $E_{CE}[\Delta Y_k]$ and $E_{NCE}[\Delta Y_k]$ respectively, should identify strategic behavior provided that families in the two sets of states share the same spell month trend ξ_k. If income reporting is not enforced at all, families in the 12-month renewal states face the same incentive as those in the 12-month continuous eligibility states, and the comparison does not help identify labor supply responses. The estimand $E_{CE}[\Delta Y_k] - E_{NCE}[\Delta Y_k]$ is still informative, however, in gauging heterogeneity in strategic behavior; if no significant differences are found between the two sets of states, the model testing results from the calibration exercise in section 6.1 may apply to the 12-month renewal states as well.

[24] Recall that telescoping families start their spell at the beginning of a wave, and therefore the covariance $Cov(Y_{i0}, Y_{ir})$ is cross-wave whereas $Cov(Y_{ik}, Y_{ir})$ is within-wave for $r = 1, 2$.

The results from comparing the two sets of states are summarized graphically in Figure 3 and numerically in the third row of Table 7. In effect, equation (9) is estimated separately for the two sets of states, and Figure 3 plots the differences in the estimated δ_k's.[25] There is a very slight upward trend in the 2-year window around the beginning of public insurance spells in the 2001 panel, but the pointwise 95% confidence intervals all include 0. The trend is essentially flat in the 2004 panel. To formally test whether the difference in the rebound magnitude is 0, I adopt the assumptions and method used in section 6.1 to account for the seam bias because families in the two sets of states had exhibited varying degrees of telescoping behavior. Combining previously introduced notations, the resulting estimator is the sample analogue of $\Delta E[\Delta\bar{Y}^{adj}] \equiv E_{CE}[\Delta\bar{Y}^{adj}] - E_{NCE}[\Delta\bar{Y}^{adj}]$. As shown in Table 7, the point estimate of $\Delta E[\Delta\bar{Y}^{adj}]$ is 287 dollars in the 2001 panel but is not significant at the 5% level. The 2004 point estimate is quite negative, providing no indication that the families in the continuous eligibility states had a larger income rebound.

To summarize, the counterfactual analyses comparing high and low income families and across states with different continuous eligibility provisions do not provide compelling evidence in support of strategic behavior. Taken together with the results from subsection 6.1, the predictions from the labor supply models are essentially rejected, and the absence of labor supply response is consistent with Meyer and Rosenbaum (2001) and Ham and Shore-Sheppard (2005). The findings may be explained by factors such as the lack of knowledge of program rules and frictions in income adjustments. Perhaps the best approach to identify the precise reason for the lack of strategic behavior is a survey targeting the program beneficiaries. However, with the labor supply responses practically ruled out, it appears reasonable to treat incomes as drawn exogenously from a stochastic process, as opposed to being actively controlled by the families, in computing the optimal recertification frequency.

7 Optimal Length of the Continuous Eligibility Period

Because of the lack of income/labor supply responses to the continuous eligibility provision shown in section 6, I compute the optimal length of the recertification period for Medicaid/CHIP, τ, based on a mechanical model of individual behavior. In this model, labor supply considerations are absent from families' optimizing decisions and incomes simply follow a stochastic process. After the realization of income Z, consumption is determined by $C = [(1-t)Z+g]P + (1-t)Z(1-P)$ where P is a binary variable indicating

[25] For the results presented in the paper, calendar month effects are constrained to be the same across the two sets of states. Allowing the calendar month effects to differ has little effect on the estimates.

program participation status, and agents' utility u only depends on the consumption level C.

In order to determine the optimal transfer policy, I specify next the social welfare function, which contains two components. The first component is a standard Bergson-Samuelson functional of weighted individual utilities, W, and the second component is surplus in the government's budget, S, which can be used to finance a public good (Salanie (2003)). I assume that the two components are additive and that the welfare resulting from the public good is linear in its spending. As an illustration, when eligibility check is performed every month, when take-up rate is 100% (i.e. $P = 1_{[Z \leqslant \gamma]}$) and when eligibility monitoring is free, the per-period social welfare is given by:

$$\underbrace{\int \Psi(u(C(z))) f_Z(z) dz}_{W} + \omega \underbrace{[R - \Pr(Z \leqslant \gamma) g]}_{S} \tag{13}$$
$$\text{s.t.} \quad C(z) = [(1-t)z + g] 1_{[z \leqslant \gamma]} + (1-t) z 1_{[z > \gamma]}$$

Ψ is an increasing and concave function that weights the utilities of individual agents according to the social planner's redistributive taste, and ω reflects the contribution of S to overall social welfare relative to that of agents' utilities. t is the pre-determined marginal tax rates on income, and f_Z and F_Z specify the p.d.f and c.d.f. of pre-tax income Z respectively (assuming the stationarity of the income process which will be relaxed later). The government collects per-agent revenue R, which may contain income tax revenue $\int tz f_Z(z) dz$ as well as sources not explicitly modeled here,[26] and it is assumed that R is sufficiently large to cover program spending: $R \gg \Pr(Z \leqslant \gamma) g$.

The formulation of the social welfare function (13) differs slightly from a textbook approach (e.g., Salanie (2003)) in the following two respects. First, government surplus does not typically enter the social welfare function directly but through a balanced budget constraint. As noted by Salanie (2003), however, the dependence of utility on S is omitted in a textbook model because the spending on the public good is held constant. The specification (13) simply extends that of Salanie (2003) by allowing the production of the public good to be variable.

Second, having a "notched" lump sum transfer schedule with the associated cutoff γ as the policy instrument is not prevalent in the optimal design literature. In fact, if the income tax schedule is completely flexible and that $\Psi \circ u$ is strictly concave, then the government should choose a transfer function that equal-

[26] For example, part of the federal CHIP funding comes from tobacco taxes.

izes consumption across agents when labor supply decisions are not considered in the model (a special case is studied as early as in Edgeworth (1897)). When labor supply incentives are considered, the seminal paper of Mirrlees (1971) shows that the marginal tax rate always lies between zero and one which precludes a discrete drop in the consumption-pre-tax-income schedule if the optimal tax schedule is completely flexible. However, Blinder and Rosen (1985) and Slemrod (2010) argue that it is possible to institute a notch as part of an optimal schedule when the set of income tax instruments is limited, e.g., linear.[27] By continuing with the specification of (13), I take as given the existence of the notch-creating transfer programs like Medicaid and CHIP.

A dynamic extension of the baseline formulation (13) is called for when evaluating the optimal recertification period. I consider a T-period problem, where the public insurance program becomes available in period 1, and in every period, families' eligibility depends on their income and program participation history through the continuous eligibility provision. For example, when the continuous eligibility period is 3 months, a family is assumed to automatically participate in the program in month 2 and 3 if it was eligible for and participated in the program in period 1. In month 4 when the family's eligibility is recertified, the participation status will depend on whether their income for the previous month falls below the threshold. Formally, the social welfare function becomes

$$\begin{aligned} & \sum_{m=1}^{T} \beta^{m-1} E[\Psi(u(C_m - \phi I_m))] - \omega E[R - (gP_m + \kappa I_m)] \\ \text{s.t.} \quad & C_m = [(1-t)Z_m + g]P_m + (1-t)Z_m(1-P_m) \end{aligned} \tag{14}$$

The expectation is taken over the joint distribution of $\{Z_1, ...Z_T\}$; P_m and I_m are dummy variables indicating whether a family participates in the program and whether program eligibility is certified in month m, and they are determined by the family income histories and the recertification frequency as illustrated above. In addition to spending on public insurance benefits, each eligibility check costs the government and the participating family κ and ϕ to perform, respectively.

In this paper, I consider the problem where the government chooses the continuous eligibility period τ to maximize social welfare taking tax rate t and eligibility cutoff γ as given. The optimal τ is determined

[27] The *theoretical* properties of means-tested in-kind transfers in an optimal-design context have also been studied in Nichols and Zeckhauser (1982), Blackorby and Donaldson (1988), Gahvari (1995), Cremer and Gahvari (1997), Singh and Thomas (2000), etc., These studies typically consider the problem with two types of agents and a transfer scheme that ensures second-best allocation, i.e. the high type does not pretend to be the low type and claims benefit transfer. See Currie and Gahvari (2008) for a survey.

by comparing numerically calculated social welfare corresponding to different recertification periods. In order to compute social welfare, the joint distribution of $\{Z_1, ...Z_T\}$ needs to be specified, and I adopt a non-parametric approach by relying on the observed income processes for families who had appeared in all months in the 2001 and 2004 panels of SIPP ($T = 36$ and $T = 48$ for the two panels respectively). Assuming 100% benefit take-up, I can impute each family's monthly program participation decision based on its income history or a particular recertification period τ, and calculate its consumption accordingly.

The remaining missing piece for the numerical exercise of determining the optimal τ under full take-up is specifying the value of ω and the functional form of Ψ. In order to obtain ω, I assume that the observed policy parameter γ is the solution to the frictionless optimization problem (13) where the cost of eligibility certification is zero. That is, the government abstracts away from the monitoring problem in determining the eligibility cutoff. Since ω will be smaller when monitoring cost is taken into consideration: $\omega = \{\Psi([(1-t)\gamma + g - \phi]) - \Psi([(1-t)\gamma])\}/(g+\kappa)$, the computed optimal τ should be considered as a lower bound. I can then solve for ω following the first order condition of γ as

$$\omega = \{\Psi((1-t)\gamma + g) - \Psi((1-t)\gamma)\}/g \tag{15}$$

where individual utility u is assumed to be linear in consumption, and $\Psi(u) = \frac{u^{1-\eta}}{1-\eta}$ is used for the weighting function under various values of η. Because each family faces different eligibility cutoffs, benefit notches and tax rates depending on its composition and income, I calculate ω using the average g, γ and t families face in SIPP. The resulting values of ω are very similar in the 2001 and 2004 panels and they decrease η goes up. Finally, the monthly discount rate β is taken to be $(0.95)^{(1/12)}$ as in section 6.1.

Since the take-up rate is not 100% in reality, I also investigate the optimal recertification frequency under partial take-up. The take-up probability for a family with eligible children, denoted by p, is estimated using two approaches. First, I assume that p is independent across time and of factors such as family income and demographics. The *annual* participation rate among eligible children is estimated to be 80% in the late 2000's by Kenney et al. (2011), implying a *monthly* take-up rate of $1-(1-0.8)^{1/12} \approx 0.125$. The second approach allows p to depend on family income, race, age of the youngest child and the number of children in the family. These relationships are estimated using SIPP[28], and p is larger for low-income families, African-

[28] I pool together families with eligible but uninsured children and run a probit regression of whether the children would enroll over the next four months on the family characteristics–the resulting estimator of the coefficient vector is denoted by $\hat{\beta}$. Looking at the probability over the next four month circumvents the problem created by the seam bias; for a family with characteristic vector x, the monthly take-up rate p is estimated by $1-(1-\Phi(x'\hat{\beta}))^{1/4}$.

American families and families with young children in both panels; p is found to be positively correlated with the number of children in the 2001 panel and negatively in the 2004 panel.[29] As it turns out, the two approaches produce very similar optimal recertification frequencies, so only the results from the second–and more realistic–approach are presented below.

Welfare is computed for values of τ between 1 and 35, the latter of which is one less the total number of months in the SIPP 2001 panel (since the previous month's income is used to determine eligibility, the maximum number of months a child can be covered during the 2001 panel is 35 months). Each of the parameters κ and ϕ takes on the values of \$0, \$9.5, \$19, \$28.5 and \$38 in 2010 dollars. The median hourly wage rate for government program interviewers, which is around \$19 in May 2010 according to the Occupational Employment Statistics database of the Bureau of Labor Statistics, serves as the basis for the value choices of κ. The parameter values correspond to 0, 0.5, 1, 1.5 and 2 hours of work for eligibility recertification respectively (Irvin et al. (2001) adopt the same estimation strategy[30]; Prell (2008) also includes overhead costs in estimating κ, which results in larger estimates and will consequently lead to a longer optimal τ if applied to my simulations). There is no formal reason to choose the same values for ϕ as for κ, and I do so here simply for convenience. In general, it may be more costly for a family to have their eligibility certified because it involves finding out information, gathering proof of incomes, filling out the application forms and sometimes traveling to meet face-to-face with their case worker on a work day.

Table 8 presents the optimal length of the continuous eligibility period from simulations under the 25 combinations of κ and ϕ (5 values for κ and 5 for ϕ), four values of η, $\eta = 0, 0.5, 1, 1.5$[31] and two assumptions governing the take-up rates. The prevalence of the optimal continuous eligibility periods that are multiples of 4 under 100% take-up is again due to the seam bias; it is not so for the partial take-up results because of the random monthly program participation introduced. Under 100% take-up (column blocks (a) and (c)), the optimal recertification frequency is indeterminate for the utilitarian government ($\eta = 0$) when monitoring is costless because transferring wealth across population leads to no change in the overall welfare. When monitoring is costly, any eligibility check imposes a deadweight loss and therefore the implied optimal interval is the corner solution of 35 months. For the concave social welfare functions considered

[29] It can be shown that the calculation of ω is unaffected under partial program take-up.

[30] Irvin et al. (2001) simulates the impact of implementing the 12-month continuous eligibility provision on Medicaid coverage, payment and administrative costs using program data from CA, MI, MO and NJ between 1994 and 1995.

[31] $\eta = 0$ implies a linear (utilitarian) Ψ, and Ψ reduces to the log function when $\eta = 1$, which is the social welfare formulation used in Brewer et al. (2010). Under the four values of η, giving \$100 per month to a family with a monthly income of \$1000 brings the same increment to social welfare as giving \$100, \$311, \$1000 and \$3749 to a family with a monthly income of \$10,000, respectively.

($\eta > 0$), the optimal τ increases with η under the same recertification cost parameters. The results confirm the intuition that the government should certify eligibility every period when it is costless to do so and should check less frequently as costs increase. An increase in the cost on families, ϕ, is more likely to lengthen the recertification period than an increase in κ of the same magnitude. The optimal τ's computed from the 2004 panel are generally similar to but are somewhat larger than their 2001 counterparts under large ϕ, κ and η. The estimates from both panels point to an optimal τ of 12 months when κ is positive and when ϕ exceeds \$19 (Prell (2008) uses \$17 for ϕ in 1998 dollars in his calculation, which translates into \$23 in 2010 dollars). Under partial take-up (columns block (b) and (d) of Table 8), the calculated optimal τ's are no longer monotone in the cost parameters because of the randomness in take-up behavior in the simulations.[32] The recertification periods are much longer than those derived under 100% take-up, close to or being at the upper bound of 35 months allowed in the simulations.[33] This is because short continuous eligibility periods tend to result in coverage gaps, leading to significant welfare losses for low-income families. Table SA.1 in the Supplemental Appendix illustrates this point by showing measures of excess eligibility and under-coverage for various recertification periods. Under partial take-up, the number of uninsured months as a fraction of income eligible months decreases from above 0.95 to just below 0.75 as the recertification interval increases from 1 to 35 months. In contrast, the fraction of income ineligible months during public insurance coverages increases from 0 to only 0.05, reflecting concentrated program participation among low-income families, for whom income fluctuation is unlikely to result in ineligibility.

There are two caveats in interpreting the results of Table 8. First, given that the simulation sample consists of families that responded to interviews for all months during the SIPP panels, their income processes may be different from those not included in the sample. Therefore, the resulting optimal length of the continuous eligibility period is sample-specific and may not apply to the general population. Second, an implicit assumption in calculating the optimal τ is that lengthening the recertification period does not induce strategic labor supply behavior nor does it change the take-up probability among eligible families. As τ becomes very large, it is likely that the information regarding the program becomes more salient, more eligible families may choose to participate and that ineligible families start responding to the dynamic opt-in incentives. On the other hand, even when the average monitoring costs are very low, having a small τ may

[32] Also due to randomness, the optimal τ when $\eta = 0$ and $\phi, \kappa > 0$ is not necessarily the corner solution of 35 months. It could happen by chance in the simulation that no family participates in public insurance when $\tau = 34$ but all eligible families participate when $\tau = 35$, in which case the social welfare from $\tau = 35$ is lower because of the deadweight loss of monitoring.

[33] Since the 2004 panel is 4-year long, I can allow the maximum candidate of τ to be 47 months. The resulting optimal recertification periods are mostly above 40 months.

asymmetrically lower the take-up rate among the most vulnerable. As shown in Currie and Grogger (2001), single-parent families–arguably the more needy–disproportionately dropped out of the Food Stamp program when the frequency of recertifications had increased.

The normative framework presented in this section relates to and extends the informative Prell (2008) model in studying the optimal WIC recertification frequency along several major dimensions. First, Prell (2008) assumes constant hazard rates in the transitions between eligibility and ineligibility, which makes the problem analytically tractable and provides nice insights. In comparison, I carry out the exercise non-parametrically by relying on the empirically observed income processes and provide a computational solution. Second, Prell (2008) assumes 100% program participation but acknowledges that take-up behavior or "program access" should be carefully modeled. Estimating the take-up probability and building it into the normative framework brings this analysis a step closer to the goal. Third, the value of the transfer to different individuals is assumed to be the same from the social planner's perspective in the Prell (2008) framework whereas I calculate the optimal recertification interval under alternative social welfare functions. Using my framework, the implied optimal continuous eligibility periods under 100% take-up rate are moderately longer than those of Prell (2008) in the WIC context, while those under partial take-up are much larger. In summary, my results suggest that 12 months serve as a lower bound of the optimal length of the continuous eligibility period for Medicaid/CHIP under reasonable parameter values.

8 Conclusion

This paper presents both a positive and a normative analysis regarding eligibility recertification in a means-tested program. For the positive analysis, it investigates both theoretically and empirically the impact of continuous eligibility on the income and labor supply responses of participants in public insurance. Neoclassical labor supply models predict that a long eligibility recertification period provides strong dynamic opt-in incentives wherein families lower their income to gain program eligibility, acquire government-provided benefits for the continuous eligibility period and revert back to their "optimal" interior income-consumption bundle. Using the 2001 and 2004 panels from SIPP, I adopt the Jacobson et al. (1993) event study framework to trace out the income and labor supply processes for families participating in Medicaid/CHIP. The point estimates in the full analysis sample and various subsamples in which strategic behavior is more likely to occur do not indicate the model-predicted dip-and-rebound pattern around the time a child gains public insurance coverage. Dynamic extensions of the Saez (2010) model are calibrated using family income and composition information, Medicaid/CHIP policy parameters and income tax rates. Comparing the magnitudes of the predicted strategic behavior to those observed empirically while accounting for the seam bias in the SIPP data rejects the intensive-margin neoclassical model in most subsamples. In addition, I adopt a difference-in-difference type approach to compare the income processes between children in high and low income families and children living in states with different continuous eligibility provisions. The lack of differences in the two counterfactual analyses also do not support the strategic behavior predicted by the labor supply models.

With the positive analysis practically ruling out income and labor supply responses, I treat the income processes in SIPP as exogenous and propose a framework to answer the normative question of what the length of the continuous eligibility period should be. I derive a mapping from various combinations of cost parameters associated with eligibility recertification to the optimal length of the continuous eligibility period under various functional forms of social welfare and assumptions governing benefit take-up. Under moderate cost parameter values, 12 months appear to be a lower bound of the optimal length of the continuous eligibility period. That said, with technological advancement and improved data sharing among government agencies, recertification costs may decrease significantly in the future, in which case the continuous eligibility period can be shortened to improve targeting efficiency.

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Tables and Figures

Table 1: Provision of 12-Month Continuous Eligibility

Twelve-month Continuous Eligibility	States
Yes	Alabama, California, Washington D.C., Idaho, Illinois, Iowa, Kansas, Louisiana, Mississippi, Maine, Michigan (after Jan 2003), New York, North Carolina, South Carolina, Wyoming (after Oct 2002), West Virginia (after Oct 2002).
No	Alaska, Colorado, Georgia, Hawaii, Kentucky, Missouri, Montana, Nevada, New Hampshire, North Dakota, Ohio, Oklahoma, Oregon, Rhode Island, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Wisconsin.
Complicated	Arizona, Arkansas, Connecticut, Delaware, Florida, Indiana, Maryland, Massachusetts, Minnesota, Nebraska, New Jersey, New Mexico, Pennsylvania, Washington.

Notes: This table groups the states by whether or not they had provided 12 months of continuous eligibility during the sample period of 2000-2007. States in the first row had provided 12 months of continuous eligibility during the sample period; states in the second row had not, even though many of them allowed a 12-month renewal period with mandatory reporting of circumstance changes. The remaining states (the third row) either underwent changes in their provisions (AR, CT, IN, MA, NE, NJ, NM, WA), for which the timing of the implementation was hard to precisely pin down (the three states that made a switch in row one are only included in the 2004 panel sample), or their continuous eligibility provisions differed across components of their public insurance programs (AZ, FL, MD, MN, PA).

Sources: NGA (2000-2008), Kaiser (2000-2011).

Table 2: Summary of Institutions for States Providing 12-month Continuous Eligibility

State	Income Proof Requirement	Renewal Materials Sent (Months before expiration)	Presumptive Eligibility	CHIP Waiting Period 2001 Panel	CHIP Waiting Period 2004 Panel
Alabama	Pay stubs w/in 1 month	1	N	Y	Y
California	A pay stub w/in 45 days	Vary by local office	Y	Y	Y
D.C.	Pay stubs w/in 1 month	3	N	N	N
Idaho	Pay stubs w/in 30 days	1.5	N	Y	Y
Illinois	Pay stubs w/in 30 days	2	Y	Y	N
Iowa	Pay stubs w/in 30 days	1.5	N	Y	N
Kansas	Pay stubs w/in 2 months	1.5	N	N	N
Louisiana	Pay stubs w/in 1 month	1	N	N	N
Maine	Pay stubs w/in 4 weeks	Vary by local office	N	Y	Y
Michigan	A pay stub w/in 30 days	Vary by local office	Y	Y	Y
Mississippi	Pay stubs w/in 1 month	0.5-2	N	N	N
New York	Pay stubs w/in 4 weeks	2-3	Y	N	N
North Carolina	Pay stubs w/in 1 month	1	N	Y	N
South Carolina	Pay stubs w/in 4 weeks	1	N	N	N
West Virginia	Pay stubs w/in 30 days	2	N	Y	Y
Wyoming	Pay stubs w/in 1 month	2	N	Y	Y

Notes and sources: This table summarizes the relevant institutional details.

1. Income proof requirement data are gathered from state application forms and instructions except for Kansas and Michigan. The two states do not specify the recency requirement in their application forms, and phone calls are made to state agencies to collect more detailed information.

2. For the timing of the renewal process, information comes from government websites in California, Washington DC, Illinois, New York, North Carolina, South Carolina and West Virginia, as well as phone calls to government agencies in the remaining states.

3. Presumptive eligibility data come from NGA (2000-2008) and Kaiser (2000-2011); the set of states that provided presumptive eligibility during the 2001 panel did not change during the 2004 panel.

4. Waiting period data come from Kaiser (2000-2011); the letter "Y" indicates that a state had mandated a CHIP waiting period during the sample period for children whose private insurance was voluntarily dropped prior to program application.

Table 3: Public Insurance Spell, Child and Sample Unit Counts by Spell Types, Continuous Eligibility Status and Analysis Sample

(a) Public Insurance Spell, Child and Sample Unit Counts by Spell Types and Continuous Eligibility States

	Total No. of Public Insurance Spells		No. of Kids with Public Insurance Spells		No. of SU with Kids on Public Insurance	
	2001	2004	2001	2004	2001	2004
Pub Insurance spells	16109	23109	10656	17190	5024	8149
Left-Truncated Spells	8402	13996	7407	11929	3683	5848
Fresh Spells	7707	9113	5759	7850	3033	4390
Fresh Spells Ex. Infants & State Movers & SSI Kids	7044	8183	5294	7076	2839	4060
12-Month Cont. Elig.	2582	2821	1934	2421	1057	1420
No 12-Month Cont. Elig.	2485	2927	1927	2559	1036	1453
Other States	1977	2435	1444	2112	755	1207

(b) Public Insurance Spell, Child and Sample Unit Counts in Analysis Samples

	Subsample Public Insurance Spells		No. of Kids with Public Insurance Spells		No. of SU with Kids on Public Insurance	
Sample	2001	2004	2001	2004	2001	2004
Full Sample	2582	2821	1934	2421	1057	1420
Long Gap Subsample	419	689	419	680	255	420
No Presumptive Eligibility nor CHIP Waiting Period	417	799	321	703	181	429
Excluding Kids Starting Pub. Insurance w/ Parent	2027	2484	1606	2164	896	1286
Two-parent Subsample	1242	1596	932	1391	492	780
Construction-retail Subsample	635	667	513	609	296	352
College-educated Parent Subsample	1837	2317	1411	2022	821	1210
Families with More than Two Children	1115	1212	838	1054	306	406

Notes: The first three rows of Panel (a) show the number of public insurance spells, children and sample units by spell type. It then breaks down the fresh spell counts, excluding those for infants, children who moved across states during a spell or who were on SSI, by continuous eligibility provision status. Note that I do include children who moved across states as long as the move did not occur during a spell. As a result, the fourth row of Panel (a) is the sum of the last three rows in columns 1 and 2, but not in columns 3-6. Panel (b) gives the spell, child and sample unit counts in the full and subsamples. By construction, the first row of Panel (b) is the same as the fifth row of Panel (a).

Table 4: Variable Averages for Children in the Analysis Samples

Variable	2001 Panel (Spell Month 0)		2004 Panel (Spell Month 0)	
	Month 0	Month 1	Month 0	Month 1
Age	8.35	8.42	8.74	8.84
Female	0.49	0.49	0.49	0.49
Black	0.23	0.23	0.19	0.19
Family Size	4.2	4.2	4.1	4.1
Two-parent Family	0.51	0.51	0.58	0.58
Family Income (in 2010 $)	1888	1859	2399	2353
Fraction without Earnings	0.11	0.12	0.09	0.10
On Medicaid	0	1	0	1
On Food Stamp	0.17	0.21	0.17	0.21
On Welfare	0.04	0.06	0.04	0.05
Mom on Medicaid	0.18	0.35	0.21	0.32
Dad on Medicaid	0.07	0.15	0.09	0.15
Mom on Food Stamp	0.20	0.24	0.18	0.21
Dad on Food Stamp	0.09	0.11	0.08	0.11
Mom on Welfare	0.04	0.05	0.03	0.03
Dad on Welfare	0.01	0.01	0.00	0.01
Mom on UI	0.03	0.02	0.02	0.02
Dad on UI	0.04	0.04	0.02	0.02

Notes: Variable averages for children and their families in the various analysis samples right before (Month 0) and during the first month (Month 1) of public insurance spell. Medians are reported for the family income variable; means are reported for all other variables, for which the SIPP sampling weights are used.

Table 5: Model-Predicted Income Responses

		2001 Panel (in 2010 Dollars)		2004 Panel (in 2010 Dollars)	
ρ	e	Dip Magnitude	Rebound Magnitude	Dip Magnitude	Rebound Magnitude
1.47	0	183	183	192	192
0.57	0	216	216	244	244
0	0	241	241	290	290
1.47	0.15	260	220	286	241
0.57	0.15	300	283	345	326
0	0.15	336	336	387	387

Notes: Dip-and-rebound magnitudes predicted by the model. The model is calibrated using SIPP panel income data, federal income tax rates and published CHIP eligibility threshold and spending data.

Table 6: Statistical Tests of Theoretical Predictions

(a) Comparing Empirical and Model Predicted Rebound Magnitudes: 2001 Panel

Sample	Point Estimate	p-value: testing H_0: $E[\Delta\bar{Y}^{adj}] = y^{\rho,e}$ vs. H_1: $E[\Delta\bar{Y}^{adj}] < y^{\rho,e}$		
		$\rho = 1.47$, $e = 0$	$\rho = 0.57$, $e = 0$	$\rho = 0$, $e = 0$
Full Sample	-147	0.03**	0.02**	0.02**
Subsample 1	-1079	0.01***	0.01***	0.01***
Subsample 2	-225	0.00***	0.00***	0.00***
Subsample 3	-124	0.06*	0.04**	0.04**
Subsample 4	-374	0.03**	0.02**	0.02**
Subsample 5	-619	0.00***	0.00***	0.00***
Subsample 6	-136	0.07*	0.05**	0.04**
Subsample 7	-193	0.04**	0.03**	0.02**

(b) Comparing Empirical and Model Predicted Rebound Magnitudes: 2004 Panel

Sample	Point Estimate	p-value: testing H_0: $E[\Delta\bar{Y}^{adj}] = y^{\rho,e}$ vs. H_1: $E[\Delta\bar{Y}^{adj}] < y^{\rho,e}$		
		$\rho = 1.47$, $e = 0$	$\rho = 0.57$, $e = 0$	$\rho = 0$, $e = 0$
Full Sample	-564	0.00***	0.00***	0.00***
Subsample 1	-1409	0.00***	0.00***	0.00***
Subsample 2	-709	0.03**	0.02**	0.01***
Subsample 3	-720	0.00***	0.00***	0.00***
Subsample 4	-940	0.00***	0.00***	0.00***
Subsample 5	-735	0.01**	0.00***	0.00***
Subsample 6	-594	0.00***	0.00***	0.00***
Subsample 7	-678	0.00***	0.00***	0.00***

Notes: Presented are point estimates of the seam-bias adjusted empirical rebound magnitude, $E[\Delta\bar{Y}^{adj}]$, along with p-values from testing the labor supply model in various samples and under different parameter values. Subsample 1 is the long gap sample; subsample 2 excludes states providing presumptive eligibility or mandating a CHIP waiting period; subsample 3 excludes children whose parents took up Medicaid when the began their public insurance spell; subsample 4 consists of children in two-parent families; subsample 5 is the construction-retail sample; subsample 6 consists of children in families with a college-educated parent; subsample 7 consists of children with more than one sibling. p-values are obtained by bootstrap with 500 repetitions.
* p<0.1; ** p<0.05; *** p<0.001.

Table 7: Comparisons with Counterfactual Groups

Counterfactual Group	Estimand	2001 Panel		2004 Panel	
		Point Estimate	p-value	Point Estimate	p-value
Low-income Group	SDD_3	730	0.25	-353	0.60
Low-income Group	SDD_4	699	0.19	-217	0.77
No-continuous-eligibility States	$\Delta E[\Delta\bar{Y}^{adj}]$	287	0.09^{*}	-649	0.99

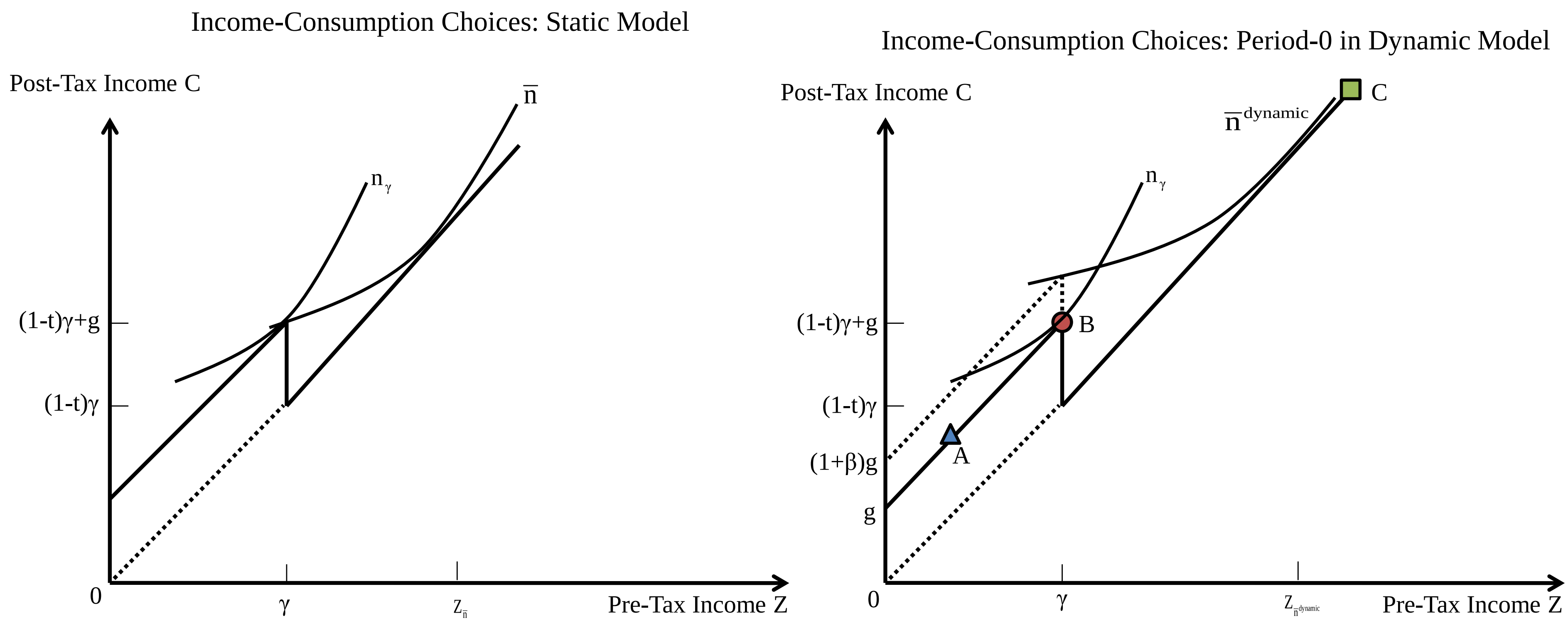
Notes: The point estimates are in 2010 dollars. The symmetric difference-in-difference estimand SDD_r defined in section 6.2 comes from the comparison of income processes between high and low income groups. The p-values in the first two rows are from the one-sided hypothesis test: $H_0 : SDD_r = 0$ vs. $H_1 :\ SDD_r > 0$. $\Delta E[\Delta\bar{Y}^{adj}] \equiv E_{CE}[\Delta\bar{Y}^{adj}] - E_{NCE}[\Delta\bar{Y}^{adj}]$ gauges the difference in income rebound magnitudes between states providing 12-month continuous eligibility and those that did not. The p-value in the last row comes from the one-sided hypothesis test: $H_0 : \Delta E[\Delta\bar{Y}^{adj}] = 0$ vs. $H_1 :\ \Delta E[\Delta\bar{Y}^{adj}] > 0$. Bootstrap with 500 replications are used in conducting the hypothesis tests.
* p<0.1; ** p<0.05; *** p<0.001.

Table 8: Optimal Length of the Continuous Eligibility Period from Welfare Calculations

Recertification Cost		Optimal Length of the Continuous Eligibility Period in Months (τ)															
ϕ	κ	SIPP 2001 Panel								SIPP 2004 Panel							
		(a) Full Take-up				(b) Partial Take-up				(c) Full Take-up				(d) Partial Take-up			
		$\eta=0$	0.5	1	1.5	$\eta=0$	0.5	1	1.5	$\eta=0$	0.5	1	1.5	$\eta=0$	0.5	1	1.5
$0	$0	1-35	1	1	1	1-35	33	34	35	1-35	1	1	1	1-35	35	31	35
$0	$9.5	35	4	4	2	35	32	35	31	35	4	4	4	33	35	32	32
$0	$19	35	4	4	4	35	32	32	34	35	4	4	4	33	34	35	34
$0	$28.5	35	4	4	4	35	35	33	34	35	8	4	4	34	34	35	35
$0	$38	35	8	4	4	35	33	30	32	35	8	4	4	35	31	31	34
$9.5	$0	35	4	4	8	32	35	27	34	35	4	4	8	33	35	34	31
$9.5	$9.5	35	4	4	8	34	34	35	34	35	8	8	12	34	34	34	30
$9.5	$19	35	4	4	8	35	33	33	31	35	8	8	12	35	35	34	35
$9.5	$28.5	35	8	8	12	33	35	34	31	35	8	8	12	35	33	31	35
$9.5	$38	35	8	8	12	33	34	32	31	35	12	8	12	35	33	33	34
$19	$0	35	4	8	12	34	35	33	34	35	8	8	12	34	35	35	33
$19	$9.5	35	8	8	12	34	31	35	33	35	8	8	12	32	33	35	34
$19	$19	35	12	8	12	35	34	34	35	35	12	12	12	35	34	34	30
$19	$28.5	35	8	12	12	35	30	34	34	35	12	12	16	35	33	35	35
$19	$38	35	8	12	12	32	35	33	32	35	12	12	24	35	35	35	33
$28.5	$0	35	12	12	20	34	34	32	34	35	8	12	24	33	35	31	35
$28.5	$9.5	35	12	12	20	35	35	34	34	35	12	12	24	34	35	34	30
$28.5	$19	35	12	12	20	35	31	34	35	35	12	12	24	35	33	35	34
$28.5	$28.5	35	12	12	20	33	35	33	30	35	12	12	24	35	33	30	35
$28.5	$38	35	12	12	20	35	34	31	33	35	12	12	24	35	33	35	35
$38	$0	35	12	12	20	34	33	35	35	35	12	12	24	35	35	35	34
$38	$9.5	35	12	12	20	34	32	33	33	35	12	12	24	34	34	35	33
$38	$19	35	12	12	20	32	28	30	33	35	12	12	24	35	35	35	33
$38	$28.5	35	12	12	20	34	33	31	35	35	16	16	24	30	32	32	32
$38	$38	35	12	12	20	35	31	34	32	35	16	16	24	34	35	32	32

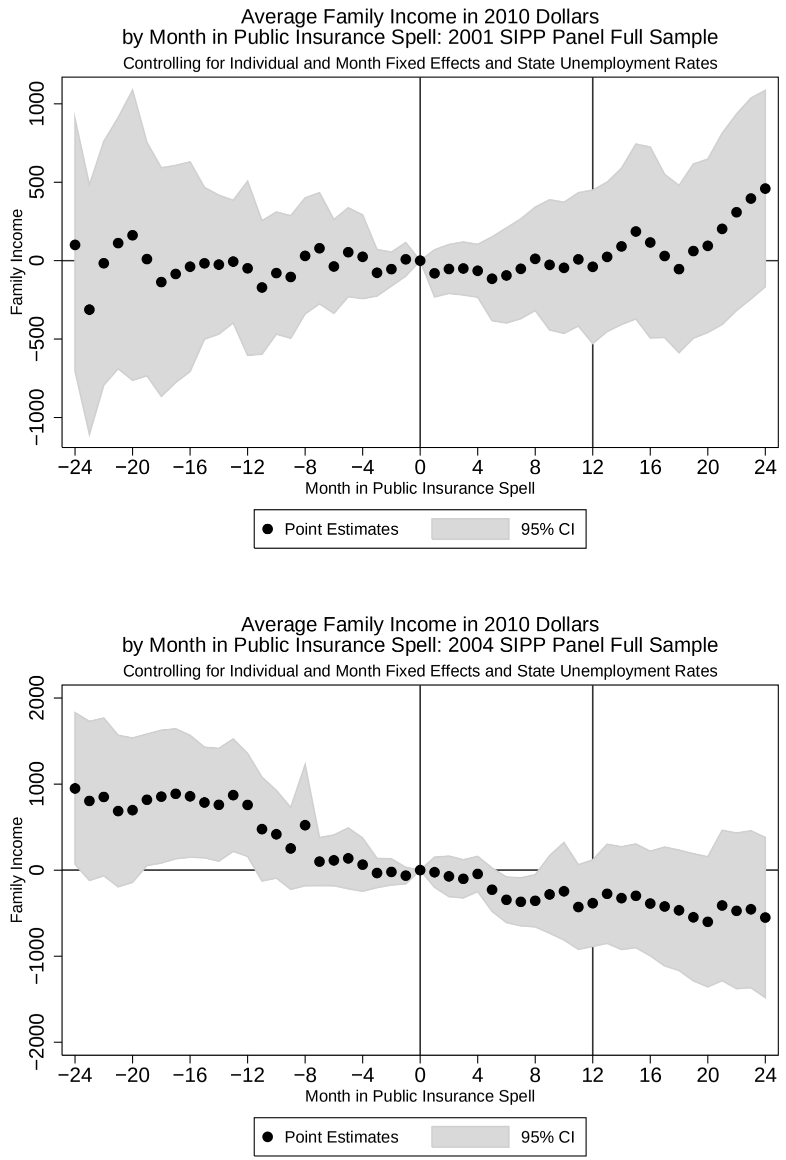
Notes: The optimal lengths of the continuous eligibility period, τ, are calculated based on the framework in section 7. The choice set of τ is $\{1,2,...,36\}$. The calculation is carried out for different recertification costs (in 2010 dollars), different values of the social welfare function parameter η and different assumptions governing take-up rates. The SIPP sample that serves the basis of the calculation consists of families who had appeared every month during the 2001 and 2004 panels.

Figure 1: Income-Consumption Choices in the Static and Dynamic Models



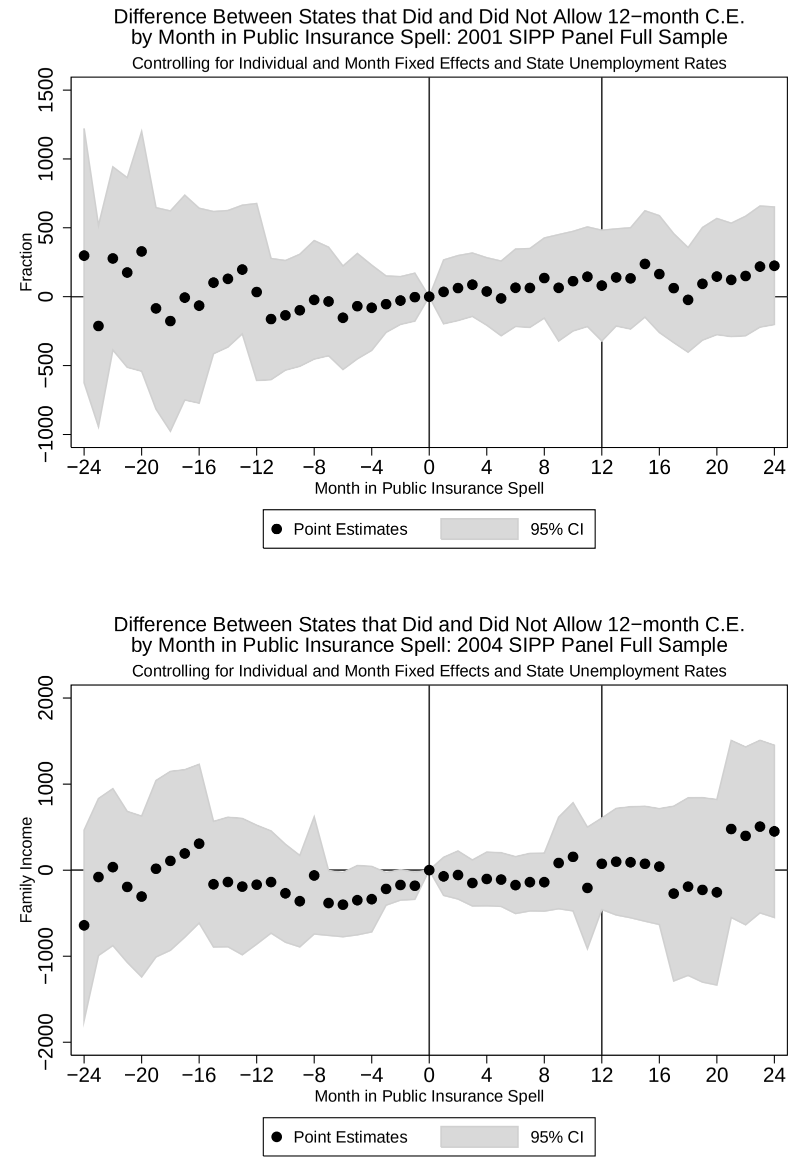
Notes: The left and right panels illustrate agents' income-consumption choices in the static (subsection 3.1) and dynamic model (subsection 3.2) respectively. The static model assumes that income eligibility is checked every period whereas it is checked every two periods in the dynamic model. In the case of the static model which is presented in the left panel, agents whose type is in the interval $[n_\gamma, \bar{n}]$ choose pre-tax income $Z = \gamma$. For the 2-period dynamic model which is presented in the right panel, agents whose type is in the interval $[n_\gamma, \bar{n}^{dynamic}]$ choose pre-tax income γ in period 0. The fact that eligibility is only checked once every two periods in effect increases the size of the benefit notch from g to $(1+\beta)g$.

Figure 2: Average Family Income by Month in Medicaid Spell: Full Sample



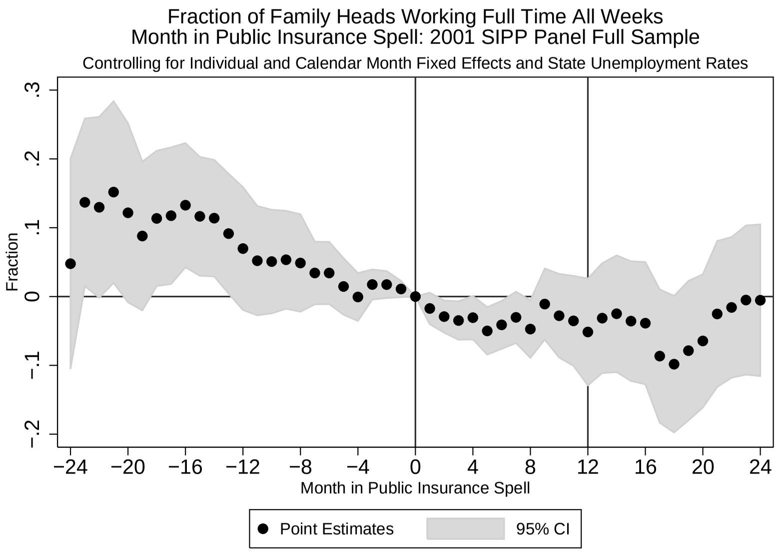
Notes: Plotted are the estimated coefficients and pointwise 95% confidence intervals from fixed effect regressions of monthly family income in 2010 dollars on a set of spell month indicators. Month 0 (the month right before the start of a fresh Medicaid/CHIP spell) is the omitted category, and the corresponding coefficient is 0 by construction.

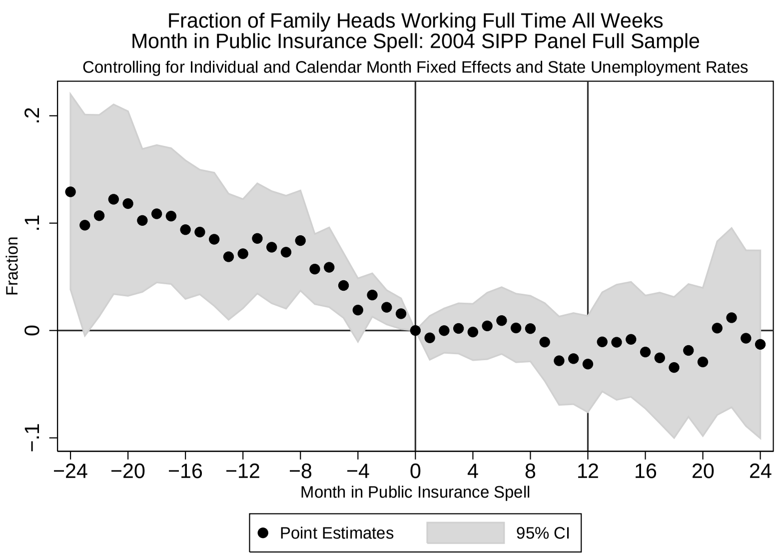
Figure 3: Difference in Average Income between States with and without 12-month Continuous Eligibility



Notes: Plotted are the point estimates and pointwise 95% confidence intervals of the differences in the income processes between children residing in states providing 12-month continuous eligibility and those who did not. Month 0 (the month right before the start of a fresh Medicaid/CHIP spell) is the omitted category, and the corresponding coefficient is 0 by construction.

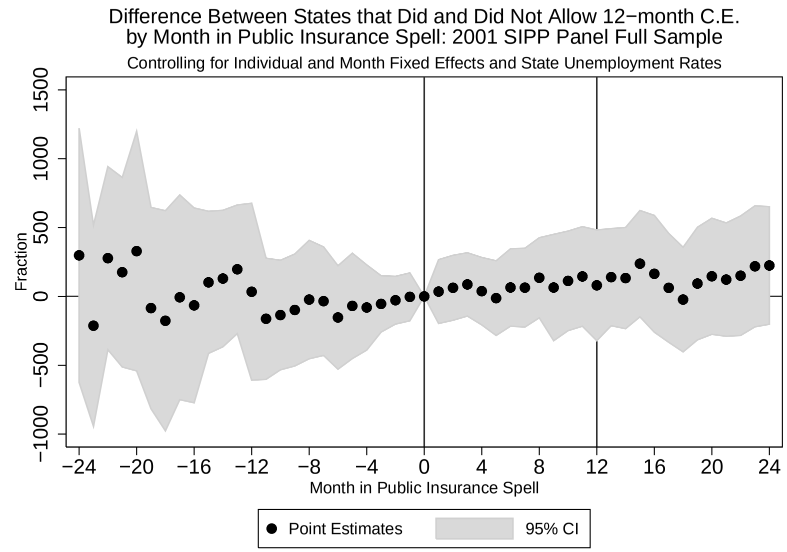
Figure A.1: Fraction of Family Head Working Full Time in Medicaid Spell: Full Sample

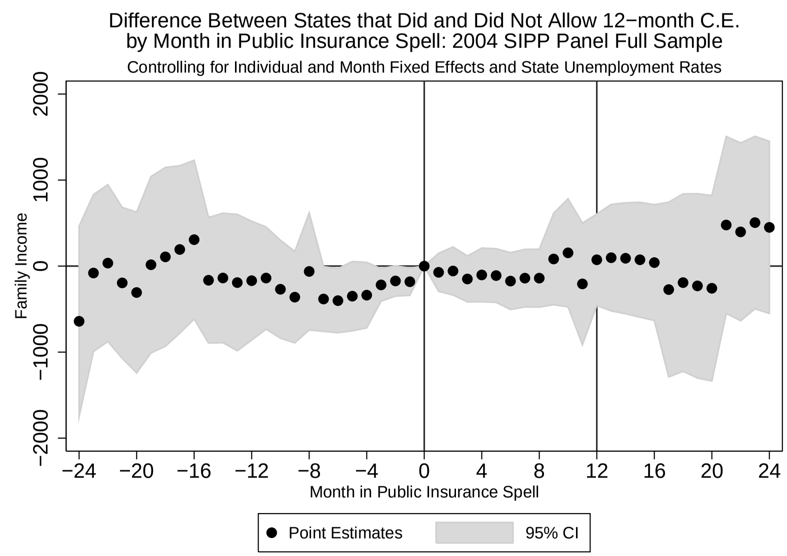




Notes: Plotted are the estimated coefficients and pointwise 95% confidence intervals from fixed effect regressions of whether the family head had worked full time on a set of spell month indicators. Month 0 (the month right before the start of a fresh Medicaid/CHIP spell) is the omitted category, and the corresponding coefficient is 0 by construction.

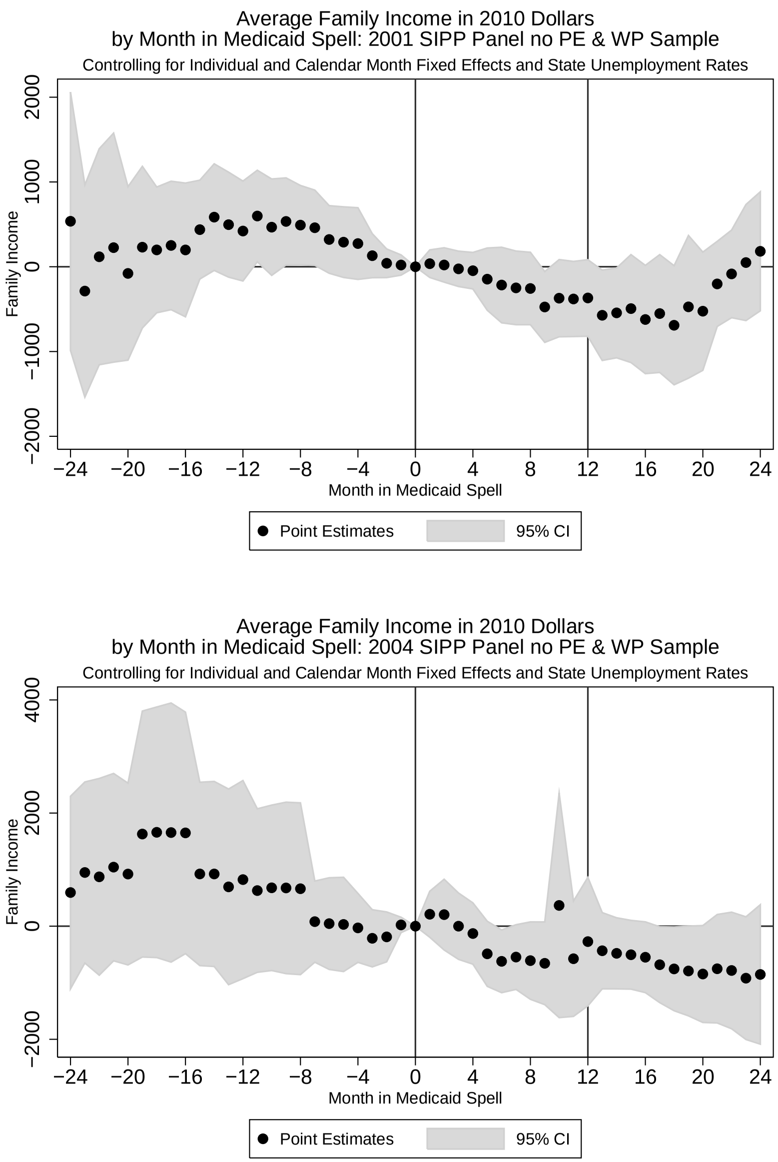
Figure A.2: Average Family Income by Month in Medicaid Spell: Long Gap Sample





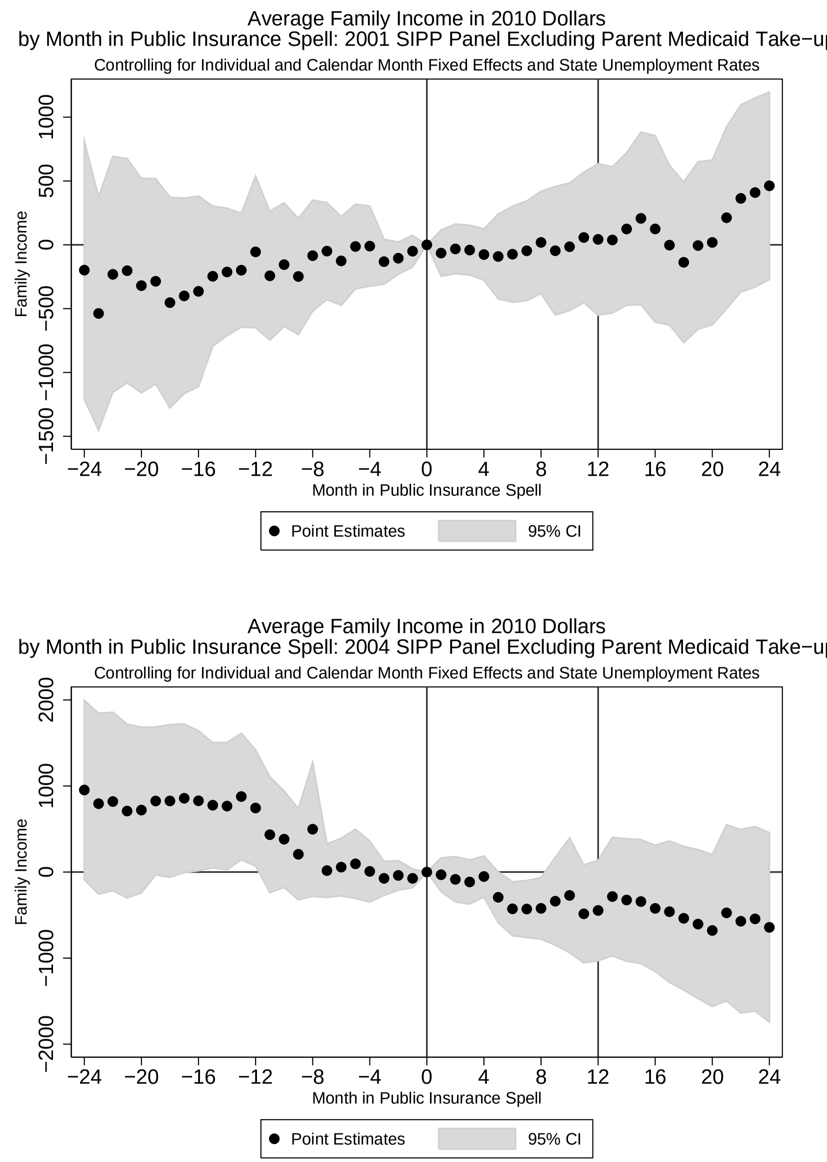
Notes: Plotted are the estimated coefficients and pointwise 95% confidence intervals from fixed effect regressions of monthly family income in 2010 dollars on a set of spell month indicators. Month 0 (the month right before the start of a fresh Medicaid/CHIP spell) is the omitted category, and the corresponding coefficient is 0 by construction. This subsample only includes children who were not covered by public insurance for 12 months before the start of a spell.

Figure A.3: Average Family Income by Month in Medicaid Spell: Subsample with No-Presumptive-Eligibility and No-Waiting-Period States



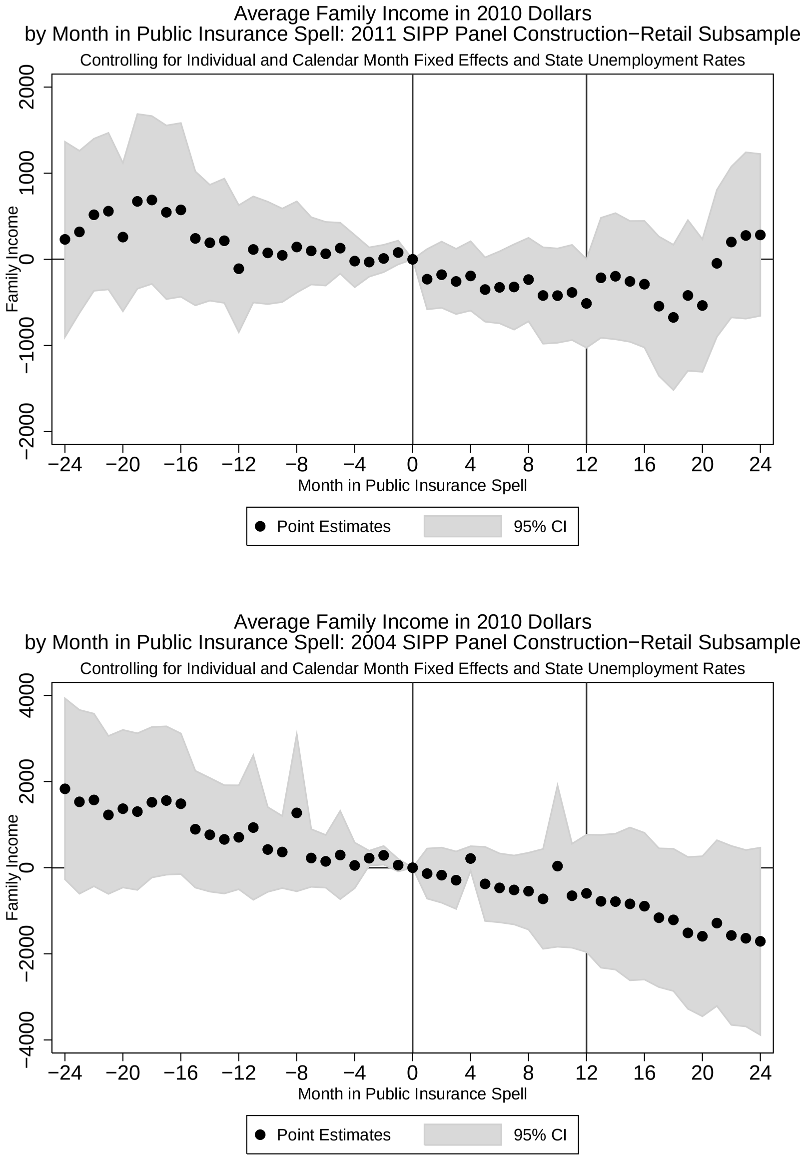
Notes: Plotted are the estimated coefficients and pointwise 95% confidence intervals from fixed effect regressions of monthly family income in 2010 dollars on a set of spell month indicators. Month 0 (the month right before the start of a fresh Medicaid/CHIP spell) is the omitted category, and the corresponding coefficient is 0 by construction. This subsample is restricted to children in states that provided no presumptive eligibility nor mandated a CHIP waiting period.

Figure A.4: Average Family Income by Month in Medicaid Spell: Subsample Excluding Children Beginning Public Insurance with Parent



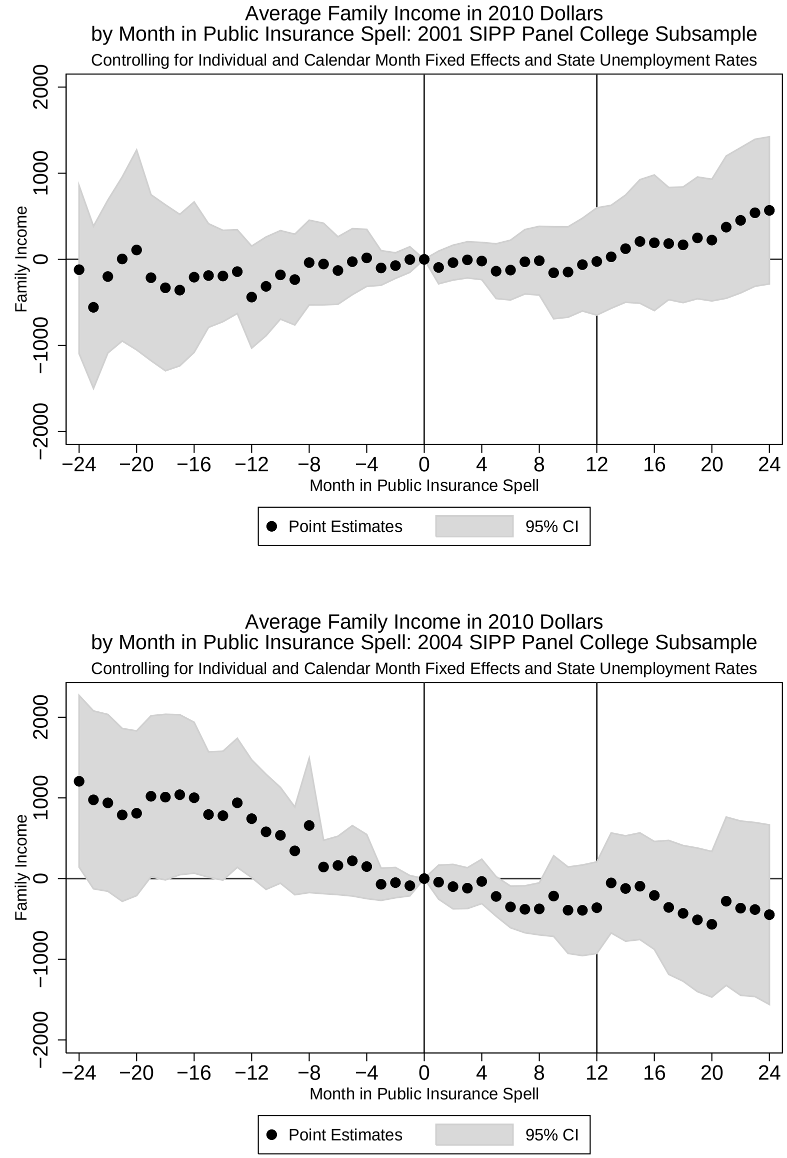
Notes: Plotted are the estimated coefficients and pointwise 95% confidence intervals from fixed effect regressions of monthly family income in 2010 dollars on a set of spell month indicators. Month 0 (the month right before the start of a fresh Medicaid/CHIP spell) is the omitted category, and the corresponding coefficient is 0 by construction. This subsample excludes children whose parents took up Medicaid when they began their public insurance spell.

Figure A.5: Average Family Income by Month in Medicaid Spell: Subsample with Two-parent Families



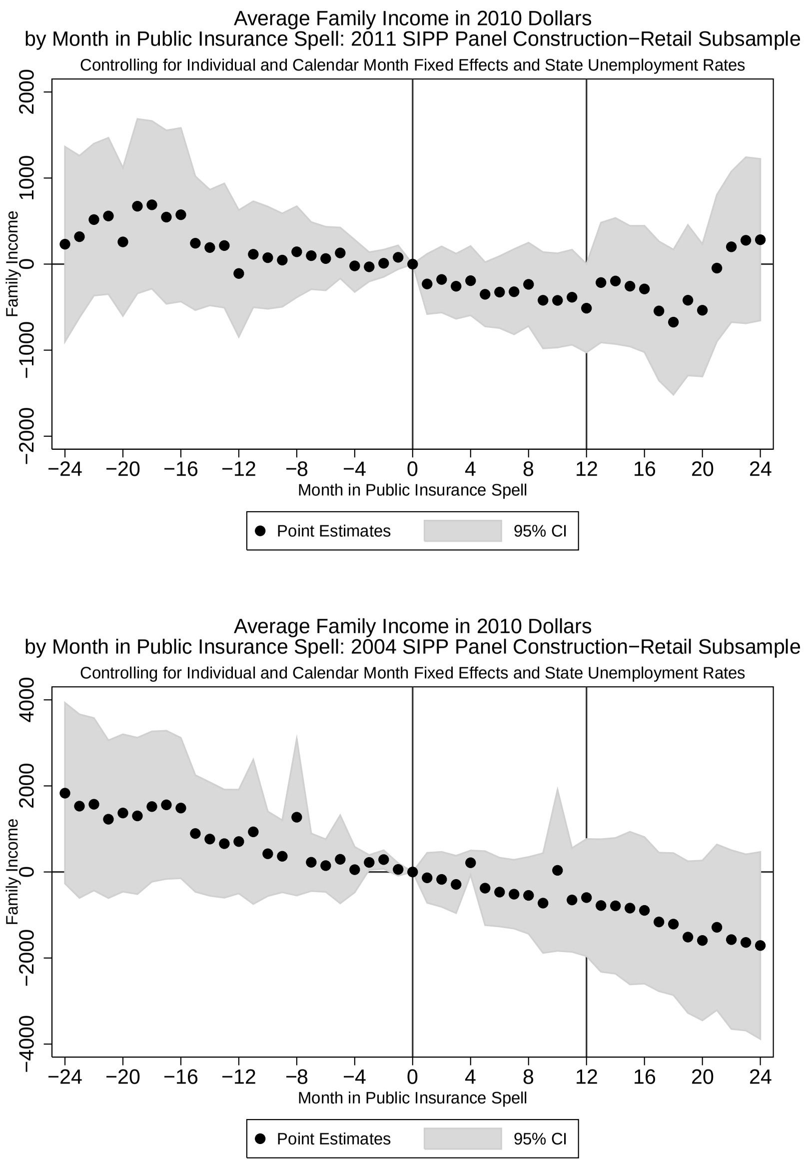
Notes: Plotted are the estimated coefficients and pointwise 95% confidence intervals from fixed effect regressions of monthly family income in 2010 dollars on a set of spell month indicators. Month 0 (the month right before the start of a fresh Medicaid/CHIP spell) is the omitted category, and the corresponding coefficient is 0 by construction. This subsample consists of children living in two-parent families.

Figure A.6: Average Family Income by Month in Medicaid Spell: Subsample with Parents in Construction or Retail Industries



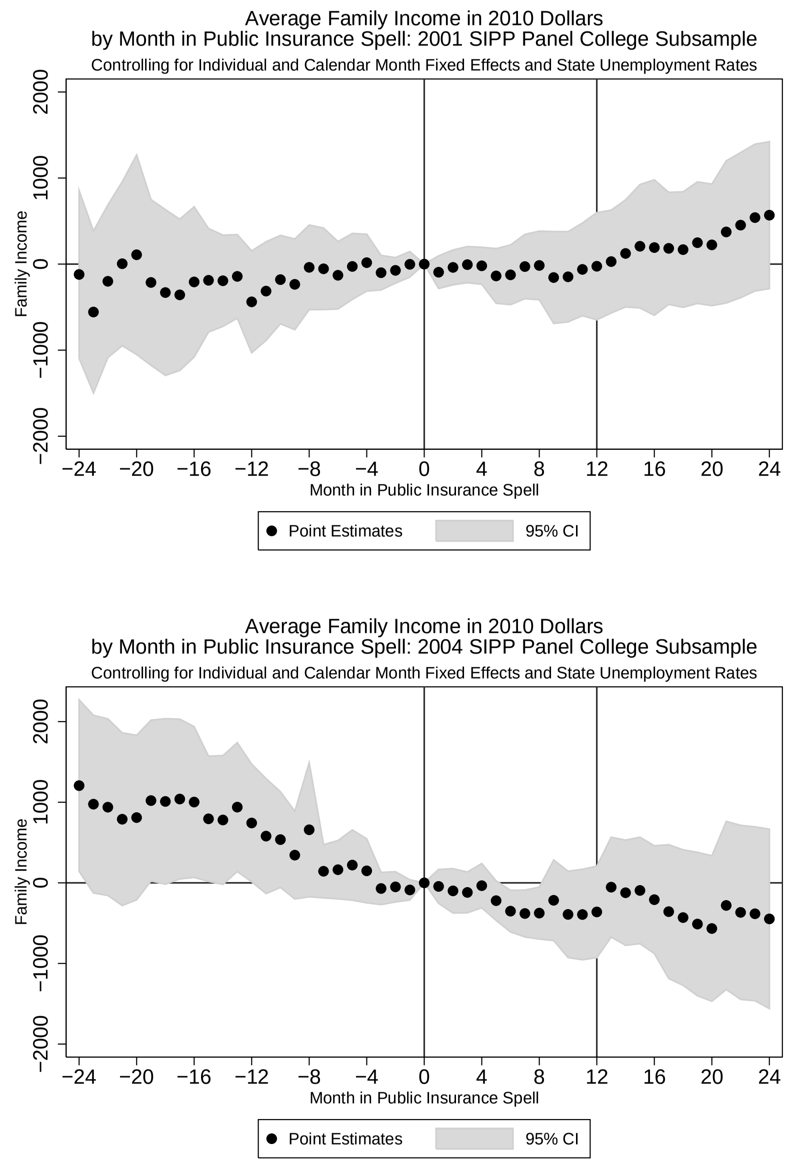
Notes: Plotted are the estimated coefficients and pointwise 95% confidence intervals from fixed effect regressions of monthly family income in 2010 dollars on a set of spell month indicators. Month 0 (the month right before the start of a fresh Medicaid/CHIP spell) is the omitted category, and the corresponding coefficient is 0 by construction. This subsample consists of children whose parents had worked in the construction or retail industries.

Figure A.7: Average Family Income by Month in Medicaid Spell: Subsample with College-educated Parent



Notes: Plotted are the estimated coefficients and pointwise 95% confidence intervals from fixed effect regressions of monthly family income in 2010 dollars on a set of spell month indicators. Month 0 (the month right before the start of a fresh Medicaid/CHIP spell) is the omitted category, and the corresponding coefficient is 0 by construction. This subsample consists of children with a college-educated parent.

Figure A.8: Average Family Income by Month in Medicaid Spell: Subsample of Families with More than Two Children



Notes: Plotted are the estimated coefficients and pointwise 95% confidence intervals from fixed effect regressions of monthly family income in 2010 dollars on a set of spell month indicators. Month 0 (the month right before the start of a fresh Medicaid/CHIP spell) is the omitted category, and the corresponding coefficient is 0 by construction. This subsample consists of children with more than one sibling.